Jan Lindsten

Personal Details

Name	Jan Lindsten
Dates	Born 1935
Place of Birth	Sweden (Stockholm)
Main work places	Uppsala, Aarhus, Stockholm
Principal field of work	Cytogenetics, Medical genetics
Short biography	See below

Interview

Recorded interview made	Yes
Interviewer	Peter Harper
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Personal Scientific Records

Significant Record set exists

Records catalogued

Permanent place of archive

Summary of archive

Biography

Jan Eric Lindsten was born in Stockholm; Sweden on January 23rd, 1935. He received his BSc from Uppsala University in 1958 and successively PhD and MD degrees from Karolinska Institute in 1963 and 1969. In 1967 he was appointed Professor of Medical Genetics at Aarhus University, Denmark and in 1969 became Professor of Medical Genetics at Karolinska Institute, also being head of the Clinical Genetics Department between 1970 and 1990.

From 1987 – 1994 he was Chief Medical Officer and then chief executive officer at Karolinska Hospital, then chief executive officer at National University Hospital, Copenhagen, 1994 – 1996, Dean of the Medical Faculty, Karolinska Institute 1996 – 1998 and President of the Royal Swedish Academy of Sciences 2003 – 2006. He also acted as Secretary General of the Nobel Assembly and Nobel Medical Committee and member of the Nobel Foundation Board, 1979 – 1990.

He is the author of over 200 publications on clinical genetics, including some of the first publications on human cytogenetics.

INTERVIEW WITH PROFESSOR JAN LINDSTEN, 11th NOVEMBER, 2004

PSH. What I was going to do Jan, if I may, was to ask you a few things about your own work, but also a bit more general because I think you can speak for a lot of other things in Sweden and so I think it would be very valuable. Can I start right at the beginning perhaps and ask, what part of Sweden were you born and brought up in; in Stockholm?

JL. Well I was born in Stockholm and my father was an officer in the Army, and I was born in 1935 and when the war broke out my father was in the reserve of the Army, so he had to join the Army and we moved to the southern part of Sweden.

PSH. That was even though Sweden was neutral, everybody I suppose still had to join just in case.

JL. We mobilised. So we lived down there for seven years and then he never left the military and so he was transferred to another city about 120 Km from here, where I finished the high school and that region belonged to Uppsala University, so then I went to Uppsala to study and I started to study there in 1953. When I had started to study, I became interested in human genetics during the lectures given, and I decided to interrupt the studies and start to do some research. I was very young at that time.

PSH. Was it a zoology or biology degree you were studying for?

JL. No I started to study genetics, but then I changed and started to study medicine in 1955, so I had a background in genetics and statistics, and that is why I became interested in human genetics when we reached that stage in our medical studies. And then in '57/'58 I interrupted my studies and started to do research at the Department of Medical Genetics, which was, as I said before, the previous institute for race biology or eugenics here in Sweden. Dahlberg at that time, who was the second professor, Lundborg was the first one, Dahlberg was the second one, he had just had a stroke before that, so I never met Dahlberg, but Jan-Arvid Böök was the head of the department and Marco Fraccaro had just come from England where he had been for a couple of years with Penrose, and so I joined Marco who became deputy head of the department.

PSH. What year was that?

JL. That was in '57. So I joined Marco. At that time Böök and Marco had built up the Department of Human Genetics or medical genetics and we started to do tissue culture and we started to do chromosome analysis. As you know it was only one year after Tjio and Levan defined the human karyotype.

PSH. Before we get on to that, can I just ask you a little bit about Böök, because I mean, Marco told me a bit about him, but he seems to have stimulated a very valuable department and yet his name seems to fade out rather. Was he a clinician?

JL. No, no.

PSH. Or a scientist?

JL. He never was a clinician, he was a kind of a scientist. He had been working on salamanders, in Lund, where he came from, and you could say he was a cytogeneticist from the beginning.

PSH. Right.

