# Professor Lore Zech

**Personal Details**

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| Name | Lore Zech |
| Dates | 24/09/1923-13/03/2013 |
| Place of Birth | Guetersloh, Germany |
| Main work places | Stockholm |
| Principal field of work | Human cytogenetics |

**Short biography**

Lore Zech was born in Germany but spent her entire career in Sweden, mainly in the Stockholm laboratory of Torbjorn Caspersson, where she discovered the banding of human and other chromosomes produced by fluorescent alkylating agents. She later made major contributions to human cancer cytogenetics.

**Interview**

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**INTERVIEW WITH PROFESSOR LORE ZECH, 12 NOVEMBER 2004**

**PH = Interviewer (Peter Harper)**

**LZ = Lore Zech**

**PH It's 12 November 2004 and I'm talking to Professor Lore Zech at the Karolinska Hospital, Medical Genetics Department, in Stockholm, Sweden. I have been asking everyone I see, Lore, how did you first become interested in science in general and then genetics in particular?**

LZ I am from Germany and I started studying medicine but after the war there came all the soldiers back and the young students had to finish, and because I didn't want to lose the time I went over to biology and studied biology and physics and chemistry. And then, when it was easier for medical students, I had come so far in biology that I didn't want to start from the beginning again, so I continued. And already as a student I worked quite a lot in my free time with chromosomes, mainly chromosomes from lilies because they are big, there are only a few and you can see a lot. It was a fantastic experience to see these chromosomes during spermatogenesis and all these processes. It was at the University in Bonn.

**PH May I ask, was Bonn the city where you were born and brought up?**

LZ No. I was born in another city in Germany, Gütersloh My parents died when I was a very young child. They both died of tuberculosis after the war. My mother died when I was two years old, my father when I was four or five years old, and I was educated in the family of my grandmother. Well, I had a very beautiful childhood because we were living in the countryside and it was very good. And at the end of the war I worked at a hospital to prepare for the medical studies and I met my husband who went to Sweden. He got a fellowship to the Institute of Torbjorn Caspersson and he should be there for one year, and after a year he wrote to me that he was not finished and he all the time told me that Sweden had so many beautiful girls and in Germany there were no men in our age because they had gone during the war. Hitler sent the young men who had not a family to the most dangerous places because then he hadn't to pay pensions and so on. So if you had a man then you had to keep him. So I thought it was best to follow my husband to Sweden and I got also a fellowship at the Institute of Caspersson.

**PH What year was that?**

LZ That was, my husband went in 1952, I went in 1953. And then we worked at this Institute of Cell Research and Genetics. It was very outstanding at that time and Caspersson's goal was to measure nucleic acids in living cells and he built instruments. He had learnt about nucleic acids at the laboratory of Einar Hamerstein who was a very important person at the Karolinska Institute and in 1944 Caspersson got a personal professorship. Well we were welcome to work there but we had to use the instruments. Everybody who didn't want to measure nucleic acids was not welcome and we didn't know anything about the things we should do but, OK we learned.

**PH Did Caspersson have any biological interest or was it purely centred around the technology?**

LZ Must I be polite or can I say how it was?

**PH I think after this length of time you can say the truth.**

LZ He was very uninterested in biology of cell growth and development and he was interested in his machines, and up to his death he always regarded the most important contribution which he had made to science was the development of these machines, and you cannot imagine, these machines were very big and large. As big as this room for one machine and they were uncomfortable, so you had to stay and look in the microscope like this, and all day, it took about 2 or 3 minutes to measure one cell. And then

the machines became smaller and smaller and Caspersson went to every congress with all the pictures of the machines and the diagrams and people found he was a very important man, but they were terrified when he started his description of the machines and how they worked.

**PH Why were they terrified?**

LZ Because it was so boring. I mean all these measurements and the theoretical mathematical calculations of the measurements and the unspecific light loss. How big was the unspecific light loss. It would take half an hour if he explained it.

