Sous-titres anglais d'une interview de

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I was born in Nancy, Meurthe-et-Moselle. My father was a businessman. He came to Paris, well, we all came to Paris when I was three and a half. And that's it. I grew up in Paris. I went to the Lycée Carnot, from the 9e to the Math-Elem included. After which I went to university, to the PCB, to the Medical Faculty where I did the first two years. The second year was during the first year of the war. I hadn't been mobilised yeSo during that first year of the war, I did medicine examinations. And in 1940 I left.

It was something that I found very restricting. There was always the threat of being tested, of being sent to the blackboard. There were tests almost once a week. There was a sort of competition between the students which I didn't really find pleasant. It wasn't bad at these competitions, but I didn't particularly enjoy them.

And did you have specific teachers that stood out, science or literature or philosophy teachers, that struck you by their teachings, by their classes ?

Not specifically. I had some very good teachers. Many were complete eccentrics. The majority were interesting. A few were very very boring. So there were the characters that you always find in cases like these : the guy who's deaf, who doesn't listen, who... but really nothing very specific.

Whereas on the other hand in medicine, you are talking about a person from elsewhere whom I discovered, Mr. Hovelacque. You say it was your first scientist...

He wasn't really a scientist, he was an anatomist. He was an exceptional character. For a start he was physically extraordinary. He looked like an El Greco. He was very tall, very thin, with a beard. He absolutely looked like an El Greco cardinal, without the hat. And so, he was absolutely astonishing. He was a remarkable teacher. Anatomy, it isn't exactly fun, but he had a way... He was able to draw anything with his two hands on the board, any bone of the skeleton. One day, he said to me : "Jacob, don't ever get married, no wife or you are done for !"

But it was a form of science, a little traditional science...

It was a form of science, yes, but it was a completely descriptive science. He would take a bone, throw it in the air- one of the little wrist bones- and say : right or left ? during the anatomy exam, which was nonetheless quite delicate.

And he is the one that has most impressed in your medical studies among the other teachers you had during those two years, none of which had impressed you so strongly. ?

He was the most outstanding individual, he was the most extravagant if you will, and the purest. During the War he had been a Zouave captain... the First World War. After four years he came back, having not taken any leaves, but before going to see Mrs Hovelacque, he went to the laboratory. Which is quite an astonishing achievement.

My maternal grandfather : The General. He came from a very modest family. He went to the Ecole Polytechnique, and at that time there weren't many opportunities for Polytechnique graduates. He stayed in the army where he became a four star general. And he was quite a character. He's the one that gave me the majority of my education.

Even philosophical, I think, even...

Yes, general.

So much so that you wanted to do Polytechnic...

He's the one who decided that I would go to Polytechnique. And I decided that I had absolutely no desire to endure mathematics for I don't know how many years, that it didn't particularly appeal to me.

And so, I had an uncle who was a doctor. I went to see him and said to him : I want to study medicine, I would like to become a surgeon. He said to me : surgery, that's really stupid, you need to be a doctor. And besides, you be able won't to handle surgery, you'll start vomiting at the first glimpse of a scalpel. So, he sent me to see one of his friends to attend an operation. I found it incredible, and decided that I would be a surgeon, in spite of the uncle.

In fact you describe the impression you had during the first operation, this atmosphere of serious work.

Well yes, I found it... he had sent me precisely to see one of his surgeon friends. I found it incredible. There wasn't a single gesture out of place, it was really astonishing.

She had Hodgkin's disease that had started the previous summer. And during all of autumn and winter she was ill. And she died on the 2nd of June, just before the collapse of the regime. Which means that in my mind, my mother's death and the collapse of the regime coincided completely. It was one unique event.

And my departure for England. All of that was one big thing in my mind. A break between childhood and the future.

Do you think that you would have gone so easily if your mother's death had not occured...

It's very difficult to answer that question. I think so. Because I thought it was insane not to take up arms in June 1940. I found accepting the defeat and the German occupation unbelievable. It seemed monstrous to me.

I think that most people could see it, but didn't want to. Because it was hard not to see it. Everything gave prominence to it. Every German move, everything showed what they were about to do. It was more or less obvious. But people didn't accept it. There was a very defeatist atmosphere, people didn't care about anything, they especially didn't want to fight and really weren't interested in any of it.

A pacifism that you sort of point at because it was often a left-wing pacifism, a pacifism of good intentions : war is a bad thing, therefore we shouldn't be going to war.

Well the two added up. The pacifism on the one side, left-wing pacifism and the right-wing non-war. It all added up to give... there was an awful atmosphere during that first year of war.

And also an atmosphere of latent violence between- in society, of confrontations in a way- precisely between communists and right-wingers close to fascism ? Could we already get a feeling for what the atmosphere was to become ?

A little. But people weren't really interested in all that. They were concerned with themselves, and not really with matters of the State and general matters. The atmosphere was very bad during that year of war.

I decided to leave. I was in Paris, I was in my second year of medicine. I took a few exams. I left on the 14th, it was the 14th of June, the Germans came in by the north and I left through the south. I left in a car with two or three friends. And we ended up... well there had been long discussions, because two of us had quickly made the decision to leave for North Africa or England, to join a war that we hadn't seen and that we considered to be necessary. And the others wouldn't have any of it. In the end, we ended up in Bordeaux. We tried to embark in Bordeaux, but the British boats had just left. We went to Bayonne, more or less the same thing. Then to Saint-Jean-de-Luz. And in Saint-Jean-De-Luz we met a guy as we were walking through the streets looking for a way to leave. And we met a guy in uniform who was a Cavalry-Lieutenant. Yes, I think that in Bayonne there was a sign at the British Consulate : For military questions concerning French people, enquire in Saint-Jean-de-Luz on this road at that number. So we stared wide-eyed. We went to that road that number. There was nothing there. We looked everywhere. We met a French officer in uniform who stared at us and saysWhat are you doing here ? So we said- We're looking for our Aunt so and so. He said- Do you want to leave ? So we said- Yes, if possible. And he said- Listen. There were Polish civilians and the remainder of two Polish divisions, who had fought with us, on the western front and who were boarding Polish boats. And the guy said : Get out of here, don't hang around, disappear until 5-6 this evening, and then you will probably be able to board. So we went for a walk. When we first set off there were four of us, but now there were only two of us left. The others had gone. When we arrived a cordon of policemen had positioned themselves. We started to leave. And right in front of me was a really short guy, who I later found out was a jockey. He looked like a jockey actually, the 'titi parisien' jockey. And the enormous policeman in front of him said- Where do you think you're going ? Because at that time leaving was prohibited for the French.

It was official ?

It had only been official for a short time, for a couple of hours. Since 4pm it had been official. So the policeman asked - 'Where do you think you're going ?' He turned around and said 'Swastika !' Which was the only word ending in ski or ska, ie polish-sounding, which came to him. The policeman was so shocked that he let him through, and we slipped in behind him.

You said that you didn't even where the boat was headed when you got onboard. You knew it was leaving France, but you didn't know if it was headed for England or Africa.