JL. The problems that arose and became more and more obvious was that he and Marco didn't get along very well, and this was partly due to the fact that Böök had a thyrotoxicosis, for which he was operated upon. So he was on thyroid drugs and sometimes he felt too slow, and so he started to use central stimulants and sometimes, relatively often, he couldn't control these two things, the thyroid substitution and the stimulants and it went so far so he was on the list. He had in fact smuggled narcotics, so he was registered at Arlanda airport and he had been caught sometime. He had ampoules in his typewriter and things like that. He also used the department for buying drugs at the local pharmacy in Uppsala and that was discovered so, and he became very moody. This increased the conflict and finally the conflict became too big, so Marco had to leave.

PSH. They didn't make Böök resign because of all this problem?

JL. No, at that time you know, once a professor always a professor, and that was before modern times, so he could stay on, even if people knew that he was a lot of problem. I came in between, because I was a young student at that time, but I had to take a position and I took a position for Marco and at that time Marco and I had started to have contacts with the Department of Endocrinology at the Karolinska hospital, because of my interest in gonadal dysgenesis, and this department had a lot of those patients, so before Marco left for Holland or for Leiden, it had been arranged so that I could move from Uppsala to Stockholm and to the Department of Endocrinology, and that was in 1961, the beginning of '61 that I moved.

PSH. Just to go back a little, how did you first come to be interested in Turner's and gonadal dysgenesis?

JL. I mean, we started to do chromosome analysis and we managed to do that fairly quickly. We were unfortunate in one sense, and that is that we had Pharmacia in Uppsala, and they used to separate white blood cells from red cells by dextran, which we used, and dextran doesn't stimulate the lymphocytes to grow, while what was it, I don't even recall. Was it Moorhead or was it . . . ?

PSH. It was Moorhead yes.

JL. Who described phytohaemaglutinin, because he had that close at hand But our intention to separate red and white were the same, but they were lucky to use the phytohaemaglutinin.

PSH. Which was an accident. I don't think they knew what it was doing to begin with, did they?

JL. No no. That's what I mean. And so we confirmed the karyotype which was nothing unique, but you have to remember at that time there were only 5 or 6 labs in the world who were studying human chromosomes. I mean it was Kemp in Copenhagen who had done a little bit, not much, and he didn't continue, and then it was Lejeune in Paris. Penrose had done a little bit.

PSH. But not very much.

JL. But then Chu, Ernie Chu and T C Hsu, and that was about it.

PSH. And Charles Ford.

JL. And Makino and Charles Ford

PSH. And then Edinburgh.

JL. Yes, and that was about it. So we, as all the others, started to look at various conditions. Now we got into problems because of Böök, with the University Hospital at Uppsala, because at that time you could almost look at any type of malformation or disorder and often you would find something, so what Böök did is that whenever he got a referral of material, he put a stamp on that when he sent the reply, and it said "all rights for publication reserved".

PSH. Oh my heavens.

JL. So the departments of paediatric and gynaecology became mad at us and we had to move to a city outside of Uppsala to get material. We went to a town called
Eskilstuna where there was a paediatrician who had written some papers about gonadal dysgenesis. So we started to study those patients, but as I said we didn't manage with the blood cultures, so we used long-term bone marrow cultures and we used skin cultures, and that's why Charles Ford came a little before we published it, because he used direct bone marrow preparations, while we had to culture them for a long time. So his publication came about a month earlier than our publication on gonadal dysgenesis, but we were on the same track.

PSH. I'm trying to remember what year your thesis on Turner's syndrome was. I have a copy.

JL. That was'63.

PSH. '63 and that must have summed up studies over the previous 5 years, I suppose.

JL. More or less everything I had done up to then.

PSH. So would it be fair to say that the Turner's work was your first kind of contribution, or major contribution, in the cytogenetics area.

JL. That's right, but of course in parallel to that we did all kinds of things. We discovered some of the variant abnormalities in Down's syndrome and, you know.

PSH. Mosaicism

JL. We discovered all kinds of unique things which anyone could discover at that time.

PSH. So you moved to Stockholm in then, was it '61?

JL. '61.

PSH. And what was the post? Was this a specific academic post then, or were you still completing your medical studies.

JL. I got a stipend for a PhD student and my project was Turner's syndrome.

PSH. So this was part way through your medical studies.

JL. That's right. And then, when I had finished that, I went back to my medical studies and completed my MD

PSH. In Uppsala or in Stockholm?

