Jan Mohr

Personal Details	
Name	Jan Mohr
Dates	1921 – 2009
Place of Birth	France (Paris)
Main work places	Oslo, Copenhagen
Principal field of work	Human genetics
Short biography	See below

Interview	
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Biography

Jan Mohr was born in Paris to Norwegian parents, and was brought up in Norway from the age of four years. He qualified in medicine from Oslo University in 1948, after interruption due to World War 2 and had already become interested in genetics by this time. He then studied genetics at Columbia University, New York, and the Galton Laboratory, London, where he began research on blood groups and genetic linkage analysis, before taking a post with Tage Kemp in Copenhagen in 1951.

In 1954 he was appointed, initially as lecturer, later Professor in Medical Genetics at Oslo University, before succeeding Tage Kemp as Professor in Copenhagen in 1964, developing the Institute and remaining there until his retirement. He spent his final years in Oslo.

INTERVIEW WITH JAN MOHR, 5th SEPTEMBER, 2005.

PSH. Jan, I have really no structure for these conversations, but there is a lot I don't know, so can you tell me where were you born and brought up.

JM. So your history is going that far back?

PSH. I find it's easier if we start at the beginning.

JM. I was born in Paris. My parents are Norwegian, but they were staying in Paris in connection with my father's painting. He was a painter. But I have grown up in Norway.

PSH. Yes. So, can I ask what year you were born?

JM. 1921.

PSH. 1921, and when you came to Norway how old were you?

JM. That was a little back and forth, so at four. At four years of age we settled in Norway.

PSH. And may I ask, you say your father was a painter, but was there any particular family influence that led you to medicine?

JM. That's difficult to say because my father had several brothers, and each of them had an interesting field where they excelled to some extent – architect, author - but I had an uncle who was a professor of anatomy.

PSH. This was Otto Mohr?

JM. That was Otto Lous Mohr.

PSH. Maybe I can come back later to ask a little more about him?

JM. Yes. So that is difficult to say. I started out in an entirely different field in astro-physics.

PSH. This was at the University here in Oslo?

JM. Yes.

PSH. And what made you change to medicine?

JM. That's very difficult to say, but partly my friends, my family, my cousins, some of my friends had studied medicine and I became, perhaps in part it had something to do with social engagement. I felt that maybe that astro-physics and astronomy, celestial mechanics, as you got down to the daily work in it, it was a bit dry and distant from daily human matters.

PSH. So you trained in medicine here in Oslo. What year was it you qualified?

JM. 1948.

PSH. 1948. Were your studies badly interrupted by the war?

JM. Yes, for some years.

PSH. Because this must have been a very difficult time, whatever people were doing. So 1948, did you have at that time any idea as to which particular area of medicine you wanted to follow?

JM. Yes, in 1948 I did. But I did not when I started the medical study, but as it happened during the war I was a refugee in Sweden and there was a Norwegian soldiery established in Sweden, but then in the summer vacations we were allowed to study at the university and I studied genetics with Professor Bonnier, Gert Bonnier.

PSH. This was where in Sweden?

JM. Stockholm University, or rather at that time it was called Stockholm College. Stockholm Skola. And there was a research station, a practical research station for raising cattle and pigs and other animals and we went up there for some weeks in the summer and I took some examinations and then, let me see, when I approached the end of the medical study, I found that I was very close to knowing enough to take a degree in genetics in Stockholm. There was no degree to be taken in Oslo. So then I went during the last term, for some weeks to Stockholm and graduated in genetics there, so when I finished my medical study I had those two.

PSH. Was there any human genetics at that time in Stockholm, or not really?

JM. No, that would be in Uppsala, rather.

PSH. I was going to ask, did you know Gunnar Dahlberg?

JM. I met him, but I didn't really know him.

PSH. So you came back to Norway and completed your studies and then, did you have to then do clinical specialisations or . . .?

JM. At that time it was not required really. One was supposed to have done enough that you understood, and I think I was only four months in clinical medicine before I got a stipend from the Rockefeller foundation, so I studied two years in the States and later at the Galton.

PSH. Where were you then in the States?

JM. At Columbia University.

PSH. Oh you were. And at that time who was there at Columbia?

JM. L. C. Dunn.

PSH. Had he taken over from Morgan or was it a separate unit? I don't know that.

JM. I think he had taken over from Morgan. I don't know if there was anybody in between.

PSH. Because he must have been a very interesting person to work with.

JM. Oh yes.