**PH So people I suppose really would be more interested in the reason he was doing it than the actual methods.**

LZ Of course. Yes. Absolutely. But he understood that there were many crazy people who were more interested in the biology and therefore he started a collaboration with biologists. Out of the interest of Caspersson in my husband, my husband was a virologist and studied infection of plant cells by tobacco mosaic virus and he studied this in the hair cells from tobacco. So he cut the tip of the hair cell, infected it with virus and then he studied under the microscope how the cells changed.

**PH Now this is a tobacco hair cell, not a human hair cell.**

LZ No, a tobacco hair cell . You could see the changes and Caspersson's interest was that was a procedure which could be measured perhaps, changes in nucleic acid content and so on. Well, I was asked to measure cells during developmental changes in nucleic acid in single cells, and there I had the knowledge from my early student days. I knew which material would be good, so we started this with lilies, Trillium erectum, that is a plant from South America, which has very big chromosomes and it was possible to measure them, yes.

**PH So at that time when you began with Caspersson, how many people were there in the group working with him?**

LZ It was a very little Institute. I cannot tell you. We had four or five engineers building the machines and then perhaps 10 people. I can check on this and tell you later.

**PH That's OK. I was trying to get a feel**

LZ It was a very little Institution.

**PH And of those people, would maybe 3 or 4 be biologists?**

LZ No there were, Lindstrom and Engstrom were physicists, but medical doctors and then there were other medical doctors and I was the only biologist.

**PH Right. At what time did the work of the group start involving DNA?**

LZ That was already at a much earlier date. Caspersson's first instruments measured DNA by UV absorption and that was already during the forties.

**PH So before it's structure was recognised.**

LZ He has written a book from that time.

**PH I didn't know that. I must find it.**

LZ Cell growth and cell function.

**PH So he was working on nucleic acids then before it was recognised that they were hereditary materials.**

LZ Yes.

**PH That's interesting.**

LZ Yes and that book is quite interesting. It was a good book from that time.

**PH I will try to find it.**

LZ It's not any longer available but I believe it was as a pocket book, I thought.

**PH Then at what point then did work start actually on chromosomes? Was your first work with Caspersson on chromosomes or did it begin on something else?**

LZ No, I did other work after these very first measurements and we had seen that it was possible to measure, then I did other work and Caspersson did also other things. But he had a collaboration with an engineer, Carlsson, and they measured, I believe, liver cells just the DNA content of liver cells. And Carlsson was one of the engineers who had constructed, Caspersson had in his group three engineers who built instruments what is the name in English for these educated, high school educated engineers?

**PH They would be, I don't know.**

LZ They are Diploma engineers in German. Then he had two or three engineers just on the gymnasium education in techniques and then there were some people in the workshop. It was a big staff around that field, I can write these numbers for you when we talk.

**PH That's OK.**

LZ And he tried to measure RNA together with Brachet.

**PH Was that Jean Brachet?**

LZ Yes. There was some trouble then about priority, I forget what it was, but somewhat unpleasant. He measured giant chromosomes together with Hans Bauer, who was head of the Max Planck Institution in Germany and when he had worked together with Leon Carlsson, that ended also very unpleasant, because Leon Carlsson built an instrument and wanted to get the patent, is it patent?

**PH Yes.**

LZ For it and Caspersson didn't want it.

**PH I can imagine**

LZ And Caspersson disliked all collaborators who succeeded to put their name first in the authorship.

**PH I noticed that. Now you can get computer publication lists and**

LZ It was one of the reasons why we always were 3 authors on the early chromosome papers, because then it was Caspersson et altere, you see from three it is et altere.

**PH And unfortunately your name began with 'Z'.**

LZ Once I was really angry. It was at the Paris conference in '71, a big conference and Caspersson invited us to some dinner and by some reason I was late. I had to talk to Ted Evans before, and there was a chair which was somewhat lower than all the others and Caspersson said it was for et altere. I was angry. And he had another collaborator. It was Henin [?] from Lund, and Albert Levan said, it is his project and he is the first author. Caspersson was angry. He has never got over this.

