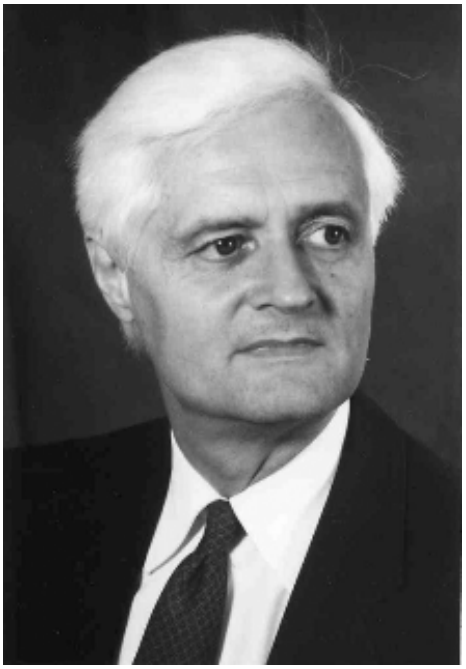
Friedrich Vogel



Personal Details

Name	Friedrich Vogel
Dates	1925 - 2006
Place of Birth	Germany (Berlin)
Main work places	Heidelberg
Principal field of work	Human mutation, Medical genetics
Short biography	See below

Interview

Recorded interview made	Yes
Interviewer	Peter Harper
Date of Interview	17/12/2003
Edited transcript available	See below

Personal Scientific Records

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

Biography

Friedrich Vogel (1925 – 2006)

Friedrich Vogel was born and educated in Berlin. After World War 2, in which he was a prisoner on the Russian front, he trained in Medicine in Berlin and then joined Hans Nachtsheim to work on retinoblastoma and human mutation, a field that became a life-long interest. In 1962 he was appointed to the new Chair of Human Genetics at Heidelberg University, where he remained for the rest of his career.

Vogel was a key figure among the new generation of German human geneticists responsible for reviewing the field after the abuses of the Nazi period; he was co-founder of the journal *Humangenetik* (now *Human Genetics*) and in 1979 published, with Arno Motulsky, the landmark textbook *Human Genetics Problems and Approaches*.

INTERVIEW WITH PROFESSOR FRIEDRICH VOGEL, 17th DECEMBER 2003

PSH. This is an interview with Professor Friedrich Vogel in Heidelberg on 17 December 2003. We met at the offices of Springer Verlag.

PSH Friedrich, first it is very good that you have been able both to spare the time and are happy to talk in this way and I would like you to be able to say anything you think is worthwhile, but what I have asked everybody I have been interviewing is, how did you become interested first in Genetics?

FV. Well, this is a very interesting question. I studied medicine in Berlin and then I did have an internship. At that time an internship was for 15 months and I was to get what you call your final approbation as a doctor and this was finished for me in February, no, excuse me, it was in January 1953. And now I looked around for interesting fields to work on. I wanted to do some science and in school I had learned about Mendel's laws and then I had visited sometime seminars of the Max Planck Institute in Berlin with Professor Hans Nachtsheim. This was an Institute of general genetics, especially animal genetics, and the rabbit was the main experimental animal but they also looked at micro-organisms also. And now I visited Nachtsheim.

Now I got the feeling that Mendel's laws were an interesting, let's say a theoretical basis for scientists. My earlier idea was to become a psychiatrist but the problem with psychiatry was that there was no real basic theory and for genetics I had the feeling there is a good basic theory and then I visited Nachtsheim and I asked him if I could work in human genetics, whether he would be interested in somebody working in human genetics. And he told me, well I shall make up my mind, maybe you come up about a fortnight later; and a fortnight later I went and he said "yes you might be interested in the field of human mutation." It was very fashionable at that time, human mutation, because of the radiation protection and so on; spontaneous human mutation rate was not being done by anybody in Germany and maybe you are interested in this field and he said "I will ask for a grant for you from the Deutch Forschungemeinschaft". This is a kind of research council, German Research Council and they are giving grants for a year or two years, or so. You might have such a grant. And then I said yes I would like this. I got this grant. It was very low at the time. It was 360 German Marks a month. But still at this time, my wife was still working and we could manage.

