Anthony Edwards

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Place of Birth	UK (London)
Main work places	Pavia, Cambridge
Principal field of work	Mathematical genetics
Short biography	See below

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Biography

Anthony William Fairbank Edwards (born 1935) is a British statistician, geneticist, and evolutionary biologist. He is a Life Fellow of Gonville and Caius College and retired Professor of Biometry at the University of Cambridge, and holds both the ScD and LittD degrees. A pupil of R.A. Fisher, he has written several books and numerous scientific papers. He is best known for his pioneering work, with L.L. Cavalli-Sforza, on quantitative methods of phylogenetic analysis, and for strongly advocating Fisher's concept of likelihood as the proper basis for statistical and scientific inference. He has also written extensively on the history of genetics and statistics, including an analysis of whether Mendel's results were "too good", and also on purely mathematical subjects, such as Venn diagrams. His elder brother John H. Edwards (1928–2007) was a geneticist.

He is also known for his involvement in gliding, particularly within the Cambridge University Gliding Club and for his writing on the subject in Sailplane and Gliding magazine as "the armchair pilot".

INTERVIEW WITH PROFESSOR ANTHONY EDWARDS, 10th DECEMBER, 2004

AE. It's Friday 10 December 2004 and I'm speaking to Professor Anthony Edwards at his home in Barton near Cambridge. So I've been asking everyone to start with, Anthony, how did you get interested in the first place in science and mathematics? Well, I can only tell that by anecdote I suppose. I can remember my prep school, which incidentally was near Barnstaple, Tawstock Court.

PSH. Was it?

AE. I can remember becoming interested in astronomy and already a political activist for science at the age of 10 or so, I remember leading a deputation to my headmaster asking if we could have just one period of science a week. And the answer was no there wasn't time in the syllabus for one period of science a week.

PSH. This was the old classical tradition was it?

AE. I suppose so. Just complete ignorance of science amongst the staff at the school. I was quite good at mathematics at that age, it faded a bit later but I was one of the best of the boys at mathematics and I suppose it was applied mathematics that interested me, but what originally started it I don't know. It might be supposed that with my elder brother John being a scientist and being seven and a half years older than me, he might have influenced me at that stage. But I don't think so because, due to parental illness and war time, we didn't see each other anyway. I really don't remember the existence of John until I was 7 or 8 or so.

PSH. Seven years between siblings is quite a large interval too, isn't it?

AE. Yes. He influenced me very much later but perhaps we'll get on to that.

PSH. Am I right then, you went to Cambridge as an undergraduate?

AE. I went to Cambridge as an undergraduate. We both, I should say, had a fantastic public school scientific education at Uppingham, which was really extremely good both mathematically and scientifically. I can't speak too highly of that and that influenced us both greatly.

PSH. So did you feel you were really getting the fundamentals of science and maths then, by the time you'd completed your school days?

AE. Oh yes, I have earned my living ever since school on what I was taught at school basically, and the little I have learnt since, the education was so good. I came up to Cambridge originally as an engineer and that arose because in those days the engineering tripos was called the mechanical sciences tripos and I actually wanted to read the natural sciences tripos, and nobody at school knew the difference, so I had passed a thing called the mechanical sciences qualifying exam and so it was assumed I wanted to read engineering, but Cambridge is a flexible place so I simply said to my tutor "Oh no, that's a terrible mistake, I want to do the physical sciences". So he said "Oh well, you go to Dr Smith instead of Dr Jones. It's quite straightforward". So I read natural sciences but I read physical sciences first.

PSH. When did you start getting involved in any biological sciences then?

AE. Well in a sense I have never been involved in biological sciences. I was for a time, I had the title of Reader in Mathematical Biology and I used to boast that I had become Reader in Mathematical Biology knowing no mathematics and less biology. But it was strictly the interest in genetics and the interest in R A Fisher and being influenced by him.

PSH. So your part one, then, was entirely the physical sciences?

AE. Yes it was, but that wasn't wholly my own fault because I wanted to read invertebrate zoology as a half subject. I must have had these biological leanings even as a physicist. I wanted to read that half subject and I was persuaded not to do it, because the examination time-table meant that it was extremely difficult if a student wanted to do half subject metallurgy and half subject invertebrate zoology. On reflection it seems that's rather a good combination, metallurgy and invertebrate zoology, but Cambridge wouldn't allow it, so by the time I'd finished my first two years I had done no biology.

PSH. And then for your part two then, what did that . . .

AE. My part two was genetics.

PSH. I see. I'm very ignorant of Cambridge structures I'm afraid, partly because I went to Oxford which was no clearer. So by doing genetics, did that involve doing quite a lot of zoology or was it very much just genetics?

AE. No, it was a strange opportunity which suddenly opened up. This was the last year in which Fisher was Professor of Genetics and his background of course was mathematical and physical, although he had done a lot of natural history in his time, and I can't now quite remember what the original influence was, but it could have been my brother John. Oddly enough not suggesting that I should do genetics, but suggesting I should learn some statistics, because as a mathematician who was turning out not to be good enough to be a real mathematician, statistics is a very good opening, and somebody said, and probably he said, well if you become a statistician you will always be able to earn your living. It's a sensible thing to do, which I think is as true today as it was then, but because I hadn't done mathematics it wasn't possible to do any statistics in the mathematical tripos. So I was rather at a loose end. I was thinking perhaps of having done part one, that is the two year course in the natural sciences, of going back to the beginning as it were and doing the one year mathematics course, which I knew I could cope with,

with a view to becoming a school master. But somebody then said, it's a terrible waste of Cambridge if you do two years in science, not to do a third year science and there were all those options, so-called part two options, so I simply went and bought a copy of the lecture list and I thumbed through it and I came across this subject 'genetics' which I thought might be quite interesting, and so I looked at the lectures that were proposed for genetics and I found that some of them were given by Sir Ronald Fisher and I thought that was very strange because I'd read the book "Statistical Methods for Research Workers" by Fisher and in my schoolboy ignorance I just assumed that he was a contemporary of Isaac Newton. I knew he was the father, or somebody of that name was the father of modern statistics, but I had no idea that such a person could still be alive and in Cambridge and teaching genetics. What was all this about? Am I reminiscing too much?

PSH. No, not at all. It's fascinating, it really is.

AE. Well the Reporter for the lecture list, it had at the bottom a little footnote which said: students intending to read part two genetics or hoping to read part two genetics should consult either Dr Wallace or Dr Owen at the genetics laboratory before the division of the Easter term. So I just got on my bicycle and I went to the genetics department, which in those days was in the professor's private house, 44 Storey's Way, which Fisher had filled with his mouse stocks. So I cycled up there, the front door was open, I went in. The next door was open too, a little room and there was a small man sitting, with a beard and pebble glasses and I thought, well I had better ask him if I could find Dr Wallace or Dr Owen, clutching my University Reporter. And so I went and asked him you know. I thought he was the hall porter or something like that; he said "Oh. I'll do. I'm Professor Fisher". So I explained that I had read this invitation to come and consult and I was a little bit interested in genetics and he said "Well let's have a talk about it. Come out into the garden". So we went out into the garden and in those days, the garden of the department was laid out in some replicates of some of Mendel's experiments, as well as there were Primula being worked on, there was *Lythrum salicaria* and there were strawberries for tea and it was all nicely organised. And so we walked up and down the experimental plots really for a very long time. I think Fisher was very lonely and he talked and he talked and I never said anything, and it was wonderfully interesting and at the end of the afternoon he said "Well that was very interesting"; he said, "Come back tomorrow afternoon and we will continue with the conversation". Well I couldn't wait to get back the next afternoon so the conversation was continued, and I only had a third class examination result to my credit at that stage, because I really hadn't fitted with physics, but nevertheless he agreed to take me. So I was the only student he took in his last year.