**PH What was your own first work then?**

LZ That was on plant chromosomes. That was the banding.

**PH So really you've been involved with chromosomes from the very beginning of your career.**

LZ Yes, I had more or less privately worked with chromosomes. I sold my, I had some jewellery and sold it and bought a microscope because if we wanted to make a thesis at the Institute, we needed a microscope.

**PH And I suppose that Caspersson's Institute didn't have the kind of microscopes that would be of use to you.**

LZ Oh that was wonderful. When I came to this Institute, I had heard about it, what a fantastic building. There was a big laboratory and there was one single microscope. And Caspersson said "Here is a microscope. If somebody wants to just to take a look on his preparations here is a microscope". So he thought you are just going look.

**PH So tell me a little about the work which led up to the banding.**

LZ Well we had a collaboration with the Children's Cancer Institute in Boston. The head was Sidney Farber who was a very important man in the United States, and at that Institute, I don't know how it was in, it was in medicine in general but everywhere the alkylating agents used for cancer treatment and well, what were they doing. So Caspersson was interested in this collaboration, and he had a collaborator from Boston, Ed Modest, he is figuring on our first paper, and he gave us a whole box of alkylating agents and he thought he might perhaps try some of them on fluorescent and he thought he might perhaps also increase this fluorescence, to put some fluorescent group on this substance and look what happened. And that was a terrible job because it took a very long time to come to a method to do it. Well, nothing happened. I believe he tested about 40 substances at least and you know how biological experiments, sometimes they work and sometimes they don't work. You have to repeat everything many times. So finally he found the substance which worked and he saw on these very big plant chromosomes, the very first band. Caspersson didn't believe in it. He thought something was wrong, but he got it all the time again, and he understood that it might be a very important thing. There was especially, the preparation of plant chromosomes was done in a somewhat different way compared with human material. You needed your thumb to squash and that meant that the chromosomes went in all directions. It was very difficult to count the chromosome number in squashed preparations because the chromosomes were distributed everywhere, but I had some pictures where I saw that the chromosomes which had about the same morphology had the bright bands in the same regions as other chromosomes. We had never thought that this was how it might be, homologous chromosomes. So these very first pictures made me think about, perhaps one should try to get homologous chromosomes and we saw that there was the same banding on them.

**PH What year was this?**

LZ Oh that was in '68. We had in our group also an Indian, Vak was his name. He's on our very first papers and we looked at many many metaphases and it appeared again and again,

**PH May I ask, were you using techniques like colchicine and hypotonic swelling on these plant cells?**

LZ No, no, nothing like that, but I had from my university time a programme for plant material, the old cytogeneticist, Geitler in Austria for instance, and they used carmine acetic acid and we used his method and we squashed. I had worked together with Kimber from Oak Ridge on protozoa and he used hypotonic swelling for the protozoa, so I learned it before Hsu established the hypotonic treatment for human cells. He used it later on for human cells but never for the plant cells. Then we found it was right to write about the plant chromosomes. It was a somewhat exotic method and very uninteresting. I know that I talked about it in England at some English meeting in London I believe. Among others were the group of Ted Evans there and then after that lecture they tried hard but they didn't succeed. Good for us! And then we wrote about it and then came the problem, OK Caspersson didn't believe in it. And therefore he suggested we shall write it in the Journal 'Hereditas', this Swedish Journal, and he couldn't say to us, the group, that he didn't believe in the technique but that was his reason. Hereditas was a very unknown journal and nobody would see it. If it was wrong, a mistake, then it would be forgotten a year from then. But if it was really something, then we had found it. The other reason for Caspersson was that Levan's 46 chromosomes had been published in Hereditas and that was OK for me, so anyhow I was the last author on the papers. And then I wanted to start the human chromosomes. I mean we looked at hundreds of plants to get a petal. There are very strange things. The usual plants that we used Vicia fava and Trillium and so on, they had bright bands on darker chromosomes but then there were other plants who had completely diverse pattern, Scilla sibirica for instance, but it was not interesting

any longer, so therefore I wanted to try other material. We were first thinking of mouse, because George Klein was at the same institution.