And now I, yes, I went and started on human mutation and then I looked at the first work by Jim Neel and his co-workers and by Haldane and so on, people who had worked in this field, Penrose, and now the problem was, which trait, which disease do I select. I selected retinoblastoma for a very practical reason. The reason was that all cases of retinoblastoma in the area where we were living around Berlin and so on were coming into the University Hospital of East Berlin and there were only a few ophthalmologists, whose addresses were known, so that I could easily find out the cases. This as you know is a

very important problem; with any other diseases where I could have worked it would have been, for a single person, almost impossible to get information.

Then of course I got this information and I started my first work on the genetics of retinoblastoma and I found out that people who had worked on it already, like Jim Neel, had made in my opinion, a slight mistake. They had assumed that all sporadic cases of retinoblastoma would be germ cell mutations and this was certainly not the case, since the selection against this disease in the parent, in the generation of the parents, was so low, one would have expected more cases, many more cases of hereditary retinoblastoma among the present cases, and therefore I decided that not all of them could be germ cell mutations. There must be a high percentage of somatic mutations or what we called at that time phenocopies. And this later on, as you know, this turned out to be

true and as you know the theory about it is of course, is the present theory of retinoblastoma not only, but of many other tumours, many somatic mutations and a couple of germ cell mutations and in retinoblastoma we do know that the binary cases are all germ cell mutations but only a small minority of the unilateral cases.

This of course I worked out at the time, it was a sort of first success. I did it completely alone, without any help, any technical help and then at that time, some people published in Switzerland, a man named Fornio [?] published a kind of survey, epidemiological survey of all cases of haemophilia in Switzerland and I used that to study the problem of mutation rate of haemophilia. This was a first for human disease where mutation rate had been estimated, by J B S Haldane. So this way I started my interest in the field of human mutation.

PSH So really this early work set the pattern for what you might say has been your biggest contribution over the years.

FV Well yes, this might be, you can say, but also I also started something two, one or two years later. I started something on the electroencephalogram, twin study on the electroencephalogram, and later on I worked on the genetics of the electroencephalogram and also on the significance for this field of human behaviour genetics and this has not set any pattern. I have not been followed. Some people have done some work, some people who had worked with me, later on they started to work out some molecular aspect of epilepsies but the entire field, and this is for me an interesting field, that now a single disease phenotype, very obvious disease phenotypes are being studied very often and now with the start of molecular genetics, all the medical geneticists went down to the lowest possible level, the level of the genes you see in the DNA and the levels in between, you see. Between the genes, the physiological levels and so on between, they are not being looked at any more and in my opinion this will come back to this field, but at the moment with this special field I have published a book on it.

PSH I saw that, yes, and I haven't read it yet, but I think I saw a nice review of it.

FV Yes I saw the review

PSH. In Human Genetics

FV Yes in Human Genetics, but the review only came out after, when I was at the Congress in San Diego, you see. Where we met in the San Diego at the Congress, I came on the invitation of Springer Publisher company because we wanted to reconstruct our textbook and then there was also an exhibition and I went with Rolf Lange into the exhibition of Springer Verlag New York in this Congress. Then my book had come out a short time ago and I looked at it. I said to Rolf Lange "It's unfortunate you do not show my EEG book. Why don't you show it here?" He says it would have been, of course they did show my textbook with Arno Motulsky, but they didn't show the EEG book and I asked them "How is it possible. Don't you want to sell it or why did (n't) you show it to the people ?" And then he said "Well this exhibition was not started by Springer Heidelberg. It was started by Springer New York and of course they did so well you see" and something like this. And then I told him "Well maybe you can ask Peter Harper for Human Genetics to take careful review on this book". And this review has surely come out and it is a nice review you say?

PSH I have seen the review but I haven't yet read the book so I must do that.

FV I think you should do it.

PSH And I think what you say is absolutely right that the genetic basis of these physiological phenotypes, I think is going to be very important.

FV Precisely yes.

