

Ursula Mittwoch



Personal Details

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| Name | Ursula Mittwoch |
| Dates | 1924 |
| Place of Birth | Berlin |
| Main work places | Galton Laboratory London |
| Principal field of work | Human genetics |
| Short biography | See below |

Interview

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| Edited transcript available | See below |

Personal Scientific Records

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

Biography

Ursula Mittwoch was born in Berlin and came to England in 1939. She left school in 1943 but was prevented by war-time regulations from attending full-time university and joined the staff of John Innes Horticultural Institution as technical assistant to Kenneth Mather, at the same time attending evening classes which led to a BSc degree. Following graduation in 1947, she entered the Galton Laboratory, London as a PhD student, followed by post-doctoral appointments. She collaborated with Harry Harris and Bette Robson on the genetics of cystinuria, and worked on cytogenetics as well as leucocytes in Down syndrome. After writing a book on Sex Chromosomes she began investigating the genetics of sex differentiation, leading to the postulate that, in mammals, male sex differentiation requires a higher rate of cell proliferation than in females which has since been confirmed. Differences in metabolic rates due to the interaction between nuclear genes and mitochondria are also likely to be involved in different gene expressions between the left and right sides.

INTERVIEW WITH PROFESSOR URSULA MITTWOCH, 2nd MARCH, 2004

PSH. I'm talking with Professor Ursula Mittwoch on Tuesday 2 March 2004 in Professor Sue Povey's Office at the Galton Laboratory, University College, London. Ursula, can you give me an idea what first got you at all interested in science of any kind, but then especially genetics later.

UM. I think I was interested as a child, particularly in biology and animals and, of course I lived in Germany first of all, and they have a book called Brehms Tierleben and I got a copy for, I didn't get all the volumes together. I have a 5 volume edition of that, I think it must have been published in the 1930s, and I got volumes of this on I think, successive birthdays and other occasions and that was my favourite reading. Certainly one of my favourite readings as a child.

PSH. Was that a kind of encyclopaedia of natural history or was it more a periodical?

UM. No, it is a pretty systematic compilation of different animals, starting with invertebrates and then one volume is fish, amphibia and reptiles, birds, mammals and then the last volume was the animals in their environment.

PSH. So each birthday you got another volume or two?

UM. Yes, and maybe sometimes in between. Yes, it didn't take the full 5 years. Something of that nature, yes.

PSH. And then when you went to college or university, that was in Germany still?

UM. No. it was not, no. I left Germany at the age of 15; actually I had to leave school at 14, and so there was a bit of an inter-regnum between leaving school and coming to England, and I finished school, first went to a boarding school in Brighton for a year and school in London; Henrietta Barnett, I don't know whether you have come across the name, in London and I didn't go to college at all because, what with the emigration and, hardly had I entered Henrietta Barnett school, that was in 19, oh gosh I forget now, was it 1940, whenever, when France was invaded.

PSH. Yes. So did your family go first to France and then to England?

UM. No no no no. Not at all. No no we came to England in the Spring of 1939, that was about 6 months or so before war started, world war 2, and then, well when my parents were looking for somewhere to live, my sisters and I were sent to boarding school in Brighton for a year, and after a year I went to Henrietta Barnett school and then, yes was it the fall of France, the invasion of Holland and Belgium?

PSH. Yes, 1940.

UM. 1940. Well that was a cue to intern certain aliens, certain foreigners.

PSH. Oh yes.

UM. OK. Now my parents had been exempt from internment because at the beginning of the war they went before a tribunal and they were exempt. Well, I hadn't been before a tribunal because I wasn't 16 and then of course I was 16 just before and so that was bad luck.

PSH. Oh dear.

UM. So I went to the Isle of Man and my younger sister was jealous, she wanted to, she liked to travel but she had to stay at home and I went off for about 9 weeks and then, you know, they brought this sort of category of, I don't know, children, teenagers or whatever and they brought them back. But it didn't do my schooling any good.

PSH. I had no idea that women were sent to the Isle of Man as well. I imagined it was just men who were involved with that.

