EMBL
ARCHIVE

Oral History: Heinrich Stuhrmann / 2017/11/16

DISCLAIMER

The information contained in this transcript is a textual representation of the recoded interview which took place on 2017/11/16 as part of the Oral Histories programme of the EMBL Archive. It is an unedited, verbatim transcript of this recorded interview. The transcript was prepared to the best of our abilities. Nevertheless, isolated errors cannot be completely excluded. In case of doubt, please always refer to the audio file.

The transcript may not be copied or disseminated to anyone unless permission is obtained from the EMBL.

Some information contained herein may be work product of the interviewee and/or private conversation among participants. The views expressed herein are solely those of the interviewee in his private capacity and do not necessary reflect the views of the EMBL.

EMBL reserves the right not to be responsible for the topicality, accuracy, completeness or quality of the information provided. Liability claims regarding damage caused by the use of any information provided, including any kind of information which is incomplete or incorrect, will therefore be rejected.

EMBL

File: 2017_11_16_HeinrichStuhrmann_transcript

Key

AFL: = Interviewer, Anne-Flore Laloë

HS: = Participant, Heinrich Stuhrmann

[??? at XX:XX] = inaudible word or section at this time

AFL: So, we're here today. It's the 16th of November 2017, and we're at EMBL Heidelberg, in Germany. This Interview is part of the Oral Histories program of the EMBL archive. My name is Anne-Flore Laloë, and I'm the Archivist at the European Molecular Biology Laboratory. Now, please would you introduce yourself?

HS: Well, my name is Heinrich Stuhrmann. I'm presently living in France, but I started my scientific career at the University of Mainz, where I studied Chemistry, finished in '64 my diploma, finished in '66, my thesis and in ’70 my *Habilitation*. Then, thereafter, I was invited to go to the Institut Laue-Langevin from '71 to '73, and in '76, quite to my surprise, I got a letter from Sir John Kendrew, who invited me ... or proposed to me to join the EMBL at a very special place. Not in Heidelberg, but at Hamburg, to use synchronisation, which at that time, just had real ... Let's say the potential of the suggestion had just been discovered by Ken Holmes and Rosenbaum and Witz published in *Nature*, and now Sir John was looking for a person who would be, let's say, the right one to start this work there, in particular, to do many things just from scratch, because nothing was there ... or almost nothing. Then, let's say ... How should it go, proceed now because –

AFL: So you've given us a nice background of your route to EMBL, so-

HS: Well perhaps ... I just told another story to the lady before, which was the first contact with Sir John, which started with a letter asking for the structure of the model of the Myoglobin molecule, which just had been discovered in '58, and where the ... Even the atomic coordinates were in order, roughly, in the beginning of the sixties and I was eager to help this big cause. Then, I could see whether the structural solutions, seen by small-angle scattering, would be the same as in the crystal. That was the subject of my thesis. Well I put this ... I wrote this letter to Sir John, but the answer which I receive, I was not sure whether he himself answered, or it was H. C. Watson. The answer was, 'Look. There are already two groups which are working on this subject, and for you, it would not be very useful to try it the third time because the other are probably far ahead and you would have no chance to come to some result in the time before them.' Therefore, he warned me to do this. Well, that was negative in one way, but on the other side, it was

extremely stimulating. Being young, and let's say, having chosen this subject by myself, to quite an extent because we were rather free in the choice of our subjects at The Institute of Physical Chemistry. That was a bit, let's say, the tradition in the institute that people who were coming there could have their choice. That is, having made this choice, I am responsible of this choice, that is I have to do something now with this situation. Fortunately, Sir John and his co-workers published the way how they found the model in a great detail. For instance, I could find all the electron density sections of the molecule ... Of the unit cell to be precise, and then of course, by taking photos, or glass slides from these cross-sections from the proceedings of the Royal Society Series A, you could then ... You end up with a stack of these slides, and then you could see, as these are transparent, the three dimensional structure. Well that was fine. It was clear to me that small-angle scattering from solutions are not a terribly powerful method, and perhaps, not very <5:00> to say ... Giving a precise answer whether there are changes in the structure if the crystal is going to be dissolved. And therefore I looked for a way how to get more information out of small-angle scattering experiments. The way how to do it after some, let's say, ways of thinking which were not very straightforward but at the end I came to the idea ... The thing which was left to do is to change the density of the solvent. Then of course, one has two extreme situations. If the contrast is very high, then the molecule in solution would appear as a shape. You would see the volume, and if the density is very low, then you would see the internal structure, which by chance would resemble more or less, to that what you see in the crystal, that is, then you would see the helices. And in fact, these profiles of small-angle scattering are quite different. At high contrast, we have something which is curvature, is monotonously decreasing with the angle, not very structured, but if you got to very low contrast, things are very different and the profile you get is exactly the same which Sir John showed in his paper when he averaged the crystal graphic data with respect that ... the rated average as a function of the angle, then the profile is exactly the same that you can see in solution, that is, you reproduce something which has been measured from this crystal in solution, which would mean the internal structure might be more or less the same which is, perhaps not too much a surprise. Later on, looking more ... Let's say reading a bit more of the literature, I found that this way of thinking and measuring has already been invented well before, by Perutz and Bragg in '52 when Perutz and Bragg used a [07:19] of different electron density just by changing the concentration of ammonium sulphate. Then, what they've found out is something like the orientation of ellipsoid structures in the unit cell, is they saw something like the shape, and they saw the arrangement of the haemoglobins in the unit cell. Low solution, typical also for small angle scattering. When you do that kind of contrast variation, the results are more or less the same; comparable, but of course crystallographers took a further step, they went to much smaller labels, finer labels, heavy atoms, and these of course led to a much higher solution, which you could observe at large angles, that is, high momentum transfer, very high order reflections from the crystal, and then of course, they phase problem could be attacked to higher solutions start to be solved. This, of course, is not the same for small-angle scattering. There you stay at the level of Bragg and Perutz, that is, you have a low resolution structure, but if you look at it more closely, the low contrast profiles are giving something of the internal structure, which at that time, was new. Well, this is also what led me to the ILL, and there of course, this new technique works perfectly fine. It was used by many people to look at viruses, at chromatin, at

