H John Evans

Personal Details

Name	H John Evans
Dates	1930 – 2009
Place of Birth	Llanelli
Main work places	Aberystwyth, Harwell, Aberdeen, Edinburgh
Principal field of work	Cytogenetics
Short biography	See below

Interview

Recorded interview made	Yes
Interviewer	Peter Harper
Date of Interview	10/12/2003
Edited transcript available	See below

Personal Scientific Records

Significant Record set exists

Records catalogued

Permanent place of archive

Summary of archive

Biography

John Evans, born in Llanelli, South Wales, first became interested in genetics while studying biology at Aberystwyth University between 1948 and 1953, then doing a PhD on plant tumour chromosomes, leading to further research on radiation effects. In 1955 he moved to the Harwell radiation biology research unit, developing further cytogenetics research, including a year at Oak Ridge Laboratory, USA, in 1960, before being appointed to the Chair of genetics in Aberdeen in 1964. In 1969, following the death of Michael Court Brown, he became director of the Medical Research Council Human Genetics Unit, remaining there until his retirement.

INTERVIEW WITH PROFESSOR H JOHN EVANS, 10th DECEMBER, 2003

PSH. This is 10 December 2003 and I am in Edinburgh talking to Professor John Evans at his home, about the early days of cytogenetics.

John, I want this really to be you talking and very little if anything from me, but just to get things going can I ask, what got you interested in genetics in the first place.

J E. I suppose it's what happens to most people. It's an enthusiastic teacher. I did a degree in botany and zoology in Aberystwyth, back in 1948 to 1953, or thereabouts; not quite sure of the dates, just after the war. And a chap called Percy Tudor Thomas, (P T Thomas) who had spent a bit of time with Cyril Darlington at the John Innes Institute and with Peio Koller and a few other interesting characters, and he was steeped in cytogenetics, lectured in genetics and it was exciting whereas zoology or botany, although quite pleasant and enjoyable, not as exciting. And those were the days when interesting things were starting to come out. People were writing interesting papers on chromosome structure and behaviour, on inheritance, on looking at single gene effects, and interactions and so on, and I got taken up by this and I ended up by getting a degree in botany actually, because Percy Thomas was an agricultural botanist. He wasn't at that time a professor, he was just simply a senior lecturer I think.

PSH. This was at Aberystwyth.

JE. This was at Aberystwyth.

JE. And I had to do, as part of my honours thesis, a project and the project suggested by , Percy (or PT as we called him) was to have a look at asparagus chromosomes, because I am told that if you can get asparagus male plants, they have far more shoots than female plants. If you could actually sex the plants before they flowered, you would make a lot of money, so let's have a look at the chromosomes and see if there is a sex chromosome present". And so I did that as my student thesis. There wasn't a sex chromosome present, but I got the chromosome number and made a lot of preparations, took a lot of photographs, and did standard sort of things and enjoyed it all. I wrote a very nice little dissertation.

Well then he got keen on me staying on to do a PhD. I thought I might in fact be off to become a school teacher or something. In those days one wasn't quite sure what to do. So I was keen enough to go ahead and say OK. So what are we going to do? He proposed quite an interesting study. He had some people come in to see him a few months earlier, two Polish men who were actually cultivating mushrooms commercially in some sheds and they were growing these mushrooms in compost and selling them. And they had come into the University, to the agricultural botany department, because some of their mushrooms had very funny growths on them. They were very abnormal. They had gills on top and were rather profuse and bizarre. And Perce thought "Oh this looks interesting. These look like plant tumours that

were being induced" and the suggestion was they were being produced because these men had supplemented the heating of the houses by burning up coal tar, coal and old rubbish as a heating source and there were fumes inside the house and these tumours were produced. And so I got the project of finding out what induced these tumours of mushrooms and to study their nuclei and chromosomes. So I spent my time making compost, growing mushrooms and building cabinets in which to grow them

and then piping in fumes of various sorts, injecting various carcinogens, etc. I had a state scholarship at the time and then I got a grant from the county to do one post-graduate year and then the British Empire Cancer Campaign paid up for me to spend two more years. I did a PhD. Published about 4 papers on this work. The growths were abnormal tumorous growths. They had lots of excess RNA in them. They were rapidly dividing syncytial tissues. I couldn't find anything in terms of chromosome, abnormalities. It was hard enough seeing their chromosomes, but I did in fact see their chromosomes and I published one of the first papers on chromosomes in Basidiomycetes. The other similar papers on chromosomes in fungi using 'modern' techniques were by Barbara McClintock on Neurospora, from the States. Anyway that was my start. That's why I got into genetics. And during my PhD we had a meeting of the Genetical Society at Aberystwyth and Charles Ford came up at that meeting, and I met him and Jack Haldane, who had recently married Helen Spurway.

PSH. What year are we in now, roughly?

JE. We are probably now in about '54 I would say, or thereabouts. I have a story about Helen Spurway and Jack Haldane too; you won't have heard about however,

PSH. Tell it.

JE. It isn't very much but we had this meeting and I have a photograph of all the people involved; everyone stayed at the student hostel, which was a brand new place and looked very nice. And it was a very good meeting and the big man at the meeting was Jack Haldane with his new wife Helen Spurway and she was quite a bossy character. And we arranged to go off to Borth, which is a village nearby, with a lovely beach and so on. And Jack Haldane wasn't there in the party when they were getting together to go to Borth. So Helen Spurway shouted "I'll get him up, get him out" and she charged to the side of the building with handfuls of stones and was there throwing stones up at a third floor window. Eventually Jack Haldane did lumber down and come out and joined the party and we all went off.

