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Oral History: Daniel Louvard / 2017/10/19

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Key

AFL: = Interviewer, Anne-Flore Laloë

DL: = Participant, Daniel Louvard

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

AFL: So we're here today, it's the 19th of October 2017, we're at the Institut Curie in Paris and this interview is part of the oral histories programme of the EMBL Archive. My name is Anne-Flore Laloë and I'm the Archivist at the European Molecular Biology Laboratory. Please would you introduce yourself?

DL: I am Daniel Louvard, former Director of the Research Centre of the Curie Institute, retired since 2013, September 2013, and currently Honorary Director of the Curie Institute, Emeritus Research Director of the CNRS and Honorary Director of the … no sorry, Professor, of the Pasteur Institute. And now we are currently in my office which has been the office for 25 years now, the office of the Head of the Research Group of Cell Communication and Morphogenesis in the Department of Cell Biology of the Curie Institute.

AFL: Fantastic. So we're here today to talk about your relationship with EMBL through the years and so the first thing that I'd like to talk about is what's the path that led you to EMBL?

DL: Well, I think I should say that I was lucky to see the conception in a way and the early days of EMBL. At the end of my PhD with a degree of PhD in Physical Science from Marseilles University, where I studied the structure and the organisation of biological membranes and the structure of membrane proteins, I moved to California, to the UCSD, University of California San Diego, in the group of John Singer, who, in 1976, was one of the leaders of the research on biological membrane and membrane organisation and function. And I came as a post-doc with an EMBO fellowship, and at that time, and I believe still today, EMBO applicant to EMBO fellowship, long-term fellowship, are interviewed by referees who write a report for the jury of the committee of the EMBO fellowships after they meet the applicant, and to go as a PhD, as a post-doc in John Singer's laboratory, the referee for my application was Kai Simons. This happened in 1976, early 1976 in fact, and I met Kai Simons at Heidelberg in the temporary space of EMBL, which in those days was located in the DKFZ at Heidelberg. Kai was one of the first people who joined EMBL, recruited by John Kendrew, the first Director General of EMBL, and given his interest in the subject I was working on during my PhD and

the topic I wanted to develop as a post-doc in John Singer's lab, he was definitely one of the best referees for my application. So I went from Marseilles to Heidelberg. It was a long night train journey. And met early in the morning, Kai Simons in a small *Weinstube* or some sort of place where we could have breakfast and interview me at that place. <5:00>

He mentioned during the discussion or during my interview I should say, that he had moved from Helsinki to Heidelberg as one of the early recruited teams or group leaders of what will be the EMBL in its premises in Heidelberg when it will happen a couple of years later.

And I was lucky to be funded, granted this fellowship, I was able to go to California, and it is … two years later roughly, when I was still considering for my future the possibility to return to Europe, and my priority I would say, what I was thinking in those days is to return to a research laboratory in France, which was not unusual but very common in those days. And I received, I forgot when, probably by the end of … in the autumn 1977, a phone call, a long-distance call these days, from Kai Simons himself, who was asking me how was going on my research, my post-doctoral experience, and he mentioned to me that EMBL was recruiting a number of scientists, or cell biologists, and wondered if I would be interested to apply as a new group leader there.

Of course I was very puzzled initially. This was something I never thought about it. Not to return to France. But then I consulted a few friends in France, and they all say to me that I was crazy! Because those days, I should have said that it was not uncommon in France … I had already a position at the CNRS as research associate, *attaché de recherche*, I was on a leaving absence, indeed, but essentially, I was warranted to be able to restart a project in a research laboratory in France. I never considered not returning immediately to my home country. And that's why a number of my friends and colleagues in France were saying that I was crazy to go to a place that did not exist, except on paper, and something which was totally unknown, in the forest of EMBL, [??? at 08:56] and of course this was a difficult decision. On top of it, I should say I was married, am still married <laughs> with a lady who came from South of France, from Provence, and when we moved from Marseilles to San Diego that was fine for her, the sun was still there every day like almost every day in Provence … and add to that that we had a new child, born in United States, so the family was …beside us was our daughter, born in Marseilles and a new boy, born in San Diego. So the idea to go to a place like Germany, to Heidelberg, where it's cold, raining, <10:00> for my spouse was not very welcome, I would say!