ribosomes, all these that support in nucleic complexes of protein, in lipid complexes, lipoproteins are very conveniently studied by small-angle scattering to get a low resolution picture of the architecture of the large structure. It was then ... say it was maybe '74 or '75 at the ILL, then there was a meeting where I saw Sir John for the first time, and at that time he had made some progress in the determination of the shape of the large sub-unit of E.coli ribosomes, which we had something simple ... paper ball, which was rather deformed to get this ... That what he obtained from calculations into something which we could see. And I remember also that Sir John looked rather carefully at this <laughs> ... ugly piece of paper, or let's say <10:00> ball of paper, but he didn't say anything. He was extremely silent during the whole meeting, he didn't say a word, but he of course, must have listened very carefully and was probably rather well informed about me, and it was in '76, then, in May or June or something like that, when I received a letter from him telling me that position in Hamburg would be open. Well ...

AFL: So, because for the first couple of years, then, the person who was leading the Hamburg project was ... I mean, where you the first head of un ... Of there?

HS: No, there was Barrington Leigh before, but he was not terribly enthusiast of this position because he notes that, 'One cannot do really science because one has to do some much other work,' and for his career, this would not be very good for him.

AFL: OK. Right, so hence, the opening a couple years later.

HS: Yes, therefore, the position was open again and I got this letter. Then, I went to Hamburg. Ken Holmes introduced me to the place, and also to the city and so on. We went to *Blankenese* and so on, just like what the first idea about what Hamburg is and how nice the city is, particularly the suburbs. Well, with Ken Holmes, let's say, one had a close contact with Ken Holmes, at least for the beginning, and the thing, or let's say the critical thing was that I was not very well known to the people who were already there, like Gerd Rosenbaum, Harmsen and so on and Robrahn, and they did not really trust me. <laughs>

HS: Oh no. <Laughs>

AFL: You don't need to eliminate it, this is just the truth and nothing else, and it was not quite clear how we could cooperate because my background was quite differently from that of Gerd Rosenbaum. Gerd Rosenbaum was a very good physicist, where he had done his diploma work at that time, at 41 with Professor Kunz and he was really expert in the synchrotron issue, whereas I myself, knew about nothing about that, and the principle, we could have, let's say, worked in a more fruitful way, but it did not really work like that. Well, the situation then was, for me, the problem, there were two bunkers, one at the synchrotron, where there was already an instrument installed of this camera for small-angle diffraction from muscle fibres. This was heavily used by Gerd Rosenbaum and in an indirect way by Ken Holmes, and it was actually the only thing which was there, but it was also, let's say, a most convenient thing to do, because synchrotron then, it was just ideal for that kind of