**PH Was George Klein then in Stockholm or in Lund?**

LZ No, no, he was in Stockholm in the Institute of Caspersson.

**PH Right**

LZ Caspersson had helped him very much, Caspersson was very generous to people from these countries, also Germany and in later years from Russia, Poland and so on. He had always some who he wanted to help. It was a very, very nice character of his. That was sincere I mean, not politics, otherwise he was a great politician.

**PH Which type of human cell did you then start working with?**

LZ We started with blood and Caspersson didn't want me to do it. I mean if you are very interested and the boss says no, you do it. You do it in secret.,

**PH So which year now were you starting to use blood.**

LZ That was at the end of the sixties.

**PH So the culture for cytogenetics of human blood was already established.**

LZ Oh yes, and 46 chromosomes did already exist and we were not allowed to ask Jan Lindsten. We were not allowed to call to Lund. All these people who could have techniques. Instead I was able on a more or less private basis, to invite a little student from Germany called Klaus Zang. He was later the boss of the Institute for Human Genetics in Saarbruggen I believe, working on meningioma.

**PH What was his name?**

LZ Klaus Zang – Z.A.N.G.

**PH So was he Chinese originally?**

LZ No he was German, Zang and he educated me how to culture blood and he helped me but we were not allowed to tell Jan Lindsten or anybody about it.

**PH Because Jan must have been now 2 or 3 years in Karolinska is that right?**

LZ Yes, he was here on this site and he had already written his thesis on the X chromosome in Turner, and he was a rising star, but he was not big at the time. Caspersson knew that he could do chromosomes, so therefore he shouldn't be involved. Well then he tried human cells and he didn't see anything and here I must say, it was worthwhile that I had the experience from the plant cells, where I had seen that there was a banding pattern existing. We didn't call it banding because we didn't know that it was on all chromosomes and all these things. But we knew that there were these bright regions and we looked and looked and looked and we got better and better optics. Caspersson had very good relationships with Zeiss and Leitz. Well I could use that too. I said I am working at the department of Caspersson and could you borrow me a very good operative. I got it and then we saw the very first band. We saw the Y chromosome and then we had real luck, because one of our students at the Institute, who is now a professor in Lund, he travelled to Germany and told everywhere, in our Institute they have very interesting methods to get bands on chromosomes and they can see the human Y chromosome, and so on and so on. But nobody used this.

**PH May I ask, was this looking at the Y chromosome in metaphase or in undividing cells.**

LZ It was in metaphases, but then we saw it in interphase too and we understood at once that it might be worthwhile for prenatal diagnosis. Well that was at the about the same time when Peter Pearson saw the Y in undividing cells. I went with these results to Uppsala to a cytogeneticist, the only cytogeneticist who existed in Uppsala, Bjorn Kessler and showed him our pictures and so on and he was not impressed at all. He said "Rubbish". So I have been angry for this many times later on. Well, we

continued and we found the bands on other chromosomes and we saw that homologous chromosomes had the same banding patterns, that was very important, which we had not seen. We had an idea about it in my very first Vicia faba preparations, had not really established it. But now we saw it in all chromosomes, that there were homologous chromosomes.

**PH So your first paper on the human chromosomes was 1969 or 1970?**

LZ Which paper are we always referring to? I believe we refer more or less to the '71 paper in Hereditas again, Hereditas and human chromosomes, because there it was really established. In the first papers where we wrote about the human chromosomes we made a lot of mistakes. For instance, we didn't realise that the polymorphic regions had nothing to do with the original. We have one paper in Experimental Cell Research. Today I am laughing about it. It was somewhat ridiculous. How we differ between chromosomes 13, 14, 15 just because of the polymorphic regions at the centromeres. But through all this, I must say that Caspersson always said he could not see any bands on the chromosomes, and he said this until his death. I talked to him two or three weeks before his death and he said he still didn't believe in the bands. He believed in the patterns which he could measure. It was such a crazy circumvention that he photographed the metaphases, and then he cut out the chromosomes and then he measured them and measured them and saw there was a top in the curve, and there was a top and then he had curves and curves and curves in all our first papers.