Anyway I met Charlie Ford at that meeting, because he came up to give a paper and he was looking for someone to come and join a colleague of his at Harwell. We had got some interaction with Harwell, because two of my colleagues, a chap called Gareth Rowlands and another chap called Roy Davies, were both involved in the idea of inducing mutations in their plants with ionizing radiation. They were working on barleys and peas I think, Percy had gone down to Harwell and came back with four cobalt 60 sources, in the department land rover with big signs on it saying 'Danger. Radioactive' The

sources weren't all that big. Big enough actually. Probably about a couple of curies each, and we had built a little lead castle in the greenhouse area and these sources were on the end of rods put in there and we were using these to irradiate bits and pieces of plants, and I actually used them myself to irradiate some roots of broad bean. However, I talked to Charles Ford at this meeting and he told me that there was a job going with a chap called Gerry Neary, a Physicist at Harwell. Charles was the geneticist. Charles and Toby Carter, were the two senior geneticists at the MRC Radiology Unit at Harwell at that time. Toby had Mary Lyon with him. Tony Searle came on later, afterwards. Charles had John Hamerton as the technician with him. That's all he had, and Gerry Neary simply had 3 physicists and he wanted to be involved in studying the biological effects of radiation.

So I went down for an interview and got the job and I then moved to Harwell after finishing my PhD. I worked with Gerry Neary, very productively I might say, for quite a

number of years because he was a very bright physicist, he had come there with a degree in physics from Cambridge. Very interested in hospital physics and radiation effects, and the Harwell unit was the radiobiology unit of the MRC and John Loutit was the chap in charge of it. It was a very good unit. A lot of very good scientists there. And I had a very interesting time. We published a lot of papers. We spent a lot of time looking at the effects of radiation on breaking chromosomes and we used various materials, broad bean roots were the main things initially because there was a lot of work being done on growth and they were used as a material to measure the effects of radiation on inhibiting growth. So we devised a method for measuring cell proliferation rate, which Jack Haldane published in Annals of Genetics, must be back in 1957 sometime, I can't remember. We compared the effects of fast neutrons, and we spent time on top of the reactor piles exposing plants to radiation, and compared X-rays, gamma rays, neutrons, and measuring the effect of oxygen in enhancing radiation effects and published a lot of papers on oxygen effects. I was growing Tradescantia pollen tubes, where the chromosomes come down the tube in a single file, we used very very soft X-rays that would only penetrate a few microns because we had to have some kind of tissue to look at the effects of these particular rays. It was important to discover in fact the spectrum of radiation energies which resulted in damage at the chromosome level.

So all that was underway and I met a lot of interesting people, David Catcheside, John Thoday, all involved in the same kind of area, a man called Hal Gray who ran the Mount Vernon radiation set-up, and I worked at Harwell, I suppose for about 8 years, something of that order, 7 or 8 years and then I went to America for a year.

PSH. Can I just ask, were you during that 7 or 8 years, were you working at all with human cells or was it all done with plant and

JE. No, towards the end of that I was working with human cells and I would have to go back and define the time by looking at the papers, which are all next door. I eventually did some work with Charles Ford, he had spent some time developing techniques and trying to get better looking mouse

chromosomes and eventually with T C Hsu's technique of hypotonic, and odd bits and pieces, he began to get good preparations, and Charles was very meticulous in the lab and he got very nice preparations. Well I did some work with him and did some work with Mary Lyon as well. In fact one of the things that I did was to introduce autoradiography in the lab. In those days you couldn't buy isotopically labelled nucleotide, you had to make your own stuff. I'm jumping ahead of myself now. Because I went to a meeting in the States, my first ever international conference, a radiation conference and a genetics conference. The radiation conference was at Burlington and the genetics one was I think in Montreal shortly afterwards. And we went, Gerry Neary and I, to New York and we saw Herbert Taylor; he had just started getting some interesting findings on segregation of chromosomes using tritium labelled thymidine - Herb Taylor and Phil Woods, and Hughes. Taylor, Woods and Hughes, that's
right. When we got back to the UK I had got some tritium labelled acetic acid from Amersham and we labelled thymidine by exchange. Anyway, we began to label various things, so we introduced autoradiography and published some papers. Some things I never published and I regret that in some ways because when I repeated and extended Herb Taylor's work on Allium roots I noticed that if I waited for 1 day, 2 days, 3 days I could see interphase nuclei with just discrete patches of label and it was quite obvious when you looked at all the chromosomes that what you were seeing were whole arms of chromosomes and they were present as discrete areas. They weren't diffuse over the

whole nucleus, so very clear evidence that during interphase each chromosome had its own area and stayed there essentially. That all came out much later.

However, I am rambling. That was quite an interesting time. We did a lot of work. I got a young man David Scott to come and join me together with a couple of PhD students and we did a lot of work in what was called radiation cytogenetics. One development was the demonstration that one could use the incidence of induced micronuclei as a measure of the effect of physical or chemical mutagens. This was an easy way of scoring damage so it has been used as a standard technique since then.