PSH. Which year was that?

AE. This was 1956. Yes, summer of '56. So what prompted that story was that you were asking me about biology. Well in the event, Fisher said, you had better come up in the middle of the long vacation to look at the flowers, as he put it, ready for the genetics course, because although it's fairly mathematical it does have bits of biology in it. So I turned up in the middle of the long vacation in July or some time, and there was Fisher, apparently all by himself again, and he looked at me and he said “Who are you?” So I reminded him who I was and that he had told me to come up and look at the flowers, whereupon he reached under his table, opened a drawer and he pulled out a big buff offprint. I had never seen an offprint in my life before, a big buff offprint. “Philosophical Transactions of the Royal Society. The theory of linkage in polysomic inheritance” and he gave it to me and he said “Study that. When you’ve understood that, come back”. Well he must have been doing, it was very naughty of him, he must have been doing it quite deliberately, because it really is the most horrendously complicated piece of linkage mathematics involving partitional functions and bi-partitions and so forth. Anyway I struggled through it and I came back at the end of the week and I took off from there.

PSH. So you could say then that linkage was sort of built into your very first steps in the genetic field.

AE. Yes it was, and the first book I reviewed was Norman Bailey’s book on an *Introduction to the Theory of Genetic Linkage*.

PSH. It’s amazing how utterly fundamental linkage is in just about all aspects of genetics.

AE. Yes, it’s a shame in a way that I haven’t worked on linkage, but it has been partly accidental and partly deliberate. Accidental in the sense that I was hired by Luca Cavalli-Sforza to go and work in Pavia in his genetics group there and he was interested in creating evolutionary trees and that kind of thing, and record linking and in human population genetics generally, but it happened that he wasn’t interested in linkage and I don’t think he has ever made any contribution to linkage. So I was set off in my career in not doing linkage anyway. But of course right from the beginning I knew the linkage fraternity. I knew Cedric Smith and I knew Jim Renwick. I knew of course John Edwards and I knew all the people who were interesting themselves in linkage. The second reason is the negative one, in that as John became very interested in linkage and started working in it, I tended deliberately not to get involved in that field.

PSH. I think that’s a very natural thing.

AE. But it has meant that I have been an observer of the linkage field, right, as you see, from the theory of linkage in polysomic inheritance when I was not yet admitted as a genetics student.

PSH. Can you tell me, the part two in genetics, how much of that was what you might call didactic and how much was based on research and project work? How did it function?

AE. It was wholly didactic. The virus of project work had not got into undergraduate education so that we didn’t waste our time messing around with little projects and then having problems writing them up against deadlines and so forth, which you see today’s students sometimes almost totally consumed with anxiety about these things, and of course for them it depends so much on what kind of project it is, who suggested it, how much support they get and so forth. No no, we were just taught. We were taught extremely well. The convention in the department was that all members of the department, including Fisher, would attend all lectures, so we would all troop down from 44 Storey's Way down into the middle of town and attend the lectures given by Margaret Wallace, by George Owen and by various people from outside the department whom Fisher got to lecture. I can remember particularly Colin Campbell who is still around in Cambridge, R C Campbell, statistician. He used to teach us biometrical genetics and it was really very funny, because Colin would stand in the lecture theatre and Fisher being very short-sighted would sit in the front row with his notebook and would take notes, and Campbell's way of lecturing on biometrical genetics was to read, basically to read to us Kenneth Mather's book, *Biometrical Genetics*. Well of course he knew and we knew that biometrical genetics, written by Kenneth Mather, was essentially written by Mather taking down notes from Fisher. So here we had this complete circularity like that children's game telling each other stories.

PSH. I think I'm right that the actual department was very small, wasn't it?

AE. It was extremely small. It consisted of something like half a dozen research students. There were actually 2 undergraduates because, although I was the only one admitted that year, David Jones, who later became Professor of Genetics in Hull before he was made redundant I think, he had been admitted the previous year to do a two year part two because he was a bit weak on the mathematical side. No weaker than I was on the biological side, but it was a pretty mathematical part two in those days. Though Fisher was extremely good at making sure we got the best education that we could get outside his department. For example he had good relations with the MRC Laboratory of Molecular Biology, which I will come to in a moment. He despatched me to Rob Race and Ruth Sanger's lab in London to learn about blood groups. He despatched me to Arthur Mourant's place at the Lister to learn what you do with blood group frequencies. He despatched me to Kenneth Mather's place in Birmingham to look at the chromosomes. So there was no nonsense about us not getting the education provided actually in Cambridge. Very, very good.

PSH. So at that stage the Laboratory for Molecular Biology must have still been in the centre of Cambridge rather than in the new Addenbrooke's building?

AE. Well now I would have to think rather carefully, and of course we could look at the dates. It could be that, yes I think perhaps I am jumping by a year, because my memory of the people from the MRC lab must have been one year later, when I was in my first year of research and the memory is this, that I don't think I had a room to myself as a research student. I think I was in a downstairs room. I remember a very gloomy room, but perhaps this is where we were made to put out our demonstrations. That's right, there was going to be a visit from the MRC so in one of the laboratory rooms downstairs, which was converted from a kitchen or something, we put out our demonstrations. Well of course I only had statistics to demonstrate, so I was interested in the statistics of the human sex ratio and I had been working on this all by myself for a while with nobody much to help me and so I put out some sort of display, some charts and graphs and so on. But everybody else had long-tailed mice or pied mice or agouti mice or something to exhibit and I daresay there were people exhibiting primula, Walter Bodmer was probably exhibiting pots of primula with long styles and short styles and so forth. Anyway I didn't expect anybody to take much notice of what I was doing and this file of about 10 people from the MRC came past and nobody said anything to me. We were standing by our exhibits of course, until the last chap and he became very interested in what I was doing, and that was Francis Crick.

PSH. That's interesting.

AE. Typically, asking all the questions, lively mind, interested in everything.

PSH. Did you have links with any of the others in that unit then, people like Max Perutz, or were they more . . .?

AE. No. I'm very sad that I never actually met Max Perutz. I read a lot of

PSH. His essays?

AE. No as a matter of fact. I have read a lot and to try and remedy this a couple of years ago I went along to a lecture of his to find that he had been taken ill and couldn't give it. So I never actually met Max Perutz.

PSH. When you finished your Part II undergraduate degree, what was the next stage?