Nonetheless, we end up there, and why did we end up there? I had the dream and the ambition those days, despite the fact that I was just about 30 years old, to be responsible myself for a project, an independent project that I would run myself. And the project I was myself very keen about which was essentially doing cell biology and understand the molecular basis of epithelial cell polarity. So this was an unusual … not an unusual dream, I'm sure other colleagues of my time had such a dream … but it was unusual for a young scientist, French scientist like me,

to pretend to be independent and to run, at early stage of the career, a project. On top of it, a high risk project, original, innovative, and when I was considering other possibilities, to return to France, several institutes in Paris, in Marseilles, in Nice, in Bordeaux, research institutes, had offered me the possibility to come back and work there, but of course it was by no means … I could talk about being independent and running the project I want to run, because I pretended that I had idea about new things for which I don't need to be supported, except financially, space available to develop my project, but the concept those days in France, it remained like that for many years, that we should work in a group, and we should work on a project that was decided by the big boss or by at least the head of a given laboratory. This was a totally different story at the beginning of EMBL and I believe it's still the case. And this, of course, was very appealing, appealing for me. I had a chance that I could do something new, on my own, and I was ready to take the risk. And that's despite the counter-argument for not going to a rising, new laboratory that still had to be built, physically, even if the concept of the EMBL was already approved by the different states, and working in an international environment, which was actually also attractive to me, and so I chose that and jumped into the water!

So the person who of course who would talk about me to John Kendrew was Kai Simons, and in the winter 1978, I actually travelled from San Diego to Heidelberg, gave an interview in front of Kendrew and the staff of EMBL and advisor of John Kendrew, presented my research, my going-on, and also presented formally, or not necessarily formally but during discussion, what I planned to do if I was hired as a new group leader at EMBL.

I know that I didn't give a great <15:00> talk. <Chuckles> I was not much experienced of this kind of interview, but … nonetheless, I was chosen by the committee and by those who were making the decision those days at EMBL, to be appointed as a group leader. And I'm saying that I was appointed, something that I find out a few weeks later, when I was back to California, and back to California after my trip to Europe, because after Heidelberg, where I had met not only Kai Simons, Kendrew, Graham Warren, Brigitte Jockusch, Bernhard Dobberstein, the people who were at that time making, building what was named the membrane group, essentially headed by Kai Simons, who had his own group in this sort of constellation of people working on membrane trafficking, membrane structure, at EMBL. After my visit to Heidelberg I took the opportunity to go to Nice, to go to Marseille, to go to Bordeaux if I remember correctly, and to Paris, where I met Head, Director of Institute, who confirmed what was the common view those days; if I come back to their lab or to their institute, I will be part of an existing group and work on a project that will be essentially something chosen by us and not by me, which of course was not my cup of tea.

So in flying back to California, I had made essentially my decision that if the job is offered to me, I will go to Heidelberg, and then I will have to convince my family and my wife that Heidelberg was a great city to live <chuckles> there, and this was a great place for me to actually develop my own research idea.