PSH Very much as we were talking about Kalmus,

FV Yes

PSH The sensory aspects, it's something which I think which people will wake up to again in a few years.

FV I hope very much that I shall still be able to see it but anyway, let me continue. This also has to do with what we are talking here in this place. We talked about my work. About 1957 I got a visit in the Institute, the Berlin Institute. An unexpected visit. And the visit was by a man Dr Heinz Götze [?]. Do you know who it was? Dr Heinz Götze was the boss of Springer Publishing company and he asked me "Would you like to write a textbook of human genetics for Springer Publishing Company?". Of course you have to remember '53 I started, '57 I got this visit. This means that after I had only worked for 4 years they wanted to have a textbook.

PSH You must have done well in those 4 years.

FV Well then of course I did write the textbook. It was my first textbook Lehrbuch "Humangenetik" which appeared in Springer 1961. Later on, Götze became a good friend of mine. He died a couple of years ago. He was 88

years old and died a couple of years ago. He was a senior boss of this company. And Götze visited me and later on, many years ago, when we were together at the Heidelberg Academy of Science – later on I tell you about Academy of Science. Anyway he was also there and when you are introduced into the Academy you have to give a report of your life for the other scientists you see. And this was I think in 1959. You are normally accepted when you are fairly old and it was 1989 and I told them the story. Well this man Götze came to me asking me for a textbook after I had only worked for 4 years in the field and this I think is a very risky, very risky undertaking. They only told me how he came to it Nachtsheim, my boss, well no, you look for anecdotes you see. Later on he told me that another textbook had been offered to them which they didn't want to take . They wanted to have something new and you know what this textbook was? It was the Human Genetics textbook by Verscheuer. Verscheuer has published such a textbook in 1959. This book is fairly bad in my feeling. You know it? No?

PSH I have never read it but I can imagine that with Verscheuer's past it would be difficult

FV Not only past. Even with a past like this it could have been an excellent scientist. This doesn't exclude. But anyway, the book was really bad when I found it later you see. Anyway I published this book in 1961 and now my colleagues, the middle-aged colleagues who were students at that time, they told me that this book was very decisive for their decision to work on human genetics and how to think in this field and so on. And now let us continue with Götze. I came to Heidelberg at the end of '62.

PSH That was from Berlin?

FV From Berlin yes. I was in Berlin with the Privatdozent, you know the Institution, the 'Habilitation' in Germany?

PSH Yes

FV I got this in 1957 and then I was asked from Heidelberg, the Heidelberg faculty, and it was important. Later on after the Congress in Copenhagen, the Congress in Copenhagen was in '56 and there I met Jim Neel for the first time by the way. And during this Congress I did meet also very interesting person, Professor of Ophthalmology in Heidelberg, Wolfgang Jaeger. He might be interesting for you when you are interested in Kalmus. Jaeger also had worked in some field of medical genetics for colour vision defects and so on. And anyway, we talked about my studies and retinoblastoma and I said what I would like to have would be a direct study on progeny of retinoblastoma cases. It must have gone up and then you have to look at the progeny. Then he said, well in Heidelberg he did have all the files from all these people, starting with 1880 or whatever and you have to send somebody into the basement of the Ophthalmological hospital and look at the old files. And then what we did, I sent there my friend and technician, by the way when you see the book on EEG, it is dedicated to my former technician, my assistant. She is still a good friend of mine. Anyway she came here, looked at these files and she found out the files of these retinoblastoma cases who had been 2, 3, 4 years old at the time when they were operated on and now, but you had a

clear diagnosis, unilateral or bilateral and so on. And then I wrote to their, what you call inhabitant officers. Well we in the year, let's say 1900, we had a person that was named from your place and is the person still living and if so please give me the address. And this is what we actually did. We did get the addresses of most of these people. Of course this fortunately was not East Germany, it was West Germany, and in West Germany the social structures had been maintained all this time. And then I wrote a letter to these people, Dear Mr & Mrs and so on, in the year of 1910 we saw you in the University Hospital of Heidelberg and we would like to see you again to see what has happened with your retinoblastoma. And then of course in most cases we got favourable answers. Then I went around and now there comes another, at this time I was not yet able to lease a car. I had to take lessons for car driving in order to rent a car to come here and look at all these people. But then we go out and we found out that the hypothesis that which I put up with my first study was mainly correct. You will see it was. The figures shifted but it was mainly correct. But it was still the first case of a human tumour with these – yes.