AE. I got an MRC scholarship, as they were then called, to do a 3 year PhD with Fisher

PSH. So Fisher was still there at that point?

AE. Yes, what was very happy, exactly for my generation, is that he retired from being head of department and being Professor in September '57 after I graduated. I then became a research student but he stayed around for another 18 months with no sort of administrative responsibility and so he kept his room, which is the room I described earlier, just by the front door and that's where the department met for tea every day, so every day, and Fisher wasn't travelling very much at that time, the half a dozen of us or so, perhaps eight to ten, would have tea with Fisher and I'm happy to say that the conversation was nearly always academic. It was a marvellous intellectual environment. So we were asking questions, it was just wonderful to have this man around. I now know that at that time he was pretty isolated both academically and personally, so he was a lonely man. He had good company in Caius College. He was President of Caius and I didn't see that side of him but I'm sure he had good company there. But in his department and in his intellectual life he was quite isolated. The Department of Genetics in Cambridge was often poked fun at by other departments, because it was said not to have a microscope, for example. It was all calculating machines. Well this was very hard on the man who had hired Luca Cavalli-Sforza to start bacterial genetics in Cambridge, who had tried to keep the blood group work in Cambridge after the war instead of letting it go back to London. It had been evacuated to Cambridge during the war which is how of course Fisher and Rob Race got together and sorted out the rhesus problem. And Fisher had bad relations with the administrators in the University, so eventually the bacterial genetics initiative faded. Luca Cavalli left to go back to Italy, Rob Race and co went back to London, so it wasn't for want of trying.

PSH. That's sad in a way and, as an outsider reading or hearing about things, I often get the feeling there have been a series of major areas where people in Cambridge wanted to make some major development, but then the authorities didn't see the need or didn't want to.

AE. Well, this is coming quite close to home actually, because I have just found some papers in the course of looking out some material for you today, which I thought I no longer had, and I haven't seen them for about 10 or 15 years, and the first one is dated 1971 and it's my paper to the University allowing me to apply to the Medical Research Council to start human genetics in Cambridge. That failed. The second is a paper from 1975 arguing that the Clinical School, which was to be founded from January 1st 1975, ought to have some human genetics in it; and that failed.

PSH. Yes, well there we are. To come back though, to your research student time, what was the actual project then, because going through lists of papers, like one gets on PubMed, I found a record of the two first publications I could find were on sex ratio and on multipoint linkage, but then it could be that you published in other journals which aren't picked up by PubMed.

AE. My first publication was actually on astronomy, but that's another story.

PSH. That wouldn't have been picked up.

AE. Well, you may well ask what I was supposed to be doing. I was actually supposed to be running a mouse experiment which had been running for many years in the department, which was a complicated system of mating which Fisher had introduced with Sarah Holt, even before he came to Cambridge in 1943. Designed to show that you can modify the expression of a gene by introducing modifier genes or presumed modifier genes from outside stocks into a relatively in-bred stock in such a way that it modifies the expression of a gene. Now the particular gene we were studying will stand for short tail, and that's quite an interesting story in itself. But I had already as a part II student become very interested in the distribution of the human sex ratio in families.

The reason I became interested in that is that, when I attended Fisher's first linkage lecture, Fisher lectured to me on linkage you see, not I am sorry to say, human linkage, just linkage in general and I have the notes here from those days. But at the end of the first lecture in which he had introduced the concept of likelihood and various statistical ideas which I didn't know about, I went to him and I said: I don't know any statistics, which wasn't quite true because I had been to one course for physicists, but I asked him what I should do about it to remedy this deficiency, and he said "Oh well I have written one or two books on statistics myself." So I bought Statistical Methods for Research Workers and I sat down to read it, and when you get to Chapter 3 or 4, there is the binomial distribution which he fits to Geissler's data on the human sex ratio for families of size eight. And he observes that the excess variance of the actual distribution over the binomial might be due to the fact that the probability of the birth being male varies between families, and I thought that's a good idea. And from that moment on I was interested in fitting distributions to such data and getting hold of the original data and so forth even before I became a research student.

Well I found Danforth's short tail rather boring by comparison with data on the human sex ratio, which in the course of which I was studying, and I was learning statistics of course, I found this very interesting. So I got less and less interested in my mouse experiment, and it's fair to criticise my supervisor too. George Owen, A R G Owen, mathematician, University Lecturer, very talented, very knowledgeable, extremely lazy, so he never did anything with his so-called research students at all. It wouldn't do these days but then, you see, like so many things have happened these days, if the climate for research students had been what it is now I would never have set off on a statistical career. You know the modern climate can't cope with people who go off at tangents. So before very long I decided I was going to do this study of the human sex ratio for my PhD, and I don't think I ever asked anybody about it, I just did it. I produced a thesis and in the course of that of course I learned quite a lot about statistics, and that is how I set off writing about statistical things. I never did write up my mouse work, though funnily enough the experiment was continued and a chap called Dick Morton wrote it up sort of collectively for all of us about 20 years later.

PSH. Tell me a bit about some of the other people who were with Fisher as undergraduates around that time, because it was a very interesting lot of folk.

AE. When the Fisher centenary occurred in 1990 I got in touch with everybody and we organised a big dinner in Caius College and put up a memorial window to him and generally made a fuss. So that was quite fun getting in touch with everybody. What I found was that there were very few formal Part II students who had taken the genetics course. Something like 2 a year or 3 a year or so, and so the influence on the whole was not through the part II course. George Fraser was one of them, an early one, and most of those whom I am still in touch with were people who had been research students. People like Walter Bodmer. Walter did Part III mathematics and then came in as a mathematician not having done the genetics course at all. Walter's career you know about. Peter Parsons, Australian, he became Professor at La Trobe, and then I think Melbourne. But the influence is really rather different; for example, Henry Bennett, who became Professor of Genetics in Adelaide and has done so much of Fisher scholarship, he was employed by Fisher, as a University Lecturer I think. These are things to check of course. He may have been a research student, he was probably a research student before. Mary Lyon was a research student of Fisher.

PSH. Yes. I have been to see her.

AE. Have you? That must have been very interesting.

PSH. Yes it was. Interestingly I went to see her shortly after seeing your brother John and his comment was “Oh well that won’t take you long”.

AE. She is a bit taciturn.

PSH. But in fact she opened up a lot. It was very interesting.

AE. That was a very good thing to have done. The famous statistician C R Rao was technically a research student of Fisher’s and I think his thesis had some sort of, I think it had the word linkage in its title.

PSH. Another person who I am due to see soon, who I think was with Fisher, is Sam Berry.

AE. Sam Berry, he was a Part II student also. He must have been a couple of years before me but he left after doing his Part II. He may have known Fisher through Caius as well. He was a Caius student.

PSH. One of the things which you have already mentioned was Fisher’s difficulty in really getting the university to back the experimental side, but I mean, he did have this big mouse colony didn’t he. And so the image of Fisher just being a statistical person must have been backed by solid biological mouse-type work.