PSH. And again somebody with very strong outspoken social and political views, I think.

JM. Yes. Yes.

PSH. Can I ask what was the topic of your work?   Did you have a specific project that you made a thesis from at that stage?

JM. In that lab everything was about mice. It was really more study than research at that time, but I also went through a phage course at Cold Spring Harbor.

PSH. And so that was two years was it?

JM. One in Columbia and one year after, at the Galton.

PSH. At the Galton yes. It has interested me so much talking with people, how many people have spent time at the Galton and, what was your experience at the Galton?

JM. I liked it very much. That surprises you?

PSH. No, not at all. Not at all. I think most people liked it that I have spoken with and have been hugely influenced by it, but it was a very unusual place I think.

JM. That's right, I was kind of warned in advance by this fellow from the Rockefeller Foundation. Mr Pomerat.

PSH. Oh yes. So did you go knowing that you would be rather left on your own and not have detailed supervision?

JM. Maybe it wasn't stated that clearly, but I expected something like that.

PSH. Did you have a particular piece of work in mind, or did you really just go to learn from the people there?

JM. I just went to learn and blood groups were the thing, another thing I was put on to. Of course one year isn't nearly sufficient to do any field work, that kind of thing.

PSH. What was your impression of Penrose?

JM. I liked him very much and he was always entertaining in a way and he always had these little tricks going on. I don't know whether you have seen these.

PSH. His models?

JM. His reproduction toy.

PSH. Amazing, quite amazing. And so you went into the blood group side. Who was doing at that time . . .?

JM. Lawler.

PSH. It was Sylvia Lawler. Because I think Race and Sanger were still at that time at a different centre, is that right, or maybe . . .

JM. They were. I think they had moved two years before or something.

PSH. So there was Sylvia Lawler. Was Jim Renwick also there?

JM. He was not there, but sometimes in and out.

PSH. And were there any other people who were studying with you or at the same time?

JM. Harris was there.

PSH. Yes.

JM. And then of course there was the mice department farther down.

PSH. That was Grüneberg.

JM. Yes.

PSH. So you had a year in the Galton. Was that really where your interest in blood groups as genetic markers began?

JM. Yes, and I was attracted by linkage studies and partly because so much in human genetics at that time was rather diffuse and unclear conclusions and all that, and linkage seems to be a possibility of getting stringently based results.

PSH. Was C A B Smith there at that time?

JM. He was there.

PSH. So am I right, this must be around 1950?

JM. Yes.   It was directly after the stay in the States.

PSH. So did you actually, at the Galton, start some specific linkage study?

JM. No.

PSH.   This developed later when you returned?

JM. Yes

PSH. How did things develop after your time in the Galton? Did you have a research post to come back to, or did you just have to start somewhere?

JM. No, I had an in between station in Copenhagen and partly, that was because I knew about the material that had collected there. So at the end of the stay at the Galton, of course I had it in mind to apply the linkage methods to the Danish material. But I considered a number of different topics for my thesis, you know medical degree, and I worked in various project and then I had this research with Lutheran-secretor linkage and branched out to cover all the material that was suitable.

PSH. So the Lutheran-secretor work, that was in with Kemp in Copenhagen?

JM. Yes.

PSH. And was there something already existing in terms of a blood group lab there, or did you start it when you went there?

JB. No I started it there.

PSH. And I'm very interested in . . .

JM. There was of course a blood group unit at Copenhagen, but not at the Institute.

PSH. And not doing linkage?

JM. No.

PSH. At that time there weren't so many genetic markers. How many did you have at the beginning?

JM. Oh, everything available, but still of course it was kind of undeserved that I found it, that relationship.

PSH. Was that a great surprise to you?

JM. Well in a way, of course I thought about this, I had been lucky you know, but I had started various projects in parallel and the probability of finding something in one of them.

PSH. OK. And am I right, at the time you made the discovery, it was really thought to be the Lewis blood group.

JM. Yes, but it was entirely clear at that stage that it was always synonymous with the secretor. It was thought about in the way that you could score the secretor by typing for the Lewis.

PSH. Were you using the Penrose sib pair method for your analysis?

JB. Yes, in the first place.

PSH. No computers or anything then?

JM. No.

PSH. And of course, from my own perspective, I'm very interested in how you also became involved with the analysis on myotonic dystrophy.

JM. That was one of the studies that had been carried out at the Institute in Copenhagen, so the material was there. I was driving around you know and collecting blood samples for it.