We wanted one or the other. We didn't know, but we soon found out it was for England. That boat was pretty surprising. There were a lot of young people on board, high school or university students, like me, who wanted to go to England. There were also a few officers who were leaving. I met one guy who asked me - Where are you going ? I said- I don't know, I'm going to try to fight. What about you ? He said- It's a unique opportunity to get away from my wife and my mother-in-law. Unexpected but as so !

As opposed to the incredible confusion and mess in France. Which was unbelievable : the defeat, the guys on the roads, it was incredible at that time, you can't imagine it... On the contrary, in England, everything was very organised. The guys took care of us. We were put in camps for 8 days, we were screened because they were scared that Germans would sneak in etc. And so we ended up in a camp. But it really was a nation which was fighting, which was completely preparing itself for war. And that's when the real bombings on London started. I didn't stay very long after that. I was in a camp in the south-west of London where most of the Free French were. So at the beginning there were only two of us. At the beginning we thought- we'll join the English. Then we found out that there was a guy called de Gaulle who was doing something. So one of de Gaulle's guys paid us a visit and explained the operation. And so, we joined that new formation which was to become The Free French.

You said that for once, it was a military formation, but without that humiliating aspect of military formation. That you were ac-

tually trained in what was useful...

But it was constituted solely of volunteers. And that changes everything. Between the guys who were recruited by... well not by force but who were recruited and enrolled to do their 'service militaire' and a troop solely made up of volunteers who are there to fight, it's definitely very very different.

And there were people from a variety of backgrounds and with different political opinions ?

There was a mix of everything in there, absolutely everything. You had communists, royalists, absolutely everything. The majority was the usual, radical-socialist, socialist. But there was... I remember a conversation between two communists who were saying- Honestly, that general isn't being very reasonable. In the end, they enrolled like everyone else. And there were also far-right guys. It was a mix of everything.

There weren't any confrontations between these very differently opinionated people ?

No. What really dominated at the time was occupied France. Especially as it was very recent, it had only just happened. And it was probably the biggest catastrophe in the history of France.

And when was the first time you saw General de Gaulle ? When did you see him for the first time ?

Well, General de Gaulle, I saw him once in the camp where he came to make a vague speech. And then I saw him again on the boat. Because I left fairly quickly, at the end of August, I boarded a boat in Liverpool on the 29th or 30th of August. On a boat that was leaving- top secret but everyone knew we were going to Dakar, because the French are very talkative. And I saw him for the first time on the boat. I was leaning on the rail and I was looking at the horizon. You could vaguely see land. And I hear a voice behind me that says : What is that land ? so I said : I don't know, it must be Ireland. And I turn around, it was General de Gaulle. It was my first meeting with the general.

What impression did he make to you ?

Well he was very imposing. He was- For a start he was huge. That occasion was a little special because he wore a colonial helmet, and it gave him a particular touch. But he was very imposing. Very.

You talk about the Gothic cathedral in your book ?

Yes, that's right. Gaulle- Because since I was a kid, I had the habit of playing with words like that, to ramble on : de Gaulle, Gaulle, Golgotha, Gothic, Gothic Cathedral, actually I think that it suited him rather well.

...admire never idolate...

That's absolutely right. Especially during the war. Afterwards, I wasn't as keen on the political aspects of the post-war. But during the war, he had been THE guy who... fulfilled an indispensable function. And he fulfilled it remarkably well.

And in your opinion, no one else, among all the politicians at the time, could have done that. You can't think of any other one ?

There weren't any politicians in England at the time. There just weren't any.

There was Pierre Mendès-France...

No, he came later on...

He came much later. Mendès-France was in prison at the time. He was sent to prison in North Africa. He escaped later on and became an Air-Force captain. There just wasn't anyone.

Well, Africa... When you take a young Parisian student who has never left Paris, who's studying at the Medical Faculty, and that you take him and send him to black Africa, and in particular, that you leave him alone in a region as big as half of France to be that area's doctor, it's quite a change, it's rather surprising. It really was something very surprising. Because during that time I sometimes feltWell, at first I went to black Africa, I left with the Dakar

expedition, where we were very badly welcomed, so we left again. Afterwards I went to Cameroon, then in the north, in Chad. And I was the doctor of a region which was more or less as big as half of France, with my two years of studying medicine. In other words, you couldn't ask me much. And it really was an extraordinary life- It was something completely new. And there, I learned a lot.

And did you have any problems with the Africans ? Or did you not encounter any in the end ? You were the doctor. Did you have very natural relationships ? What I mean is that at the time you didn't feel any opposition ?

No, not at all. There was a village and I was the doctor for both the battalion infantry, which had set up camp just north of Chad lake, and at the same time I was also the doctor for the region, meaning a village. Every morning I went to the village to do my visits. It was very picturesque. That was also something, coming from the "Hôpitaux de Paris", from the latest surgery techniques of the Saint-Antoine hospital, and landing there, where I was expected to run a small dispensary, where women came to show me their troubles, where I had to spot the cases of syphilis, because it was important for the troops. It was a very different job.

And for example did you have everything you needed in terms of medicine in order to help at least a little ?

Mostly. I remember here was a captain doctor with an incredible accent from the South of France, whom I replaced. He was very happy to see me arrive because he'd been there for six months, and he was fed up. He said to me : "Can you imagine, that here, there are no white women !" So he was very happy to arrive and to leave. He explained to me what I should and shouldn't do : "in the morning, you treat the soldiers. Afterwards in the afternoon, you go to the dispensary, and you treat the civilians". And on that note, he left, too happy !

You really learned medicine on the job.

I really didn't learn much about medicine. You know, soldier medicine isn't real medicine. "What's wrong with you ? - My head hurts Lieutenant. - Take

an aspirin tablet." That was essentially it, medicine. When there was something more serious, we would send it to the hospital. But there, miles away from anything in Chad, you needed two days to get to the hospital in Fort Lamy.

There was one operation... they brought in two soldiers who'd stolen some cows, and had been captured by other soldiers who'd beaten them with rifles. One died when he arrived and the other one had his skull bashed in. So, I told myself : while we are here, why not try trepanning. His skull was visibly bashed in. So I had something to make holes, a sort of saw. And I took out a small piece of his skull. He was bleeding, I made a ligature, and it went so well that two days later, he escaped from the hospital. That was typical. There was another one that fell out of a tree. I don't remember what he was doing, he broke a leg. So on the first day I administered the medicine that we were practising in the hospital, I don't remember which one, in Paris. So I made him a cast. No ! I made him a bandage, the kind that stretched. Well the next morning, I came back and he'd cut it. So I made him a cast, the next day I came back, and he had cut the cast. So I made him a huge cast. The next day when I came back the guy had gone. His family had taken him. You see, that was the sort of medicine that I practised.