AE. Very much so and I can give you an offprint, which is of a talk which I give to statisticians, when they ask when they discover that Fisher was a Professor of Genetics and never a Professor of Statistics, which describes some of his, most, I hope, of his genetical work. He was handicapped by his eyesight of course, but he had this great interest in mouse genetics, and interest in creating the linkage map of the mouse, and it would be interesting to talk to people who know about linkage in the mouse now, or know about the mouse genome, to see how much of that work is still remembered, if any of it; but linkage in the mouse was very much Fisher’s priority and the design of the experiments, these complex multipoint experiments which would lead to efficient destination of linkage, that was what was going on in the department on the mouse side. And Fisher had been working with mice all the time he was at, not all the time but latterly, at Rothamsted while he was the statistician at Rothamsted experimental station. The whole story is given in Joan Box’s book. These were mice that somebody gave the children and he got interested in them. That work carried on all the way through his University College time as well and then he was interested in Primula. He was interested in self sterility alleles and he was interested in the plant *Lythrum salicaria* and that I think is a tetrasomic plant, but I think he and Mather managed to work out that it was tetrasomic and not hexasomic simply by studying the segregation ratios, which is an incredible piece of work which I don’t suppose anybody ever reads today.

PSH. Was that before anyone had looked at its chromosomes?

AE. Oh yes. Remember we are talking about almost the pre chromosome, not as far as plants were concerned, but certainly as far as man was concerned.

PSH. The *Lythrum* work interests me because it goes right back to Darwin, doesn't it?

AE. Fisher was the best read of his generation of statisticians, so things like self sterility alleles and the work on *Primula*, *Lythrum salicaria* and a lot of his theoretical ideas go back to Darwin. One of the things I am trying to do, and you can see six volumes already collected over there, in the light green bindings, is to collect together the thirteen Volumes of the John Murray edition of Darwin's works which Fisher was given at his request as a schoolboy at Harrow as a mathematics prize. He opted for the thirteen volumes of Charles Darwin's work, and he read them. So anybody who studies Fisher has to have precisely the John Murray edition to know what it was that Fisher's inspiration came from, and it was hugely from Darwin.

PSH. When you had finished your research studentship, what was your next move then?

AE. Well, I had to earn a living, with a wife and two young children, this was a difficult prospect. But I saw advertised a Darwin Research Fellowship of the Eugenics Society. What I didn't know was that these research studentships at the Eugenics Society were not after Charles Darwin, they were after Leonard Darwin, and I didn't know until many many many years later that they had been invented by Fisher himself in honour of Leonard Darwin, when Fisher was active in the Eugenics Society. So not surprisingly Fisher must have been quite keen that I should get one of these things, so I don't know if he pulled any strings, but anyway I became Darwin Research Fellow of the Eugenics Society, with a pretty crazy collection of things I was going to study and which if I had stayed on for more than the year which I did in that capacity, would probably have been totally disastrous. But anyway it appealed to the people in the Eugenics Society, but fortunately after one year Luca Cavalli-Sforza, who had been lecturer in Fisher's department, in fact the first appointment that Fisher ever made in the university department was Luca Cavalli-Sforza. He just happened to bump into him at the Human Genetics Congress in Stockholm, found that he was a bacteriologist and hired him on the spot.

PSH. That's amazing.

AE. Anyway Luca was well set up in Pavia by then, and he simply came talent-spotting, and he wanted to hire somebody who knew his statistics and if he came from Fisher's department and been taught by Fisher so much the better, so without much ado he offered to double my salary and I thought well, that will enable me to feed the children a bit better, so off we went.

PSH. So how long were you in Pavia?

AE. For three years, and I only left because I had come to the crunch point, were my children, were going to be, we had 3 by then, were going to be Italians or were they going to be brought up in an English-speaking country? Otherwise I might well have stayed for a considerable length of time.

PSH. Can I ask, at that point was Marco Fraccaro back in Pavia or was he still in Sweden?

AE. He was still in Sweden. Now there is a Swedish side to this story too. My wife Catharina is Swedish and of course I had incentives to go across to Sweden, and in particular I thought it would be fun to go to Uppsala. There were good reasons for doing that because there was data on the human sex ratio in what was then known as Statensrasbiologi Institutet, The State Institute for Race Biology it was still called, and that was headed by the great Gunnar Dahlberg, so I applied for some money to our Scandinavian studies fund here in Cambridge to go and spend a month with Dahlberg. What I didn't know was that Dahlberg was dead.

PSH. Yes, because he died in about 1950 was it?

AE. Well I don't know when he died. As a matter of fact I have been meaning to look it up to find precisely when he died. But certainly nobody I was in touch with knew that Dahlberg was dead when I tried to go there in 1957. Well maybe they did, but they didn't tell me and perhaps the question had never arisen. I was applying to go to the Institute, it was then headed by Jan Böök. So off I went to Uppsala and there I found Marco Fraccaro, whom I'd never met before, and he was doing very interesting things with chromosomes. So I watched the whole of the chromosome development, we are talking about 1958, which of course is a critical year in human chromosome development anyway, through Marco Fraccaro and my brother John, so I watched that side of human genetics developing too and I did my fair share of cutting up karyotypes and trying to stick them all together in the right way. Anyway Marco had some data which he was analysing on the human sex ratio but he had got into a bother with his Chi-squareds, he didn't know about degrees of freedom and he asked me to help him and eventually we produced a joint paper, a very early one. There's Fraccaro and Edwards I think it is, I forget what it is.

PSH. Yes, I came across it.

AE. It exists, and that was part of my sex ratio studies and Marco and I have been friends ever since. So he came back whilst I was in Italy, I think; he came back, not to Pavia but to Istria because that was where the Euratom laboratories were, and I think you will find he first went there with Euratom and then managed to persuade them to set up a unit in Pavia.

PSH. Yes indeed. I have interviewed Marco and he told me about that and one of the very interesting themes which keeps coming up is, how much of the early work on human genetics was based on the whole radiation and atomic energy situation.

AE. That is true isn't it? It's true of Luca Cavalli of course, that was atomic energy authority stuff.

PSH. And all the Harwell stuff. It's something that really needs a proper historical study in its own right.

AE. And then the whole of Jim Neel's stuff of course, too.

PSH. Yes. So just before we come back to Pavia, would I be right in saying that you met your wife when you were up in Uppsala, or was that quite separate?

AE. No no no no. She joined the University gliding club here. She came to see her sister, who was already living in Cambridge and married, and they were both glider pilots, and my older brother was a glider pilot, John was a glider pilot, so we are a gliding family.

PSH. Now then in Pavia, I get a little confused with the different parts of the University set-up in Pavia. Luca Cavalli-Sforza's chair, was that in genetics or was it in something more general?

AE. You will really have to ask him. The unit that he was running, in which I was employed, was technically an outstation of the International laboratory of genetics and biophysics in Naples, run by Buzzatti-Traverso, so that's where the money came from proximately. Their money I think came from the Italian National Research Council, and probably from the Atomic Energy Authority in the United States as well. Well of course, everybody in Italy is a professor anyway, so the precise connection of Luca with the professoriat in Pavia is not known to me. Presumably he was professor in the department. My goodness me, there must have been a sense towards the end of my time there that he was the head of the department. He certainly behaved as though he was. But you see there are all sorts of people around who might have been performing that function. I just don't know.