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So I finished my postdoc in the summer of 1978, travelling back to Europe, I had in my suitcase or in my pocket, a flask, a culture flask of cells, which was possible those days, and so I had them with me on the plane, not with my big luggage, and these cells were the base of my future research at EMBL, and I had almost accidentally, during discussions with colleagues on the campus of UCLD, discovered that the property of this cell line, called MDCK, Madin-Darby Canine Kidney, which actually matched my interest in trying to understand how epithelial cells form an epithelial monolayer in culture, and organise in such a way that these cells can transport iron in a vectorial fashion, secrete iron, transport protein across the cells, and building membrane domains which we call apical membrane and basolateral membrane, of different protein and lipid composition, sealing the monolayer with very tight contact, called tight junction, that prevent lateral diffusion of water and iron from the external milieu down to the so-called internal milieu, bathing the cell at the base. This was a unique, an incredible, unheard cell model system for the biologist <20:00> interested in this basic principle of cellular organisation that is very common in differentiated epithelial cells in the body of multicellular organisms, from fly to human. I said fly, I could also say *C. elegans.* So understanding how you can create different membrane domains, that in itself generate asymmetry and cellular function of asymmetric separation, asymmetric transport, protection as a whole, because of the monolayer formed by cells in close contact, was something that we could not approach with model system. We had, we knew this was the basis of important cell teleology of normal physiology, and that this thing didn't work properly at the origin of a number of diseases which affect this epithelial cell, let me insist on the fact that they represent 70% of the different cell types of the body. More than 150 different cell types in other words. So this is what I had, as a treasure in my pocket, in my bag on the plane. And I talked when I arrive at EMBL, I discussed with Kai Simons and other colleagues, the properties of these cells and I have to say that in parallel, in a totally independent fashion, David Sabatini and Enrique Rodriguez-Boulan from New York University had uncovered, independently, the property of such a cell line, and they actually were one step ahead of me because they had already started a research project where they demonstrated the capacity, the fact that these cells had an intrinsic polarity function, because when these cells were infected, by different viruses, some viruses will bud or will be released from the cell, infected cell, from the top of the cells, and are essentially released in the bathing medium of the monolayer that the cells form when it grows, while other viruses will bud, or will be extruded, and will leave from the cell, from the basolateral side. In other words essentially viruses and the product, the gene product of the virus, when the virus assembles inside the cytoplasm, was targeted into one domain or another. So that's another … that was evidence that this cell can actually sort out different signals and different information in different domains and space. And I knew that the cell that was brought to EMBL, that I had got from a group of kidney physiologists at UCSD, I knew that these cells had an intrinsic polarity because looking at different membrane markers, none that exist by looking without proper antibody in vivo in kidney section, that different markers were localised in this cell line, either on the basolateral membrane or in the apical membrane. So the cell can sort out its endogenous membrane proteins to different domains and as such, using another system, Sabatini and his colleague had showed that different viruses can bud from different domains.

When I told Kai Simons about this observation, Kai was a cell biologist interested in membrane, and virus with membrane envelope, and when I told him he said, 'That's impossible.' But Kai has an incredible personality that <25:00> he could say that bluntly, but then he will think about it and he was, and he is, bright enough to change his mind, to listen, to reconsider what could be considered as a dogma and to make it as a research project.

So when I described that to Kai, he said 'It's impossible. Nobody has ever seen that.' He didn't mean in vivo, he meant in cell culture. And so he became very interested in the system I was planning to develop with different approaches, and actually progressively put his own group of research working on viruses and viral infection on this model system. And he had actually developed his, built his own scientific career from there, using this system, while I was myself developing from a different direction.

So this is the story of how I came to EMBL and how my project works. My project was essentially if I want to summarise, a lucky encounter of someone who will referee my application for an EMBO, someone who did not forgot about me when he was important to recruit new scientists, and one thing I didn't say, at that time, I didn't say so far … is that EMBL being a multinational laboratory, sponsored and founded by different governments and different states, so far the recruitment of French scientists had not been successful; either because nobody wanted, no French scientists wanted to leave their home country, at least some of the best potential candidates possibly, or because the candidates were not up to the expectations of those who were recruiting the young people. And it just happened that they must have liked my project, my track record, I'm sure they had reservations about my presentation <laughs>, but they overcome probably this possibility or this, let's say, pitfall <laughs> and I was offered to join EMBL a few weeks after I returned to California. So the fact that I was, as Kendrew said, 'Oh finally, I'm so pleased that finally we will have a French group leader at EMBL.' And it is true, I was the first French group leader and not only French group leader, these days, there were not so many students and postdocs, so it was a time when at the canteen we could start to speak French among the few French people working at EMBL. So this is my … how I started.

When we arrived at EMBL, when I arrived in September, of course there were two aspects I had to prepare, the arrival of my family, we came in October. Of course I have to say that these days people like Konrad Müller were very helpful actually to find a good flat and I was lucky that for the family I could rent a flat in the old city. It was easy for my daughter, five years old, to start to go to school at *Hasenleiser*, the so-called international school, so from that point of view, and the first autumn at EMBL, or at Heidelberg I should say, was a sunny fall, which actually was a good transition for my wife! <Chuckles> I would not say the same for the winter that came after that, which was very rainy! So for <30:00> this consideration, so the support of the administrative staff at EMBL was perfect. Very helpful. For the practical aspect. And in the lab, the lab was just barely painted or finished; it was just brand new I should say. So I was on the same floor as cell biologists, membrane group of those days, settling the lab, organising the lab, was