PSH. So you had the family information, but you yourself had to go around and visit the families.

JM. Oh yes, I collected all the blood.

PSH. You must have learnt a lot about myotonic dystrophy in passing, at that stage.

JM. Not so much really, because I focused very strictly on . . .

PSH. And my understanding and reading from that is that there was this rather small hint of linkage with myotonic dystrophy, but nothing very conclusive.

JM. Well it certainly was sufficient to make me very suspicious. It was also the sib method, it couldn't really get hold of all the information and there was a little bit more, my impression was. And then as you know, Renwick later on, with a better method from the same material, got a significant result.

PSH. When you completed that study, am I right that your first paper on Lewis, Lutheran and secretor was about 1951 or something like that?

JM. Yes that's right.

PSH. And then came your – I don't know was it a thesis – your book? I remember reading it when I was starting to work in this field myself.

JM. At first I got this Lutheran-secretor-Lewis linkage and then of course it was a very natural expansion to go to all the other families where you could get linkage information, and that took maybe a year to collect all that, and then that was a thesis for the Copenhagen University.

PSH. And how many other disorders? I know you looked at several other disorders, I think?

JM. I don't remember exactly, there was the cataract, myotonic dystrophy, Huntington's chorea, but the material wasn't really large enough. And then Thomsen's disease, that was very small also and, what was the other one?

PSH. Was it osteogenesis imperfecta?

JM. Of course it was, yes. Osteogenesis imperfecta. But there was some indication with the ----[?]---- I think.

PSH. So how long did you have in Denmark at that stage?

JM. It is not quite easy to say, as I also did in later years handling Tage (Kemp) in between Oslo and Copenhagen, but maybe 1½ or 2 years perhaps.

PSH. What was, you pronounced it, Taye?

JM. Tage, or in Danish Taye, Kemp It's very difficult to say in Danish.

PSH. How was he as a person to work with?

JM. He was easy to work with, in a very different way from Penrose. Both let you do what you wanted to do. Well of course the linkage was not Tage Kemp's interest. He was very much more practically interested in the counselling. All these practical aspects, that was his main interest. Of course his systematic collection of material.

PSH. Yes, I'm interested that people like him formed some kind of bridge between basic genetics and the first steps in human genetics, and it's very interesting talking to you, who remember people like him who go right back to the beginning. As an institute was it quite big already at that stage?

JM. Physically it wasn't very big, but there were a number of persons coming in and doing a thesis and then leaving again.

PSH. Were these people from different medical specialities?

JM. Yes.

PSH. That's my impression from the monographs which came out, that there were all these different specialties and they came for some years and worked on genetics and went back again.

JM. That was typical. There weren't many positions at the Institute. There were two and some secretaries, and fund financed assistants.

PSH. Had cytogenetics developed there at all at that point?

JM. No. Not at all and it was even discussed very seriously later on, while he was still there, whether it ought to be included under human genetics at all!

PSH. Did you have much contact with the people in Lund, such as Albert Levan and those folks?

JM. Not very close contact, but I visited. They had a little society, I sometimes visited there, so I knew some of the people there.

PSH. When was it you went back to Norway to a definite post?

JM. I will have to look up that in the papers, but let me see. You see, I also had the military service in between.

PSH. I see.

JM. Actually I wrote the final edition of my thesis while I was in the military. Maybe it was 1954 that I had a position.

PSH. Was this a position specifically to develop human genetics, or was it some more general department you were in?

JM. No it was more general. You see there was no institute or unit for human genetics in Norway at that time. There was a unit for general genetics, which had been there since the 1920s or something, but that was the initiative there was from the zoological side, Professor Bonnevie, Kristine Bonnevie. She was a very strong professor in zoology, and there was great emphasis on maintaining unity in genetics between animals, plant and human genetics, partly with a view to stringency. It was thought that these more stringent approaches from Drosophila might somehow influence what you did in human genetics. Partly the motivation may have been these exaggerated racial concerns. There was some anti-movement against those and that was also reflected in the Norwegian Society of Genetics, a general society where, from a very early stage, there were strong reservations against those things from Germany. But it was very general you know at that time, you know the racial, very directly from Darwin and Galton and so on, but then distorted of course on the way.

PSH. Am I right that your uncle, Otto L. Mohr was also very against the eugenic applications?

JM. Yes, and you probably also know that there was a particular person, Mjøen [Jon Alfred Mjøen]

PSH. I heard about him, and so I can understand that must have caused a lot of tensions.