PSH that is really fascinating.

FV And then of course, and now this man a few years later, this man became the full Professor of Ophthalmology in Heidelberg and his influence in the faculty was good enough to take this young man from Berlin. There was another important person. We did have a surgeon, K H Bauer. This man Bauer was a leading surgeon in Germany after the war and he was also important in the faculty and he had written a book some years ago, 'Das Krebs Problem'. The cancer problem in German. And therefore he was specifically interested in the retinoblastoma story and he also helped very much. So the faculty decided in favour of me. But in the last moment there were complications. It was very interesting. All that I tell you now you should not write in any book, but you can actually know.

Some of the established anthropologists in Germany, for example Gieseler in Tübingen, they didn't like a young man coming from Berlin and so on and he wanted to have here an old Gieseler in Tübingen, I always say SS comrade It was true. They were both in the SS. An SS man named Schade who later on became a Professor in Dusseldorf He wanted this man. He was older than me and so on and anyway, this is but maybe it's not true that the Nazi connection was actually the important point, but still the facts are there you see. Anyway the faculty, he even came here to talk to the Dean about these things but the faculty said 'No. We have decided in favour of this young person and we do not change our decision. But anyway this is why I came to Heidelberg. Then in 1964 I was again contacted and meanwhile in '61 the book had appeared – the textbook – and the textbook had excellent reviews, even excellent international reviews. One of the best reviews by the way was by Franz Kallman in the American Journal of Human Genetics; excellent review and he claimed and Götze knew this of course and Götze contacted me again and said "Well, we do want to make a new journal in the field of human genetics. We do have a certain journal but the main promoter of this journal is the psychiatrist Kretschmer, the constitutional research Hans Kretschmer. He has died in the meantime and we want to do something absolutely new and why don't you get some, collect some people and you

should start a completely new journal in human genetics". And then I thought about it who to pick, and there was an excellent older, slightly older human geneticist who was a very good friend. This was Peter Becker. Peter Becker, muscular diseases and so on, you know?

PSH. I knew him well ---

FV You know him also quite well? He was excellent, he was a good friend of mine and he is an excellent medical geneticist. One of the leading figures but then the second person I decided in favour of was Helmut Beitsch. Herman Beitsch in Freiburg at the time. He had become Professor of Human Genetics at Freiburg and Gerhard Wendt. He was professor at Marburg, who started the first genetic counselling unit in Germany. Organised genetic counselling unit and then of course we said we cannot have a journal only exclusively with German editors and we do have to have a very good American and at this time Arno Motulsky had become a very good friend of mine, which he still is. And then we asked Arno.

PSH How did you come to know him in the first place?

FV Well I can tell you. It was very interesting. Arno, I met him at the International Congress of Human Genetics in Copenhagen. The Copenhagen Congress was very important for interconnections between human geneticists. And in Copenhagen he gave a paper which was very interesting for Nachtsheim; as I told you Nachtsheim my boss was an animal geneticist and Arno gave, now I forgot, you may have checked, a paper on some animal blood disease. I forgot precisely which it was, other than it was a kind of blood disease of some animals and Nachtsheim anyway got interested in it and Nachtsheim invited Arno for a lecture in Berlin at the seminar of our Max Planck Institute. And Arno came to the seminar in the winter '56/'57. He gave this lecture and afterwards, with Nachtsheim and me, we went together to a certain restaurant and then we talked about many things and at that time we started to realise that we had similar views for many problems in human genetics. Then of course it happened in 1958. '58 there was an International Congress of Genetics in Montreal in Canada and after this Congress I had asked for it. I stayed from August to December in Ann Arbor for a while.

PSH With Jim Neel?