Well I didn't participate in many battles. The one that I most participated in... What was it called ? I've forgotten the name. I'll remember it later. It was a battle in the south of... it was called Ksar Ghilane. Ksar Ghilane is a well in the south of Tunisia. So we went up. I was in Brazzaville. Afterwards I was appointed in Chad. Leclerc was in charge of all of that. And Leclerc undertook more and more deep and complex raids in the south of Italian Libya. And at the beginning, I didn't take part in the first one, in Kufra in particular, which was rather incredible, and where Leclerc, with 200 guys and 20 trucks, jailed I don't know how many thousands of Italians who surrendered with satisfaction at the first canon shot. So we resumed, but after the Italians, we ran into the Germans and then it became a completely different story. So that is what constituted my war... coming up from Fort Lamy, or perhaps a little higher, through the desert, to attack the Italian camps at Seba, Mourzouck and so on. And so the real achievement then, was to bring a set number of soldiers and canons in trucks, thousands of kilometres away from Brazzaville and Fort Lamy. All that in the Sahara, in the middle of the desert. That was an extraordinary achievement. More than the military

aspect. As for the soldiers, as long as we were dealing with the Italians, there wasn't any trouble. The day we ran into the Germans, that's when it became a little more difficult. And that was at the famous Ksar Ghilane where this time the battle lasted all day. In the morning, we were in holes buried in the sand. And on the one side there was, what is called the big erg, which is an huge desert of sand where it is impossible to walk or drive. And on the other side is the Matmata mountain. And one morning the Germans charged down on us and started shooting. They attacked us a first time around 7-8 in the morning. We beat them back. They started again around two in the afternoon. We beat them back. They started again around 5 or 6, and were breaking through when the Royal Air Force arrived and destroyed everything. Fortunately, because at that point, General Leclerc's Free French Forces were completely disappearing. We were lucky then. It was luck and the Royal Air Force.

You have a great admiration, it seems to me, for the Royal Air Force.

Well yes, of course and with good reason !

And also for the efficiency of the British army.

Yes, yes of course ! They were great. So what was very surprising, was that in the morning, the Germans arrived and bombed us first. They then left. At that point the Royal Air Force arrived and started firing at them. And it went on like that all day, first one and then the other. Until 5 in the evening when they arrived at the same time. And then, we witnessed an exceptional performance. There were approximately 30 planes or 40 planes on each side that were firing at each other from every direction. And in the end, the Germans left. I don't know how many planes were taken down. It was really beautiful. It was a magnificent show.

I had been sent, I don't remember why. Ah yes ! I was part of a battalion and it had sent, for some reason that I have forgotten, had sent a company, or at least a set amount of men quite ahead, close to the German lines. And at one point the Colonel thought : "but what if we have wounded men, they don't have anyone, I need to send somebody over there." So he sent me with two black male nurses. So we headed in that direction. The two black men

vanished very quickly. I ended up on my own, making my way in the night, which wasn't very pleasant, in the direction of the German line. And I ran into a captain, and I asked him : "Where are the guys over there ?" He told me : "Over there, you walk for 30 minutes, you turn slightly right and there they are." So everything was pitch black, and I can't see very well at night. Some people can see well in the dark, but not me. So my two guys vanish straight away, and I find myself at the back of beyond trying to reach the guys who were ahead. So then what's surprising is that, I ran into this guy who was just standing there, leaning on a rock, his submachine gun under his arm, and who was just staring at me. He followed me. I don't know why he didn't shoot me. I was alone, he was alone. There I was walking around with a box of bandages, him with his gun. He followed me like that. I pretended not to see him. I carried on. I guess he didn't want to attract attention because there was a lot of people around, and so in the end it wasn't necessary to shoot, he would have been spotted.

You don't think that there's also the fact of not wanting to shoot someone who is basically so close, that can be seen ?

We will never know that. But the fact is that he didn't shoot. So I arrived. And so what's funny, is that I got very scared when there was no reason to be scared. At one point, I ended up lying face down streaming from sweat and from fear. Even though there was nothing there. And when there were things like this, nothing. It's incomprehensible, but that's the way it is.

Algiers- I didn't really like it. It wasn't very pleasant. It was extremely crowded. The first FFF, it was something very pure. They were guys who were all... The people who were in London, there were very few, I don't remember how many, 4-5000 or 6000. They were guys who really wanted to do something and fight. From the moment, we came across the North African people again, who for the majority had no desire to fight, or who did terrible things. I had gone with one of my friends with whom I'd been to school. We left together. And when we were in Africa, he was more or less, he was Second Lieutenant. He headed a black platoon, and one day, one village stole another village's cows. So terrible drama, they started fighting and my friend was sent over with his platoon to separate them. One guy hid behind a baobab and stabbed him in the heart with an assegai, dead. He had a brother who was also killed, on the day of the American landing in Algiers, by a

member of Algiers 5th Regiment Chasseurs, who shot him in the back. The two brothers, not bad ! Why am I talking about this ?

Because you found their parents...

So, yes, afterwards I found their parents. Yes, they lived in Algiers where I stayed for two weeks. The thing is that I had had a choice to make. Following the campaigns, we didn't get any leaves for years, two or three years. So we were entitled to one leave and there was the option of going either to Algiers or to Lebanon. And I thought, for some reason or another, that we would probably be going to Lebanon later on, and come back through the South of France. So I thought : Lebanon, I'll see it later on, let's go to Algiers. Which was a miscalculation. I should have gone to Lebanon not Algiers.

I got back on a boat... on a LST, the landing boats where the tanks were placed. It must have been in March 1944.

And did you know it was D-day ?

Yes.

How was it for you psychologically ?

It was the end. As soon as we knew- until 1942-43, it wasn't clear at all. It really wasn't obvious that we would ever see France again. But from Stalingrad, it had been over.

That was... but from Stalingrad, it had been over. In your opinion, was Stalingrad the turning point of the war ?

Yes Stalingrad really was the turning point. There was Stalingrad, and the North African war.

El Alamein ?

Yes that's right. That was the turning point.

And then you knew that the landing meant that the end of the war

was near ?

Yes.

We weren't at D-day, we were elsewhere. D-day took place on the 6th of June. And we arrived in France on the 1st of August. And I was injured on the 8th of August. It happened very quickly. So it was, so we were... We reached the south of England in June, near Southampton. That's where we boarded the landing boats. It was also very picturesque. My company was on one of those small boats, and there was an absolutely exceptional commander. Who had a British sailor's face and a ginger beard. And we board the boat, it was dark, we leave, we hear the sound of two-three impacts, and the engine stops. We stopped moving. That was really a little worrying because it was the English Channel. So we wait. The next morning, we looked... It was a sort of boulevard that went form England to Normandy, there were boats going in one direction... Not a soul around, and eventually, the commander came out, this tall red-headed man. He took one of those cans of soups, those that you just heat on the fire and eat. And on that note he looked into the horizon and said- Over there, and he walked off. We were on our own in the Channel, which was very unpleasant, because there were lots of other people and planes around. But it all went well in the end.

It's when we were going south to bypass Avranches and cut off the Germans, they made a north south counter-attack, and I was injured in a plane bombing that accompanied that counter-attack. And I was knocked out, I was struck by 50 or 60 grenade fragments.