a piece of cake I have to say. The *Lager*, the place where you can actually get the instrument you need from the pipette to the spectrophotometer or microscopes – standard microscopes was actually available, and it took me … it was very short, I had to organise my lab. It was very convenient. Very quickly I had a position for a technician. I had a position to recruit a scientist, EMBL staff, and I could quickly recruit, a few months after arriving, post-doctoral fellows. So it was, despite the fact I was totally unexperienced in creating and building a lab, everything was made easy and was made easy even compared … I had no clue about what could have happened if I returned, go to other places, but at least there was nothing I would have to complain about. And very quickly all the facilities, all the organisation, tissue culture facility, animal house facility, microscopes, all that was extremely well organised. Kai Simons was the mentor of all of us these days, was making our life not only easier but creating a very friendly atmosphere, and not only a friendly atmosphere but also putting us in a context that we had a unique chance and we should take that chance to actually make EMBL famous. Something that could be seen, not only from the rest of Europe, but also could be seen overseas. And one of the things that Kai and his colleagues did that actually contributed a lot to this very rapid raise of EMBL and visibility of EMBL among the people working on this aspect of membrane traffic and trafficking, endocytosis secretion, that Kai created a series of lectures, a series of course supported by EMBL and symposia, to which the best known scientists in the world would come, and particularly from United States. George Palade, David Sabatini, Günter Blobel, Jim Rothman, Randy Schekman – I'm just flashing here names which are so famous that they all got Nobel Prizes. And very soon those people got to know us, our work, and as a return not only to be friendly but also because they realised we were doing same sort of frontline of the research in this field, we were invited to United States, to the big meeting in Gordon Conference and Cold Spring Harbour. So the cell biology or membrane group as it was called those days, before it became the so-called programme in cell biology, was rapidly known and we, as young scientists, at the early days of our career, were exposed to the international scene and something that of course was extremely incentive, and nourished our ambition and our desire to contribute to the best of science. So this is, EMBL was these days really relatively small. <35:00> When I left EMBL four years later, I would say almost five years officially because my next move was the Pasteur Institute, I'll tell you more in a second, in fact I remained part of my group or what was left, my small group of five, six people, remained for another year at EMBL before I closed down completely – the lab was completely closed down in 1983 and so I was going back and forth, and within five years' time I was in a situation where my research, as part of the research of the membrane group, put on the scene at the highest level possible and my research got to be known by many scientists. I could publish the work in the best journals. And so the conditions these days at EMBL, there was no worry about budget, issue of money, which of course something that in the real world I will have to rediscover or to discover! <Chuckles> But this period, which I will call myself the golden age of EMBL, of the pioneer, was something that was unpredictable, as research can be, and also is something that we contributed – when I say we, the different groups there – in a very special environment of friends. The groups where I learned these days was optimum size for the group. If it gets too big, the ratio between productivity and the size is not in favour of the productivity. So if you're too small, that doesn't work; if you're too big, it doesn't work. If you're too small, at least if you are the right size, you are obliged in a way if

you want to be efficient, to interact with the neighbours, with the others, with people who complement, overlap your research and have something else to bring. And this is a big lesson I learned there, is the interaction with other groups, people who have different experiences, different culture, but common aims and common objectives. That these things can actually complement each other and synergise, and collaboration with other people for fixed, well-defined objectives, was a motto. And the collaboration with loyalty, respect and I would say equity in what will be the outcome of your venture. And I had, at my time of EMBL, developed my own project, my own tools, which I shared with other people. Like I became, as Kai Simons used to say, and others, was the guru of making and producing antibodies. I made not only polyclonal antibodies but special procedure to inject, immunise rabbits, but also was among the first at EMBL in the early days of monoclonal antibodies, to produce monoclonal antibodies. So that, I was very good in microscopy. My physical background helped on that. I was very good in generating probes. My chemistry background was very helpful on that, combined to biochemistry and cell biology, I could share that with other colleagues, and they will bring to me things that I didn't know. So this was a very incredible time and one thing also which actually was very … a couple of anecdotes.