What else did we do at the time. Well, back to Charles Ford and Mary Lyon. Having got all these labels to use to look at chromosome segregation and as markers, Mary came up with this "inactive X" hypothesis of hers which was a terrific idea. I will never forget talking about it over the tea club. We had tea club every day and had tremendous discussions and debates. Mary is a shy, but quite determined lady and she had this rather novel notion of random X-inactivation and we all discussed it, and yes it sounded sensible. We had in the lab a mouse strain carrying a translocation known as Searle's translocation. Searle's, (Tony Searle) translocation has an added bit to the X chromosome, so that heterozygous females with a translocation had one X slightly larger than the other so they could be distinguished cytologically. And at that time we had evidence that late labelling was associated with heterochromatin, and that one of the X chromosomes was late labelling *and* replicating. I injected these translocation mice with labelled thymidine and then sampled bone marrow at different times after exposure. Scored the

slides, of course blind, and was able to show that in fact in 50% of the cells the long chromosome was late labelling, and in the other 50%, it was the short chromosome or the Xs that were late labelling, This was the first demonstration of the randomness of the late-labelling X as such. That was published in Nature, again I would think it probably somewhere around the '60 mark or thereabouts. It was 40 years, 45 years ago.

PSH. It might have been possibly a bit before that because the human chromosome number

JE. Well I'll tell you about that . .

PSH. came up

JE. Charles is not here, I mean it is a pity Charles is not here

PSH. I would like to hear about it, because I have heard about it a little bit from people like Paul Polani but I would like to hear about it from your perspective.

JE. You should hear about it from John Hamerton too, if you could. But what happened there was, Charles had got these techniques going fairly well following T.C. Hsu and Jo Hin Tjio. (Tjio was quite a character). Tjio and Levan, anyway had used these techniques and showed that in human somatic cells the chromosome number was 46 not 47, or 48. Charles was talking to somebody at Oxford and we had some relationship with the radiotherapy department in Oxford where a policeman was due to come in to have a testicular orchidectomy, and radiation therapy and Charles managed to persuade, I forget who it was now, I know the chap, I can't think of his name. Long dead.

PSH. The surgeon involved?

JE. Radiotherapist involved actually, because he got the surgeon to get the sample out. So Charles came back from Oxford proudly carrying a tube with a piece of testis in it. So it was a policeman's testis that was the interesting bit about the story, an Oxford copper
donated the material from which Charles showed the 23 bivalents at meiosis. That was quite an interesting time, because we discussed things at tea. As I said before, we had these teas in the afternoon, always exciting, good discussions because we had, well Toby, Charles, myself, Toby Carter, Mary Lyon, Tony Searle came later, he'd joined Toby Carter later on.

PSH. Bruce Cattanach?

JE. No that was later when Bruce came.

PSH. But a wonderful group of people.

JE. Oh yes. I mean it was a very, very interesting bunch of people. And Paul Polani coming up occasionally because he and Charles had a collaboration.

Paul was providing materials from the clinic and so they did their Turner's syndrome work. Paul was very involved in Turner's syndrome. That was one of his big interests then, and then we had Peo Koller who used to come up because Peo was looking at tumours and so was Charles. Peo was looking at Walker rat sarcomas and Charles was looking at various other tumours in mice, they had a lot in common and a good collaboration. I had Stanley Revell come up quite a few times, from Chester Beatty. Interesting fellow Stanley. Good thinker and we also collaborated.

PSH. And what happened then after this phase? Where did you go to next after Harwell?

JE. Well, I had a number of American visitors because there were not many people in this country doing the kind of work that I was doing. Catcheside had more or less stopped. Leah his colleague in Cambridge died, or had given up at that time, and so Neary and myself were the physicist/biologist partnership in the UK looking at radiation effects at chromosome, at nuclear level. And there were colleagues in the States, Sheldon Wolf especially, it was a big lab at Oak Ridge and a big lab at Brookhaven and a chap called Arnold Sparrow was one of the Brookhaven people. And Arnold came through and said you must come and spend some time with us. We have got a gamma field, we have got radiation sources in greenhouses, we've got mammalian tissue culture cells, and I was growing hamster cells at the time, and looking at cell survival and radiation-induced chromosome damage. And so we went to the States. We went over on the Queen Mary with my wife and two young kids and spent a year at Brookhaven. Did a fair bit of work at Brookhaven, published a number of papers and one big review in International Review of Cytology, which Danielli wrote and asked if I would do. It's a very big review of radiation effects of chromosomes and I spent some time down at Oak Ridge because I knew Sheldon Wolff, I had met him at meetings with Dan Lindsay, who worked on Drosophila, and a whole range of very bright young scientists. It was a very active lab at Oak Ridge. So I did a year there and then came back and I headed a new cell biology section at Harwell. The MRC Unit made a cell biology section in addition to the existing genetics section and cytogenetics sections and I had a couple of young scientists, PhD students join me, two of them went to the States and took jobs at Oak Ridge or at Baltimore, one with McKusick.

PSH. Who would that have been, because Victor McKusick was very clinically orientated?

JE. This was a chap called Robinson. He has not done all that much since then. He went there and became an administrator and ended up in NIH in Bethesda. He had a PhD in
entomology in Oxford with Dick Southwood and came to me and he worked with me on Chinese hamster cells looking at cell killing under different conditions of oxygenation, different radiation types and so on, and he went to McKusick and I don't think he was there for all that long. I think he ended up by going to the National Cancer Institute, NCI at Bethesda. I met him some years later and he was then acting as an administrator on the site in NIH. Then one of the other young men, nice young man, now old man probably, went to Oak Ridge, forgotten his name now. Come back eventually. But I

mean over the last, I've been in this game for 50 years dammit, or not quite but not far off, yes, and there have been a lot of people. There must have been at least 40 or 50, no 100 post docs come through, more than 100, and at least 50 or 60 PhDs, at least a dozen or two MDs, remembering them all, I could look up and see who they all are . .