FV With Jim Neel yes and especially with Jack Schull. Jim Neel at this time happened to be in Japan with Jack Schull and I studied for the first time, I looked for a genetic counselling unit, an organised one and also some other field. You may even know Eldon Sutton. I worked in the laboratory of Eldon Sutton and so on and I looked around in the international, and Jim, Jim Neel already knew my work on human mutations, retinoblastoma but also haemophilia and so on. And then after this, after my visit I made what you normally do when you are in a country like this, kind of lecture tour. I first went to San Francisco, to Berkeley to Kurt Stern, you know his book, Principles of Human Genetics?

PSH Yes and I remember him well also

FV Yes, yes, very nice person and I went to Kurt Stern and then from Kurt Stern I went to Seattle to Arno Motulsky and I was for the first time, we talked about many things. And later on of course, contacts were intensified with some of the meetings on anthropology and so on, then we met at a certain meeting, I there asked him whether he would be interested in co-operating with me on a new version of textbook.

PSH So the book of yourself and Arno Motulsky, did it grow out from your original earlier book in Germany.

FV Yes

PSH Right

FV Yes definitely. And also as I told you, at that time Heinz Götze and I had we were very good friends, we had become very good friends since I worked in Heidelberg; he was in Heidelberg, I visited him at home. He visited me at home and so on, then we talked and then he told me why don't you think of a new edition of your textbook and now it was in the early 70s, but now it should be in English and it should be if ever possible be with an American co-author. This was very important also for selling the book and therefore I contacted Arno and he co-operated and he even arranged that in 1976/77 I could spend together with him an Academic year in Stanford, Centre for Advanced Study into Behavioural Sciences. Very interesting place in Stanford. And we stayed there and worked on it.

PSH Because that book really was an absolute landmark in the development of human genetics.

FV Oh I see, I am glad to hear this, yes.

PSH There is no doubt, because this was the first time there was a really comprehensive textbook that everybody could use.

FV Very good to hear this, and the book was also successful you see. Let me tell you the book also came out in 79. It was reprinted as it was, since it sold so well in '82 and then we had new editions in '86 and in '97, something like this, a new edition. And then we had translations. Now you will actually be surprised. A book of about 800 pages, not simple to translate. We had translations in Italian, Portuguese, Russian, Japanese and Chinese.

PSH And I expect that the Chinese and the Russians didn't pay you any royalties!

FV They did.

PSH They did. That's . .

FV Well they normally, they had to kind of arrange something with Springer Publishing Company of course and the situation was that the whole of, half of the money goes to the publisher and half of the money goes to the authors

and this means, oh it was several thousand German Marks at the time. Not quite as much as I got for the English edition but as you now, but still it was

PSH Worthwhile.

FV Worthwhile yes.

PSH Can I take you back Friedrich, to after you came to Heidelberg; when was it that you were able to develop a full Institute? Was this from the beginning or did this happen at a later point?

FV No. From the beginning. They had of course, a problem was we had a kind of Institution, well let us start again with the International Congress '56. And at that time there was a feeling that when you look at the papers given of course at the beginning of the Congress, the keynote lecture for the congress was given by Muller, the discoverer of radiation genetics, and then at the Congress the Minister, the Minister of Denmark gave an introductory remark and so on and saying that human genetics is so very important because of the danger of ionising radiations in humans and so on. And this was what happened in Germany. There was a conference in '59 on human mutation and so on and on radiation dangers and there was also, and we do have an Institution called Wissenschaftsrat and this is a kind of scientific council, the Government has introduced this institution in order to have recommendations which sciences should be stressed in the future and so on and they gave the recommendation in 1960 that any medical faculty in the course of time should have an Institute of Human Genetics. And here in Baden-Wurtemberg in the state where we are living, they are normally very active in following those recommendations and they started a new Institute of Human Genetics in '61 in Freiburg, which was Beitsch, Helmut Beitsch, and then also in Heidelberg which was '62 and this was not only a Chair it was an Institute. It was an Institute, I think I got 5 positions for scientists and about 8 or so positions for technical and other persons. This means it was quite an Institute, and then of course we were able to go up later on. It was very interesting, in 1965 or late 65/66, my former faculty in Berlin, the Free University of Berlin, they also wanted to found an Institute of Human Genetics and they offered me this chair, but I asked them, I went there and asked them what are the conditions. What do you give for the Institute, positions and money and so on and then they were absolutely surprised that I was so well equipped and they told me it is completely impossible. We cannot have anything similar like this.