We had regular group meeting every week, where we had sort of presentation of the on-going research by <40:00> a given group leader, in particular when we were close to write or to start to write a draft for a future paper, to present our work. Very … not only in detail, but also showing the results, the data, our hypothesis, our conclusions, and this was very commonly and in a friendly fashion, but critically evaluated by the other group leaders, post-docs. I remember Graham Warren, who came from Cambridge, training at Cambridge University, was the most critical guy, as the British can be, taking as a sort of game or as an exercise to say yes when you say no, or to say black when you say white, and challenging us continuously, and this was for me a very interesting experience. And one day Ari Helenius who was working with Kai Simons on a separate project but related project of Kai's group itself, he came to this meeting and said, 'You know, there is one thing that I cannot stand in those meetings. That everybody presents the data, micrograph for instance or electron microscopy, a gel of electrophoresis or curve of binding affinity for a ligand for a receptor, everything is perfect …' He said, 'In my lab, we have results, the EM micrograph is not that well focussed, the immunofluorescence image is a little blurred or there is a piece of dirt in one corner, the gel … electrophoreses, the bands are not perfect, the gel is cracked' and so on and so on. And he said, 'This is not real life. We should not give the impression to the others that in your lab everything is perfect and there is never an imperfect experiment. In my lab the experiments are not perfect. We will keep repeating and sometimes we have a perfect experiment and that maybe the one we will publish. Therefore I don't believe that I say I'm going to put in the corridor two more on the wall – like a poster – all the experiments that didn't work, all the dirty gels or the unfocussed, and then we can discuss on that because there is scientific matters and information on those, and I don't want to give the impression that everything that we have on a daily basis is polished.' And of course we knew he was right, and this was something that actually had a very profound and important … because when he did that, the group of EMBL started to be known, we started to build our fame. It was a couple of years after I arrived. And giving the

impression that we were super-stars and everything we do was perfect. And of course then the exchange of information between scientists and especially not from the group leaders but for the post-docs, or the students – if a student has always a dirty gel and another one shows only perfect gel, he feels very intimidated. And we know this is the story, this is how it happens, and this message still goes for today. And I'm sure it will remain like that for next decades – that we don't do things absolutely perfectly, but nonetheless we should get the confidence on we will improve, we will get a better experiment, and from those dirty imperfect experiments we still learn something in science. And then it has … these days we are unfortunately talking about all the problems of fabricated <45:00> data, frauds, and I think this is part of the problem. This is part of the issue – the pressure on young scientists is so enormous, it was strong force, but not so … it existed … I'm going to tell you an anecdote of that, but we have to say that we create this environment. In creating an environment that gives the impression that everything we do is always perfect, it's not the reality of experimental science. It's not the reality of life by all means. So like that I will say Ari Helenius did play, in this context, like going to the psychiatrist, a very positive impact which actually made those meetings we had after that, and after … not to feel intimidated or ashamed of doing an experiment that as a good scientific context, but remained technically to be improved. That doesn't mean we will not improve, we will not publish a cracked gel or blurred image, definitely.

Now I mentioned the fact that we were definitely not concerned but aware about the fact that being at EMBL was being lucky and privileged to be in such a condition, for those who actually joined EMBL at the early days and hopefully, I hope those who come today still feel that way. And let me tell you one thing I will never forget in my life. Weeks or months after I arrived, as it was not uncommon I understood later, I received a phone call from Waltraud Ackermann, Secretary of John Kendrew, Sir John, and Sir John, as she said on the phone to me, 'is glad to invite you for tea next Wednesday.' Don't take my word for next Wednesday but soon after … 'to discuss with you. Are you available?' I said, 'Of course,' you can imagine that I said, 'Of course I am available!' I would have cancelled every possible meeting … but nonetheless I'm sure in these days, as a young investigator I was more busy in setting up my experiment than going to a meeting where I would have to say, 'No, I cannot go to John Kendrew's invitation'! So I did. And I went there, tea time – *noblesse oblige*, as a British Nobel Prize winner and founder of EMBL, first Director General of EMBL – and we had a discussion which must have lasted roughly 30 minutes, but most of the discussion was extremely relaxed, talking about many aspects of what he did in his own life, how life with my family was in Heidelberg, in the lab, and so … this was the majority of the discussion. But by the end of the interview, which of course I could imagine he must have received …however I have no proof of that, a warning from the secretary that the time has elapsed, he said, 'Oh, you know, Daniel' and this is essentially almost word for word, at least what is in my memory, he said, 'You know, Daniel, you've been chosen among the best scientists, the best young molecular cell biologists in the world to join EMBL as a group leader. You will be successful, no matter. EMBL provides for you all the conditions, the resources, that allow you to be successful in your project, which we know, that's why you've been chosen, it's an interesting, original and risky project, but you will make it.' And then

he said, 'But on the other hand, you know EMBL, we should be successful, and your success will provide the success of the whole EMBL.' And he said, 'If <50:00> you're not successful, I will make sure the rest of the world knows about it.' Full stop. And then he said, 'But … if you need advice, don't hesitate. My door is open.'