PSH. No

JE so it is difficult actually. Anyway I came back and I headed this section and I was then looking at human chromosomes and looking at peripheral blood and looking at monkeys too. They were marmosets and Charles had this colony at Harwell. They were full of nematodes, but nevertheless they are still good. It isn't very often you can look at a slide, look at the blood sample and what do you see, wriggling nematodes.

PSH. Gosh

JE. Not very nice actually but . . .

PSH. And then, which year have we got to now, would this be . . .?

JE. 1960 or thereabouts

PSH. 1960. And what year was it then that you moved up to Edinburgh, or was there a stage in between?

JE. There was a stage in between. 1960 back to Harwell and then in 1964 I went to Aberdeen. A chair came up in Aberdeen, a new chair in genetics and I forget who was on the short list altogether except Walter Bodmer, myself and what's his name who went to Swansea eventually . .

PSH. Oh John Beardmore.

JE. John Beardmore, were shortlisted and were up there for interview, and Walter came across from California. And we were chatting and both Walter and I said we weren't interested eventually. I was not too keen. I had arrived on a dismal grey day on a train. I had never been up that far north before and Aberdeen when it is raining, doesn't look too exciting I can tell you. However, Walter certainly was not interested so I gathered eventually, and I had a letter offering me the job and I didn't want to take it but there was a chap who you probably don't know, called Alastair Curry. Did you meet Alastair Curry?

PSH. No.

JE. Alastair was a pathologist. He was a pathologist at ICRF in London. During the war, he was an army medical officer. I think he was fairly high up actually. However, Alastair
was on the MRC and came on a site visit to Harwell and he met me there on that site visit. He was there for 2 days I think, one of those big site visits that went off very well. And he got the chair of Pathology in Aberdeen. Alastair is from Islay and did his degree in Glasgow, a real Scot. He died about 6 years ago, at a guess. And he persuaded me to come, to take up this chair and so

did Vero Wyn Edwards who was Professor of Zoology. Vero was the one who was organising the genetics chair. So we agreed to go and so in 1964/65 we went to Aberdeen and set up the Department, got the MRC to put in a fair amount of support, got very nice microscopes, we were looking at human chromosomes, we had written a number of papers on radiation damage in human chromosomes - and I should go back for just a second I think, before I continue with Aberdeen.

While I was at Harwell, I met two characters called Richard Doll and Michael Court-Brown, and Richard and Michael came to the unit. Michael was a radiotherapist at Hammersmith. Richard was a physician and Michael's senior slightly. Both slightly bombastic characters, both of whom became very great friends of mine and I didn't see much of them at Harwell, because they came because they had been asked by the MRC to look into the possible effects of iatrogenic radiation in inducing leukaemias. This is a well known story now. But what probably isn't known is that they had to have dose measurements made on these patients and they had a whole list of patients with leukaemia, non-leukaemia, and exposure to radiotherapy of one sort or another. But somebody had to decide what the radiation doses were and the person to do this was Gerry Neary. He was ideal. So Gerry was asked if he would actually look at this and do it by Harry Himsworth who was secretary for the council at the time, and Harry used to come to Harwell. So Richard and Michael came to Harwell and worked with two young men at Harwell, one a chap called, I don't remember his name now. One of them ended up in Oxford and became the Chairman of the Science Publicity Committee - Mulvey, John Mulvey. Do you know John Mulvey?

PSH. I know the name but I have never met him

JE. OK well, John Mulvey. Well John and the other fellow, Gareth, I think Edwards, spent their time just going through patient notes and diagrams and doing dose estimates. Anyway I got to know Michael there and they did this work, published it and then Harry Himsworth suggested to Michael that he could set up a little group of his own. Michael came back up to Edinburgh, set it up at the Western General Hospital. It was called the Clinical Effects of Radiation group on . . . look it up actually. It was a group. He wanted to have some people to work with him in the group. He had talked to Charles Ford.

There had been a lot of talk about broken chromosomes in tumours. He had talked to me about broken chromosomes and radiation effects and inducing tumours, inducing leukaemias so the idea was that radiation had caused chromosome damage in humans and might have given rise to leukaemias. He talked to Mick Callan, now Mick was somebody who was at John Innes Institute when Percy Thomas was there, so I knew Mick from that – way back. He talked to Mick Callan and Mick had a young lady called Pat Jacobs who was doing an MSc on, it was a grasshopper I think. Was it grasshopper chromosomes? I've forgotten. I have got Pat's original paper somewhere. Anyway, he sent Pat down to Harwell to Charles to learn how to handle mammalian chromosomes. So Pat came down to Harwell, while I was at Harwell. I have gone back. Back-tracked down to Harwell. So Pat came down and spent, she must have spent at least 6 months if not a year with us, with Charles principally, but we got together quite a lot and she actually

stayed at digs quite near where I was living with my wife and a couple of kids. And after a few months David Harnden arrived. That was the other person who Michael Court-Brown wanted to appoint to help him get his group off the ground and then of course they came back to Edinburgh, and they set up in part of a wing off radiotherapy, which the unit still has actually. It has been refurbished and extended since I arrived.