Can you remind us of the circumstances that accompanied that injury ?

Well, we were heading for the south, precisely to try and bypass the Germans who were there, and then go back up with Patton. There had been a German offensive trying to cut us off, and in particular a plane bombing. The planes arrived a first time, and I had a friend, a very good friend who was seriously injured. So I went to take care of him. That's when the planes came back. Everybody hid in holes, and he was unfit to be moved. So the planes arrived. He said to me "don't leave me". I didn't leave him and he was killed there and then, as for me, I was seriously injured. I didn't leave him. Yes, he died

next to me in the hospital.

And then starts a year-long period where you will be...

Oh yes, dreadful, dreadful. With constant operations, patches, this and that. A really dreadful time. Even once I came out of the hospital, I was operated again I don't know how many times, because I had shrapnel wounds that were weeping here and there. It took me a long time. My elbow had burst out. My thigh was broken, the entire right side was filled with fragments. And it took me a long time to recover. It was very long and painful.

So I came back to Paris. And the atmosphere that I found there wasn't very pleasant. All my friends who had stayed in Paris, who had gone on with the extern exam, the intern exam, who were all more or less doctors, came to see me with heartless looks, saying "Ah yes, here comes the glorious soldier, very good, continue!" It was very very unpleasant.

With nonetheless a certain admiration wouldn't you say ?

I'm not really sure. No, I had friends who, were my competitors if you like, during the first years of medicine, for the exams which, I didn't take anyway because my first exam had been cancelled. They had already become very important players in the world medicine. Which annoyed me tremendously. But they were full of commiseration, but that's all.

You say that you tried to get accelerated exams, or at least have the right to present yourself earlier to the exams, which you were denied.

Yes. That's something which... because I wasn't an extern student at a teaching hospital. And you know that in the French system, you need to be an extern to become an intern, etc. So, I was 4 or 5 years behind these guys, I went to the office of the "Assitance Publique". And I asked them for the right to present myself, to be an intern without being an extern. And that my tests get marked and that's it. They almost fell to the ground. To present yourself to be an intern without being and extern, that was unthinkable. And that completely disgusted me, I decided not to continue in that direction and do something else.

That you would never practice medicine- As a doctor- You did it to get the diploma, yesThat I would never PRACTICE medicine.

I still got a doctorate in Medicine, but without practical medicine, yes.

That was also very special. I had a little cousin who was very pretty. And so, I arrived in Paris. I was evacuated to Paris afterwards. When I was first injured, I was sent to the American hospital, 101st Field Hospital. Then, from there, I was sent to Cherbourg where I stayed for quite a while. And when Paris was freed, I was sent to Paris. And then I travelled by train for three days, with peanut butter sandwiches as food. It was very distressing. So I got there. And as I was saying, I had a very pretty little cousin, who had arrived. She lived in Lyon but had sought refuge in Paris. So she was looking for me, and didn't find me, but I quickly found out from the others who told me- there's a girl, she says she's your cousin, but it's probably not true, she's too pretty for that ! And eventually, that's how I was reunited with my family.

De Gaulle had created a medal for the war battles. At first, in particular he didn't want to give a "Légion d'honneur", from when we were still in England, until he took over the entire government. He didn't want to give "Légions d'honneurs". He created a medal for the people of Free France, which was called the "croix de la libération". There were 1020 or 1030, I think. I think that there were 1030. Now there are 120, the rest are gone.

Is that what constituted the order of the "compagnons de la libération" ?

Yes, that's right.

And is it something that was very important for you ?

It was THE decoration of Free France. There was another one afterwards that was called the "médaille de la résistance', but which wasn't really as chic.

And was this the start of four difficult years until your admission

at the Pasteur Institute ?

Yes, very difficult. Very difficult. I really didn't know what to do. I tried a variety of little things. I had no idea what to do. Because surgery was what I really wanted to do. But with my arm, surgery was out of the question. And so I really did some things... a little of everything. I did all sorts of things.

There was in particular, the work that you did to try and produce penicillin, or equivalents of penicillin in France. That was at the end.

That was at the end. Yes, penicillin wasn't produced in France. There was this guy, a division captain who I knew, who had been demobilised and who decided to produce penicillin. And the only way he had found to make some, was to retrieve it from the urine of the patients being treated at the hospital. Which wasn't exactly brilliant. But there was another antibiotic called tyrothricinum, which had been invented by Dubos, and which was a localised antibiotic, which couldn't be used in injections but could in localised treatments. And I wrote my thesis on that. I stayed for a while, but then I left because it wasn't working very well.

L'antibiotique marchait pas mal, non ?

The antibiotic itself was working pretty well. But external antibiotics are very limited. We can't do much about it.

And why wasn't it working better, this French Penicillin Centre, as it was sometimes called, the Cabanel Centre ?

The Cabanel Centre... because it was something unusual. It was the gunpowder engineers, Polytechniciens, who had decided at the end of the war to convert the powder factories into antibiotic factories. Just because there were vats, big vats where you could make powder or whatever chemical treatment, they decided that, because penicillin was made in vatsbut the vats needed to be sterile and no-one was able to make these vats sterile. So it was impossible to manufacture an ounce of penicillin in those vats. For a while I was even, there was a powder factory in Morcenx - Morcenx in the south-west of France. And I was appointed manager of the Morcenx factory. And I went

once a week, I would take the train in the evening, arrive the next morning in Morcenx, and I would leave in the evening and return to Paris. It was pointless. It was impossible to do anything in Morcenx.

Well, it so happened that I really didn't have a clue what to do. When the war was over, I tried a series of things.

I think that your cousin by marriage, Herbert Markovich, played an important role.

Yes. He was in the same sort of situation as me. He hadn't been in the war for as long, but he had still seen some war. He had tried to do some research and he had ended up at Ephrussi's laboratory where he was doing some interesting things. As for me, for a while I really tried a few things. I almost presented myself... following a friend called Pauphilet, who was the son of the Normale's Principal, whom I'd met during the war, who told me - you know, there's a new school of administration that's just been created. There's special examination for those who were in the war. I'm going to take it you should come as well. So all right, good idea. I bought a law book, it fell from my hands, and after two days, I said... I'm over and done with, with the administration school. So I did a series of things like that. And then eventually... so yes there was Markovich with whom I had dinner. His wife was my wife's cousin, so we had dinner together once in a while. He told me that he had gone to see Boris. He had done more or less about the same thing as me. He didn't know what to do more than I did, he was at the same stage of incompetence. He was working for Boris Ephrussi, where he was doing some things that really interested him. So I tried to get some tips. There were two guys who were Normale graduates, one was Lwoff's student, the other, who I knew, was Ephrussi's student. I went to interview them. Because of the things I had read I had nonetheless, by reading small things here and there, I had reached the conclusion, that between bacterium, nucleic acid and genetics, something was more than likely to happen. So it was in that direction that we needed to look. It's the only thing that came to my me at the time. And so, I talked with those two guys, through which it appeared that there were two laboratories that could take care of these sorts of things, which were Ephrussi and Lwoff. And, the two guys added, Lwoff's laboratory is much more interesting than Ephrussi's laboratory.