UM. Far more men went than women, but it depended to a certain extent, people who lived on the south coast were more likely to be interned than those who lived in London. Anyway neither of my parents were interned. You know the aliens, they got certain categories. Those who were interned immediately. Those free of internment and then there were the in-betweens. So my parents were exempt from internment altogether. And I was the intermediate one. Of course I hadn't been before a tribunal, but I thought it was logical, I didn't protest against it.

PSH. Everyone I have spoken to has been amazingly tolerant of this process. People like Paul Polani and Max Perutz seem to have been, to my mind, extraordinarily tolerant of what they were put through.

UM. Oh I don't know. There was a war on and people didn't know what they know now and so on, and officials had to do their jobs. No I . . .

PSH. So what happened next? When you finished school did you get a chance then to go to college or did you have to go straight into work.

UM. No, because when I finished school I was 19 and then there had been, I don't know whether it was a law, but anyway a regulation, this was to prevent people to go to University instead of doing war work and 19 was too old. I think you had to be 18 and a certain number of months, and I was 19 and in fact 19 and a half, and so I couldn't go. I did take an entrance exam for St Anne's Society in Oxford. They said that I had reached entrance standard, but because of the regulations of the Ministry of War, National Service or something of that nature, they couldn't offer me a place. So then I had to do something for the war effort and was fortunate in finding a place at the John Innes, which was then the John Innes Horticultural College, no not college, it was called the

PSH. Institution.

UM. Institution yes, who were in south Wimbledon.

PSH. Right, that was Merton was it?

UM. Merton, exactly.

PSH. Before it moved out to Norwich.

UM. Before; it moved somewhere else first [Bayfordbury]. But anyway, certainly before it moved away from Merton. And I was there from, oh dear, was it 1943 to 1947 and I was a technical assistant to Kenneth Mather.

PSH. Right, yes. Was he the director at that time?

UM. No the director was Cyril Darlington and Mather was head of, I think it was head of genetics. Darlington was cytology and I think Dan Lewis was something called Pomology – apples.

PSH. Yes. So how long were you at the John Innes then?

UM. Well, I was there for I think three and a half years and went to evening classes with the view of getting a degree. Now these evening classes were to last 4 years and I think I took the last 6 months or so off in order to prepare for the exam, the final exam.

PSH. And your work when you were working at the John Innes with Mather, what did that involve, because I always have the image of Mather as very much a theoretician, but presumably he must have been doing practical work as well?

UM. Yes, there was practical work. Well, there was a question of, and partly also because, for the war effort growing tomatoes out of doors was important. So that involved a certain amount of, well there probably wasn't so much breeding but various ways of cultivating the tomatoes and see what types, first of all how much food they yielded, so I think we did a certain amount of weighing of tomatoes and so on. And somebody else extracted their vitamin content. But then there were crosses of, dear me, Nicotiana, yes Nicotiana certainly and Antirrhinum and also Lythrum. Now Lythrum, I should have checked that. Isn't that the one that has three styles and crosses between these ones?

PSH. Yes, Darwin used to work on it I think, in the beginning.

UM. And of course these are very labour intensive operations. Also maize, of course sweet-corn was an important crop, war effort and otherwise, so you have to bag the, depending on the species, you have to bag up the flowers so the wind or insects can't get at them and you have to emasculate.

PSH. So this went on for three years or so, was it?

UM. Yes, over three years, so that was a lot of outdoor work and then there was, of course Mather was keen on *Drosophila* so he did lots of crosses and so on of *Drosophila*. Bristle number was one of his specialities. Now I wasn't myself involved in the *Drosophila* work. Right at the beginning, I did a bit of making up the medium, cooking the medium. But mainly my main work was in calculations on a sort of semi-automatic calculating machine and he seemed to think I was good at it, so I got quite a lot of that stuff.

PSH. And then did you finish at the John Innes Institute at the end of the war or.?

UM. Yes. Certainly, oh definitely the war had ended by then, yes. Of course the war ended in '45. I went there in 1943 and I graduated in 1947, so I left early in 1947 after about three and half years.

PSH. And what was your next step?

UM. My next step, ah yes. I was hoping to get a degree, and the war ended, and that seemed a good opportunity to make up for my lost education. So the idea was to work for a PhD. By then, I was quite interested in genetics, and I filled in an application form to become a PhD student in genetics at University College London, giving mycology as my second choice, which was asked for. The application went to JBS Haldane, who said that he didn't have any room, but that Hans Kalmus had expressed an interest in fungal genetics. Kalmus was then with Penrose's group in the old Galton Library.