PSH At what point was it that you were able to bring clinical genetics and genetic counselling into the Institute.

FV This is a very good question. Well when we started, I had, I told you, a number of science positions and one of these positions was what we call over-assistant, my chief resident, or something like this. Meanwhile a good friend of mine who I knew from my schooldays, Walter Fuhrman, he had become a Paediatrician and he had become recently Privatdozent in paediatrics in Berlin and he had been to America for a couple of years and so on and he had started working on, also I had taken some influence, I had started working on human malformations, and the hereditary aspect of human malformations and looking for families in paediatrics. Therefore I asked him

whether he would be interested to take this position of a Chief Resident in my Institute, since I had very little or no clinical experience and to do some work on medical genetics. And he came to me and he started. Also it was very important for us to go to the University Hospitals to see our colleagues over there and to have some connections.

And then independently of this we did have one also one nice thing. I heard from my cytogenetic lab. I had hired a female medical doctor who only got her doctor's degree at that time in another field, Dr Traute Schröder, and Dr Schröder built up our cytogenetics lab. And it was, and she started of course under very primitive conditions in the beginning of 1963, I had taken over in November '62, 1963 she had started in the basement of the building which we had rented to do some childhood genetics and so on but she was very able. And then at the end of '63, at the beginning of '64, we were asked to come to the University Hospital, to the Medical Hospital for medicine and they had an interesting case with what would be Fanconi's anaemia. And then we started looking and we should look at the chromosomes. Dr Schröder started to work on this case and his brother, a very young man, with Fanconi's anaemia, later on both of them died from cancer by the way. Anyway she looked at them and then she found something very strange with the chromosomes. Chromosomes looked scattered in a certain way and then some strange where they were broken and so on and then we looked at it and I had known from my work in human mutations that there are mutator genes somewhere in genetics and then we thought this might be the effect. This might be a kind of genetic basis for chromosome breakage. And this is what we published also and here I did have the big advantage of being some of the editors of German *Human Genetics*. This means I could accept and publish such a paper; with any other journal with any other editors it would have been turned down. I am absolutely sure, it would have been absolutely turned down, since people would say, well this a kind of culture, kind of dirt,

PSH Artifact

FV Artifact of some kind, dirt and the culture or whatever. Anyway, yes and we did publish it and this was in fact the discovery of chromosome instability. And which has become very important. I don't have to explain to you, that it is very important for cancer research. Now the more recent situation in this has been published in the Conference where Dr Schröder has participated, which was in Würzburg. Maybe the German, *Medicingenetik*. It was published in German and there was a whole Conference volume on Fanconi's anaemia

PSH That's nice because that was a major discovery.

FV It was a major discovery in medical lives, of course and Dr Schröder, then later on was also very interesting, 1966 there was a congress in Chicago you may remember, International Congress in Chicago. She went there and after the Congress she visited a couple of American cytogenetic labs. Obviously as you normally do when you go to a Congress like this, you look at some other such institutions. She went there and then people normally would say, "Now we shall show you the really first case of Fanconi's anaemia". They opened their files and showed them here chromosome breakage in this case and that

case and so on, only nobody had published it since everybody had believed that this is artifact. But do you understand?

PSH It's amazing how you can misinterpret things if you want to.

FV Same as anticipation. Exactly the same thing. Well this is how we started. Then genetic counselling of course, you may know that together with Walter Fuhrman I have published a booklet on genetic counselling.

PSH I wouldn't call it a booklet. It was a very nice book.

FV Anyway, but it cannot compete with your book you see. It was never the idea that it should. But, no, but it was at that time.

PSH It was a very good book.