So I knew Michael Court-Brown pretty well and I was now in Aberdeen, not very far away, so I used to come down and see Michael and we'd chat about what they were doing and how they were getting on, and he had Karen Buckton join him, and they were looking at radiation effects on bone marrows of patients being given therapy and so on. And I'd set up my department and I had a number of interesting people working with me. I had a number of workers from abroad. I had a girl called Svietlinksa from Warsaw, had a girl from Greece that spent a year, and a girl from New Zealand. There were 2 or 3 PhD students and quite a busy little department was building up. Then Michael died and I was on the Council at this time, on the MRC, or maybe just before. And Alastair Curry was on the Council, the one who got me to go to Aberdeen. And John Gray was then Secretary of the MRC and they discussed the possible future of the Unit with Pat, and the other senior persons in the unit. The MRC had agreed to put up a new building for Michael, an extension, and that hadn't happened but it was all planned out as to what they might do. So I was persuaded to come down to Edinburgh, and reluctantly I'll tell you, as I enjoyed Aberdeen. My first impressions of Aberdeen were completely wrong. I think it's a very exciting city. It's a lovely place to work. The Aberdonions are marvellous characters, quite unlike the penny-pinching individuals depicted in cartoons. They are generous people and I liked it. It gets a bit cold but the countryside is fantastic around there. So we enjoyed Aberdeen.

PSH. What year was this that you then came down to Edinburgh

JE '69. So I arrived here in April 69 and we looked around for houses. We had 4 kids at that time, 4 boys and getting 4 boys into school is not easy but we were looking around this area because there were 2 schools, Watsons and Heriots, which were 2 schools we were told to go for. David Harnden had been dux at Heriots and of course extolled its virtues and I talked to the Headmaster at Heriots, who was a keen rugger fan. I wasn't playing rugger then but I was still a rugby fan and we got the boys into Heriots, so we bought this house, which is quite a big house and we've loved living here ever since.

PSH. I won't ask you whether you support Scotland or Wales at rugby.

JE. It depends which one is playing which. If Scotland and Wales are playing one another I support Wales. If either are playing Ireland or England or France I support them. Scotland if it's Scotland against them or Wales if it's Wales against them.

PSH. Yes. You must have come in then to the Edinburgh human genetics unit really just as it was at the start of very major development.

JE. Absolutely. Well let me just say that I agreed to come eventually. I had to, they weren't going to put the building up unless I was coming. So they told me this "A brand new building for you. It's going to house I don't know how many staff. It would become the biggest MRC unit in the country", it is the biggest MRC unit in the country and I eventually said "OK. You build me an animal house and I'll come". So I was given the money to build an animal house, given the go-ahead to the new building and I moved down. And I have got a lot of interesting memories around that time because it was a very exciting time and we did a great deal of work on chromosome variations in human populations; subfertile, radiation exposed, etc. At that time Tjorbjorn Caspersson had just begun to start getting somewhere with fluorescence microscopy and using quinacrine, and Lore Zech worked with him. Lore, she did all the work actually, but for Tjorbjorn everything had to be done with a spectrometer. He needed to see a map and he published a paper showing that you could distinguish human chromosomes, from their fluorescence profiles with quinacrine. So we got hold of atebrin, which is the anti malarial tablet quinacrine, and found that this worked just as well. It stained the chromosomes and so did at the same time, Peter Pearson down at Oxford, with Martin Bobrow. And I never forget it was, I think it was at Christmas, it was winter, sitting in the office and going over my pictures I had taken that day of human lymphocyte fluorescing chromosomes, I had Adrian Sumner and Richard Buckland working with me. And I could see bands. In fact we could identify all the chromosomes without any recourse to any fluorescence profiles at all. You could see the bands were there. We called them 'q' bands and we wrote a paper.
Now that we had been able to pick up these chromosomes, these bands by eye we could see which was chromosome 21 and 22 for instance. It was thought at that time that the chromosome that was abnormal in chronic myeloid leukaemia was chromosome 21, because Down's syndrome patients got leukaemia. We showed it wasn't, it was 22 in the leukaemic cells, not 21. So we wrote this up. This was, it must have been Christmas, maybe November, it will say in Nature. So I wrote a paper up pointing out we can now distinguish 22 and the chromosome for leukaemia is 22 not 21. I think it's Evans, O'Riordan, Robinson and Buckton or something. However, you will find that if you go back and look in the very last issue of Experimental Cell Research, on December 20th or whatever, it is edited, by Tjorbjorn Casperson, a brief paper showing exactly the same thing. Now he was the editor and he'd nipped in very smartly and got the year 1970, ours is in theory a year later, actually a month later when it came out.

At that time we showed that there were polymorphisms present on fluorescence on chromosome 4, chromosome 3, chromosome 9, chromosome 1. We actually traced these through families. We showed on a population basis that they were inherited. We thought we might be able to use them as markers to look for genes. They are not very good, because they are mainly centromeric, which is a pity in that sense. We found big variations in the Y chromosome. We showed that you could look at sperm and find YY sperms as opposed to single Y sperm. We showed that using this technique you could show that couldn't separate sperm, as somebody else had been claiming in the States, into X bearing or Y bearing sperm.

I then did a very important paper which has not been really made much of, published in Chromosoma with Elizabeth Ganner a visiting post doc from Switzerland, where we used tritium labelled thymidine and labelled human, peripheral blood cells, and then double stained them with quinacrine with ASG. We will come back to ASG in a second. This paper actually shows that the bands are late labelling bands. I think it's the first paper that suggests that those chromosomes where the highest proportion of quinacrine fluorescence are those containing the least genes, and that it's the non quinacrine sites are the genetic material.

PSH. Absolutely.

JE. And you find that in that paper. It must be 1971 or '70. In that year we got the ASG technique. Pat Jacobs went off. And so the idea was to go and spend a year with Dan Lindsay and off she went to spend the year with Dan Lindsay. Well she came back for a quick visit during the year. We had, actually, Adrian Sumner and myself found that by staining these slides with Giemsa you could see the same bands as with quinacrine. It wasn't uniform. We struggled to find out why. We called this the Acid Saline Giemsa technique, the ASG technique and we had done a lot of work on it. We published a paper in Nature.