PSH. So Marco Fraccaro came back to Pavia while you were in Italy?

AE. I think he must have actually got back to Pavia, because our two wives are friendly, because Marco's wife is Swedish too, and I don't see how that would have happened if there hadn't been that overlap.

PSH. So during those 3 years, what were the main areas of work you were concentrating on?

AE. Oh, I was wholly concentrating on phylogenetic trees. This was the great explosion in methods of constructing phylogenetic trees. When I got to Pavia the promised computer had not turned up, as is the way with promised computers, so I tended to fiddle around for most of the year, but I was learning Italian and so forth, so it was quite a useful time and continuing the tradition that we'd had in Fisher's department where PhD students didn't have one big project they worked on, they were encouraged to do lots of little things, so you will see from my early publications that there are all sorts of odd bits and pieces which have come out, which were peripheral to my main PhD subject and that was how we were taught. It was a very good education. So I simply carried on, I mean I was writing papers on number theory and so on when I first got to Pavia, because there was no computer. Once the computer arrived, Luca Cavalli had had this idea that one should be able to reconstruct human evolution from gene frequency data of human polymorphisms, principally, in those days, blood groups of course. Now in a sense it wasn't original because people like W C Boyd and indeed Arthur Mourant had realised that there was a lot of ancestral information in the blood groups, but Luca was much more focused on this. He thought that if we could extract information from gene frequencies by postulating an evolutionary tree of descent, that although that would be a very crude approximation to reality, it might provide the statistical basis for doing the computations, and I tried to persuade him that this was not going to work, because it seemed to me that there was not enough information, and that was probably true at the time, to do the complex calculations that would be necessary, and the analogy I used was linkage. I said, Luca, what you are asking me to try to do is to solve the linkage problem of the associations between characters when you are offering me, not a linear chromosome, but a multi-branching tree-like chromosome whose structure you don't know. Now we can't solve the linkage problem yet, how on earth do you expect me to solve for you the multi-branching chromosome problem when I don't even know how many branches? So that was my analogy, but he was very persuasive and of course he was my boss, so we got going and I started writing the programmes and it turns out in retrospect that those are the original phylogenetic tree programmes.

PSH. Can I ask, and I'm really very ignorant on this, were phylogenetic trees, at that point, already well established for other species and experimental organisms?

AE. Well in the Darwinian sense, people were drawing branching trees, yes.

PSH. I was thinking more of an experimental sense. Could you start with a set of data and then turn them into a tree.

AE. Well there are the taxonomists and the systematists, and they and their supporters had got as far as the work of Peter Sneath, P H A Sneath and Sokal, and they had written a book called Numerical Taxonomy, so the taxonomists were busy applying computers to trying to create branching structures to do taxonomy, but of course most of them were not interested in phylogeny and I think what was so striking about the human genetics development was that we weren't in the least bit interested in classifying the races of man, we were interested in deducing the phylogeny, so we had a different focus right from the very beginning and looking back, probably the important thing about that work was that we introduced the notion of statistical estimation into phylogeny, which was completely alien to the taxonomists' tradition, and of course the reason we did it is we were both students of R A Fisher. It's as simple as that.

PSH. When you finished at Pavia what did you come back to, so to speak, were you coming back to Cambridge or was there another step in between?

AE. No, then I set off to the department of genetics at Stanford. There is a great sort of strange connectivity which works between Walter Bodmer and John Edwards and Luca Cavalli-Sforza and me, which involves bits of

Cambridge, bits of Stanford of course, bits of Pavia, family bits inevitably, Walter Bodmer is my son's godfather, just to add to the confusion; Oxford, because John succeeded Walter in Oxford. So it's quite a sort of big academic family and Walter was at that time working in Stanford. Lederberg was there. Luca Cavalli had not yet started his Stanford career but knew Lederberg very well. So Walter got a grant from NIH or somebody for me to go across for a year, I don't think with any specific project in mind. It could have been the phylogenetic trees project because that was what I continued to work with when I was there and that proved very beneficial to the project, but I was only there for a year and I think I spent most of my time arguing with the tax authorities in San Francisco about being a non-resident alien, successfully I may say. But then David Finney offered me a senior lectureship in Aberdeen. Now David Finney of course has strong linkage connections and I'm happy to say that I had a letter from him only yesterday, which I will show you in due course, and I had already decided that I wouldn't go back to the United Kingdom unless I could be at least a senior lecturer. I wasn't going to go back for temporary appointments. I wasn't going to go back for anything more junior. So I came back as a senior lecturer, in statistics I may say, so then I really did have to learn some statistics.

PSH. Yes. How long were you in Aberdeen then?

AE. Three years. And then David Finney moved to Edinburgh and I felt I didn't want to move my family again. We had a nice house in Aberdeen and I couldn't think of really uprooting them again, so I declined David's offer of a senior lectureship in Edinburgh. But then things rather collapsed in Aberdeen and my attempt to get, I wanted to move to genetics in Aberdeen. I was already beginning to realise that I didn't want to make my career wholly in statistics and I had good MRC support in Aberdeen and there were plans for developing that. I ran an MRC group, called the Human Genetics Computer Project. That was great, but I realised at that time there was no future in Aberdeen and I saw an advertisement for a bye-fellowship in medicine, a bye-fellowship in medicine or science it was called, Gonville and Caius College Cambridge.

PSH. I don't know what a bye-fellowship is?

AE. A bye-fellowship is . . .

PSH. Is it b.y.e. or . . .?

AE. It's b.y.e. What happens in these colleges is there are the statutory fellows of the place, and then people sometimes leave money for research for fellowships in particular subjects, which are not teaching fellowships and it's not appropriate that the holders should become full Fellows of the College, so these are called bye fellowships and I was lucky, there was one in Caius which I saw advertised. I was unhappy in Aberdeen with the way that I was failing to persuade the university to move me to genetics. Of course I really, I didn't understand the system and I didn't have anybody to advise me up there, so I was hitting my head against a brick wall really.

PSH. Who was in genetics, was it John Evans?

AE. John Evans wasn't yet there. In Aberdeen genetics, first of all I tried to get them to get Walter Bodmer interested and Walter came across but he took one look around and said no, not for me. So John Evans got there later. I think he may have got there round about the time I was leaving anyway, so it was by then too late to repair the damage, but anyway I got fed up and I saw this bye fellowship in Cambridge, I applied for it and got it. It was just for two years but I'm still around.

PSH. And was that attached to Caius College?

AE. It was wholly a Caius enterprise, which was money which had been given by, I think it was a famous obstetrician, Sir Comyns Berkeley in the 1930s for a bye fellowship and it was advertised, as I say, and I was lucky to get it. And then during my tenure I used the bye fellowship to write a book called 'Likelihood' which had been sort of been boiling up in Aberdeen because having had to learn some statistics, real statistics because I was supposed to be a senior lecturer in statistics, I realised I didn't agree with very much of it. I wanted to do things differently. That was my Fisherian and genetical inheritance of course, so I couldn't understand why people didn't plot likelihood functions, it seemed such an obvious thing to do. But I discovered that nobody was doing it, so I had to write a book telling people to do it. Well, I don't think they took much notice of my book, but they now do it anyway.