So when the young people in my institute, in my lab, said that they have too much pressure now, which I recognise they have pressure, and when they think it was easier for us to make it, to be successful, definitely then we were not so many, the conditions were totally different, but those who think we were not under pressure or at least we should fulfil what people were expecting from us … and this was, I would say, the essence of the spirit those days. And too actually … since I think it's the right time now to mention that, this was a time where we had opportunity to meet other people besides of the membrane group, I was particularly interested about what Jacques Dubochet, recent Nobel Prize, who did his work, his early work, at EMBL as a group leader and head of programme there, starting for what later will be recognised as a major achievement in electron microscopy, and the cryomicroscopy. And I had many discussions when I was crossing in the corridor, going to conferences, Jacques Dubochet with his indescribable Swiss accent, always very charming, very optimistic … he was in a sense, for me, in front of a formidable task, to do something which was beyond our understanding in those days. And I admire his courage, not only from the time of EMBL but all the … *endurance* that he had actually to go through, to find what was the right solution. Similarly, I have seen the early days of the work of Christiane Nüsslein-Volhard and Eric Wieschaus, working in their own area. And I could ensure you that most of us, if not all, could not appreciate and understand what will be the consequence for development biology of their early screen of the so-called maternal gene in the fly zygote. Eric was a very lively and very interesting chap. Christiane, *Jani*, was more reserved and I would not say serious but Eric could talk and smile and talk about futilities and Christiane, I never heard that she would. So in these people was the type of environment we had at EMBL those days.

So now I move, I left EMBL, 1982, officially but as I said earlier I maintained part of my group for another year, and in 1982 I left EMBL to go to the Pasteur Institute, where I was appointed as a new group leader, the word at Pasteur was not group leader, was Head of Unit, which means about the same, *chef d'unité,* in the Department of Molecular Biology. For which later on, a couple of years later, I became Chairman. It's a rotating position. <55:00> It's a duty that those who have not done it should do. And so it's not a position forever there, when you are Chairman. And why did I actually move from EMBL to Pasteur? I think it's again … not an accident – unpredictable event – or something that definitely was not planned. Neither by me nor by others. Possibly by John Kendrew. François Jacob was invited, I think it must have been in 1980 roughly, he was invited to give a talk at EMBL, by John Kendrew, and of course François Jacob was a big hero in molecular biology. He was also, I think it's good to remind, a great supporter of the concept of EMBL and EMBO as well of course. Which would be, the concept would be a place like the CERN has been for physics, a place where different countries will put their resources to do things that could not be done in a single country, and where young scientists will actually be recruited to actually embark on projects with

a high risk and with the frontline of science, with unlimited, in a way, resource and support.

And so François came to the EMBL, he gave a lecture there, and again the Secretary of John Kendrew, Waltraud Ackermann, told me that I was among the people, the scientists that François Jacob will meet. I don't know if this was a choice of François Jacob, or the choice of John Kendrew, or both, I will never figure out. But anyway, François Jacob came to my little office, the second floor of the first building of EMBL, and he asked me why I was there and what was my project, what I was doing. The visit took maybe half an hour, where I present my ambition to understand cell polarity, which is still a big problem or a big issue, unfinished task, not only for me but for the rest of many scientists in the world. And François, as usual, did not talk much; he asked me a few questions, and I never figured out – I did not figure out immediately or at the end of this discussion whether he likes it or not. He complemented me for being the first French group leader of EMBL, but I had no idea whether he liked or not my project.