Pat came along from the USA and saw this and said "Oh there's somebody in the States doing this or getting something like this. When did you do this? I said "It's all been done. There's a paper in press" and so on. A number of people then published papers shortly afterwards with the same kind of result. We also published papers then on centromeric heterochomatin with Adrian Sumner. Adrian was very much involved in this, showing the polymorphisms and how they are inherited. Richard Buckland and I then studied the mouse. We did the first paper distinguishing between different mouse chromosomes, because normally you can't distinguish between them because they all stain the same. And that's published in Experimental Cell Research. We looked at Bovidae and a whole range of other animals, looking at X chromosomes, and did quite a lot of work on banding all around. That was a very prolific high yielding period I think. That five years or thereabouts a lot of things came through.

Now one of the important things was that we had a conference in Paris in 1971, just when all this had been breaking or had broken. And we met at Runjis, which is a little place just outside of Les Halles an old place where there is a big market just outside Paris. And we stayed at a nice hotel with a little swimming pool outside, and we would sip whisky out there late at night and swim, and go through the fence, into Runjis for food where we got the most marvellous Bouillabaisse, because they served all the truckers from this huge pot full of crabs and God knows what. That's by-the-by, because during the day we worked very hard and the idea was to sit down and sort out all this information coming through on human chromosomes. And so I chaired half of it I think, and Jerome Lejeune did the other half because they were also coming through with interesting bits of information. Jerome had got Down's syndrome, Pat here had before that had got XXY Klinefelter, they had done the XO and so on, and we had just published, or we had in press I think, our paper defining 'g' bands and defining 'q' bands and suggesting how we

might number these, how we might actually do something about getting a nomenclature, and so we had a nomenclature discussion and this Paris nomenclature ended up by saying ‘q’ bands and ‘g’ bands. That came from us actually, ‘q’ bands and ‘g’ bands with no interbands and how the numbers were going to go. But one of the amusing things at this meeting was a problem with a chap called Klaus. Do you know Klaus Patau?

PSH. Only by name.

JE. Klaus was there, difficult fellow, Klaus. A bloody nuisance actually. He was holding things up all the time, being very awkward and difficult. You should ask Pat about this, because I remember telling Pat, she won’t tell you this, but I said “you go and sit next to him and disturb him”. She was very attractive looking and so she was deputed to go and sit next to Klaus and distract him and when he was going to make difficult noises tell him to shut up, or at least intervene. And she did very successfully and we had a very good meeting. Must have been 2 or 3 days before the actual main conference and that Paris meeting came out with perhaps the biggest, the major, leap in terms of human cytogenetics nomenclature and technique at that time. It was encapsulated in that particular meeting.
And it went very well.

PSH. Thinking back at your unit, during that phase and years afterwards, who would you see as the key people that you had working with you there at the time.

JE. Well there were quite a number. Well Pat first and foremost, Pat Jacobs. Karin Buckton who was Pat’s second in command in that section, Ann Chandley was third in command in that section. Adrian Sumner was an important person. John Gosden - less so, he was one of the scientists at the time involved in DNA sequencing, trying to sort out some of the satellite DNAs. Of course we had Ed Southern and Adrian Bird who came over from the MRC Mammalian Genome Unit.

PSH. When did their units come over and merge, because there was a lot of movement wasn’t there, perhaps in the ‘80s was it?

JE. That’s right. Much to my annoyance in a sense, because I was again involved with the Council on a site visit to the Mammalian Genome Unit. Peter Walker, was the Director, Peter was Director of the unit and Ed was his second in command, and Ed was a radiation biologist actually. They had a site visit when I suppose Peter was going to be retiring and it was a very good site visit, very good people. There was Peter Walker, Ed Southern, Adrian Bird, who else was there at the time? Well we will come back in a minute. However, despite the very good site visit, the Council wanted to close the unit down. They were trying to adhere to the doctrine that they had, that they would set up units for a limited time and small units certainly, once the director retired or moved, they would just disappear. Which I think in some ways is sensible. You can argue for or against it. And this was the case. They were going to close that unit down. They did the same with Bill Hayes’ unit up at KB, which again was a big mistake as far as I was concerned. So I was asked, would I take the unit on and I said I didn’t fancy taking on a unit the

other side of town. I already had - we must have got something like about 130, 140 staff and about 60 or 70 post-docs, visitors and Ph.D. students - over 250 people down at that site which I was responsible for, essentially. Two thirds the MRC paid for and the rest were non MRC financed. But that was another small MRC unit up there; so I ended up by agreeing to embrace it. So I talked with Ed and said, OK we will set it up and he can run a molecular genetics section, and keep it up there, not bring it down, keep it up there and keep it going in that way and so Ed and Adrian Bird were at Kings Buildings. Eventually Ed got the chair in Oxford. Adrian Bird moved down and ran a section within the unit and he had a year off too. He went to Switzerland for a year, but he ran a molecular genetics section in the unit.

Now I'm jumping about here, because there were various other people, with Adrian was Howard Cook. Howard came over. I had already appointed Veronica Van Heyningen, who Walter assured me, Walter Bodmer, that she was one of his best PhD students and she was and she is, and she has done very well. So I had Veronica and Howard Cook. Before that I'd got Chris Bostock a chap who did his PhD with Murdoch Mitcheson at KB. He ended up by being the head scientist of the Agricultural Research Council Research Unit down near Reading.

PSH. It'll come back.