JL. In Stockholm. I have remained in Stockholm since then.

PSH. Right, so you completed your medical studies and then, how long was it before you came back into genetics, or did you keep your research going continuously

JL. I mixed it, so I was in genetics all the time and fact I got a Professorship in Denmark, in Aarhus in Denmark, in 1967.

PSH. I didn't know that. So . . .

JL. Before I got my MD, which I got in 1969.

PSH. So were you in two places at once so to speak?

JL. No, I was in Aarhus, but they had expected that a local person should get that position, but he didn't. That was Therkelsen, and I got it, but due to that fact, there were no labs for me and no department, so I spent two years working in Stockholm but at the same time travelling up and down to Aarhus in Denmark, teaching and planning a new lab. But during that two year period they arranged a chair for me here in Stockholm. So when that building was ready in Denmark I left, but that building still exists and Lars Bolund, and all the other people in Aarhus, are still working in that department which I drew.

PSH. And did Therkelsen then follow on from you?

JL. Yes.

PSH. Yes.

JL. That's right. So the Department exists, so I think I did quite a good job for human genetics anyhow.

PSH. So when you then developed cytogenetics in Stockholm, can I ask was this on the basis of being a research laboratory, or was it also a diagnostic laboratory?

JL. It definitely was a diagnostic laboratory. Already from '61 I did diagnostic work for the hospital, but when I came back from Denmark, that was in '69/'70, they had arranged a chair for me here in Stockholm and a separate laboratory, an independent laboratory separate from the department of endocrinology. And this was a combined clinical diagnostics laboratory and a research laboratory, which then developed and became a kind of a model for the other laboratories at the other university hospitals in this country.

PSH. Yes. Am I right that a part of the time at this stage, Maj Hultén was also in your lab?

JL. That's right. She became a PhD student with me and, she was not easy to handle by the way, but we did some good work and she did some good work, and after that she moved to England as you know. Her family was very interesting by the way. Her brother is a very well-known curator of a museum of modern arts.

PSH. Is that right?

JL. He was the founder of the museum of modern arts here in Stockholm, which was one of the leading centres for modern art here, and then he moved to Paris and made a similar thing there, to Venice, to Los Angeles and

PSH. I didn't know that.

JL. And her father was an extraordinary person. He was a botanist, who was a specialist in the Arctic flora and who made a lot of expeditions to the Arctic regions of Kamchatka, so it was a very special family.

PSH. One of the things I noticed, going through the list of publications, was you kept collaborating and publishing with Marco for very many years after the two of you had been in Uppsala. How did that happen? Did you go backwards and forwards, or was it more indirect?

JL. No, it was direct. You see I had a very, very small unit to begin, when I came to Stockholm. I was alone and I was in the department of endocrinology, I was not in a genetic surrounding, so I was lacking teachers. I was lacking input from other geneticists, so for me it was a kind of umbilical cord to stay in contact with Marco, whom I knew from before, and so in order to increase our capacity, both of us, we collaborated.

PSH. That must have been very fruitful.

JL. It was very nice, and I went to Pavia now and then and Marco came here, so we worked at a distance but still it worked quite well.

PSH. Can I ask, how much contact over those early years did you have with the people in Lund?

JL. Not much. Not much at all. Levan was the person there of course, but Levan, he was a very kind person, admirable in so many ways, but he was a lonely wolf so to speak. He had his own department and he had his own pupils and I was not one of them, so we didn't have very much contact. We met and had good relations, but we never worked together in any way.

PSH. Because it has always interested me that, after the discovery of the chromosome number, there was no real development of clinical cytogenetics outside cancer in Lund for many years really.

JL. No. Levan's interest was basic cytogenetics, and you can say cancer cytogenetics, that was his interest. He was the one who put Felix Mitelman on to study the tumours in the rat and you can say that Felix has followed that line ever since, more or less, but that is his main . . . and I was never particularly interested in tumours, so that is another reason why perhaps we didn't collaborate. And I didn't continue to collaborate with the people coming in Uppsala either, Gustavsson and Kjessler and the others because I thought they, I had had to leave Uppsala because of this conflict and I was a little bit irritated by them, because they didn't, or didn't want to see the drawbacks with Böök and the problems there, so I left them to their own.