PSH. As part of this fellowship, was this turned into what you might call a more formal university post in due course?

AE. Not directly. In Cambridge, unlike Oxford, the college posts are administratively and bureaucratically distinct from university posts, but you can hold both. You can be a university lecturer and a college lecturer and if your university lectureship collapses you will probably be kept on by the college as a college lecturer. This is the way that the process works, so people who come to Cambridge, or people who came to Cambridge in those days on temporary tenure like my 2 year fellowship, unlike people who came on senior research fellowships from research councils and so forth, in those days there were very few of them but it was very likely that you could get a university post.

PSH. So you sort of got a university post?

AE. Then it turned out that there were two people after me for a university post. One was John Thoday who was Fisher's successor as professor of genetics, and George Owen who we mentioned earlier said that he was leaving his university lectureship to take up a post in Toronto to study poltergeists. But he hadn't actually written the letter of resignation. George Owen was responsible for teaching statistics, population genetics within the department of genetics, my old department. I turned up with this bye fellowship in Caius, Thoday not unreasonably wanted me to succeed Owen. At the same time there was a post in the Department of Human Ecology which was offered to me, which, I didn't know at the time, was intended for statistical consultancy work and it was put to me by the then professor who was a fellow of Caius, Leslie Banks, that it was a post which was going free and which I could do my work in, which involved of course also trying to persuade Cambridge to get some human genetics going. Well that was actually on offer. Of course I was uncertain what to do, whether wait for the post that I would really prefer in genetics, which was not absolutely certain, or to take the one in human ecology which was actually on offer. So I went and sought the advice of David Kendall, the head of the statistical laboratory whom I had got to know through working there in my bye fellowship and he simply said a bird in the hand is worth two in the bush. So I took human ecology and thereby hangs a long tale which we won't go into just now.

PSH. Right.

AE. But it was a university post and although the university tried to remove me from it in due course and make me redundant, I stood and argued and I am still here.

PSH. Can I ask, at what point did you start to have your own students working with you. Was that before you came back to Cambridge, because I noticed for instance you had Bill Hamilton with you at one point?

AE. Bill Hamilton was a couple of years behind me in the department and of course I was still a research student, so I never had any students in the department, but they used to have a mentoring scheme, as it would now be called I suppose, and Bill Hamilton was assigned to me, so I talked to him about the work I was doing on natural selection and the sex ratio and I was then starting a mouse experiment in which I was hoping to change the sex ratio, I mean it seems unbelievably naïve and nobody told me not to do it, but I was going to change the sex ratio in this mammalian species in the inbred line that I was breeding from; it couldn't possibly have worked, but I remember Bill Hamilton pointing out, technically it couldn't work anyway because of the design of the experiment. So I think I must have had some influence on Bill's interest, because later on of course he became very well known for his work on sex ratio in other species.

When it comes to more formal students, in Aberdeen I was only there for 3 years. I had two who were actually registered research students. One was Ann Eyland who worked on migration matrix models in population genetics and eventually went back to her native land of Australia and became, amongst other things, the President of the Women's College in the University of Sydney, that's when I last saw her. And then Chris Cannings and he is now professor in Sheffield, I inherited him from Cedric Smith. He'd done I think one year as a student of Cedric's and then he took a post of Assistant Lecturer in Aberdeen, so he needed a supervisor and he did a very nice thesis on population genetic models. I think I tended to steer him away from the statistical work which he'd started with Cedric, but he has an interesting paper which sometimes comes up in correspondence with John, about the estimation of gene frequencies in some circumstances which Cedric had put him on to. I can't retrieve the details now anyway and that perhaps would be too detailed in any case.

I remember I came to Cambridge, of course for the first two years there was no question of me taking any research students, but then when I got more established I had three, the first from anthropology, and he didn't last very long. I mean he did his three years but the main thing I taught him was to write good English I think. His English was appalling when he started but by the time he left me he could write good English. I don't know what happened to him in the end. Then I had a splendid chap called Gordon Hopewell from the department of genetics, and I set him to work on writing computer programmes in human genetics and in particular for the calculation of inbreeding coefficients. You appreciate there were no decent programmes for computing inbreeding coefficients from pedigrees and he produced an incredible Fortran programme of I think it was 32 or 64 or something instructions. Very very clever in his first year, but his real ambition was to captain the university at croquet and in those days you didn't really have to do much work for the research council to be persuaded to pay for another year. So he never even bothered to submit at the end of at the end of his third year. He just went off to his father's furniture-making business in Leicester as far as I remember. I suspect he never did intend to do anything academic, but he did write this wonderful programme.

And then I got more involved in the statistical laboratory and I used to take students for their practical subject. The Diploma in Statistics farms out its students to people in departments around the university who are doing vaguely statistical things, so I got involved in teaching the part III mathematical genetics course through the Statistical Laboratory and it was one of the options in the Diploma in Statistics, and that's how Elizabeth Thompson came to me. She was a student of mine for her year doing the Diploma and was sufficiently interested in what I was doing, and of course I became extremely interested to have her as a research student because she was exceptionally good. So she became a research student of mine. Now at that time the Department of Human Ecology was at Fenners and it was fairly isolated, so there was no way in which a group was growing at Fenners and I couldn't get MRC support. They had supported me in Aberdeen but I couldn't get them to support me in Cambridge. So all my attempts to expand failed in Cambridge.

So after Elizabeth, there were really two reasons why I never interested myself much in research students again in the sense of trying to go out and get them. One was this isolation at Fenners, which later became isolated in the middle of Cambridge by the hospital development, and one was that, once you have had a research student as good as Elizabeth Thompson, you probably wouldn't want to take run-of-the-mill people anyway. As it happens I had no guilt complex about it, because people didn't come to me and I didn't go out foraging. I didn't have the unit which it would have been desirable if I had wanted to apply for MRC studentships and then advertise them, so by that time in any case, having been disappointed in my attempts to get human genetics going in Cambridge, because nobody knew what the two words stuck together meant, and of course the idea that computing had anything to do with human genetics was totally alien so it was completely impossible to get a hearing for the sort of computer-based human genetics which I wanted to get going in Cambridge. I just gradually became more interested in other things. I mean one does.

PSH. Talking about other things, one thing I wanted to ask about was your work on the original Mendel experiments and the statistical aspects which blew up into a controversy in the mid eighties. How did you get involved there?