JE. It'll come back. He was another important person in the Unit. A very important person.

PSH. And did Nick Hastie come at that point?

JE. Yes. What happened was that when I got Ed to run the section, I wanted somebody in the unit at a molecular level, a chemist, at least someone with a good biochemical background involved in looking at DNA, because we only had John Gosden, the names will come eventually and I haven't mentioned Denis Rutovitz yet have I? No. But Ed and I discussed who we might have to come in. Ed had a number of names, and I didn't know Nick Hastie, and one of them was Nick Hastie. So he had had good reports about Nick. He was a young man who had come from Liverpool, had gone to Cambridge, gone over to the States and was at Buffalo with a very small group in Buffalo, couple of people, I think a scientist and a couple of technicians. So I said OK, I will go and see him. I am going to be in the States. I will be in New York, I was doing something or other there and so I arranged to meet Nick Hastie and went along and met him and was quite impressed by him. Stayed the night with himself and his wife Alison. Had a day in the lab there. We talked about what we might and might not do. Got very excited about it. And said right you know you've got to come. You can come but you've got to go to the Council to be interviewed, you know go through all this palaver. I'll never forget. So he came over, and Ed and I went down with him to Park Crescent and we had a chat with the interview panel – can't remember who the panel was now. All I can remember was after Nick had a session with them, he came out and he was actually almost in tears. He was just so hyped up. He had a good interview, it went very well, but he was really quite emotional about it and I can imagine why too. He had come all the way from America. He had come in and come down and had to sit with all these old fogies and had to tell them

what he was going to be doing. Anyway, so he was offered a permanent appointment for the job and he came and he brought with him Bob Hill his assistant, who is still there, and we weren't quite sure what we would do but I can remember sitting down having a meeting and saying right let's have a look at this. We are going to go for one of these chromosomes. "Chromosome 13 with a retinoblastoma gene", "or chromosome 11 with a Wilms' tumour gene". We had a big debate, about 15 of us downstairs in a little common room, and decided we would go for chromosome 11. And we set Nick up with a section and he had Veronica van Heynigen and he had Howard Cook. Who else did he have? He had Bob, Bob Hill. Adrian Bird had a separate section. Ed was about to go to Oxford. Can't remember then what.

PSH. Well it proved a very effective area of work.

JE. Terrific. They did a very good job. They have done very very well indeed. I mean they have made tremendous progress in looking at chromosome 11 and looking at Wilms' tumour and all around it. And I think Veronica has done tremendous work on eye development, all the genes involved, the Pax 6 and everything else. That has all been very front-running science, very good.

PSH. Tell me a little about Denis Rutowitz and that area of technology.

JE. I'm about to go to that now.

PSH. Because that is something very different really from the rest of the work in the unit, in a way.

JE. That's right. It is. It was. When I came down from Aberdeen I had met Denis Rutowitz but he was based in London. He had been in Cambridge, did his degree in Cambridge. Denis, I think was once in the Israeli Air Force flying jet fighters, he is quite a character is Denis, hell of a nice chap. He married at least 3 times and is an interesting fellow. But Michael Court-Brown was keen on the idea of being able to rapidly analyse chromosomes by machine and he had met somebody and discussed this possibility I think with Donald Michie, who was here in Edinburgh. Do you know Donald Michie?

PSH. No.

JE. Donald is a mouse geneticist who, you know Anne MacLaren,

PSH. Yes

JE. Well Anne's husband,

PSH Oh right.

JE. First husband. You will find a Michie and MacLaren paper in J. Heredity about 1969, on looking at paternal expression in mouse inheritance, I think it's called. Anyway Donald is a very interesting fellow, and he became very interested indeed in machines. Machine Intelligence. And ended up taking a

chair in machine intelligence here and in Glasgow, making robots and God knows what. I think Denis and Donald were very friendly, and I think Donald and Michael Court-Brown were friendly via Anne. I think that might have been the connection, but I am surmising that's the case. However, Michael Court-Brown was hopeful that you could use a machine to do all this chromosome analysis and so the Council set up a little group and it was set up in London, there was no room in Edinburgh in the building at the time, and so they had a place at the back of King's Cross, right in the brothel area, interesting part of town. So I went down there when I took on this job and I'd met Denis and had a long talk with Denis and Daryl Green and two or three other people there, and then they eventually moved up to Edinburgh and were developing systems for two things, one was to assay cervical smears, to find a technique for picking out abnormal cells in cervical smears and that was one programme of work. But the main programme of work was developing a machine to actually count chromosomes and analyse them and pick out translocations, bands, chromosome number, you know an automatic way, so we ended up putting a slide or battery of slides, 6 slides on a microscope overnight, come back the next morning and having a print-out saying what we had got there. And I mean it was quite an advanced look ahead as to how one might do things. In fact they ended up by building a number of machines which worked. I mean they were front-runners in this area, really. And there are now machines of course, commercially, all over the place that do all these things.

PSH. Yes.

JE. Well that is very much due to Denis. They were real pioneers here, and they were a lot of bright people. Denis Rutowitz, Daryl Green, Jim Piper who is about here now. Well, what has happened from that, if you talk to Nick he will tell you is that before I retired a couple of people down in Denis' section and a couple of people from my section who were involved in looking at mouse development and they have ended up by developing computerised data and programs to look at which genes are being expressed where, which RNA is involved and when, what tissue is formed then and how these patterns emerge interact during embryonic development.

PSH . Yes

JE. And they have done a tremendous job actually. It is a very impressive show.

PSH. I have seen Nick talk about it. It's magnificent.