PSH. At what point did you start working or collaborating with Caspersson and
Lore Zech?

JL. That was 1970, and I knew of course that they had started to work with quinacrine mustard.

PSH. Was this in the Karolinska or was it in a separate . . .?

JL. Yes. It was in the preclinical department, which was called research and cell genetics. You see that was another reason why when I came to the Karolinska Institute. Caspersson was there. He had already a circumscribed area of interest and that was cell genetics.

PSH. OK, so you couldn't really . . .

JL. So I couldn't go in there and in order to create something for me, I chose because I also liked it, clinical genetics, and that's why I started to do clinical genetics at the Karolinska Hospital and then in all Sweden, and it later became a clinical specialty as you know. But then Caspersson and Zech found the quinacrine mustard pattern, and they wanted to have a clinical counterpart and we started to work together. I don't recall whether I took the initiative or they took the initiative, but some of us did, and we came in close

contact when we started to work. Now even that became a problem after some time, because Caspersson was so fascinated by machines and measurements and he made very good machines. He was very good at that. When he had made one machine, which could determine something with one decimal, he wanted to make another one that could do it with five decimals. So he was not really interested in the biological consequences or the use of these machines for biological purposes. He was interested in measuring in itself, which is legitimate I must say, but that was not my interest. So Lore and I had so many problems we wanted to do, but Caspersson more or less prevented us from pursuing all these ideas we had.

PSH. He must have been a very strong character.

JL. Oh he was. He was. It was interesting but, you will hear about this from Lore, but when he had found the pattern, he wanted to construct a machine that would recognise the pattern automatically. That was his main interest, not how you could use the pattern for anything. He wanted to. But it all worked in the way that his technicians looked at the negatives and told which chromosome was which, and then he had the machine to analyse what they had already discovered. Of course now you can say there are machines that can do it, but that has not added very much to the basic knowledge and to the application, but as Lore and I wanted to continue we had so many ideas of what to do, but he stopped it more or less. Of course I could continue with the banding techniques myself, but he stopped the collaboration between me and Lore because it became tangled. It grew too fast and too much and he hadn't any control over it.

PSH. When was it that you started to move beyond cytogenetics to what you might call more general clinical genetics.

JL. I realised that if I was going to run a department of clinical genetics, I couldn't stick to cytogenetics, because clinical genetics has only a function if it can do something for patients which other clinicians cannot do, which meant I had to become more of a geneticist myself and as I said before, that was my Achilles heel, that I didn't work in a genetics department, so I had to teach myself and get the knowledge So I started to build up the clinical side with outpatients, seeing the outpatients having genetic counselling. I did a lot of quite interesting work together with the psychologists about genetic counselling. And then I started to recruit PhD students, apart from Maj [Hultén], who already was about to leave at that time, but other ones and then I got Lennart Iselius who was very good at genetic epidemiology and I started then after that, at a later stage, to build up molecular genetics, together with other people, although that was not my main sphere of competence.

PSH. Can I ask, was there any clinical genetics in Sweden at that time?

JL. Hardly any. It depends on how you say - there have been a lot of human geneticists in Sweden, or people who have been dealing with genetic matters in the clinic. You have Essen-Möller in Lund, a psychiatrist, who was interested in genetics. You have Tage Larsson in Stockholm, Torsten Sjögren in Stockholm. You have Dahlberg in Uppsala. They all were interested and made some very interesting contributions to human genetics. No one made a

department, a unit for trying to consolidate genetics as one field. So I think that was what I wanted to do.

PSH. Who was your kind of model for that? Did you look to other countries and other set ups when you were planning that or did it just happen?

JL. Not particularly. I mean I participated in congresses, met people, knew a lot of people, genetics wasn't that big at that time, so you met a lot of people all around the world and picked up things everywhere and tried to do your best.

PSH. Sure. I'm going to suggest we pause from our recording now.

[LUNCH]

PSH. If we have a minute to continue where we left off, can I take you right back to Uppsala and ask you a bit about, now is it Lindborg or Lindberg?

JL. Lundborg.