AE. I was familiar with Fisher's paper on Mendel in 1936 as an undergraduate, and I was aware that Fisher had done goodness - of - fit tests using Chi-squared and when I wrote my book 'Likelihood' it was in a sense a sermon against the use of significance tests, so I said to myself, one day I must look at Mendel's data and look at what Fisher did, because I'm putting myself in the intellectual position of saying you shouldn't use these repeated-sampling tests of significance, or tests of significance based on repeated sampling. So what should you do? What likelihood functions are you going to draw? So I had always had in the back of my mind that one day I must study Mendel's data carefully and the opportunity arose when my wife became ill in Sweden one summer and I had to rush over to look after her, which was very fortunate that it was in the summer, and fortunate that I was employed as an academic, and I realised that I would want something to work on when I was in Sweden so I simply swept up all the papers that I'd got on Mendel, and I have the habit that if I am interested in something, I open a file on it and as the years go by I just pop bits of paper into it, which is why my files are bulging on linkage. So I'd got a file and I'd got a copy of the original paper and I'd got Fisher's paper and I took a calculator with me and off I went to Sweden. So I did it by hand whilst waiting for my wife to get better, which I am very glad to say she did.

PSH. And am I right that your conclusion essentially was that Mendel or whoever was doing the work with Mendel, were not likely to have done anything deliberately remiss in the work. Is that a fair sort of summary?

AE. Yes I think so, I really came to the same conclusion as Fisher, that anybody who has worked with Mendel's data and read about his life and so forth just simply cannot bring themselves to believe that he would have deliberately falsified anything and of course there is no direct evidence for that. All we have is a set of results where there aren't as many extreme results as one might expect and so then one begins hypothesising as to how this might have come about and I think anybody who studied it very closely is reluctant to come to any firm conclusion. So one just has these ideas. Fisher once tossed off the idea that perhaps Mendel's gardeners knew what the old man wanted and therefore tended to move things towards the middle for him. It might have been a subconscious effect, I don't think we are ever going to find out. It's there though. Statistically it's there.

PSH. This brings up one of the things which I keep encountering in some of my own historical reading, and this is there seems to be a tendency among, I don't know whether it's true historians but people in the historical field, to go looking for conspiracy theories and it's really very worrying, especially when most of these people are dead, can't answer back, and the same with Darwin, Wallace and the whole lot of them, as things get erected on what I always feel are the flimsiest of foundations and then it's very difficult to get rid of these things.

AE. There is some truth in that. I find it with my work on Fisher that people are always wanting to pin the eugenic label on Fisher as though the whole of the subsequent work from that extraordinary man can be explained by an interest in the improvement of the human race when he was a young man. I mean it's complete nonsense but they want to stick what has become an unfashionable label on him and I think so often you see people quoting, quite wrongly, Mendel as an example of the falsification of data and they've no evidence for that whatsoever.

PSH. I have come across it again personally because I was closely involved with Bernard Kettlewell and all the Melanism data and again . . .

AE. Oh really – yes.

PSH. But anyway. Just to get back to the main theme. One thing I wanted to ask you about was the European Society for Human Genetics, because I know from our previous correspondence, that you and Jim Renwick were perhaps the prime movers in getting something off the ground.

AE. Well we were indeed. You see Jim and I were fairly thick as thieves through two connections. One was, we were both in Scotland, he was in Glasgow and I was in Aberdeen and one was the Genetical Society. We were both regular attenders at every meeting of the Genetical Society, so we got to know each other very well. And we both left Scotland in a sense partly for the same reason. Partly that the Scots were going through one of their nationalist phases and they weren't making us very welcome but it was partly because, in both cases we were academically a bit unhappy, so we used to talk about these things and anyway we got to know each other very well and not only did we go to meetings of the Genetical Society but of course we went to International Genetics Congresses and we talked to each other a great deal there, and there was an important meeting Jim Neel organised in Ann Arbor as a preliminary to the 1966 Chicago Conference. It's called Computers in Human Genetics or words to that effect. There were really only 3 people in this country who knew anything about computers and human genetics. One was John, one was Jim and the other was me. I don't think John was at that meeting as it happens, but anyway Jim and I were there and we simply fell to talking about the future of human genetics in Britain, which didn't look very rosy at the time and I remember that in the course of several days of conversation we went out for a walk one day and we found ourselves talking about the fact that human geneticists didn't seem to have any forum in which to meet and then we realised that here we were in the United States, a lot of people from all over western Europe talking, and about to talk to each other in Chicago, to each other and we didn't meet in Europe and this seemed crazy so shouldn't we do something about it. So Jim, who was more forward in taking the initiative than I was, talked to Bob Kirk and I think you know the result because eventually we had a little paper in draft which somehow had got into a bottom drawer and got forgotten about, when Jim died, so I resurrected it and we published it in the Journal, just saying what I remembered. Of course there were many people who I mentioned in that little note who were still alive and could have challenged anything and nobody challenged anything, so I suppose that's the gospel really.

PSH. Can I just ask you, from having thought about it and discussed it with Jim, what were the next steps that you and others took to actually get it off the ground?

AE. Well Jim and I talked about it in Ann Arbor, that was where the original idea was, we then both went to Chicago. We then started talking to people, I suppose together and separately, I don't remember, in Chicago. What I do remember is that Jim either put up notices himself or got Kirk to put up notices. For some reason he thought that he ought not to be seen to take the initiative directly and Kirk was at W.H.O. I think at the time and somehow was neutral ground. I remember there was - Jim had some worry about the Israelis and whether they were going to be counted, that's one of these politically difficult things which he was concerned about but anyway, I think the meeting itself that was called, was technically called by R L Kirk who I think was W.H.O. at the time and I have a memory of the room in which we held the meeting and deciding that we would do something about it. I suppose Jan Mohr was there because we had got the invitation to Copenhagen for the first meeting of the European Society which was when it was founded in '67.

PSH. And where was that preliminary meeting held remind me?

AE. Where was the preliminary meeting?

PSH. The meeting where you sort of got together.

AE. That was in Chicago at the Congress.

PSH. Ah yes.

AE. At the Congress in Chicago. So far as I know nothing happened in terms of people actually meeting together between Chicago and Copenhagen. I think Jan Mohr must have said "Oh I'd like to get this off the ground. Everybody come to Copenhagen next November. I'll give you . . ." and so on.

PSH. So Jan became kind of secretary of the society from the beginning.

AE. I think he was everything, right from the beginning.

PSH. One thing that always intrigues me, which the society sadly has lost is, that there was a point I remember built into the constitution about 'frugality.' Was that you and Jim or was that a more general statement?

AE. Well of course in the first place it was never intended as a medical society. It was intended much more from the Human Population Genetics and linkage, that kind of thing, but it was before the doctors had actually discovered human genetics. Very early on and we had no plans as to how it should turn out, but I think we could say we were extremely satisfied how it was run at the beginning and it was a shoe-string operation and we didn't really have a constitution. There was this curious idea that everybody who'd run a meeting stayed on the committee I think. So the committee grew and grew and grew. Anyway it worked alright for the first few years.

PSH. Then after that it's probably fair to say that as that generation grew more senior there came a point when it got a bit stagnant, didn't it, mid '80s, late '80s?

AE. Well I suppose it did yes. I just kept on going loyally to the meetings for the most part, nothing much seemed to change, which is what you mean by being stagnant I suppose, and then it suddenly became professional and it took off.

PSH. Jim Renwick is one of the key people in this field and John may have told you, well you know that John helped to rescue Jim's records.