So there were two laboratories in Paris where one could hope to do that sort of thing. So there was Boris Ephrussi... And Boris Ephrussi was a rather difficult guy. For example, there was a guy that I knew, that I knew pretty well, who was very nice. And one day, he was doing an experiment, on a Saturday afternoon. Boris Ephrussi gets there and asks him "What are you doing ? - I'm doing an experiment". And Ephrussi says "But you didn't tell me about this experiment ? - No Sir, but I thought it would be interesting to."... Ephrussi takes the test tubes and empties them down the sink. So it wasn't very encouraging ! On the other hand, everything that I was hearing from the Lwoff-Monod group was very tempting. And that's when I ended up at Lwoff's laboratory and, innocently, I told him "I'm not good, I haven't done anything, but I would really like to work for you". He told me "You seem very nice, I really like you, but I don't have any openings". And for nine or 10 months, I came back every month, until May or June, asking if there were any openings. And the last month he told me "You know, we've found the phages induction". So I put as much admiration as I could in my answer and said "no, really ? I didn't know what it meant". I left and went into the first available bookshop, to try to find out in a dictionary what induction and phage meant.

I'm guessing that there wasn't anything there ?

There was something on induction but not on phage. And so eventually, I started to understand what it was all about. And so on the 1st of September 1950 or 51, I joined Lwoff's team. And I was very lucky then. Because he was incredibly kind. He sort of treated me like a son. He didn't have a son. He was extremely kind to me.

When you defended your thesis, he basically didn't even think that he needed to show that he was your supervisor.

He was in America. He had gone for a year with his wife to learn the culture of cells with Dulbecco. He wasn't going to come back for that, and I wasn't going to wait for him to come back. So he wasn't there.

Many supervisors would have wanted you to wait for their return.

He wasn't like that, he really wasn't. He was amazingly generous. Because he always pushed me, and even when I started making progress and moving for-

ward, he continued to push me without second thoughts. That's astonishing!

André Lwoff was particularly generous, at least with me. André Lwoff really was a headstrong guy. Meaning that he had his friends and his enemies. If you were his enemy, you needed to be very careful because he would make nasty remarks which cost him a lot in his life. He said to some very important people... At one point, he wanted to be a lecturer at the College de France, and there was this guy called Courier, who was both lecturer at the College de France, secretary whatever at the Academy and I don't know what else. He was everything. And Lwoff didn't like him. And not only did he not like him, but he let him know that he didn't. And people don't really like to be told that. Which means that with his straightforward manner of telling people "you're all idiots", people don't really like that, and he ended up a little stuck. He never became lecturer at the College specifically because of that. Which means that when they came to get me, to go to the College, I thought "I have to go see André and see what he thinks". And he was really generous then. I said, "Well, sir, I know that you have had problems" - he had applied twice and been rejected twice, even though he was a much superior man to the one they took. "I know that you have had some small problems with the College. I have been offered a chair at the College. I won't go if you don't approve". He said "Of course, go ahead etc." But it wasn't easy.

And you say that with you he has shown great generosity ?

Very, very generous. Really. He always pushed me and always helped me. Incredible.

And that you were sort of a son to him ?

A little, I think.

We get the impression that it was someone that didn't accept bad quality, whether it was of thought or of behaviour ? Or of speech.

He was very attached to language. Or of speech. He was very attached to language. Yes, that's right. But he could be unbearable. Because he really did tell quite a few people what he thought of them. And in particular, a guy whose name I won't say, because there really is no need to mention it, but

who is an appalling idiot. And he said to him "It's an absolutely incredible story". They were in a CNRS commission, and the guy in question... they are examining his lab so he leaves. André Lwoff says what he thinks of it. And on that note the guy comes back in. And straight away, his neighbour tells him what was said. And he says to Lwoff "Sir, I ought to slap you. - Very well, Sir, you will have my witnesses in the morning". And the guy disappeared.

Everything happened in the corridor. Meaning that every single thing... every time a guy had an idea, he came out with it in the corridor, it would be taken apart, discussed once again etc. And it was at a time when with phage and bacteria, we would have an idea in the morning, at two o'clock we would do the experiment, the next morning we had the results, therefore we could discuss the results once again etc. It was exceptional. All day long we would talk in the corridor, with Monod, Wollman, the Americans- because there were very few French students. Lwoff and Monod weren't famous. It was before Monod started at the Faculty. People didn't know who they were. And I was the youngest student there and I was already over 30.

And this atmosphere mainly came from André Lwoff, from his personality ?

From everyone. Monod was also very close to these guys, and would talk to everyone. It really was a very exceptional atmosphere. Yes, mainly because of Lwoff.

And the fact that there were so many Americans that were coming, was it linked to André Lwoff's prestige, to his international recognition ?

Yes, it's the fact that they were... In 1946, Lwoff and Monod went to Cold Spring Harbor. And there, they met all the guys that were more or less working on the same thing. And they started inviting people. And from that point onwards, there was a continuous flow of Americans who came to work with us.

Which means that you felt part of that international community.

Yes, exactly. What was extraordinary, was that there weren't any French

students. Even though those were worldwide recognised laboratories. Monod became a biochemistry lecturer at the faculty. He replaced, what was his name ? the old lecturer from here who was both pastor and... I've forgotten his name, it doesn't matter. So when Monod became lecturer at the chemistry faculty, he started getting students. And Lwoff got a microbiology chair. Because until very late, there was no microbiology chair in France. That was unbelievable. Even at the Faculty of Medicine it happened very late. There was a microbiology chair that Lwoff got, I don't remember what year, but very late.

If we could slightly get back to the works that could have influenced you, precisely before or just after you started at the Pasteur Institute. You mentioned Erwin Schrödinger, *"What is life ?"*

Yes. It was an important book because it was a completely different way of viewing the world, from the way of the little medicine student that I was until then. It really was.. he was a physicist and it was completely different. For me it was a completely different world from the one I had imagined. So there was him and there was Brachet, because Brachet... that was nucleic acids. And nucleic acids were beginning to play an important part. And there was a third one, you say it was Huxley ? Yes.

On Darwinism, the theory of evolution.

Yes.

And among the influences that directed you towards biology, there was the Lysenko affair.

Yes, because the Lysenko affair, that was incredible. It was utterly unbelievable. The guy who... The guy who dismissed all of biology, on the basis that it was incompatible with Marxism-Leninism, it was quite astonishing. That annoyed me so much that I think it was one of the factors that lead me towards biology. Towards genetics in particular, yes. Because for Lysenko, genetics didn't exist. That was extraordinary.

And that really struck you. It really mattered at the time ?