PSH. Because one of the things that intrigues me is that a lot of the people involved in eugenics were also actually very competent, either medical people, or competent geneticists. They weren't just charlatans. Am I right that Lundborg was a psychiatrist?

JL. I don't know very much about Lundborg. I know that he was mainly, I would say, an anthropologist. He was interested in the shape of the body, the face, the skulls, he was up in Lapland photographing, wonderful photographs of the people up there, registering the way they looked and publishing that. Unfortunately he was mixing those observations with his ideas on race and racism.

PSH. I mean one of the things which is interesting for me is, that Sweden was neutral in the second world war, but from talking with people, it seems clear that there was a lot of pressure coming from Germany, that many people in Sweden felt that they had to kind of be cautious, or keep on the right side and there was a group of people who felt they had to somehow adopt at least some of the German kind of philosophy.

JL. Absolutely, the German influence in Sweden before and during the Second World War was quite considerable. Even I had German as first language in school.

PSH. As first language?

JL. Yes, as first foreign language.

PSH. Right.

JL. So no doubt that German had an influence and of course, people were impressed by the changes that occurred in Germany when Hitler came to power, and they rejected, or didn't want to see or hear what they learnt about,

what was going on and especially in academic circles and in military circles I think that Germany had a very prominent position. I know for instance that there were physicians who were dressed up in uniforms here in Stockholm and

PSH. Yes and Lundborg was a strong influence.

JL. He was a racist, but I don't know very much about him. But together with an artist, who was very good in making pencil drawings, he has published a book. The artist's work is fantastic, but of course the interpretation, this is Aryan blood, is something that Lundborg has added, but the drawings themselves are very, very good.

PSH. So did Lundborg actually set up a genetics department, or was it more an anthropology.

JL. It was more an anthropology, race biology you could say, and it was Dahlberg who turned it into a genetics department. Dahlberg was anti racist and he wrote officially against Germany and racism.

PSH. Am I right that Dahlberg had already taken over from Lundborg before the war?

JL. Yes.

PSH. The reason I know that is from reading the autobiography of Lancelot Hogben, who not only was a close collaborator of Dahlberg, but was in Sweden [actually Norway] when the war broke out.

JL. That's right. Lancelot Hogben came to Sweden and the only one he knew was Dahlberg. Then, when he had to go back, he had to go all the way

PSH. Via Siberia

JL. Via Siberia, to get back to England again. I have heard that story. But Dahlberg, you could say, was a population geneticist and he made significant contributions to population genetics. I always used one of his expressions when I taught population genetics because he was interested in isolates and said "Thank God there were places where dances were being held" So there was some exchange of blood.

PSH. And his book, which was translated into English, I think maybe by Hogben.

JL. That's right, he got the Swedish version and translated it into

PSH. Race, Rubbish and Reason.

J.L. John Edwards knows this work very well, no, Anthony Edwards knows this work very well. I spoke to him about it last year.

PSH. And then, did Dahlberg then die young?

JL. I'm sorry to I admit I don't know how old he was, but he had a stroke and he couldn't speak and he wasn't there when I was there, so I never met him.

PSH. So then Böök would have taken over from Dahlberg in the early 1950s?

JL. In the middle of the fifties and Böök was instrumental in changing the name of the department to medical genetics.

PSH. So before Böök, really there was no medical element to the work in Uppsala?

JL. Both yes and no. Yes, because Dahlberg was the only competent biological statistician in Uppsala, so every thesis that was presented in Uppsala, it was better that he had seen it before it was presented, because if he had not,

PSH. Yes, you'd get criticism after.

JL. he would be an extra opponent and he would cut it into pieces because he was very bright. And you know the library at that department was fantastic. The collection of books they had, about as I said before, race biology but in general, because Dahlberg was very instrumental in writing encyclopaedias, so he had a lot of general literature of science and other biology and things, so after Böök's retirement the department was going to move and I was on the committee to appoint his successor. I insisted that they should take to the protocol that the library should not be split, but kept together and moved to the central university library because of this uniqueness.

PSH. Yes indeed, because as a historical perspective it would indeed be unique.

JL. And I was sitting there in a small room with all the glass, the films, the photographic glass plates that Lundborg had taken up in Northern Sweden. They were surrounding me. I wish I had taken a box.