**PH I have always noticed that, that in all those publications you have these peaks, these troughs alongside the picture of a chromosome. But I imagined that those peaks and troughs were derived from some separate primary measurement rather than from the actual chromosome picture.**

LZ No they were from the pictures.

**PH So really, they didn't show any new information that the chromosomes didn't.**

LZ No. Caspersson at that time was really a big man for all his other research. I believe he was really at the top 3 times before, for his studies on DNA, RNA, but for the chromosomes he didn't believe it, and the most funny experience was the Paris conference. It was '71, yes?

**PH It was '71. I was there.**

LZ Ah, so you see Caspersson was already on the airport to go to Paris when, what is the name for the man who makes all, the chauffeur from the institute?

**PH The driver?**

LZ The driver came with big cartoons and these curves. They were curves that were measured during the very last days, where Caspersson had been on the countryside and went from there to the airport and he came with all this material which Caspersson wanted to take to Paris. It was quite nice. I was close to Hamerton. Now Hamerton, I don't know to whom he talked but he said "Caspersson has come with all his curves. What shall we do" All the curves. Because everybody in Paris, well not everybody but very many people, had already fluorescent pictures and I mean Giemsa banding had already come and so on yes, and Caspersson came and wanted to demonstrate that you needed curves to identify chromosomes.

**PH Can I ask in terms of the actual microscopes you used, did you at that point have to have a special attachment for fluorescence or . . .?**

LZ No, nothing existed. Fluorescence had come during the fifties to biology, but the microscope equipment was so poor that you couldn't see very much. I was later on invited to give a little course in Copenhagen at the Institute of Jan Mohr and he wrote to me "we have a nice fluorescence microscope." You couldn't even see the Y chromosome. It was a very interesting course. But I have to say one thing, when we started to see the bands on human chromosomes, Caspersson didn't believe in them, but he could measure them and I must ask my old collaborators whether he at that time measured in microscope directly and looked at them. I don't think so, but anyhow he didn't like it generally spoken, but I had talked about the Y chromosome at the conference in Reykjavik and

unfortunately I have never saved my manuscript. I had to show Caspersson my manuscript and I had written about it and he wrote on the big space on the side 'This is ridiculous. You shouldn't talk about it. People will laugh at you'. But anyhow, Albert de La Chapelle was there and he became very interested and the next opportunity he came to Stockholm.

But when he came to see my chromosomes, Caspersson thought there might be something with them. Anyhow I showed him.

**PH Can I ask, were the fluorescent stains you used initially very short lasting?**

LZ No, not quinacrine mustard. All the others yes. I mean you can get bands with proflavine, acriflavine, there's very many other substances, and also there is this medication which is used for some influenza type. Lots of all this big collection of samples which we had got from Boston, there was just only this quinacrine mustard which worked. All the others were short lasting or had a very weak fluorescence. I mean today you can see fluorescence with the very weakest staining, with all this microscopic equipment. It was at that time much more difficult and most of them faded away when we looked.

**PH At what point did you start to use these techniques diagnostically?**

LZ Much later. Much later. I retired in '89 here, at the Institute of Karolinska and I am living far in the countryside. It became too expensive for me to continue. Jan Lindsten offered me to work here, but it became too expensive so therefore I went to Uppsala to the Rudbek Institute at the University Hospital and the boss there was Karl Eric Gustavsson. He was working on mental retardation and I asked him, I said "I want to work with fluorescence and chromosome identification" and he said "but I don't think that they have a future, but anyhow you are so welcome and I am happy if you come" and he helped me also to get a microscope. I borrowed the microscope from here, which I had got from the research council, and with permission you can move it to another place. So I worked for the very first years in Uppsala and I got free of charge material from Vysis, the company which now is Vysis. I don't know what the name is.