Yes. It mattered quite a lot. There were fervent discussions in the papers. There were guys like Prenant. Prenant was a very good biologist. He was a lecturer at the Faculty of Science. But at the same time he was a member of the Communist party. So he was torn. There was him, and there was also a woman called... who was a pharmacology lecturer at the Medical Faculty, who was also a member of the Communist party. And they were all bringing genetics down, and Lysenko was all they talked about.

And what about Aragon, with whom you are a little harsh ?

Well, Aragon was also exasperating. Aragon was a guy with incredible talent, but who also came up with unbelievable things. Unbelievable ! He was a doctor, so he had a hazy idea of things.

Lwoff had worked on the baci megaterium. And so he thoughthis whole idea of lysogeny, was based on megaterium. And in particular, he had done a series of very delicate experiences where he would take a bacterium, measure it out it drops, and once in a while, a bacterium would disappear and phages would appear. And there were no phages until the bacteria had disappeared. So, he had concluded, only through micro-manipulations, that for bacterium to give- because in a culture, you find approximately one particle of phage per lysogenic bacteria. So there were various hypotheses. Either during each division the bacterium excreted a particle of phage. Or either once in a while, just like after an infection, the bacterium would think- let's release a hundred particles. What he showed through this, was that the second hypothesis was the right one. So he was working on that, and when I arrived, he said "We need to check if that's the way it is, not only in megaterium but also in another organism". He told me "why not try in pyocyanic". I went to the Pasteur Institute's microbe bank, and I picked up 35 pyocyanic strains. And so, I religiously put a drop of each on each other's plate to see if there were phages, if it was lysogenic. There were some. I took out lysogenic lines, and I tried to reproduce with pyocyanic, what he had done with megaterium. That's when Elie Wollman came back from the United States where he had spent two years with Delbrück. And he came back from the United States with Escherichia Coli K12, which Lederberg had shown him could recombine itself, and which Esther Lederberg had shown was lysogenic for a phage called lambda. So we decided that was what we were going to work on, because it was a very favourable material. And we started working together on lambda

and K12.

And because genetics could be done. Tools were available to do a genetic approach.

We could research the genetics of bacteria, the genetics of lysogeny and the genetics of the phage.

Gunther Stent had invited him to go back to the United States for another year. Elie and I did a lot of things at the time. In particular, we did all of the... when studying lysogeny and its genetics with K12, we mainly did the K12 system, the study of... we dissected the mechanism and the injection etc. That's right. Where we were able to map out with time, etc. With that, he returned to Stent's laboratory and nonetheless we had equipment... That K12, with its injection system, was nevertheless exceptional to analyse cellular functions. So, with Monod we decided "we're going to look at how lactose works". There was a specific lactose region, we could take receiving bacteria with the deletion of the lactose region, inject lactose, see how the Z(+) gene, the three genes and I(+) manifest themselves. So we started dissecting all of that system. And then... Elie had vanished. And we did the majority of our work in his absence, he never got over it. Never. He got the impression that he'd missed the right moment. Yes. All the more so since that's what we got the Nobel Prize for and he didn't really like that.

And you had kept fond memories of your collaboration with Elie Wollman ?

We kept them until the day he left. And when he came back... At that point I started working with Monod, we did a lot of things. When Wollman came back, I invited him to come back and work with us. Never ! That was typical. It was something we had started without him, he wasn't going to jump back onto the wagon.

What really got us excited for a while, which is quite funny now, was to know whether it was inserted into the chromosome, or attached to it. And attached to the chromosome... when you think about it, there is no attachment system on the chromosome. I don't know how we could attach it on tyrosine. There is no tyrosine in the chromosome. It isn't obvious how we can... so, eventually,

as soon as the structure of the DNA became clear, which was more or less around the same time, it became obvious that it needed to be inserted. All that really troubled us.

Basically, it's in fact at that time, we didn't have much data on the chromosome. So an attachment model really wasn't unreasonable.

And we didn't have any DNA, well very little that is.

Yes, we favoured the attachment, but very quickly, it slipped through our fingers.

Yes, but it isn't much in relation to the results that you obtained.

Yes, but it was very important to see how... because the thing that... what really troubled us at the time, was that there were pieces of chromosome that we could add or cut off. And when we would say that, Ephrussi would say, throwing his arms up saying "A chromosome cannot be manipulated". And in fact, that was the first thing we started to play with. Up until Campbell showed that it was circular, and that the two circles... that we inserted one circle into another one in a linear way. In a very simple way.

And that aspect, precisely the idea that genetical material couldn't be manipulated, that the chromosome couldn't be manipulated, that it was something that was beyond... it's something that you permanently fought against, because with Monod... you also had to convince him a little. At first, Monod, didn't like it at all. That's true. He was very quickly...

He quickly got into it, but at the beginning he didn't really warm up to the idea of adding to and taking from the chromosome. But he quickly got into it. As soon as he understood it, he was very quick.

On lysogeny, wasn't it also maybe a model for understanding phenomena like cancer ?

Yes, a little. Yes, it was also mainly presented by Lwoff and a little by me as a way of understanding cancer. Actually, it didn't really teach us much. The

cancer virus probably doesn't work like that. They insert themselves next to a gene that they either activate or inactivate, more than anything else.

Nevertheless, at one point you had made the hypothesis that precisely, this sort of activation phenomenon and gene inhibition through the insertion of the phage could be linked to what was happening in cancer.

Yes, but it was quite easy, until the moment.

Yes, it was one way of seeing it.

Well, we really liked it because like the cancer model, it brought a lot of interest to this little phenomenon of lysogeny.

And isn't it in those years that André Lwoff is going to slightly abandon his work on lysogeny ?

Yes, he decided that he'd had enough of bacteria, he wanted to start working on real viruses. He and his wife went to the United States for one year, and they went to a series of laboratories that were working on mammal cells and on mammal viruses. He went to see Dulbecco, and three-four other guys like that, where he learned and came back with the techniques. And so he started cell cultures and things with viruses, real viruses.

But it's an important moment where we were sort about to understand what lysogeny was, what it consisted of. So, eventually he now considered this to be your work ?

Yes, afterwards. He took the viruses and left us the...
It was an incredible model. The key was to know to what extent lysogeny was or wasn't a real model.
Eventually, it was a rather good model, but unfortunately it wasn't as simple as lysogeny.

While studying lysogeny, what we wanted to know, was the nature of what we call prophage. We considered that the lysogenic bacteria kept, in their chromosomes, the phage's genetic information in the form of what is called

prophage. And the question was : How is this prophage settled in the chromosome ? And it appeared that it was inserted in a linear way. Why am I talking about this now ?

Precisely because conjugation played a key role in positioning it on the chromosome.