PSH. One of the things which, I mean, you may not be able to answer me very much, but am I right, you became the director of the Karolinska hospital as a whole at some point?

JL. That's right, at the end of the 1980s I became Chief Medical Officer at the hospital and at that time I became more and more interested in management, so in 1990 I changed career completely. That means I quit human and clinical genetics and became Chief Executive Officer of the hospital. And at that time I said to myself, I had to make a choice, molecular genetics came into the picture. I knew what they were doing, but I was now of no value at the bench because that was not my field, and I was very interested in management and so I said to myself, why should I sit and block this position for another 15 years when all these new young people are coming. And I still think that that was a wise decision to make, because the department, not exploded but increased much more rapidly after I had left than when I was there.

PSH. When you finished, how many people were there in the department?

JL. About 20-25 or something like that and then it grew very, very rapidly. Of course I had paved the way in many ways. We had increased space. We had laboratories. There were people who were knowledgeable in molecular genetics, but I couldn't have contributed to that; what I could have contributed to is, lets say the attitudes to the need for clinical genetics, the needs for patients, which I think is one of the risks they are running now, that unless they have a service which patients are in need of and which no one else can provide, then there is no need for clinical genetics any more.

PSH. That's true.

PSH. Seen from your perspective then as a Chief Executive of a hospital, how do you see clinical and medical genetics fitting into the broader scheme of things now and in the future?

JL. I think it still can very well, in contrast to some of the other clinical laboratory disciplines. You know, there was a time in the seventies when there was a production of very good scientists at the pre clinical departments and they wanted to create positions for them and there was a need in the university hospitals for methodology which could be used for scientific work, which the clinicians didn't have access to. So it was created clinical chemistry, clinical physiology, clinical pharmacology etc. Nowadays the need for these disciplines are much less, because the clinicians are not satisfied. They know much more now, but they are not satisfied with the expert knowledge that these clinical disciplines have. They want to have the real expert knowledge which is available in the pre-clinical department, which means you can out-source the clinical chemistry for instance to a private lab or something who can do that, and physiology is more or less, the cardiologists can do a lot of the clinical physiology, radiology can take over other parts of physiology etc. But for genetics still there is room for it, and there is a need for it, because of the rapid development and the clinicians don't really have time to keep up with everything that goes on in genetics, but provided that you give the patient something more than the clinicians can do themselves. The day when you cannot do that anymore, then there is no need for it anymore.

PSH. I agree with you completely and what we find at home is, that more and more of our time is spent, not so much providing direct services, but educating our colleagues how they can provide those services, but you still end up with a core of services which really you have to be expert in, the clinical, the family and all these other aspects to deliver really well, and I think, because the field is developing so fast, however much you pass on to other specialties still leaves a lot for the clinical geneticist.

JL. What we did to try to show that we were of help was that, whenever we have had a patient, we wrote a letter after the visit to the patient, summarising what we had said, so that they could show it to their other physician and that physician would then know what the patient had been told. The other thing was that whenever we replied to a physician who had referred a patient, we appended reprints or copies of papers to show them the background for the

way we were reasoning. We organised conferences with the clinicians. And I think you cannot just sit with your arms crossed. You have to work actively to show that there is a need for you.

PSH. That's true. And the need changes from year to year.

JL. And there is nothing which says if you are not needed, why should you be there.

PSH. You don't have a divine right.

JL. No. I guess everyone reasons the same way, because in that way we could change and have a lot of structure rearrangements within the health care system.

PSH. Yes.

JL. So I don't know if whether I answered your question really.

PSH. No, you did I think. I mean as far as anyone can answer because it is changing all the time.

JL. When you become Chief Executive of a hospital, your views on your colleagues change radically and there are so many things which change. For instance, it seems as if the more beds you have the more powerful you are, and in fact it would be the opposite, that the less beds you have the more powerful you should be. I mean the day you don't need to hospitalise any of your patients, the more successful you have been.

PSH. How did you come to be part of the Nobel Committee?

JL. Mmm

PSH. You may not, I am sure there are things that you wouldn't wish to say.

JL. There are certain things which I am not allowed to talk about.