PSH. So Kalmus by then would have left JBS Haldane's group and moved across, because I seem to remember he started off with Haldane and didn't quite get on, is that right?

UM. This could well be the case. Yes he was with Penrose. Penrose of course had come in 1945, or was it 1946, after the war?

PSH. Yes.

UM. Certainly he was here before me. And Kalmus was, well not actually in this building but in the old Galton. I don't know, are you familiar with that?

PSH. Yes.

UM. Yes. So I went there for interview. There was Haldane, Kalmus and Penrose, and I rather felt they were all talking at cross-purposes, but anyway Penrose said yes he had got room. I mean he had only just started up, but I think neither he nor Kalmus knew anything about PhD students, and Haldane said, well, Miss Mittwoch first has to graduate, which I don't think occurred to them particularly. Anyway I got this degree. I got a second class. There wasn't any upper or lower.

PSH. Same as at Oxford.

UM. It was lucky in a way, because I hadn't even realised that one had to get a good class you know. I went to these evening classes just to pass. Anyway I don't think it would have mattered really. There weren't any rules and PhD students, obviously there hadn't been any in the department. So this is where I started off.

PSH. So you were the first PhD student here?

UM. Yes. And yes, I was given a desk and a waste paper basket and told well, do research. Kalmus said, what I ought to do is to get a bucket and spade and collect horse dung, and I wasn't at all sure how one sets about collecting horse dung but I thought, you know in order to be a PhD student one has to be inventive, and so I got on a bus and went to a riding school up in Mill Hill, and I mean they all thought it was rather amusing. Anyway I got this horse dung and my mother gave me some pickling jars, and lo and behold, some *Coprinus* came up and I collected the spores and started off some cultures. I think it was pretty fortunate that I had that training or experience at the John Innes, of how a laboratory is run and how to deal with matters, because really there was very little supervision – hardly any.

PSH. I guess you had been taught to be methodical.

UM. Methodical, yes, I mean that you have to keep stocks, yes, and keep notes and that sort of thing.

PSH. So what did you do with the fungi when you had got them?

UM. They grew. Oh I see, when I got them, I collected spores and they were seeded out. I should say that fairly early on in that procedure, Kalmus arranged for me to visit Pontecorvo's lab in Glasgow to learn some microbiological techniques. It was rather strange. I talked about it recently with David Wilkie, because he is a Pontecorvo person. It was really pretty early on in Pontecorvo's career, so he was advertising this course. Really it was meant for senior biochemists. He was working on *Aspergillus*, and actually his technique hadn't been standardised by then, so you plated out these *Aspergillus* and you came back in the afternoon in order to do something else, but they hadn't grown yet and so you had to come back later. It worked up to a point, but it hadn't been properly standardised. But I learnt some technique and particularly also from the other students, who were the senior biochemists from industry, and they said, you know "I'll show you a trick", and I learned these tricks, so I went back and I knew how to plate out spores into culture medium. I got mycelia and Kalmus arranged for me to have them irradiated by somebody in anatomy, X-rayed, and true enough it got morphological mutants which I crossed. I got some results; they segregated, also this *Coprinus lagopus* has four mating types, so you had to put together compatible mating types.

PSH. Did you end up publishing that, because my indexes only go back to the early 1950s; probably it would have been before that?

UM. Yes, It must have been roughly 1950, at least two papers, yes.

PSH. And when did you first . . .

UM. The genetics of *Coprinus lagopus*.

PSH. So when was it that you moved from studying the fungi to human types of work.

UM. Well, after my PhD, and Penrose asked me to stay on, and the reason for that is he wanted some work done on chromosomes in Down's syndrome. He was of course a Down's syndrome person. Grüneberg advised me against it. He said, you know the professor doesn't know about chromosomes, but I thought if

Professor Penrose asks me to stay on, then he must think I am good, which was a mistake to think that, I think, but he obviously thought I would be useful.

PSH. Yes, and it was an opportunity.

UM. It was for me?

PSH. Yes.