Yes, that's right. On conjugation, we tried to...So, we wanted to see if the prophage acted like a piece of bacterial chromosome, or like a group of bacterial genes. So we crossed them. When we crossed the two lysogenics together, which differed through markers on prophages, everything went well. After combination, the male chromosome ended up in the female, it went very well. But when we crossed a lysogenic male with a non lysogenic female, it wasn't working at all. And we noticed that when the prophage entered a non lysogenic cytoplasm, it started up and began developing. And that was the first argument to say that, in the cytoplasm of lysogenic bacteria there existed something which was probably negative and that prevented the development of the prophage, and that we called repressor. We were working on two things at a time. We were working on lysogeny and prophage on the one hand, and on the other, on Monod's lactose system. And eventually, little by little, it appeared that the two systems looked strangely alike, and that in both cases, independently there were structural genes that determined the formation of specific protein molecules, cellactose molecules or phage proteins. That it was the same mechanism in both. In the cytoplasm there was a substance that blocked gene expression, of either phage, or lactose.

But the work on conjugation, was to clarify both prophage and its localisation

but it was reciprocal, meaning that the two worked together. In fact, we set off to try to understand prophage, but very quickly, it enabled us to study the exact mechanism of genetic recombination, of conjugation and of recombination.

And can you tell us about the principle of the experiments you did at the time, about uninterrupted conjugation ?

Yes, so that was... What we noticed was that... Let me just remember how

it happened...

I know that William Hayes had found strains, it was quite important, wasn't it ?

Yes, that's right. In the Colon Bacillus system, there were males and females, there were donors and receivers. The majority were low frequency donors, meaning that there were recombination with ratios of 10^{-6}, but there were particular donor strains that had been isolated, on the one side, The Italian who worked on ?

Cavalli Sforza ?

Cavalli Sforza, and on the other side by Hayes, who injected markers at much higher frequencies. So we tried to analyse it and Elie had the idea of of separating the happy couples during conjugation by placing them in a waring blender. It was an experiment that Hershey and Chase had done with phage. By marking proteins with S35, or nucleic acid with phosphor, they tried to see what did and what didn't go in the phage particle. And they showed that it was the nucleic acid that went in and that the protein came out. So, we tried to do that with conjugation, and we noticed that... let me see...

From an outside perspective, I imagine that you saw that specific markers were going in at different times.

Yes, that's right. The aim was to separate them to see when the chromosome went in. We would mark it either with sulphur or with phosphor, and the idea was that conjugation should occur. And at some point, the male chromosome is going to end up in the female. And when we had done that, and we had separated them at different times, as it happens we separated some at 10 minutes and others at 30 minutes, and the results were different. And we noticed that if we separated them in a abrupt manner at different times, we would see that the chromosome went in through one end. It was the so-called spaghetti experiment, which infuriated Wollman.

The chromosome being a long spaghetti that enters the bacterium.And afterwards, you found that depending on the bacterial strains, the same yet slightly shifted order could be found.

That's right. It has a name. To permutate, yes. In other words, the only way of explaining it, is to put it all in a circle that would be open at different ends, the opening being the front and the back.

Which at the time was revolutionary, wasn't it ?

Yes, the circle was revolutionary. I remember the first time I told Elie that the chromosome was circular, he was beside himself. I don't know why but he was furious. Then he accepted it. The idea of a circle wasn't obvious. It was already clear for the phage. We knew that phage chromosomes were circles. But, it was quite a nice explanation, a circle that you open at different points and that goes in, it wasn't bad. It held out.

In Lwoff's laboratory, there was an important person, and that was Monod. Lwoff was working on lysogenic bacteria, and Monod was working on the system of the use of lactose by Escherichia Coli. At the beginning, in the fifties, everything was set up on the other side in the attic of the old building. There was a long corridor, with at one end Lwoff and his group, and at the other end Monod and his group. Lwoff worked on lysogeny, on lysogenic bacteria and Monod on the system of use of lactose by bacteria. It was a comfortable place to talk. But it really wasn't a comfortable place to work. Meaning that they were old labs. There were too many of us, but that was really good because everyone would meet in the corridor to talk. As soon as we had an idea, we would rush to the corridor to talk with the others, who would quickly try to destroy it. It was extremely lively, active and particularly stimulating.

Meals were eaten together, weren't they ?

Meals were eaten together. For a long time they were in my lab. You weren't there then, you were too young. I had a lab at the end of the corridor with a very large table. And because of that table, everyone had their lunch there which infuriated me, because I liked to start my experiments very early after lunch, and the others hung around over coffee, and it was very difficult to get them out. But we all ate together and talked a lot. I have a very pleasant recollection of that time and of that group, in particular because of Lwoff and probably also because of Monod. In general, we talked things over in the morning. At best, we had an idea that we would carry through in an

experiment that afternoon. And when all went well, we had the result the next morning, and we would start all over again.

Those were also the advantages of genetics.

It was above all the advantages of bacteria which go very quickly, and of genetics in particular. But the system went very quickly. In the morning, we would discuss the results we had obtained the day before. We prepared the experiment which was carried out at two o'clock, at 9 o'clock the next day the results were in. At 10, we would discuss it again and put the next experiment together. It was an absolutely unbelievable race.

And was André Lwoff open to discussion ?

He was very open to discussion. He, himself, didn't speak much, but yes he was very open to discussion.

Could you go ask for his assistance when you were faced with a problem ?

Yes. He was very generous. He was very nice and very generous. In fact, he was a guy who... He had people he supported and people he was against. If you got on with him he would do anything for you. If you didn't get on with him, you were sure to get torn to pieces and to have the worst problems. He was an enthusiast, he was very warm.

He wasn't even ironic if you ever came up with a bad idea ?

No, no. He was a little like everybody else, but no more so. Monod was much more ironic. Monod was harsher.

And after that, did the tradition of common discussions continue even when Monod went downstairs, yes.

Until then everything happened in the attic of the old building. When Monod was appointed chief of the biochemistry department, he got the large department downstairs, on the ground floor. And then, the lunchtime discussions changed and moved to the ground floor. But the system more or

less continued. The two groups saw a lot of each other. I practically worked in the lift. I was doing the experiment with Monod, but my lab was on the second floor so I spent my time going up and down. Yes, people talked a lot.

And about everything ? Science, politics ?

Absolutely everything. Yes, about science, politics. On the whole, people more or less agreed. They were slightly left-wing, not too much, definitely left-wing even and there were no political battles. There were discussions like there are discussions between people of pleasant company, but without fights. But they were still firm discussions. We also talked a lot about the personalities of the scientific world. And then we were harshing.

Well the Americans were there for a year. They brought in fresh ideas. They also brought the links with the American laboratories. At the time we often went to the United States. As a matter of fact, I think that after the war Lwoff and Monod were the first ones... the first ones to rush to the United States. I think they had both received Rockefeller grants. So there were links. They also got money for the lab from Rockefeller. I also got some straight away, as soon as I started to emerge and write papers. So it worked out very well.

And thankfully, because you weren't getting much help in France ?

Not much, the Pasteur Institute didn't get any, and from the extra-Pasteur Institute, from the ministries we got very little. So, we had a little American money which was valuable. Which was especially valuable because we bought a lot of things in the United States. There was an extraordinary person, you knew her, Sarah. Sarah Rapkine who was the widow of a researcher called Rapkine, who was a friend of Lwoff and Monod. She was the one in charge of buying things in the United States. So we talked to her about the things we needed to buy, about money problems.