investigation, but you couldn't do anything else in this hall except this type of experiment, that is, because if someone accesses the instrument, then of course, the beam is shut and there's no way that another instrument might work in parallel because there was not protection or separation from this instrument from the rest. That was one beam hall, then, we had another one at the storage ring DORIS, which was completely empty, there was nothing, but the radiation emitted by DORIS was not very attractive for people who did muscle diffraction, for two reasons. First, the wavelength, let's say, the energy was lower and therefore, the intensity emitted in the x-ray range not terribly high, and then of course, the focus of the electron beam was larger than that of the synchrotron, then of course, he had let's say, a poorer optic sensitivity due to ... did not have finer solution, which you could obtain at the synchrotron. That is, the geometry was a bit worse, but the future that was quite clear was on the side of DORIS, because it was promised that at the end, DORIS would run at very high current. People even ... some people told ... I note the letter from Gerd Rosenbaum, who wrote, 'While there will be a current of six amps stored there, electron beam stored in the storage ring' which of course, never has been reached, it was just, let's say, a dream, which at no ring has ever been reached, but you see these dreams or <15:00> these rumour which went around was extremely stimulating, but ... OK, the things developed much more slowly. We concentrate, of course, on DORIS, because the hall was empty and the we tried to find ... Well, first I engaged an additional scientist who had my confidence. That was Michel Koch, a Belgian Physicist … a Chemist, I would say more a chemist than a physicist, and who helped me a lot at the beginning, because he was practically the only part with whom I could speak. <Laughs> [???] The others where, let's say, not that accessible and had different ways of thinking. Well, we had to cooperate in one way, that was quite clear and if we want then to organise teams in the hall, then of course we had to do muscle diffraction as well, but perhaps that was the main aim from my side, we should do additional things, we should also have a good small-angle camera, we should have also SAXS. We should, of course, also have protein crystallography, which at that time we didn't have at DORIS. And then of course to do this, we had to separate all these instruments that each person or each group could access his instrument separately, that is, that we shouldn't shut off the beam for the whole hall, because one group needed to have access to the instrument. We had just big construction of concrete, which were both good … to separate instrument and also you could on the higher level, put another instrument, because they were solid enough to carry it, and thus we had not only on the ground floor, protein crystallography, SAXS, muscle diffraction, but on the upper floor, anomalous dispersion of small angle scattering on a very elongated double monochromator system, which at that time, never has someone else had been envisaged, but which at the end was not that easy to handle, but it worked at the end. This was my responsibility as it just was a bit, let's say, my hobby, anomalous small-angle scattering, which ... Well, I had discussed this point with Sir John before, and he agreed on this and therefore he gave me green light. This just tells you again, Sir John was not only crystallographer, he also thought of other possibilities, he had a much wider horizons and he said, 'Why should you do protein crystallography? So many people are doing this. It's boring. Do something else.' Yes. <laughs> ... But at the end, there was no way to prevent protein crystallographers to, let's say, to becoming major player even in the use of synchrotron. Well, that was how things started. It was not easy in the beginning because we need, of course, for this

mechanical workshop. The mechanical workshop, we had only two persons that was Viktor Renkwitz and Wolfgang Behrens. Victor was good mechanic ... *mechanicien*. Yes, and-

HS: Mechanics? Yeah.

AFL: Mechanics and the other was more that he was also expert in preparing tools, which more than just doing mechanics. This was an extremely competent person that was just starting within a very interesting thing about this new instrument on the upper level, and how to prepare the house for the monobit and things like that, but unfortunately, this very competent person died in a car accident a few months later. That, of course was really a very hard blow to the outstation because the workshop was the backbone for our constructions and we had only one person left that of ... first of all people were very sad about this event, that of course, the moral in team *le plus bas possible*. Then ... Well, we've got an additional person they recruited, but again, he's had to learn things again and.... We lost about a year or so in this way, while ... thus the growth was rather slow and moreover, the beam was not always there. We were only parasitic users all the time, and it was only, I think, in '79 when we had ready some instruments, <20:00> for working, when at noon, the Director of DESY, Professor Schopper came to me and he was looking around the lab and saw, well, people had worked, their notebooks were open, things like this, quite clear that it's an active room, and he told me, 'Well, you have the chance to use, now, the DORIS storage ring, for your own. This is the main user. You will get the beam you like for a limited period,' because DESY was fortunate enough to get PIA, the positron injector, running in a very short time, that is, this injector was then running the positrons into DESY'S storage ring and then, of course, ejected into PETRA. Before that, DORIS was used as intermediate storage ring, just to fatten the positron bunches and then to eject them into PETRA. That was no longer needed, and then of course, we had this ring, luckily, for our own, that was in summer, where people take holidays of course. That is, we then had to alarm all our potential clients, to tell them, ' We have now beam, and fantastic beam. Please come here immediately.' And in fact, we got people and many people did profit from its muscle diffraction [???] [21:45] and also [???] Manuel Cruzan did experiments with his ... I ... sorry.

AFL: The name escapes you.

HS: Yeah, I will get there <laughs>.

AFL: That's fine.