UM. Well yes. I mean I could no doubt have done other things. I remember that the external examiner, Ingold, he was a fungus expert and he said "Oh what a pity because there are so many people working in genetics, but fungi". However, I thought, the Galton Laboratory, University College, and this is how I got roped into the chromosomes of the Mongol. Which I didn't enjoy at all. Not at all. I mean chromosomes altogether. Chromosomes, occasionally they had some other people, I mean men with testicular biopsies and I had to go to some hospital to collect them. I was always very pleased when they didn't turn up. But anyway, so that was my next project but that wasn't the only thing I did.

PSH. You must have been the first person to do chromosome analyses at the Galton?

UM. Oh sure, yes yes. At the Galton, yes.

PSH. Because really they were only just getting going, if going at all, anywhere else at that stage were they?

UM. They weren't really being done at that time, I don't think. Well maybe in the States.

PSH. Because Charles Ford didn't really start looking at human material until later on.

UM. Oh, that's right. It was really after Tjio and Levan in 1956. Oh yes, yes. And it certainly wasn't something I enjoyed doing and it was of course, in some ways it wasn't a full time job, because the material wasn't really available. Also, the other reason why I was useful was that Harry Harris had

been to Italy and was interested in establishing a microbiological technique for measuring lysine and I think arginine and

PSH. And cystine?

UM. Cystine is a different type of amino acid and I really have to check that.

PSH. I found one of the papers, but I'm not sure which one it was, but I was interested because, would I be right that Harry Harris was at the Galton, because I thought at some point he was over at King's College.

UM. Oh that was later. Kings? I think that was much later. He went also to the London, but he was also in Biochemistry at UCL. The problem with the Galton was that they didn't really have jobs, or if they had them they didn't sort of give them away.

PSH. No. I've got the papers here. It says excretion of amino acids in cystinurics. But I guess it was probably quite a few different amino acids.

UM. Lysine and arginine, are they the basic?

PSH. Dibasic. Yes they are.

UM. Now cystine is different. Anyway, so you had this lysine and arginine deficient moulds. I'm sorry I don't even remember what species it was, but . .

PSH. I don't know either, I'm afraid.

UM. Anyway you had this agar plate, you had these deficient organisms and then you added the lysine or the arginine to the medium. Well what you do is, you made holes in the agar, and I used to put in urine samples from patients and then you let them grow for a bit and then you measured the diameter of the growth against, well some illuminated object in the dark room, yes with calipers and I did this for, I don't know how long, at least a year and then I thought, really one doesn't need a PhD for that. But that is the problem, nobody had any money. The idea of employing any, you know.

PSH. Can I ask, were you getting paid when you were at the Galton, or were you expected to live on thin air?

UM. Well no. I got, was it a stipend? I got something. Penrose was quite good. He had connections and he got some money from Rockefeller and so on, got perhaps £250 a year or something like that. So I didn't live on thin air, but not very much.

PSH. Not much left over.

UM. Not much left over, no. And of course there was no development, so it was not at all certain what would happen next year, and so usually nothing happens and sometimes he said to me "Oh it's because you don't bother me". Other people asked him for more money. But I thought, surely the professor

knows I would like some more money but if I sort of pester him he would be even less likely to give me some. Yes there was a small salary, or whatever you call it, and I suppose in a way it must have been administered through College, yes, after a while, yes.

PSH. Can I ask was there any kind of well set-up laboratory, like a cytogenetics laboratory or a biochemical laboratory at that stage, or was it just some, did you just have a general laboratory where you did any experiments that needed to be done?

UM. Well, I think the fungi, I think a lot of the work I did in my room, and then there was a larger lab with autoclaves and so on, and I suppose bits of apparatus were there if needed, accumulated.

PSH. I was thinking of microscopes and that kind of thing, were they really good, because, especially if one was having to look at chromosomes, that must have been quite a big factor, having a really good microscope to look down?

UM. Where was the microscope? Yes, I can't remember where this microscope came from. There certainly was one and I don't think that, I mean there certainly was a microscope, yes. I remember my first incubator was a gas incubator for the fungi and that was in my sort of office.

PSH. Yes. So just pausing a moment before we leave that stage and go onto your work on drumsticks. Is it fair to say that really you only did a small amount of actual chromosome studies and that you didn't regard that as your main area, or was there a lot of work and just a lot of technical difficulties?