PSH. Of course. I quite understand.

JL. The situation is as follows. At that time there is a Nobel Committee. It is composed of 5 people, but each year are added 10 extra members of the Committee for that particular year only. The regular members of the Committee can stay on for 6 years and these ad hoc members are chosen because of the need for competence that particular year. It depends on what kind of candidates are nominated and what expertise you need to evaluate this. So I was chosen as an ad hoc member a few years and of course I don't need to tell you more, because you will understand why my expertise was needed.

PSH. Sure.

JL. Then there was a change in the organisation and a new secretary had to be elected, and for one reason or the other I was considered a suitable candidate for that job. There were many people who wanted to have that job, but I was considered suitable and I accepted. It was one of the most interesting jobs I have had.

PSH. It must be amazing, actually, and extremely difficult.

JL. Yes it is difficult. But still the most interesting job I have had was to be the Chief Executive Officer of the Karolinska Institute. It was fantastic. I mean it is like research to run a university hospital. I had a wonderful time as far as the internal relations are concerned, but it is an impossible job. So if anyone would ask me "Would you take that job again?" I would say "no", because there is no chance you can manage that job as the situation is today, with the political . . .

PSH. This must be very different, because what years was it that you were chief executive?

JL. 1990 to '94, and I resigned after that because it was impossible. They didn't want a leader. They wanted, what you call it in English – someone who is a boss for a certain place selected by someone who is supposed to do what the politicians want that person to do. A kind of puppet on a string and they are prepared to cut the strings at any moment if it doesn't fit their agenda.

PSH. So if there was a problem, you would be there to get the blame rather than the politicians.

JL. Yes, and the politicians are not, those politicians who are dealing with healthcare, are not interested in the success of the healthcare system, they are only interested in their own re-election; and that was my greatest mistake, that I didn't fully understand and act accordingly. I didn't understand their world and their conditions well enough to get them to propose what I wanted them to do. I was too rational for the job.

PSH. Yes, it is a very different world. Do you feel though you were able, despite these difficulties, were there any real successes you feel you could say when you'd finished that you'd achieved that as being Chief Executive.

JL. Yes. The only thing is, that I should have stayed for 2 or 3 more years, because I had started some things which were very successful, that had to do with the logistics of patients within the system, and we changed the efficiency of the hospital and quality of care tremendously by improving the logistic flow of patients within the system. So if I had been able to push that a little bit further, the hospital would have been much better off. But now when I resigned, we had only passed the pilot stage really and my successor wasn't at all interested in it, so it fell back to the situation that was before. But what I did manage to do was good research organisation for the hospital, and I was successful in getting the county council, which is the organisation responsible for the healthcare, to invest in research and development at the hospital with quite a lot of money, so that was quite successful.

PSH. Looking back Jan, one of the things I have been asking everybody I have seen is, if you had to choose one piece or field of work that you feel especially proud of, or you identify particularly with, what area or piece of work would you choose?

JL. First, I don't think that I am a particularly good scientist. I think that, if I had to choose again I would probably have gone into management much earlier. After my MD I would have supplemented that with a business administration, thing like that. I am not a particularly good scientist, but I think the work we did, I think we had good intentions. We saw the problems and at an early stage to trace the chromosome abnormalities, to localise genes. We didn't have the tools but we saw the problems and we could do that. I think the work we did with Maj [Hultén], then which was followed up with the mapping function which we tried to analyse with Newton Morton, that was quite interesting, that work. So I think I am better in seeing what are the problems that should be done, than really to be able to carry it out and pursue the thing.

PSH. The other thing I have been asking everybody is, are there particular people or a particular person who you feel has especially influenced your career in human genetics?

JL. Marco definitely. He is the one who has done . . . As I said, it was quite a small group of people and we knew each other. I mean I had very good relations with Charles Ford. He was very interesting and I should have said before that, perhaps that has to do with my interest in management, because one of the works which I enjoyed very much was being Chairman of the Standing Committee for Human Cytogenetics Nomenclature.

PSH. Oh yes.

JL. And if any paper has been quoted more than any of my other papers, that's the one and I think it's amazing. There are two things which are amazing. Firstly that cytogenetics is still a field which is going on, I mean it started in the fifties and it's still useful, and not only clinically but scientifically. Of course now it is combined with molecular genetics but it still is valuable.