HS: I will tell you later. And that is, we could then now use, let's say, a fantastic beam. The only group which has some problems was [22:12 IA] because it happened that at that side, the beam touched a steel plate, which was not noticed immediately by the operators, and then of course, the beam on that side was not so good, but for the others, it worked nicely, and it was also for me, the first time that I could really do anomalous small-angle scattering at the k-edge of iodine at 1.74 Ångström in a

way which clearly showed the way how small-angle scattering varied as a function of the wavelength near the edge. That was the first good experiment of anomalous dispersion, and similar successes were obtained by the other groups.

AFL: So, how did you actually find delivering both the services and carrying on your own research?

HS: Well ... we had to, of course, to share the possibilities, particularly from the workshop and also from our engineer, Hans Ludwig who was then a bit the person who was supervising a bit the mechanics, and then of course, everything had to go more or less in parallel, and I think this worked, and I must say that what I needed to to help, at the end, I got. That always takes time, of course, this is a normal thing. Everyone who has been working in practice is used to this situation. Well, if you have synchrotron radiation, which is very intense radiation, then of course, you need to detect it. That is, we have to consider, and that was from the beginning a main problem, the problem to have efficient detectors, which count quickly, because muscle diffraction is an experiment which requires thymol solution. The muscle is shocked bioelectrically, then it will react, and that is something which goes on in milliseconds, and exact milliseconds at that time were not ... could not be reached, but seconds yes, and then of course, we have fast electronics. At that time, that was difficult to have. Today, it's no problem, but at that time it was really problem, and also to get the data still saved from the detector to computer, we had Peter Clout who did a lot of it to help us, and then also we had André Gabriel, whom you might have seen, already interviewed as well, <laughs> because you –

AFL: But he was based in Grenoble. <25:00>

HS: He was based in Grenoble but at that time I was also at Grenoble and therefore I met him. I met even before I came to EMBL, and I got the detectors from him even before I'd get there because we used his detectors at the University of Mainz. Even a two dimensional [???], his first one based on carbon fitted glass fibres, extremely fragile, but it works on small one, and which at that time was very impressive. It was the first one which we had in Europe which was working, and then with this might have also been the same reason why I have been selected to go to Hamburg, because detecting radiation is an important part of the experiments, and as I had some experience in this field, and had such good links to persons who know about this, and who happened then to be at EMBL, at the beginning it was not the case because then he was at the ISN, at the *Institut du Sciences Nucléaires de Grenoble*. It's there where I met him together with Yves DuPont, and then of course, it was only later when André Gabriel joined the EMBL, and then after that I did the same thing. Well, his help was absolutely crucial to do good small-angle scattering, particularly the two dimensional character, and it had reasonable high solution, and we also had then support from the electronics group to get the data stored correctly and also have it read out such a way that we can analyse them, that all was done by the EMBL staff. Many things just never did exist elsewhere and we just had to get it together in a way that it works. Well, I got the contract for three years.

AFL: Three years?

HS: For three years in the first step and in '79, the EMBL decided to extend my contract for another two years. Then, of course, they had to ask the University of Mainz whether they would agree. They said 'Yes, but that is the last time. You will not get another two years, because after these two years, you must decide whether you will stay at the EMBL, or whether you join the university.'

AFL: So, you were on secondment from the University?

HS: Yes, that was clear and ... already in '79. Then in '71, already in '70, Sir John only had then asked his advisors and committees, 'What should we do?' with me, and it was decided that I better leave the EMBL in '71, which –

AFL: In '81!

HS: In '81, yes, sorry. That was a mistake from me. And I think, looking at this as a whole, it was a good decision from the scientific point of view. For the family it was not so good because we well have liked to stay at Hamburg ... Actually, we did stay, but then of course, I had to go to Mainz and to give the lecture there at JGU, going from Hamburg to Mainz, back and forth, and that of course is a bit, it's a kind of stress. Well, that is the story of EMBL. Now, you may have other questions? Just –

AFL: Yeah, just one question, 'cause you eluded to the relationship with site in Grenoble through someone like André Gabriel, but how close were the contacts between the Hamburg outstation and Grenoble, and also between Hamburg and Heidelberg? Did you -

HS: Ah. Well, the relationships between Hamburg and Grenoble were nearly non-existent, <laughs> because I knew Andrew Miller perfectly well as a good friend of mine, <laughs> and we also wrote a paper together and so on, but let's say, [???], it was not really collaboration in this sense ... Andrew Miller had, let's say, quite different problems from the … which were quite different from mine, and then of course the whole thing was relocated with the CEA and access was difficult to go out and it just, you know, the fence and all this story, you know that's just another thing, and therefore ... And then, of course, there was a lot of activity in the field of neutron small-angle scattering contrast variation from other groups. Therefore, he then snipped or was more or less sticking to his collagen which he had liked very much, <30:00> but there's not so much to do about this with neutrons. And I think he was much more happier when he went to ESRF later because that was of course where he was really like a fish who was finding water again! <Laughs> But therefore the relations were friendly, but let's say, scientifically speaking not very intense. Heidelberg of course, I went to Heidelberg regular for the meetings, particularly those where decisions were taken or where a report from my side was necessary, then of course I went to Heidelberg. Therefore these contacts were there. More on the administrative level. Not so much on the scientific level,

because when it was thought that for instance Gerd Rosenbaum, who stayed in Heidelberg might have closer contacts with Hamburg and thus cooperate from his side … Heidelberg together with us in Hamburg, which to some extent he did, but at the end it was not that fruitful because technical developments went in a different way.