UM. Well, there was a fair amount of work from time-to-time when a chromosome donor became available, but it wasn't all the time. And then of course the Down's syndrome one was, well that was organised by Penrose, clearly. Yes.

PSH. But that was, the first publication was '52 or thereabouts.

UM. Yes that's right.

PSH. And then you were also looking at there was a paper in 1959 I think, the patient with Down's and Klinefelter's that you were involved with so . . .

UM. Yes. Was it as late as that?

PSH. I think it was the year everything seemed to happen, in 1959.

UM. Yes that's right. Yes. Yes.

PSH. So would you have been looking at occasional samples over the years as they came in, so to speak, or was there a time when you ever did quite a big special project on the chromosomes?

UM. No, I don't think there was a time when I did a special project. The Down's syndrome and Klinefelter came about after the Tjio and Levan paper. And also of course, there had been quite a lot of sex chromatin work done by then. So Penrose got what's his name, Michael something or other, in Harperbury.....

PSH. Michael Ridler?

UM. Michael Ridler, in Harperbury, yes to look for I think, drumsticks in male patients and one of them was this Down's syndrome patient, so that caused of course a lot of excitement and various people were alerted and organised; I think Charles Ford did the chromosomes on this and I just handled the material before it was sent there. I was quite pleased that it worked. I mean that the material was still in working condition. Now one of the visitors then was Jack Miller, who was a visiting American and he was really lucky, very fortunate he was in on that.

PSH. Because he was an author on the paper I think?

UM. He was an author on the paper, but he was really lucky yes, he came at the right time.

PSH. Can I ask, did you have much contact over those years with Paul Polani, because Paul came up, I think, to learn his genetics from Penrose, I suppose it must have been immediately after the war, and I wondered whether you had had much contact with him when he was at Guys in those years.

UM. Much contact, I don't really think so. Do you mean me personally or the departments or . . .

PSH. I was thinking with the work on sex chromatin perhaps, because he was also involved with studies on that before.....

UM. Turner's yes.

PSH. Chromosomes were possible.

UM. I don't think so, no.

PSH. And Charles Ford at Harwell, the Galton as a whole probably had quite frequent links, or maybe not?

UM. Well there were links, but I don't know that they were particularly frequent. I went there for I think, maybe two weeks to learn some techniques. I do not recall exactly at what stage it was, but it must have been before certainly the 1959 paper.

PSH. Looking at your papers, there seemed to be quite a long period where you were analysing the drumsticks in leukocytes, and I was wondering what started that work off, so to speak?

UM. Oh yes. Now that again was Penrose's interest in Down's syndrome, and there was a paper by Turpin and Bernyer about the segmentation of neutrophil leukocytes in Down's syndrome, that there was, I think it was called 'shift to the left', too many unsegmented or too few segments anyway in these patients. Well Penrose had some connections with some Down's syndrome families, and sometimes on a Saturday the parents would come with their Down's syndrome child, and I suppose he liked to have somebody around and asked me to look at the leukocytes, which obviously wasn't very informative because I just had these blood films and no controls. And I wondered at one point whether there were drumsticks in males and went off to see, Davidson, what was his first name, anyway he was the one who discovered, Davidson and Robertson-Smith discovered the drumstick.

PSH. Was this after Barr and Bertram's work?

UM. Oh yes. Oh definitely. Barr and Bertram was about '48 or something like that [1949] and the drumsticks were, I'm guessing, about mid fifties [1954]. Yes. And he really put me right in what was and what wasn't a drumstick, and also how to make blood films, and he made them between two cover slips and you got much better films that way than if you pushed them around on a slide. In any case, you got, I think one type of cell is the polymorphonuclear neutrophil leukocyte. So furthermore I knew how to make blood films and what to look for, and then there was Valerie Cowie, have you come across her?

PSH. Yes

UM. Is she well?

PSH. She worked in Cardiff for a time and I haven't heard from her for a year or two now, because after she retired she went back to, I forget quite where, but yes I had a lot of contact with her at one point.

UM. I was just thinking about her, really.

PSH. But I haven't heard for a year or two.

UM. For a year. I haven't for longer. It could be my fault.