He must have liked my project for the following reason. A couple of weeks after his visit I got a phone call from the Head of the Department of Molecular Biology at Pasteur, who was Antoinette Ryter who said that after the visit of François Jacob at EMBL, my name was mentioned during a meeting of the Council of the Department. The Director General of Pasteur at that time was François Gros, who was himself, had his own research group in the Department of Molecular Biology, and Antoinette Ryter, the Chairman of the Department, said, 'Your name was given as a potential candidate for a new position, because we have space, laboratory in the department <60:00> because the laboratory of Avrameas, immunologist, is moving from this department to the new Department of Immunology at the Pasteur. So if you are interested, apply for the position and contact the Director General of Pasteur, François Gros,' which I did. And finally my application was selected and I was appointed group leader. And there's no doubt that in this sort of transition in my career it is absolutely clear to me, although he never told me blankly, that François Jacob was definitely the person who put my name as a potential person to come, to move from EMBL To Pasteur. I see no other reason, possible explanation, and I'm grateful for François Jacob, with whom, when I moved from EMBL to Pasteur, I established a longstanding friendship and discussion over the years, and I have published that in paper which was published by Pasteur Institute, it's called *A Tribute to François Jacob, My Move from EMBL to the Pasteur Institute.* And it's a couple of page description with my interaction and the role played by François Jacob. So indeed, this happened in 1981. It took another year before things were wrapped up. I had of course mentioned that to Kai Simons, my sponsor at EMBL, and Kai, who was not particularly keen of seeing my leaving, he says it's too early, you should stay to actually get more of your time at EMBL … he said, 'Your contract will certainly be renewed.' But it was clear that François Jacob actually has contacted me at one point, because I was hesitating whether it was time for me to move or not … stay longer … and François Jacob said, 'You know, there is an open space. You take your chance or not.' Well, when you're a young scientist of 34 years old, given a position of head of unit at Pasteur, by the way I was the youngest ever recruited Group Leader at the Pasteur Institute at that time. I don't know if they did it since I left, but since that time, but still I recognise …

being surrounded in the department with François Jacob, François Gros, Changeux and Maxime Schwartz who become Director General … that was a very intimidating environment. I would say almost even more because of the history there, more than EMBL itself.

So I think I also took the decision because my daughter meanwhile had grown up and we had, as well as the boy born in California, we had another boy born in Heidelberg, and the decision was where are we going to educate our kids? Are they going to be totally educated in the German system? Despite we had nothing to regret on that. Or are they going to … readjust to the French system if we ever go back? So this was part of the choice, but I have to say the word that François Jacob, the sentence, he said, 'We have a slot. If you don't take it … you may not come back.' Well, it was <chuckles> fair enough. So then I moved to the Pasteur institute. And I continued over the years to have a lot of contact with EMBL, lot through <65:00> my colleagues and friends, Kai Simons particularly but also others. But also I served in every possible committee <laughs> for EMBO, where I was in the early days in the workshop committee, fellowship, EMBO Council, and then the SAC of EMBL after that, plus I was in the site visit several times for the cell biology programme and also the structural biology programme.

So now quickly these contacts remained very strong, intellectually I would say, or in my way of understanding science, and also scientific organisation. When I was asked by the President of the Curie Institute to move from the Pasteur Institute to become Director of Research of the Curie Institute, with the possibility to refurbish all the hospital of Curie and transform it into research laboratory, the place where we are now, where my ambition was to build a cell biology department at the Curie Institute, which did not exist at the field of research, which did not exist either at Pasteur, where at that time I could not convince the Director General of Pasteur that cell biology was essential to understand interactions between cells and pathogens, such as virus, bacteria and parasites. When I moved there it was very clear that I will move, will create at the Curie Institute an environment that will mimic the principle, or will apply, implement, the principle which was so impressive for me at the early days of my career, and I believe were not only still standing as the excellent principle and one of the best solutions, actually, to create a dynamic and scientific environment – in other words base the strategy on the faith that you have for the youth and young investigators, to which you give the independence and responsibility to be successful. And I bet that it was easier, if risky, to identify a young scientist who will become the future star than to recruit a star which usually behaves like a diva from a big university, from a big place, and to create a new department or new group. And I have seen a lot of disappointment in this strategy and I had in my own experience less disappointment or no disappointment I would say, by putting my strategy in choosing the young people that actually are keen, ambitious to move science at the front line. And this is what I did at Curie, with the principle that I will recruit young people who of course are promised to be the future leaders in their field, I recruit those people and give them freedom, independence, and tell them if you fail, I'll make sure the rest of the world knows about it! As Kendrew said.