AFL: Sorry, what was the name of the person?

HS: Gerd Rosenbaum.

AFL: Oh of course …

HS: He's well known in EMBL.

AFL: Of course.

HS: Well that is the relation to Heidelberg. The other thing was, I was told, 'There's a lot of money, you only need to use it. Act now and use it!' But to use it, you need to have the persons who can use it. If you have a very weak workshop and a very weak engineering office, then you can't use the money! <Laughs> The infrastructure was not there needed to make full use of what EMBL Heidelberg offered to me. That was a pity and I was told this several times. But we did what we could do, we did our best, but it was probably we might have been quicker if we would have had a more efficient group. But you cannot have everything for the beginning. It was at least let's say not so bad, and at the end it did work. At the end, we ended up, and there was a list with about 17 persons who were at EMBL and we had very few in the beginning.

AFL: And how about the relationship with the campus and DESY in particular?

HS: We had very good contacts to the operators of the storage ring, which was crucial because you had to know in advance what is going on, and also if something did not work correctly and if there was no beam, we were always informed in great detail about the technical reasons for this failure, which is very good to know. DESY was extremely cooperative. They did everything what was supposed to be on their side, but one has to nevertheless see that they had their own programme. This is high-energy physics and of course what the so-called 'Soergel-Doktrin' said, 'Adapt to what we offer.' If you can't adapt then you are lost.

AFL: The 'Soergel'-Doktrin?

HS: Professor Soergel was later, after Schopper he was the director of –

AFL: Adapt to what we can give you?

HS: Yes. I think he came from Heidelberg and he moved to Hamburg and he was succeeding to Herwig Schopper. But what was good about the storage ring was the intensity, that was fine. What was not good was the beam position which was moving all the time. And if you want to do let's say anomalous dispersion working quite near to the edge of an element, then you need to be very precise with your wavelengths, otherwise things change dramatically and that of course with the beam position not correctly working, means a different geometry, means a different path through the crystals, means a different wavelength. And that, of course makes the work difficult except for the time when we had main user time. That was fine. They could do <35:00> things, especially for me, do things which I never could do before nor after. <Laughs>

DESY was, I must say we had good experience with DESY. DESY did everything for us. There is no reason to complain about DESY. It was, the cooperation was perfect. And I must say also, Sir John had played a very important role because he had established the good links and therefore people trusted each other and that was, for us, very good way to work.

I must tell you, the other story will follow thereafter about DESY, let's stay at the moment and we will surge on. We had Sir John of course from time to time at Hamburg, and then he was staying at night at the Carl Jacobs Hotel, it was very nice, old … hotel, very Hamburg style hotel, yes, and we had him at some evenings as a guest in our house and we enjoyed this very much and that was a very pleasant time to have him with us. And Sir John did everything to make our life and our stay at EMBL as comfortable as possible. Clearly the case. He was, I would say, almost kind of a father to me and of course he had to be a bit critical about what I did and that was normal, but he helped me quite a lot and of course it was also the sense of the outstation at the end. As a person I liked Sir John, but the older I am, the more I see my own life, I admire him even more! <Laughs>

AFL: So what was the path of your life after EMBL? So you alluded to, you were in Mainz for a bit?

HS: Well after my stay at EMBL I went back, I gave my lectures at Mainz, regularly each week, went by train back and forth, stayed there for two days and spent one or two nights in Mainz, and then later on I used to start one day, I went very early in the morning to Mainz by train and came back late in the night, after having done first the lecturing and second also the supervising my students who were still there, and then of course in parallel I was trying to find national funding for things which I would like to do. I still had my ideas. Of course enormous scattering, in Brisbane, was working and I was aiming to have an own beamline at the just founded HASYLAB. And fortunately I was lucky in the way that I could get the beamline A1. Very good, that was. We started exactly as I would like to have it, and it was operational over a very broad wavelength from 1 Å to 7 Å! That is you could then go through all the whole periodic system down to elements as light as silicon. You could work at the silicon k-edge. Which we hardly did. We worked much more for the phosphorous k-edge and the sulphur k-edges, there are several which are close to each other depending on the chemical state of sulphur. There is a

remarkable shift of the edge if you go from the balance state plus six sulphates to minus two sulphides. <Laughs> And that we used and that is on the poster!