FV It was at that time, it was a nice book on it and it also became very popular you see. And there were also translations into English, Portuguese, Spanish, Polish, Japanese. Two Japanese editions. Anyway it was very, at that time it became very popular. Later on we didn't follow up. And now we did have, people came to us asking for counselling you see. This is how it started. It started the public asking for it. And then we had a conference in 1969. My colleague Gerhardt Wendt made a conference on genetics and society in Marburg. And at that time I gave, during this conference I gave a lecture on genetic counselling explaining some of the problems. And now, immediately after the conference there were also some official people from the Ministry of Health and so on and immediately after this time, Wendt told me "What would you think about a kind of model institution on human genetics in Marburg". And then he started a kind of model institution which was specifically paid by the federal government and so on and special money and special organisation and so on and this was what they did. And then immediately afterwards Frankfurt got it and we got it and this means it had started in the late 1960s or early '70s.

PSH Can I ask, was this money from the Government to develop genetic services, was it put into the same institutes that were involved in research, or were the activities quite separate?

FV No, not quite separate. It was given to the institutes for research, in Marburg and also Heidelberg and was directly given to the institutes but with the specific purpose to build up the genetic counselling unit.

PSH Right.

FV This means the genetic counselling unit was always and is still part of the institute and as long as I was head of the institute it was what was called a section inside the institute. My successor, he has directly, he directed all this special institute as well, what we call polyclinic at this time. I thought it might be better to have a separate section and the man who did it, well there was a couple of people who did it, but the man who has done it since 1980 is a man named
[?], I think he was quite a . . . . . .

PSH Yes. One of the things I was wanting to ask you was about some of your main colleagues who helped to develop the field in Germany. One of the people in particular was Widukind Lenz.

FV Precisely. Yes

PSH Because he must have had a huge influence in the development, along with you

FV Of course. He did have. Widukind Lenz, of course you know his father had been very important, Fritz Lenz, and I gave a kind of, when Widukind Lenz became 70 I gave the – what do you call Laudatio?

PSH Oration

FV Oration for him. Let me see, we were really very good friends and his main influence was not so much, lets see now, his influence was based not so much on medical genetics but also the thalidomide story, this made him famous. It's not very logical but medical genetics has become popular in this country in part or mainly due to the thalidomide story, despite the fact that it has nothing to do with genetics. He also became the successor of Verschuer in Munster in 1966 or '65 no it was 65 yes, 64, 65. It was also a very interesting development. I don't know whether you know the book of Gerhardt Koch?

PSH I do

FV Yes you know, and Koch describes the discussions in Munster regarding the succession of Verschuer. The first candidate they had was Peter Becker and this was what happened, an excellent choice, no doubt about this. And then Peter Becker joined us, as I told you, in the Journal Human Genetics and Götze had told us well, we have to make this journal without Verschuer and Verschuer has to be out and this mustn't, for these there was no problem. There was no problem with Peter Becker. However, the people in Munster said, well, he has joined this activity with these young people over there with the journal and now he cannot become successor of Verschuer.

PSH Yes.

FV This is an anecdote you see. But when you read it in Koch's book, there you can find it. He also wrote it.

PSH This must have influenced the climate around the way people were appointed.

FV Precisely

PSH. And so . . .

FV And like myself of course, the story with Gisele who came here to tell them that

[?] should be the man and not me

PSH So Widukind Lenz was then appointed in Munster . . .

FV Precisely.

PSH after Verschuer.

FV after Verschuer, immediately after Verschuer yes.

PSH Would I be right in thinking that he and Peter Becker and yourself probably have been the three people of your age who have developed things most strongly in Germany?

FV Yes, I would think so. I should actually think so and our emphasis was on different fields, emphasis was on yes, different aspects and so on. The person who was also important, especially from the point of your organisation, was Gerhardt Wendt in Marburg, studying in genetic counselling and so on.

PSH I had connections with Gerhardt Wendt through the work on Huntington's.