AE. From the basement at University College?

PSH. Well firstly from Jim's flat and then they were in the basement of the Galton, but I don't know if he told you that we've managed to get funding through our historical initiative from the Bath University Archiving Unit who are professionally archiving the whole set.

AE. I did hear that from you and that is extremely important. Jim is the unsung hero of all this really, because he thought deeply about the Bayesian approach, and dear old Cedric, whilst dear old Cedric was sort of mumbling along not quite being a Bayesian, and not quite abandoning tests of significance and not quite making up his mind, Jim simply said, right we are going to be Bayesian. So in that, he was following, I'm not sure how much he realised at the time he was following, but he was following Bell and Haldane. That's really the key Bayesian paper of course, Bell and Haldane.

PSH. You are thinking of the haemophilia linkage paper.

AE. Yes. That is simply Haldane being a Bayesian and it's only recently that I've got to know about Julia Bell and discovered her. I mean she is so remarkable. I knew about the Treasury of Human Inheritance, but I thought she was sort of Karl Pearson's sidekick or something. Here is this woman who read the maths tripos and then became a doctor. I mean to be both female and mathematician and a doctor in those days was quite exceptional

PSH. I think she's a wonderful person.

AE. Wonderful. Digging her out when I did that work for 'Mendelism and Man', that was a great joy and I got to know Greta Jones, who has done her entry in the new edition of the Dictionary of National Biography too, which is very valuable. I'm not sure I have printed that one. As a contributor I have been able to get access to some, I can access the DNB myself anyway.

PSH. I would be very interested to see that because I've actually written something on Julia Bell myself, but I didn't know about that.

AE. Yes, there's a new DNB entry. I must make a note of that. I think that Jim's article in the British Medical Bulletin, which I have here, telling us how to be Bayesian is absolutely brilliant. I mean, I'm not myself very keen on being a Bayesian but that's what they are all doing, though they don't admit it. And you see if you look in the original preface of my 'Likelihood', which I may say is still in print after 32 years.

PSH. Is it Cambridge University Press?

AE. Well it was, but they dumped it. They had a paperback and after it had been in print about 20 years or so, the accountants moved in and they decided it wasn't selling enough copies so, they stopped it and the American community said hey, we can't buy copies of 'Likelihood' anymore. What is going on? So some of them wrote to Johns Hopkins University Press to get Johns Hopkins to invite me to do a reprint which I was very happy to do and Johns Hopkins not only keep it in print but they've now sold more copies than Cambridge University Press ever did.

PSH. That's good.

AE. But you will see that in the original preface:

Many people have contributed to the lively debate on statistical inference in human genetics but those who have influenced me most directly are Dr J H Renwick, who has deliberated longer and more deeply than anyone on the problems involved in detecting and estimating linkage in man, and my brother Professor J H Edwards, whose capacity for independent comment is fortunately inexhaustible.

So with the two of them and one other friend here in Cambridge, Ken Machin, who taught me to depart from the standard statistical fare.

PSH. Can I finish up Anthony by just asking you, and I haven't read it yet, but I've seen it reviewed, your book, Cogwheels of the Mind.

AE. Cogwheels of the Mind.

PSH. On Venn diagrams. I saw it reviewed in *Nature*.

AE. It got a nice review in *Nature*.

PSH. What stimulated you to write that?

AE. Oh well, that's the Caius connection. I haven't mentioned that my undergraduate college in Cambridge was not Caius, it was Trinity Hall and it was my extreme good fortune that this bye fellowship which I got was in Caius College, because that was Fisher's college. So I suddenly found myself surrounded by people who knew Fisher extremely well and that led me to become more interested in Fisher's work itself, which over the years I'd been away from Cambridge, I'd not been particularly interested, in and for the centenary in 1990 which obviously then fell to me to organise, I was interested in doing that, I had the idea of putting up a stained glass window of the Latin square, the colourful Latin square from the dust jacket of the Design of Experiments and that was to fit in a nice square window, and above that window was an arched window and I needed some sort of pattern to put in there to complement the Latin square, and so of course I thought of a Venn diagram. I thought Venn also was a Caius man, I was beginning to learn that he was part of the Caius tradition, so what more natural than to put a Venn diagram up, and it was in the course of persuading my rather reluctant colleagues to allow me to put up these colourful things in the hall that I began to wonder how you did a Venn diagram for 4 sets and when you've done one for 4 sets how do you do one for 5 sets, so I became interested in Venn diagrams.

PSH. That's fascinating. I want to finish now, but before I finish I have been asking everybody two things and one of them is, of the various areas of work you've been involved in over the years, which is the area or piece of work you feel most affinity with, most proud of? Can you think of a particular area or is it really more of a continuum.

AE. Is it what?

PSH. I was thinking, is it difficult to name a particular piece of work or . . .

AE. Yes I think of myself as an amateur in all the fields in which I have worked. So I don't think there is anything in particular. It is just very, very general. I have just been interested in a lot of things. Because of the peculiarities of my employment in a way, I have not been able to do that which I was really trained for as it turns out, because in the light, in the early seventies to find yourself with a training in genetics, human genetics too, insofar as there was any human genetics to be trained in, and computing, there was a career cut out there for me which I have not been able to pursue, and consequently I've been doing a lot of other things, mainly on the historical side and, well, Bernoulli had a spiral, the Bernoulli spiral, equiangular spiral engraved on his gravestone, so I suppose that's the pattern that he most wanted to be remembered by or somebody thought that. So I suppose you would have to put a Venn diagram on my gravestone!

PSH. That's fair enough. And then the other question I have been asking everybody is there one particular person who you would single out as having been a special influence on your career?

AE. That's actually very difficult to answer. In a way I might have to say R A Fisher, but in another way I would have to say Luca Cavalli-Sforza; in another way I would have to say my brother John, but finally I would remember my schoolmasters.

PSH. Yes. And then, Anthony, are there any other things which I have not asked about that you feel are really important that you would like to kind of have recorded?

AE. But with some relevance to human genetics?

PSH. Well anything really.

AE. Anything academic. Well, because my interests have been so broad of course we simply haven't touched on some of the things I've been interested in, and they are not terribly relevant, but everything I have done somehow comes out of that early interest in statistics and human genetics brought about by having been in Fisher's department, so my book 'Likelihood' comes out of that tradition. Even the book I wrote on Pascal's arithmetical triangle comes out of that tradition. It's not obvious to anybody who picks it up and I don't mention it, but it's a scholarly account in the history of mathematics, but I only did it because I was interested in the binomial distribution and I was only interested in that because of genetic segregation. So there are bits and pieces lying around and, as you've seen, if you get interested in human genetics and then in a sense become a bit frustrated that you can't practice it, you get interested in Pascal's triangle and Venn diagrams and goodness knows what. But it all goes back to this training in genetics, and human genetics, such as it was at the time, and statistics. All of them in an amateur sense because I am not a professional mathematician, I am not medically qualified, I'm just interested in things.

PSH. Anthony, thanks very much, I am going to stop and turn the machine off and then we can go on talking about anything else we like.

End of recording.