AFL: Oh great.

HS: But anyway, logic in my way of doing research and when I'm planning something that goes over long periods, contrast is gold, having contrast variation with wavelength variation is even better. And that way of course, the work of Wayne Hendrickson was quite important because that was of course far away from the k-edge but he saw the anomalous contribution, weak as it was, carbon <40:00> was just a good candidate to use this addition to get the structure. And we then, as a second step, a further step, we went that close to the k-edge of sulphur, 5 Å, penetration depth of this radiation, 30 microns. Thinner than a hair. And for phosphorous k-edge only 20 microns. Well, then this instrument of course had to have let's say an almost completely evacuated beam path, that is everything what is in the beam is absorbing – therefore best no material, no windows and so on, but of course that wasn't quite possible as there was a very thin window between the storage ring and our instrument. Then you also knew, having several monochromaters, like a double monochromater, that is too much. One monochromater only, and of course you have to have the instrument swinging around this monochromater if you want to change the wavelength. And then of course we had to pre-monochroatise the radiation with mirrors. That is you have to cut off the higher harmonics, that you can really get cleanly the low energies and not have it contaminated with higher orders which of course would be disturbing! <Laughs>

AFL: So this is the work you did on the beamline –

HS: That is the work which we then did. Then of course I had to work together with Mogens Lehmann from the ILL, who was the first to try the sulphur k-edge at the beamline, which was very difficult because at that time everything what we really needed was not yet there, because you have to have a crystal in an evacuated environment – that is then you have to have thin windows which are solid enough not to burst and then the crystal should be cold as well, and then you have to see this crystal in the beam, that you have some microscope inside the instrument which at that time you didn't have. Mostly we then work almost blindly but you got some reflections after days, one or two reflections, and then got a dozen the day thereafter. That was the beginning of this kind of working. It's far away from the thousands of reflections which you get easily at 1.5 Å in radiation if you take this long wave and the diffraction pattern blows up to large angles. You see only very few reflections and forward directions with an ordinary plate or detector. That, that is working with this radiation, is what the French crystallographer told me, 'ce sort des … maudits. Il ne faut pas faire ça!'[43:09 IA]'.

That's the advice!

AFL: <Laughs> Right.

HS: ‘cause many crystallographers will tell you the same thing. You never should try this because everything what can go wrong will go wrong there! But the good thing is the fact that you can distinguish from one of and the same crystal at one kind, with one element, between sulphates and sulphides, just the same diffraction line, you just go through the very big thing and you see at different wavelengths, cysteines and methionines and then you go a bit further to higher energies, about 20 eV further, and you just see the sulphates. And that is fantastic. You could even get more because you can even distinguish between methionine and cysteine because these edges are also a bit separated and look different in shape, but there are other clues. It’s only 2 eV difference. If you would have a very precise wavelength which would be very, very fine you could do it. These are things which we have done and which since then have never been repeated. That is, we are still the best and I should say the person who is the best was my daughter. She did it. And she has really the hands and particularly she is also a very good engineer, if it comes to using this machine and getting the separate environment quite the <50:00> things, she was … she did this very well. I’m not the supervisor of my daughter, the supervisor of my daughter was Ada Yonath. I think you must know her. And she was guiding her, and what my daughter Sigrid then did for particularly was to look at the phosphorous of the small subunit of ribosomes and to see whether you can get the anomalous diffraction out using k-edge of phosphorous to see whether you can get some additional such information why this is … these experiments are extremely difficult because you need a crystal of Yonath size <laughs>, say 100 microns long and 20 microns thick and so on, extremely fragile, and then this you have to use to environment which must of course be cold, and evacuate it further away, and then working then with these x-rays which really don’t go very far but with these small crystals they didn’t even … then of course it did work, but because they’re very thin and very small, then of course the signal is not very brilliant, but it diffracted, and Sigrid did manage it but it is so far a unique experiment that will be very hard to repeat because it requires a person with a very special skill! <Laughs>

AFL: From listening to you, it just sounds as though your science and your career just evolved and kept on changing and changing, reinventing itself almost.