PSH. But how did the drumsticks quite lead you into the area of sex determination and the whole area of intersexes and the relationship between drumsticks and sex, because . . .

UM. In addition to, well drumsticks of course were related to sex chromatin and we also measured drumsticks from different sexual abnormalities; Marco Fraccaro of course. He was at the Galton for a while and, after he returned to Pavia, he sent me some films, and I measured the size of the drumsticks and they were coded, and found there was a correlation between the size of the drumstick, of the head of the drumstick and the X chromosome constitution and that the iso-X, the large X had large drumsticks, the deleted X, small drumsticks, and this was a good verification that the drumstick is related to the X chromosome. Now for some reason I did also some work on fibroblasts, I

don't quite know how this happened, and on Barr bodies, a sort of relation of the drumstick, and then I was invited to write a book on sex chromosomes. Early 60s and I thought I would like to do that. I mean there were lots of discoveries about, well, sex chromatin and X inactivation and all the sex chromosomes in different animals. But I felt that in a book of sex chromosomes, one should also say something about the role of the sex chromosomes in sex determination; so when I wrote a synopsis of the book before I started writing it, the last chapter was going to be the role of the sex chromosomes in sex determination. And I spent many hours in the library, and I saw what had been written wasn't very convincing.

PSH. Yes.

UM. So it is really in some ways, the same all over again, it happened in the last 10 years or so. Very often people start off with some idea, to have a gene or other, and then you have to have additional genes and making more and more assumptions, and I thought this wasn't satisfactory. And I am not entirely sure why it occurred to me that it could be that the male gonad grows faster than the female; partly of course it was known that the male gonad differentiates earlier so it grows faster, a faster type of development. And I thought I would like to test that, which couldn't be done in the Galton because we didn't have any animals, and I managed to get some collaboration at the London Hospital. Eventually we did it with rats, and Felix Beck, who was a reader at that time in anatomy, I don't know that he was all that keen on this research, but he agreed to collaborate. Anyway he managed to, we got the rat embryos at the right age. Joy Delhanty did the chromosomes, is that correct? Yes it is, because I had an idea that with rodents, the sex chromatin doesn't really work, which isn't really the case necessarily, and we had a pretty primitive set-up of projecting the sections onto a screen and measuring them. And true yes, it turned out that the XY gonads grow faster than the XX gonads, before Felix Beck, who was an anatomist and embryologist, could see any histological difference.

PSH. Did that lead you on then to your interest in intersexual states, hermaphrodites and related things, or was this an interest which you already had all along the way?

UM. Well, as regards true hermaphrodites, it was certainly published that the ovaries are more often on the left than on the right. That was one reason why we looked at human fetal gonads. And in addition, of course, the question arose whether human fetal gonads are like those of rats, whether testes develop faster than ovaries (which we found to be so). Our first publication attracted much attention from journalists, who evidently liked the idea of a new theory of sex determination depending in the growth rate of cells, with male cells growing faster than female ones. But the idea was not popular with scientists, and one objection was that the bigger size of the testis was a result of testis differentiation, for instance testosterone production, and not its cause. So I thought that if one could relate the asymmetry of gonad differentiation, rather than fast growth being the result of testis differentiation. We found that right gonads did in fact grow faster than those on the left. However, the idea that laterality might be involved in the choice of genetic pathways was even further removed from the thinking of molecular geneticists of the time.

PSH. Yes and all the molecular work has confirmed it since.

UM. Oh yes. Well the molecular work has confirmed it since. But I don't think that has made the hypothesis more popular among molecular geneticists.

PSH. That's fascinating. Ursula, I don't want to tire you, I probably have already, but I would love to finish if I may, because you were at the Galton for such a long time, have been and still are,

UM. A very long time. Well yes,

PSH. It would be lovely to get a bit of an idea from you about some of the other important people who were here, and just from a personal perspective, Penrose I suppose is such an important person, I mean, what impressions did he leave on you?

UM. Well first of all, I mean I think I gained an enormous amount, my whole approach of quantitative investigations were definitely shaped by the Galton. It obviously hadn't occurred to any of the molecular geneticists or indeed any of the geneticists who work in sex determination, and on the contrary, I think they thought it was a complete irrelevance, that quantity was nothing to do with it.