JE. That's right. I started that. It's a long time ago now and one of the chaps who is very very much key to that is a young man who is one of the oldest serving members in the unit. We were talking about Muriel earlier on. It's a chap called Duncan Davidson. He came to work as a technician with Michael Court-Brown way back, I would think in about probably 1964, '65 or thereabouts. Very early on. And then, he was a bright fellow and was responsible for developing autoradiography in the unit, that was in later years, and then he went to do a degree at Edinburgh. Got an MRC bursary or whatever its name. Got some money to go to college, go to University.

Wouldn't happen in these days. He did his degree, came back and ended a PhD with us, actually with Tom Elsdale, that was another important person. I'd forgotten about Tom. He retired a while ago and Duncan was involved in developmental genetics and development generally, looking at tissue cultures to see how cells did this that and the other. I haven't mentioned David Harnden either, have I. So many people and it's going to be a bit of a rigmarole, to and fro. However, Duncan Davidson was the guy who was really, I think, key to the computerisation of gene expression and development in the mouse and making up the mouse map and the human map eventually. So that's very important. Now I should say that when I came down from Aberdeen in '69, there were really 2 sections in the unit. Pat with the chromosome section and David Harnden with a development virus-leukaemia section, because leukaemia was still a big interest in the unit.

PSH. Had David come while Michael Court-Brown was . . .

JE. Yes. Yes, Michael Court-Brown appointed David. He was from Edinburgh. Did his PhD with Murdoch Mitcheson and went down to Harwell to learn techniques of growing human cells and mouse cells. Came back to grow human cells and mouse cells, and to look at viruses to see whether they had induced these leukaemias, which was a big thought at the time. And he was there with Michael, I would think for about at least 4 years if not more. During that time I remember Pat was doing Burkitt's lymphoma, virus induced tumour in Africa and with Denis Burkitt and they went across Africa. Pat went to Africa to look at Burkitt's cells, and chromosomes therein. We had a big line in the unit with human lymphoblastoid cell lines. We did a lot of work with them with Michael Steel and I haven't mentioned any of that. We also had a big collaboration with Harry Harris in London. We had a huge amount of lymphoblastoid cell lines. The biggest bank in Europe probably, possibly in the world and we did a lot of gene mapping and gene expression using lymphoblastoid cell lines, and I collaborated with Harry down at University College and Sue Povey and Hoppy, and Dallas Swallow and everybody else. Wrote a lot of papers.

PSH. I have seen them.

JE. There's a fair amount, and interesting ones too. Anyway David Harnden, what happened was that David was offered a post that came up in Birmingham, they wanted to appoint someone to run a cancer group, British Empire Cancer Campaign, as it was called then I think, and David applied and got offered the job and took it. It was a professorial appointment which was a step up. So he left as I arrived. He didn't leave because I kicked him out, or because we didn't get on. We were very friendly actually, and always have been ever since, but this was a post that came up and it was an opportune time to take it and so he went down to Birmingham. So I don't think I ever published with David. I might have, because our paths have crossed on numerous occasions. I chaired the Cancer Research Campaign for many many years. I was on the board for many years and David was part of that. We used to see one another quite frequently.

PSH. Just thinking broadly, if you had to choose one part of your work that you felt stood out from the others in terms of being important for you, what would you choose?

JE. A difficult question. The two major things in my work, I guess were chromosome structure, chromosome banding, that's been a large part of it and the other part is the effect of mutagens on human chromosomes, which is again, major. I haven't mentioned sister chromatid exchange, which came out of the unit too. I produced a technique for looking at that too and it has been used again to pick up effects on chromosomes. We did quite a lot of work on looking at the distribution of satellite DNAs and different sorts of DNAs around the chromosomes and then there is a very nice paper on ribosomal RNA, by myself and Mary Lou Pardue. This is 18s28s RNA and it's the first discovery pointing out that this RNA is made at constriction sites on chromosome 21, 22 where we knew that the nuclei are, but we actually showed that these RNAs are produced at those sites using xenopus RNA and autoradiography.

PSH. Yes

JE. Mary Lou, Mary Lou Pardue is her name, Mary Lou got a chair in Boston, I think it was. She was a bright girl. So that was another area of interest.

PSH. Just one final thing, because I have taken up plenty of your time, you were born and brought up in South Wales. Have you kept links? You must have had family back there over the years, but you never actually, once you left Aberystwyth, you never returned to work in Wales.

JE. I left Aberystwyth, I should say first of all I am a Welsh-speaking Welshman.

PSH Yes. From the Swansea valley?

JE. From Llanelli.

PSH. From Llanelli.

JE. And I went to Aberystwyth. Went from there to Harwell, Abingdon, went from there to New York, Long Island. Went from there back to Abingdon. Went from there up to Aberdeen and then from Aberdeen down to Edinburgh. That's my track as it were, but I'm Welsh.

PSH. Yes

JE. My kids were all born in England and they still think they are Welsh, although they don't speak Welsh.

PSH. Well it's interesting. John thank you very much. Are there any things you feel that I have not mentioned that are really important? I am sure there are, but

JE. I can't think. What I should have done and I haven't done is go and sit next to you in my study and just look at the papers and see what . . . As I say

there are a couple of hundred publications and I write all my own papers. I did not stick my name at the end of the papers. I actually worked in those areas. Of that I am very pleased, and I kept telling Nick that is a vital thing to do when you become Director

PSH. That's very true

JE. You've got to really get on with it yourself.

PSH. John. Thank you very much indeed. I am going to turn the machine off now.

JE. OK and you hope it's all in.

PSH. Yes.