HS: It kept on changing. Let’s say in some ways it went on in a logic way, and then of course let’s say it was now in the … Sigrid then had done these fantastic experiments with ribosomes, and I was also with trypsin, that was the other thing which of course was easier to study because the k-edge of sulphur which is much easier than the k-edge of phosphorous. Then of course HASYLAB had to see whether my work at this beamline was really good and really had a future, and there was a very let’s say … contradictory discussion about this project, about the results. For crystallographers used to have let’s say a very nice collection of many, many reflections, this result was absolutely disappointing, but one must say it was foreseeable because if you go to long wavelengths then you never have many reflections. It’s quite normal. Then of course it was far too difficult to do because who else would do this experiment? And then of course how big, how many people really want to have it? Some people wanted to have it, and they defended me, but other people said no, we don’t need it – why should we continue with this? And then of course I was told, ‘Well,’ in ’97, ‘you should shut down your beam and

someone else will take the beamline.' While this has been decided, then I was thinking what to do and I contacted my friend at ESRF and asked him, 'Well, this thing did not turn out to be … it's come to an end, and perhaps you could do something better at ESRF.' One thing was definite advantage, the beam there is stable, absolutely, and intense. The conditions to work would be much better than at DORIS. Well I knew some people, Christian Riekel, Peter Bösecke, and had a discussion with them and then there was at that time Carl Brändén still director and then it was decided well, if you find a position, let's say a place where you can stay at Grenoble … then I went to the IBS, *Institut de Biologie Structurale*, there was Joe Zaccai … 'No I would not like to take this, that's a bit too difficult, too hot, I don't want to touch this, but I can recommend you someone else who is really engaged scientist and that is Richard Kahn. He will help you.' And this is what he did. Richard Kahn is a person with firm opinions which are extremely left-wing, but he has ideas and he is working as a serious scientist. And he is not afraid of what other people say. A remarkable person. And fortunately, thanks to him, I could get an office at IBS. I got a salary from GKSS, *Geesthacht*, Germany, I could stay there, but for a limited time. And well, I made a first trial, developed a project for the construction of the environment of this sample for x-ray diffraction, but this came to a committee at ESRF but there it was turned down. That was very bad. And then I was told, 'Well,' from GKSS, 'If you are not successful then you will have to return to *Geesthacht* and come back.' <Sighs> That was a very difficult period, that time, but there was something else that happened. There was a seminar on radical proteins in the *Château de Sassenage*, not far from Grenoble, and I went to this seminar and then the last one, just before noon or before lunch, there was a lady who showed the EPR signal, which she had resolved from a radical protein which looked very similar to that what I knew from another radical which was routinely used for dynamic polarisation of proton spins. This by the way was something which was running in parallel to the work after '81, parallel to what I did in HASYLAB. I also had a beam line at the reactor at GKSS for polarised neutron scattering from dynamic polarised protons in solids, in this case from ribosomes, which were largely deuterated except for one or two proteins, and then by varying the polarisation of the protons of the proteins, you can then get the precise information where these proteins inside the ribosomes are. That is another way of contrast variation, which then just uses the fact that the scattering length of the protons changes enormously with the mutual orientation of the spins. If neutron spin and proton spin are parallel then you have a scatter length of about 10^{-12} cm. If it is anti-parallel then it is -2 10^{-12} cm. This difference is much bigger than that which you would have if you exchange normal hydrogen by heavy hydrogen. That is a technique which is often used at the HD exchange. But this technique is about three times more efficient in the contrast and it works. Just, you have only to turn on the switch and you have the other contrast. You have only to change either the wavelength or the way how the flipper works.

That was already going on in parallel. I did not tell you about this because it had no relation to Sir John. Now we come to a point where EMBL, let's say my stay at EMBL has been very important. If I had not been at EMBL it would not have been possible.

Well, this lady at the *Château de Sassenage* showed an EPR spectrum which looked very familiar to that what I knew from a radical which was routinely used for the dynamic polarisation of protons. Seeing this, it came to my mind, well, if it is the same then this radical, which was a tyrosine radical, a tyrosyl radical to be precise, should be as efficient for DNP as this chromium(V) complex which I was using before. After the lecture I went to this lady and asked her, I told her about my impression and told her that one could do something very interesting with this protein, that is one could use this tyrosine radical as a source for proton polarisation, <55:00> using this method of DNP and then it would be possible to have a local polarisation, at least for a short time, of the protons, which then would enormously enhance the amplitude close to the radical side and give a much clearer picture of where this radical inside the protein is located. Fortunately, Hélène Jouve, that's her name, understood me very quickly. <Laughs> I must say, though she's a biochemist, she had a very good physics background. And that was of course an important point now. I have found something which is new and which of course could keep my stay at Grenoble! <Laughs> That was first for me but the scientific one was certainly more important because this new preparation I had to of course do, first get these samples prepared inside this CEA Grenoble, because there we have Jacques Gaillard who's responsible for the EPR, who did a lot of things for me. Then we had to take the samples to a place where we can dynamically polarise. The closest is the PSI at Switzerland. We went there. Tyrosine in fact *does* support DNP, very important, it works. Then some months later we tried the same thing with catalase, doped with tyrosine radicals. This also works but much more slowly, but for the physicists who were [???] they were really disappointed! Very weak, weak increase of polarisation, but later … yes, but it was enough to start a collaboration. And then of course also the head of this group told me, 'I'm going to stay here for another five years, then I'm going to be retired, but let's do something reasonable for the last five years and let's do something which is interesting for the group.' You must imagine, I came there without any financial support, nothing, and only the idea! <Laughs> And based on this idea, I got started collaboration, that is, I must say, fantastic. It's almost a miracle how it worked!