JM. He lived 300 metres from here, by the way.

PSH. So when you went back to Oslo then, there was Kristine Bonnevie in zoology and Otto Mohr was Professor of Anatomy, is that right?

JM. Yes now he, let me see. Kristine Bonnevie was not alive any more then. Either she had retired or had died, but there but it was Føyn another zoologist, her successor, Bjørn Føyn Professor of Zoology, he was director of this journal, institute and there were two positions, amanuensis they were called, and I got one of them and a Drosophila geneticist got the other one. But then, of course I may say I felt from the experience, when you look some years back, it was not a good arrangement any more. It was directly under the Collegium of the University, not under any particular faculty. Neither the mathematics etc, or the medical faculty. It was directly under the Collegium. As you might believe this would give it a particularly strong position, but that was not the case. So I worked from the start to split it up. So very early I got it split into a human genetics laboratory and a Drosophila laboratory. Then gradually I got it transferred to a regular formal institute for medical genetics, and at the medical faculty of course.

PSH. So were you able to do that during this period when you were in Oslo, before you went back to Denmark, had you established a medical genetics faculty in Oslo?

JM. Oh yes. I was ten years in Oslo before I went to Denmark, and then I also was promoted then from this position and I also promoted the establishment of a regular chair in medical genetics.

PSH. This must have been one of the very first chairs in medical genetics in Europe.

JM. Well, of course Tage Kemp already had one.

PSH. I didn't realise that was specifically medical genetics.

JM. Well it was termed something else. It was termed a chair in, it's difficult to say these days, but human biology and eugenics I think. I'm not quite sure, but it was not, I think you are right it was not called Institute of Medical Genetics. It was something, perhaps Arvebiologisk Institute, *hereditary biology*. You see these names are different from time to time.

PSH. They change their meaning.

JM. Oh I remember over the door it was Arvebiologisk Institute, Hereditary Biology Institute so I guess his chair was named like that.

PSH. What was it that made you return to Denmark rather than to stay in Norway?

JM. Now that's a very interesting question. Partly it was personal because of family relations and so on, and also I had a strange idea at that time, that 10 years should be enough in any position.

PSH. It's a good idea, but it often does not work in practice.

JM. It doesn't, but also they were rather late in finishing the establishment of this chair, and very shortly before they had finished it, I got these initiatives from Denmark. They wanted to have me there and I actually said yes. Then Professor Alf Brodal, he was Dean of the medical faculty here, discovered this and he called me in and he said he had a unanimous faculty in Oslo, which would like to call me to this chair. And possibly, had it been today, I might have said to the Danes that things have changed and so on. The whole situation is different now, but those days were ethically higher, so I had already said yes.

PSH. And also it happens so often that it's only when you go somewhere else, that people then promise you.

JM. Yes.

PSH. It makes them think.

JM. But I think it's 99% certain that if this Danish thing hadn't been there, I would have automatically gone into the Professorship.

PSH. Did you directly follow from Tage Kemp?

JM. Yes.

PSH. . . . or was there somebody else in between.

JM. No. There was a little gap in between. He died and then one, two or three years perhaps.

PSH. So when you returned to Copenhagen, am I right then you then developed your genetics laboratory on a bigger scale than you had been able to before?

JM. Yes, of course the resources in Copenhagen at that time were much larger, so it was in a way a larger position. It had of course been rather heavy working when I was very near establishing the Institute on a big floor of what is now the biology building here. The top floor there was originally assigned to me or to the Institute of Medical Genetics, but then you know, all these political things you know, so suddenly I didn't get that after all. So although ten years - it seems a short period now - I felt it would be good for somebody else.

PSH. When you went back to Copenhagen, am I right it was around this time when the protein genetic markers were adding to the blood groups.

JM. Yes. The enzymes, yes.

PSH. So who came to work with you in those areas. Was Kåre Berg one of the first?

JM. My first connection with Kåre Berg was here in Oslo. He was working at the Institute of Forensic Medicine and then he discovered the Ouchterlony method, you know, this precipitation of gels, he discovered the Lp(a) system. Then I had my family material, and so I kind of took care of the genetic part of that. We published that together then, 'The Genetics of the Lp(a) system'.

PSH. Did he come to Copenhagen to work for a time or was this in between you in Copenhagen and him in Norway?

JM. No, the thing with the Lp(a) system was entirely here in Oslo, and with Norwegian material. You understand I had also established a little family bank here, so I had ready samples from a number of families, so I could very quickly test in families his Lp(a) system. I had the samples, I sent them to him, he tested them, I analysed them. By Cedric Smith's stringent method.