PSH. Yes, it's been very adaptable and inventive.

JL. And the nomenclature, I don't know whether we were clever or just by intuition, that we created a nomenclature which has been useful, and I think at that time we were unique in a sense in human cytogenetics, that we all came together and we spoke the same language. In immunology, for instance, they never managed to do that and it has been chaotic.

PSH. Am I right, Marco was at the Denver conference, is that right?

JL. Yes.

PSH. But you weren't yourself at that?

JL. No because we were two and only one was allowed to go, the other was not.

PSH. You must have been involved from the beginning.

JL. Oh yes. We did everything together.

PSH. It's interesting to me that you look on Marco, in a way, as a bit of a mentor, because I suppose you were still a student at that stage weren't you and he was, if not established, he was already an investigator.

JL. But you see he came from Italy and he was brought up scientifically with the Italian geneticists, Cavalli and the other people in Pavia. Pavia was very strong at that time. A lot of very, very good geneticists, Montalenti and other people.

PSH. Yes.

JL. So he came from genetics. I didn't come from genetics really. I didn't come from a department. I didn't get that input. I had to create it myself and that was a little bit hard, because I didn't get the input from others.

PSH. Are there any other things Jan, that you feel I have missed out and haven't really asked you about at all? I suppose I could have mentioned your interest in genetics of diabetes.

JL. Yes, you could. I became interested in quantitative characters in the inheritance of that, and was I working to get Newton [Morton] to analyse quantitative traits, something like that, which is still interesting; I mean genetic epidemiology was interesting in the time of Dahlberg, but then it has got a renaissance you could say when molecular genetics came into the picture and I started to go into genetic epidemiology with Lennart Iselius a little before that, because I saw the implications of it, and that is what I meant before that. I could see the need for various things, but I myself was not able to carry it out.

PSH. And it has proved to be an awful lot more difficult than anybody thought at the time, so probably your decision was a wise one, because you know, ten years later we are still really not a lot further forward except in realising how complex it is.

PSH. Am I right that you were the first centre in Sweden to develop pre-natal diagnosis?

JL. That's right. We did it, in 1971 we started routinely. That means we started to culture cells already in 1970, so in 1971 we did I think altogether 23 investigations. That is far less than is being done in one week now.

PSH. But at that time more than almost any other unit probably.

JL. The one thing I would like to say is, I don't think I published many papers which are of any great significance, but one thing which I think is a little bit sad

that people never quote me for, and that is that I published, together with Caspersson and Zech at the very early stages of the 1970s, a paper with one patient where the clear conclusion was that Down's syndrome was not due to the complete extra chromosome but it was due to part of that chromosome 21, and we could even say that it was the distal part of chromosome 21. And we said it so clearly in that paper, and no one has ever quoted that paper, because it was the first paper in which one could say that Down's syndrome was not due to the entire chromosome.

PSH. Which paper was that Jan?

JL. I don't recall it right now but I can definitely pick it out.

PSH. And do you remember the year?

JL. Could have been '70, '71, '72 something like that. You have my whole bibliography?

PSH. From PubMed you can find all these papers.

JL. I can find it for you and I can show it to you.

PSH. Let me see if I can find it.

JL. I can pick it out and give it to you tomorrow. I think it is extra G-like chromosomes, something like that.

PSH. Yes. Distinction between extra G-like chromosomes by quinacrine mustard fluorescence analysis; in Experimental Cell Research – 1970. Yes. So that's a really a very important paper.

JL. I thought that at that time that it was interesting.

PSH. But nobody quoted you?

JL. I said it several times at that time. I had good intentions. I mean I started, together with Caspersson's daughter, Gunnell, I tried to trace the origin of the extra chromosome. The means were very primitive. We had the ideas. We tried to localise genes by studying translocations and things like that at a very early stage. No one had done it before, but we attempted to do it. We saw the problem and the same with the X chromosome abnormalities. We tried to localise where the genes were.

PSH. Well I think that's important. So maybe that's a good point at which for me to finish the recording. Thank you Jan very much indeed.

End of tape