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And I also made a very important choice that I want to have a balance of gender between young women and men, and not putting my choice on the balance but putting my choice on equal opportunity I would say. And knowing that there are conditions that will favour the success of young women who have families, and creating a proper environment so they find a place to live, a place to put their children, young children at school or nursery, <70:00> and give them their confidence they are just as good as men. And if when I left, when I retired as Director of the Curie Institute, I had recruited more than 70 groups altogether in the institute, more than half or almost half of the heads of department were women, and more than 30% of the group leaders were women as well. And with an average age below 40 for the group leaders. So this is something, and I insisted that the groups should remain rather small, never more than 10 people. Those who want more space, they should accept actually to have more responsibility and become head of department where their group is part of the department. This is what I did for Genevieve Almouzni who is my successor. She started as a young investigator, with a very small group. A couple of years later, we had to push a wall to accommodate the group because she was very successful in attracting people, and I offered her the possibility to be the head of department where I need a person to replace another one who was taking her retirement, another woman. And same was applied to Edith Heard when she joined Curie from the Pasteur, and 16 years later Edith has climbed from a young post-doc in Genevieve Almouzni's department, to move head of a new department of developmental biology that I created from scratch on the campus including the building, and now I'm so glad that she became, and has been chosen as the Director General of EMBL, and that's a woman on top, who has a British and Greek history and now working in France, and now moving to EMBL. I think, what else.

And when I say I am so glad that I took the strategy of recruiting young people, I'm glad that over the years I have seen, using the basic principle in EMBL, translated I would say, because you cannot do in EMBL what you do in Paris or in France and vice versa, so it's just a matter of plasticity, like life, we are very adaptable, dynamic. And so the principle I apply to when I was in France, was actually based on that with … I forgot to say there was one step which I actually developed in parallel, in parallel with the development of the Curie Institute, based on EMBL principle, is I am the founder of what is called the ATIP Programme in France, which is *Action Thematique et Incitative sur Programme et Equipes* – in other words, it's a programme that aims to recruit young investigators after a post-doc, searching for an independent position in an incite laboratory in France, and they are given security, they have a job at CNRS, they are also given the possibility, not only possibility, the obligation for the head of the department they integrate to have guaranteed space, guaranteed access to core facilities, a budget that is not part of the budget run by the Director, their budget, from which they can … it's like a grant, and they are the owner of the grant. And the Director of the department cannot actually puncture some money. They may have to pay core facilities cost but that's another story. And this programme started in 1989 and started with an action on cell biology as a base and CNRS developed it for other fields like genetics, developmental biology. The chemist also did it and the INSERM <75:00> came after and this is now called the ATIP-avenir program. But it's on the same principle. And why did I say that? I said because the Curie Institute

is a foundation that is supported by national grants, CNRS, INSERM. The positions for the scientists come either CNRS INSERM, INSERM for university, and of course the foundation of Curie cannot pay for everything. So we need the help and the support of CNRS and INSERM and university to actually contribute to the budget of this independent group, and also the research of independent group. So this committee that I created from the beginning was an international committee of people I knew from my EMBL time, who accepted … Paul Nurse, Günter Blobel, Randy Schekman, again some people that actually … who realised that what I wanted to do in France was actually a translation of what I actually experienced so profoundly at EMBL. And that's actually … so the programme existed, because I moved in Curie in 1993, the cell biology department refurbishing of the building was finished two years later, and I moved from Pasteur to Curie at once in one step, and then that's when I start and actually a year before, to recruit the new group in this department. And I could actually do that through the principles that were nationally accepted and sponsored by the CNRS to recruit young people. And I recruit those young people like Genevieve Almouzni, Catherine Dargement, later editor that I mentioned, much later. And of course I rely my choice what fits with strategy and the topic of the Curie, but I rely based on an international committee that actually was supporting this concept and also the CNRS that actually put the administrative framework to actually implement this concept, which is totally derived from EMBL spirit.

I think I told you most of my life and I think that's enough. Thank you!

AFL: That's fantastic. Thank you very much Daniel. Thank you.

DL: OK.

<End of interview>