PSH. Then one of the people who must have come quite early to join you was Hans Eiberg?

JM. In Copenhagen, yes. I hired him partly because it was very important at that stage of course to strengthen the biochemical side of the Institute and he was educated as a biochemist. The real reason I hired him, I think, was that he was so clever at making orchids sprout.

PSH. Oh really.

JM. It was a very difficult thing to do. He was very inventive in such things.

PSH. I've always considered him very much a 'hands on' laboratory person.

JM. Yes he is.

PSH. One of the things which I wanted to get your impressions of was the very beginnings, the founding of the European Society, which you were so much involved with. Am I right that you, along with Jim Renwick and perhaps Anthony Edwards, were the people who saw it begin?

JM. Yes, you could say that, but there were others too, Lars Beckman and Anders. In Chicago, let me see, you were at the Congress in 66 in Chicago?

PSH. No, it was before my time.

JM. Oh really? You are a child!

PSH. Yes.

JM. We were warned very much against a certain street. We shouldn't go through that street. But we wanted to anyway, because then we came to a nice park region, and Jim Renwick and I, we went through illegally, in a way, through that street. And then I remember we walked up and down and back and forth and considered a number of models for the society, also he very criticised the still very long functioning and traditional way, and I had similar discussions with Beckman. Some discussions also with Jean de Grouchy.

PSH. One of the things I remember about the constitution there, which I'm afraid has rather disappeared, was its insistence that the meeting should be 'frugal'. I seem to remember that was written in. I don't know whether that was Jim Renwick or you who put that in?

JM. Both I think. He was very miserly. No, I would still, you see at that stage I had very clearly in mind the first International Congress of Human Genetics in Copenhagen, and I happened to see from close range the kind of destruction it wrought on the Institute, because you know the staff was not very large and Tage Kemp took on an enormous project. They didn't do anything, you know, for a couple of years, and it was politics and fund raising, and there were all kinds of disagreements, so at that stage when medical genetics in Europe was not very strongly developed quantitatively, I felt it could be a disaster if we should have that kind of thing every year somewhere in Europe. That may be part of the reason I emphasised the frugality and simplicity, even if it had cost, part of the cost was that it was not very democratic.

PSH. During your time in Copenhagen, what were the main steps that allowed you to develop this into a broad human genetics institute. For instance, cytogenetics. When did this first come into the Institute?

JM. That had already entered a bit when I arrived. Let me see, there was already from a long time back, there was Bent Harvald and Mogens Hauge, who were twin people.

PSH. I remember that, but I didn't think they were involved with any cytogenetics

JM. No no, I am coming to that. And Mogens Hauge he was strongly opposed to having any cytogenetics at the Institute, but then later on of course it was very obvious they would have to re-think, particularly when they started it at the Rigshospitalet. So I had some conferences then with the professor of obstetrics and gynaecology.

PSH. Was that Fritz Fuchs?

JM. No, that was not Fritz Fuchs, it was, what was his name? Anyway, we discussed how we could co-ordinate our efforts, whether one should do it and the other not at all, or whether we should kind of share it, and then he hired Philip, John Philip I think was his name. He did cytogenetics with him and I got in Margareta Mikkelsen and there were also some others. Mikkelsen got on very well together with Mogens Hauge by the way.

PSH. So cytogenetics came and presumably, the twin studies then were more along the epidemiological side, perhaps.

JM. And also they did not continue very much at the Bent Institute because Mogens Hauge had the professorship in Odense and Harvald also got the leading position in Odense and they took along the twin studies.

PSH. And the Register.

JM. The twin register, and when you come to think of it, I actually started the twin register in Copenhagen during those one and a half years. But I don't know what happened to that one. Probably that is incorporated in Odense now.

PSH. How did you come to be interested and involved in the polyposis work?

JM. Oh that was accidental, like many things are.

PSH. Was it through linkage, do you think or . . . ?

JM. I don't really know. I was asked, but actually I didn't take any initiative myself. I was asked if I would be willing to run it and Meera Khan was prime mover in that field of interest.

PSH. One of the things which I would be interested to hear you say something about was the very early work of your uncle, Otto Mohr, because from what I have read, his work was very early indeed and, am I right, he worked with Morgan for some time?