FV Oh I see, yes, of course I can understand this very well. You are interested, you have been interested in this and he has been. Only the collection he did, well this was in a certain way a failure, this collection of Huntington cases. I also took advantage of them since, this is one of my minor things, sometime I studied the EEG of chorea Huntington and this was not very . . There was some interest in reports of literature which could have been interesting for early diagnosis but my study showed that this was not the case. But anyway to get cases for this with the EEGs Wendt helped me. There was of course negative result, but of course this is not important but anyway Wendt helped me, but the entire study was planned in the wrong way. They wanted me to contribute to the problem of mutation rates, and you know in Huntington's this is completely nonsense, there are almost no or very few new mutations and you could have known this before since there was the people of the Neel group. You remember?

PSH Yes.

FV Which already showed that there is nothing like this. This is not a very good trait for these things.

PSH Yes. Yes You mentioned when we were talking before, East Germany. It must have been really very difficult - well not just difficult for you but even more difficult for them. How did you manage to maintain links during those years.

FV This is a very good question, but I did have these links. Let me tell you another story. You know that we had in 1986 we had the International Congress in Berlin.

PSH Yes indeed, I was there.

FV You were there. You were there and we had a kind of so-called planning/permanent committee which planned for the congresses and gave the congress to us and the main reason for making the congress in Berlin was to give people from Eastern European countries the opportunity to come and to participate and this has actually worked. And now let me tell you how we could co-operate. We said for those people it is important that they don't have to pay the congress fee in valuta, what we call valuta, the German Mark. They should be able to pay it with their own money which had an internationally much lower standard, I don't have to tell you, and Irena Witkowski collected the congress fee always for the official exchange rate, not the black exchange rate, but the official exchange rate and she collected the congress fees and now they could pay in their own money and so on and now they could participate in the congress. And now comes the question. What to do with this money?. What to do with this money. And here another of my personal connections came in; it was, the Professor of Law, of legal medicine over there. What was his name? The name, at my age of course, I forgot some names a moment. Anyway a very influential professor of legal medicine in East Berlin, who was basically an Austrian by the way. But he had connection with the Eastern Germany Academy of Science and we arranged so this man, he arranged with this man, he arranged for us, that the congress abstracts, they are printed in East Berlin and for eastern money, not valuta, eastern money. And one day before the congress, some trucks came over, brought the, when you look at the abstract volumes, you see, you might even see the paper was not quite the paper which we normally have for things like this; but this was printed by eastern money and the man who organised it was a legal management specialist. He has organised it for us.

PSH So you were able to use the subscriptions from the east European people to pay for the programmes.

FV No for the abstracts.

PSH For the abstracts of course.

FV For the abstracts. And this is what we used and this worked. And now a problem came up and now they even got permission to go with their own money, to go West Berlin, by train and so on for the Congress. All these things we have arranged. And now a problem came up with the social programme and the social programme was a kind of sightseeing tour, and we first announced we wanted to allow sightseeing tours to west and east Berlin. The eastern authorities told us east Berlin doesn't exist. This is Berlin, capital of the GDR. You have to say sightseeing tour to the capital of the GDR. Then we told this to our western governmental partners and they said east Berlin is not the capital of the GDR. Its name doesn't exist on international control committees and so on. It doesn't exist. Now how to call it? And later on it worked, we said both parts agreed that the sightseeing tour goes to the west and the east of the city. When you hear today of course, it sounds absolutely ridiculous, but these are things that might normally create difficulties.

PSH I think we should finish soon Friedrich, but can I just ask you, going back to this difficult transition after the war.

FV Yes

PSH Did you feel, going into human genetics at an early stage, did you feel you were in some ways battling against accepted opinion and public opinion and that this was a difficult field to go into politically because of what had happened in the past?

FV Yes it was difficult. But on the other hand I always wanted to do something where there was not so much competition, you understand that. I wanted to be, let's say, I don't want to say I wanted to be alone but I wanted to have very few partners with whom I can communicate and so on, all this and I did have this feeling, yes.

PSH Friedrich thank you. I think we should finish there. I hope I haven't worn you out too much.

FV. No no no. It was very interesting to hear your questions also. Very, very, very good.

PSH. Thank you. I am going to turn this off now.

End of recording.