**PH I think it's still Vysis.**

LZ I got I remember, a very enthusiastic letter, when I wrote that had used it for diagnostic experiments and it was not at that time used for that, and they were very happy that it worked and that I should continue in that direction.

**PH What about the years from the mid seventies to mid eighties, because am I right, Caspersson must have retired at some point during the seventies.**

LZ Yes he was very astonished that he had to retire because he had expected that they would keep him because he was so important, but I believe he retired at 67. He was born in 1910 so he was 77.

**PH So did you continue working in the same lab after Caspersson retired?**

LZ Yes.

**PH or did you move labs?**

LZ No, no, I was in the same lab. After Caspersson came Nils Ringertz and that was very tragic for him perhaps because they didn't go along with each other and Ringertz, all the years, he died last year. It's almost two years now. He always had the feeling Caspersson was against him, but Caspersson did everything for him. Caspersson helped him to become editor of "Experimental Cell Research" and that resulted in Ringertz got contacts lots of people who he had no chance otherwise to meet.

**PH And did Ringertz develop the more medical links, or was it still very separate.**

LZ No, no. It was completely separate.

**PH Am I right this was in the Institute building across . . . ?**

LZ On the other side of the street.

**PH And so, I'm trying to work out in my mind, whether there still were any contacts with Jan Lindsten's department.**

LZ No no no.

**PH So not really.**

LZ No contact. Contact has been at the very early time, because Caspersson when we had the banding and when we had published it and then came the G banding and so on, then Caspersson wanted clinical material and it was a great opportunity to contact this Kessler in Uppsala. I thought he was somewhat if not so close, perhaps Caspersson will agree, but Kessler said all the scientists want material and we are doing the job. Finally we had no possibility to get material. Finally Caspersson contacted Lindsten and Jan was very enthusiastic and it was a wonderful time for collaborators here. He was fantastic and he has such easy writing, so he wrote. We made the experiment one day and the next day he had the paper. Then came the work with photographs. It was such a nice and beautiful time, and at that time came Maj Hülten. She was a student in Jan's lab and her thesis was finished and then she wanted to get it nice, this fluorescence also, and identification of the meiotic chromosomes and then she came over to us and made fluorescence in our lab and we helped her with identification.

**PH I suppose then after a few years with giemsa staining, there was less need for fluorescence on a diagnostic basis.**

LZ Oh it was a big difference, because giemsa was cheaper, you don't need the expensive microscopes. You can sit in the normal room. Nobody is very happy to sit in the dark for more than two evenings, so that was a big difference. But I remember Janet Rowley in one meeting in Chicago where we discussed banding, she talked about giemsa and said "No technique has caused so much blood and tears as giemsa", because it didn't work at the beginning. It took years before the routine technique would be used as today.

**PH Did you collaborate with Janet Rowley at all, in terms of chromosome 22?**

LZ Yes. Oh yes, we did a lot together. It was very nice.

**PH And then what year was it you moved to Uppsala?**

LZ 1990, many things are still left here.

**PH And you have continued to work there ever since.**

LZ Yes, I was able to equip the department there with very nice fluorescent equipment a confocal microscope from Leica which unfortunately became out of date quite soon because it was not able to get DAPI together with red and green fluorescence. It was just therefore the old fluorescence picture that you see in all Journals have always opidium iodide as background. Now it is always there, DAPI, so therefore you need now other microscope. But at all the time it is used for diagnostic purpose.

**PH Are there any other people who you worked with, either as colleagues or collaborators, that we haven't mentioned that you feel were closely involved in your work in these years, that we ought to talk about?**

LZ It was this Indian guy Vlag who on the very first plant papers. In that connection it was also strange. Caspersson believed that the alkylating agents might indicate the breakpoints in chromosomes. That was never - I mean you have in all fields, you start something and then you have to stop because it doesn't give anything, and as I said, Ringertz has never worked with banding.