JM. Yes, but at that stage I believe Morgan was semi-retired so it was Sturtevant and Beadle, also and let me see. I think those are the main persons he mentioned. He had one year at Columbia University.

PSH. Would that have been around 1918 (?), or something like that?

JM. I am not sure.

PSH. And he brought that work back with him then to Norway, for his own research?

JM. Yes, but then, as you know, he also had some studies in human genetics.

PSH. Yes.

JM. And that was by co-operation, particularly between him and a fellow Wredt from the School of Agriculture on the ----[?]----.

PSH. What was his name again?

JM. Wredt. W r e d t. So his engagement was in the genetic part, not so much in the collection of material, but this Wredt was very enthusiastic. It was the same then with Fölling's disease. Fölling of course did the biochemical work. My uncle was concerned about the genetics of it.

PSH. Yes CB [Hagemann] showed me a joint paper, I think from 1945, that they had written on phenylketonuria. I did not know about, I knew of Fölling's earlier work, but I didn't know about that paper. Would it be true to say he had laid some foundation for studies on genetic disorders then, even before the war?

JM. Yes, by doing the things I just mentioned. But they did not establish any larger set-up for systematic initiatives. Actually, I think it was in 1936 or 37 that there was an initiative from the Rockefeller Foundation. Probably you know about this, you have read about the background, to establish units in human genetics in Europe and actually Tage Kemp got a large grant for human genetic studies from Rockefeller. They financed actually the original building and Tage was there. And here in Oslo my uncle got a similar offer, but he felt that human genetics was not yet sufficiently advanced in methodology and so on, that it was really worthwhile. So he kind of got the offer changed to support the Drosophila laboratory, which consisted of just one big room and one small room beside it. He didn't like Institutes.

PSH. I'm interested that you say this was an initiative from Rockefeller, because I had imagined that the initiative came from the European centres and the money came from Rockefeller.

JM. That's quite possible, of course there was an interplay with, they didn't just come down from heaven, because they explored in advance where they would land..

PSH. Was this do you think in relation to the concern about radiation risks and the possibility that research in Europe might give information on that?

JM. Not at that stage. This was before the war.

PSH. This was before the war?

JM. 1936/37. The Danish institute was established I think around the mid 30s.

PSH. Do you know I didn't know that there had been any initiative of that kind in human genetics before the war. It must have been very far sighted. Do you know who was it at Rockefeller who . . . ?

JM. No I don't know that.

PSH. That's very interesting.

JM. After the war of course it was Pomerat.

PSH. Was this the same Pomerat who worked with T C Hsu in Texas, or maybe different?

JM. I don't know. I know him only from the Rockefeller Foundation.

PSH. Jan, I have been asking everybody who I talked with two questions, and I might ask them to you. One question is, can you identify, is there one particular person who you feel had the most influence for the development of your work and career in human genetics? Does any single person stand out?

JM. Asked like that, it might be Lionel Penrose.

PSH. Yes. And would this be for any special reason?

JM. Well, as I mentioned, I liked his concern with the possibility of getting clear answers, although he didn't always get them himself. I'm thinking about anticipation.

PSH. That's true.

JM. I remember very well in 1950, he talked about that you know and was kind of smiling about the data, which apparently in his view showed anticipation, and he felt rather confident I remember that it was just a statistical bias.

PSH. That's interesting, I suppose that must have been at the time you were there when Julia Bell had finished that study. That's interesting, because I always feel there that Penrose in fact was rather close to being right. That all these biases did exist, but at the same time there was also another factor, which none of us knew about.

JM. No of course the amount of information was so scarce then. The statistics, they were dancing on an edge very often.

PSH. The other question which I have asked everybody is, to ask whether there is one particular piece of work which you feel is the most important for you, not necessarily the most important for everybody else, but the one you feel you identify most with. Does anything particularly . . . ?

JM. You mean in general?

PSH. No, I'm thinking in terms of your own contributions, whether there is one which if you just had to hold on to this one contribution and be remembered for only one thing, would you choose one particular piece of work or whatever?

JM. It is difficult to get away from that triplet linkage, of course.

PSH. You don't have to get away from it, but I wanted you to say that, not for me to say that! Because it was very early, so much earlier than almost anything else.

JM. Yes.

PSH. Is there anything else before we finish, Jan, that you would like to tell me about that I have missed out? There must be important areas which I have said nothing about. Anything you would like to bring up?

JM. It doesn't come to me immediately.

PSH. Well, in that case I won't wear you out any more.   I will turn off the machine but thank you very much indeed.