AFL: Yeah, just it all fell together.

HS: Yes. Now of course we have … OK, I have dropped one thing which was important before, because I told you that we were doing polarised neutron scattering from dynamic polarised protons in ribosomes, at GKSS. Before we could do that, of course I had to get this technique from somewhere, because I'm not, let's say, an expert in low temperature physics. I'm a physical chemist, but far from expert there. I had the idea, of course I knew, as someone who has worked at the ILL before, that this effect of the strong spin dependence of the scattered x exists, this I knew. And therefore I would have liked to use this technique but the problem was of course … it happened that the KFA Jülich liked to have someone who would find money for a new small-angle scattering experiment at Jülich to build it. They should write a demand for money from the BMFT in Bonn, and then invest this money in Jülich, and then of course I could use this instrument, because at least I could then organise the research there. Then I was at Mainz, or at least I was at Hamburg but … administratively linked to Mainz. Yes. And the thing of course was to do … I got also the money for this but for in a way which was really surprising

because they day before this meeting, they had proposed this instrument and was agreed to this, but also said if I want to get this instrument they also would like to have the polarised target station, to have the protons polarised and the polarised neutrons as well. <60:00>This I've written there and this I submitted, this was a condition – if they get both then I agree. And then I remember Professor Springer who rang me up and told me, 'Well you will never get through this project. You will not get anything there!' And 24 hours later I got everything! The thing was then I had the money, but now how to get the polarised target? I have to find someone who does it for me because I'm not an expert in this field. I had to collaborate in a way, but with whom? Well then of course looking around a bit, Bonn, at Braunschweig, but then I got the contact with a person at CERN, a certain Tapio Niinikoski, who was then the head of this polarised target group at CERN. And I wrote to him, I got the invitation to see him. It was some Christmas evening and you [his wife, J.G.] were with me and we were discussing for two hours at CERN the project. And fortunately he understood me, which is also a crucial point, and they said, well that interests me, that is even more crucial.' And then he said, 'Well, you can do these experiments in March, when I have some time for you. Stay there for a week,' and so on. And then I went there, by the way together with Professor Otto Schärpf, a Jesuit who worked at the ILL, who was expert at neutron polarisers, which I needed of course, thereafter. Well, we did the experiments then at CERN, I went there, and of course too, we went to the canteen and whom did I meet in the canteen? The Professor Schopper, the person whom I knew very well from the time up at EMBL! He came to me, this was only due to the fact that I had been at EMBL, otherwise he would never have seen me, and told me, quite friendly, 'Well, I know you, you're a chemist. What are you looking here for, among high energy physicists?' <Laughs> I explained this in brief and asked him, 'Well, could I have some minutes at the end of our test series?' He said yes, he will give for me. And we did the experiments, it worked perfectly well because the problem was, do the radicals in solutions, it was a chromium(V) complex in a deuterated solvent, transfer their polarisation to the protons of the protein in solution. That is, would there be contact good enough to have a uniform protein polarisation everywhere? I got different kinds of information from some people, very pessimistic, but others who were experts, said, 'Well it should work.' And we went there at CERN and it did work and then at the end of this successful series I got the ten minutes to see professor Schopper, and I explained this to him but I had the impression he was already well-informed before. He understood me very quickly and I was quite surprised, I never had <laughs>, rarely seen something like that! And then he told me, 'Well, we will help you within the [???] of our budget.' 'cause the budget is quite high at CERN, the [???] is extremely important! And in fact I got the whole equipment, magnets and the NMR-system and the cooling system and the microwave system all together was put on a lorry and transported to the neutron Source at GKSS where we started this experiment. This would not have been possible if I had not been at EMBL!

<Laughter>

I will say this quite clearly. And if we had not had this opportunity, all these things which happened after where I stopped five minutes ago would not have been possible either, because only with this experience which we have then gained at

GKSS and also with the reputation which we had gotten in this field, this Swiss physicist we had then convinced, well, with you we can do it because you are an expert, OK, we do it together. And all this is linked. It's like if you would take away one of the building <65:00> blocks, below all would collapse!

<Laughter>

AFL: You know, I think this is an excellent place to stop! We've come full circle with a great EMBL story, and so I'd really like to thank you for your time today. Thank you very much.

HS: Thank you as well.

<End of recording>