**PH Did you have links with Marina Seabright at all?**

LZ I know her from conferences and so on but we have never collaborated, but I should perhaps say the R banding which usually is not discussed very much in the literature but many institutions are using it and prefer it, that is a fluorescence technique and I mean for routine G band is better.

**PH If you look back over your work, and I have been asking everybody this question, is there one particular piece of work that stands out in your mind as being something you are especially proud of and especially identify with?**

LZ Oh, the first detection of the Y chromosome was a very outstanding experience, because you could imagine there were lots of questions which could be solved by easy identification of the Y chromosome, and then I was most interested in tumour chromosomes and I worked a lot on leukaemias and lymphomas. Actually when Janet Rowley had seen the 22 translocation we had seen it too. We didn't understand it, but OK.

**PH And can I ask, the Y chromosome, before you used banding it must have looked very similar to the other chromosomes.**

LZ Yes you couldn't distinguish it.

**PH So that must have been quite a spectacular finding for you to suddenly see this chromosome.**

LZ I mean you saw the 21, 22 and the Y. If you had 5 chromosomes one must be the Y, usually if it wasn't a Downs. And the morphology was somewhat different; you must know all chromosome research was done at the beginning was done on quite contracted chromosomes. The goal was to count the chromosomes and the more contracted the chromosomes were, the better you could count.

**PH But the less good the morphology.**

LZ It was not interesting, Morphology was totally uninteresting. Nobody could see a translocation or anything.

**PH And then with your fluorescent techniques, I suppose the Y just stood out immediately**

LZ Yes of course. At the beginning we didn't see it. The optics were not good enough, but then with better optics it became more and more and finally we could see it in the interphase nuclei.

**PH The other thing I have been asking everyone, is, is there any one particular person who especially influenced your career and research that you feel you can identify?**

LZ Well, I have been educated at the lab of Caspersson. I'm very good at optics and that has been very valuable for all our instruments. Still if you are buying instruments normal students or if they come from high up positions don't know anything about optics and it is so important.

**PH Would it be fair to say that although Caspersson must have been an extraordinarily difficult person to work with, it was still a very good experience to be working in this lab?**

LZ Oh it was a fantastic experience. I mean there came people from all the corners of the world. Caspersson had lots of money and he was very social so everybody who came was invited to his country house and all the institution was invited once a year to his home and it was a fantastic atmosphere. It was so stimulating in any way. And I mentioned it was deeply tragic that Ringertz didn't go along with him. So Caspersson left, when he retired, to the hospital because he understood that he couldn't be in the building any longer. He got a room yes in the basement, far away from everything else, but of course Ringertz was perhaps afraid that Caspersson would have mixed up

**PH Looking back on your time in research, it must have been a wonderful time to be able to work in human cytogenetics.**

LZ It was wonderful. It was completely fantastic and Caspersson was very generous; so he quarrelled with my husband and my husband left in the June, in rage really, but Caspersson never took it out on me. I mean he could have said please find another place to work. That was before the banding, he had many many very very good teachers and all these people came. I mean what a time, everything has changed. I remember, Hg what is it, the English name for it?

**PH Mercury?**

LZ Mercury was distilled in the lab, in the open lab, then we got two American scientists and they said to Caspersson, if that isn't changed we leave tomorrow and they had big fellowships and then it was changed. And the early instruments of Caspersson they were one of the special tricks we are proud about was the microscope table, the movement of the microscope table and that had to be very exact. He needed two cylinders with mercury, two litres of mercury in two cylinders and they were covered by a petrie dish. Then it was completely dark in the room where the measurements were performed and then somebody came into the room and asked for somebody and one of those cylinders fell down and then the cleaning lady came with a normal . . .and I mean it is incredible, she was so astonished before when she put it on this, what is the name for this,

**PH mop**

LZ Then she put it on this table it was so heavy.

**PH Well you seem to have survived.**

LZ I mean, you must see how everything has developed from that time

**PH Thank you very much indeed. I am going to turn off the machine now. It has been a great pleasure and privilege talking.**