PSH. So the classical genetics approach from Penrose really was something which you were able to make a contribution, where the others weren't

UM. I am sure they wouldn't agree with that, but it has, yes I think it has certainly directed my approach, yes.

PSH. I mean, one thing, reading about Penrose, I always get the feeling that he was, although very supportive, not easy to approach and tended to leave people alone unless there was a problem. Was that a problem? You mentioned the finance but in terms of directing your work, did you feel you got the backing and support you needed? It's very different to how things happen nowadays.

UM. Yes, very different. Very different. Well no, he wasn't necessarily supportive. He was, if I went to him with a question, a scientific question, he could be very helpful. Very helpful. But in practical things I wouldn't say that he was, either to me or to anybody else, in the department. And in fact in a way he didn't do anything. Sylvia left and I don't know that she had, I mean she certainly couldn't have made a career here, and neither Jim Renwick.

PSH. Did you overlap for quite a long time with Jim Renwick I suppose?

UM. Oh yes. Yes.

PSH. Because he is a person who I knew well, but again he's not around to tell the story himself sadly.

UM. No and he got really nothing. I don't know how he, I have no idea what happened when he collaborated with Sylvia on the linkage. He was around but, no idea. I mean the College, if I go through old calendars, presumably they must have some, they must tell you something what his status was, but certainly he never had a proper job, a proper appointment, so he went to Glasgow. He worked with Ponte before going to the School of Hygiene.

PSH. And then Kalmus. Who I mean, did you work with him after your PhD?

UM. Well I didn't work so much with him during, because he went to Canada after one year or so. During my PhD our collaboration was quite limited, very limited. Although as I say he set me off at the start, he told me what to do and also got the fungi irradiated, but other than that no. Later, we did collaborate on something on honey bees - yes. Because I managed to acquire an integrating microdensitometer with which to measure the DNA values of individual cells. And of course he was interested in honey bees and we looked at the larvae of the males and the females, the male haploid, females diploid, and that was published, I mean you have come across it?

PSH. I have actually, yes, but I had forgotten that Kalmus was involved.

UM. Oh yes, yes, yes. I presume he got the larvae from Harpenden of course, yes.

PSH. And the other person of course who was very much around I suppose would be Hans Grüneberg?

UM. Well he was less around in as much as at, to begin with of course he was over with Haldane and then he, let me think, was he in the old Galton, of course he was in this building on the top floor.

UM. Yes. But he was certainly in this building, but there was a limited amount of connection, not a tremendous amount.

PSH. If you look back over the years, was there one particular person you could identify that had a particular kind of influence on your work and how it developed?

UM. I think it must be Penrose.

PSH. He seems to have influenced so many people, even without doing anything very much.

UM. Well yes, he influenced people who were sort of ready to be influenced and I suppose this quantitative, rather objective approach suited me and yes, I think science should be exact. I tell you somebody who was very helpful, that was Cedric Smith, who would always help with the statistics and I sometimes think that if he hadn't been around and so helpful I might have been more independent, learnt more statistics but he was accessible.

PSH. One last question, Ursula, which piece of work over the years that you have done gives you most satisfaction to think back on?

UM. I don't think I can answer this question on the spur of the moment. I remember when I sent off the Nature paper, which came out in February '69, I thought, I wonder whether this is the best paper, the best work I have ever done.

PSH. Perhaps I'm not phrasing it properly. I suppose I was thinking, not perhaps of a paper or an individual experiment but, you've worked in a number of areas and I was wondering which area of work that you have been involved in is the one you feel you've made the special contribution in.

UM. I think it must be the sex determination. I am right now in the process of correcting proofs of a paper, which I think is potentially important, suggesting that SRY and other testis-determining genes increase the metabolic rate via the mitochondria, so . . .

PSH. That's not so far removed from your work on the growth rate.

UM. No it isn't. It is definitely a development. Its just 30 odd years later.

PSH. Certainly that is the area I always associate your work with most and so it doesn't surprise me you feel it gives you most pleasure.

UM. No, I regard it as my life's work, yes.

PSH. Yes. Well many thanks Ursula, I am going to turn the machine off here and I am very grateful.

(end of recording)