Yes, the biggest joke was to use the same word to qualify things that we considered to be completely different. And it was only little by little that, by analysing each of the systems, we noticed that there were strange similarities between the two systems, and that eventually, it lead to a shared experimental model. And that model, was the so-called Operon model, namely that

there were structural genes that ruled the synthesis of one or several proteins, and that there were regulator genes that made... we haven't already described this ?...

No, no

...which made a product. It took us a long time to understand what it did. That it made a cytoplasmic product, that acted to regulate the other's activity. And we hesitated. At first we thought that it was a nucleic acid, because at the time, no one had talked about the protein affinity yet, that there were proteins that had affinities with nucleic acids. Then very quickly, we were lead to believe that it was a protein, because there were regulator mutants, thus of the regulatory system, which were sensitive to... What are they called again ? That replace the... Suppressor, exactly. Suppressor genes, which we knew were replacing an amino acid by another, or which placed an amino acid where there was a hole.

And the first experiment that you did together with Monod,and which was sort of at the origin of the Operon, was that famous experiment called PA JA MO- PA JA MA - PY-JA-MA ?

Yes that's right, with Arthur Pardee. Arthur Pardee was doing an internship, well a post-doc, not post-doc, on sabbatical that's it.

He came over on sabbatical to Monod's laboratory for a year. With Monod we had reached... after having worked together for several months, we had reached the conclusion that we needed to use the conjugation system where we... Have we talked about conjugation yet ? Well we need to talk about it.

Well you talked a little about conjugation but not really why it might have been a tool.

Conjugation had an astonishing virtue, being that there was the injection of the donor chromosome inside the receiver, and we could, by separating the happy couples... We would cut the chromosome and were able to find-Wollman and we noticed that the chromosome went in through one end and progressed linearly at more or less constant speed. So, on the one hand, we could make a genetic map, not only through cross breeding and through ge-

netic distance, but also through the injection time of certain markers with a specific pair of bacteria. So, the lactose of which Monod had located the genes, there were several genes... There was an enzyme that split lactose into glucose and galactose, which is called galactosidase. And there was a permease. There was another protein whose use no one knew which was called acetylase. All of it was determined by three adjacent genes on the chromosome and regulation was practised by a close but distinct gene, called I for Inductible. And so the question was... with Wild-type Coli, the synthesis of these proteins, galactosidase and so on, only happens in the presence of lactose or galactoside. They are said to be inductors. But there are mutants, which make these proteins, even in the absence of inductors, and that are said to be constituents. So the first thing was to know... the first idea was... until then Monod's idea had been that the constituents were making an internal inductor. We studied the question and by cross-breeding, we noticed that what was dominating, wasn't the constituent called I-, but the inductible called I+. With the transitory diploid I+/I-, the system was repressed. Which meant that the dominant was I+ and that the gene was making what we called a repressor.

Who had the idea for this experiment, to use conjugation to... ?

With Monod, we decided it as soon as we saw thatthe analysis of the conjugation was done with Wollman. But it became clear when we analysed the conjugation, that we could show that there was a chromosome that went in at constant speed starting at one end, and that markers could be located according to the time of entry, it became an exceptional system for the analysis of cellular functions. We placed the lactose-galactosidase system in the conjugation mechanism and we tried to analyse it like that.

And did Monod accept the idea of the repressor quickly ?

He hesitated a little, but he was curious. He still didn't like to abandon his ideas to adopt those of others, he really didn't like that. But at the same time, once he had accepted that it was necessary, he became an ardent believer in the new model. Meaning that at the beginning, he wasn't very interested, but very quickly he became very much in favour of it.

For a month we thought that it had to be a nucleic acid because, foolishly,

we thought that chromosome paring was a very good system. But it quickly became clear that there was a protein, because there were protein suppressors.

And the very idea that the repressor acted directly on the DNA, you said that it was when you were preparing a conference.

That was one of my set ideas. It's while preparing a conference I had to give at Harvey.

I was very proud of it and searched everywhere for someone to discuss it with. Monod was on holiday, Lwoff was on holiday, Wollman was in America. My wife was the only one there. I explained it to my wife who said "Well, I thought everyone knew that"... Which I found incredibly exasperating. Then I get back from holiday, everybody gets back from holiday and I tell my idea to Monod who, at first, was very much against it, very very much against it. And for reasons that I have now completely forgotten. But there were at least 5 very strong arguments against it. And little by little, one by one, we took down the arguments.

And do you know why it was while you were preparing that conference that you got that intuition ? Was it because you were basically forced to make a synthesis of your works ?

Yes, it's mainly due to that. It was while preparing that conference. And then I reached the conclusion that the reasoning was the followingfirst of all, the two systems were similar, the phage, prophage and lysogeny system and the lactose system. In both cases, there were structural proteins that were made and a regulatory system that did something. So very rapidly, it appeared that this something could hardly be what we call positive. Meaning that the I gene produced an inductor, but that didn't last very long. So, it made a repressor. Once again, what was very surprising, was that the two systems were different, they didn't allow the same type of experiment, but the substance was basically the same. And that we could do certain experiments with one of the systems, for instance phage genetic is a lot easier to do than enzyme genetics. But on the other hand, to measure enzyme synthesis and protein synthesis, when for instance we release the repression, it's much easier to do in the lactose. So it was complementary and we could go from

one to the other. It was very useful.

And is that when, for two or three years, a sort of ping-pong game between the two systems started ?

Yes, that's right.

When you made the hypothesis, you were eventually going to look for the mutants in one of the systems.

Yes. And when we found a mutant in one system, we could predict that the same one would appear transposed in the language of the other system, and we would find it. For instance, the constituents were the same. I once found in phage a mutant that wasn't inducible. Phage induction, of prophage, meaning in lysogenic bacteria, there are phage genes that are inserted in the bacterial chromosome, there was a cluster of 20 or 30 genes. And those genes are not active. And there is a repressor, which is synthesised by one of the phage genes which blocks the synthesis of all the other ones. So the two systems are very close. But the reasoning had been that for all phage genes, meaning the 25 or 30 structural genes, to be repressed by that very repressor gene, there needs to be a system... there needs to be a joint regulation system somewhere, of the 20 genes. So it can either be on the messenger or on the DNA. And for reasons I can no longer remember, there were many more arguments for it being on the DNA than on the messenger. I really liked that because for quite a while, I was convinced that the repressors worked on the level of the DNA. At that point, I had long discussions with Monod, because he didn't like it. He didn't like it because he had received a traditional genetic education. And traditional geneticists didn't like things to settle in a chromosome, or activate or deactivate it.

Yes, working with Monod really was incredible. Because we saw each other everyday, at least two hours a day in front of the blackboard, talking nonstop. And we discussed things at full speed, because we were used to all the little details of genes, of mutants, etc., it was very difficult for someone else to keep up. I remember Georges Cohen for instance, who wasn't very far and was interested in all that. Once in a while he would come over, and he would have great difficulty keeping up because we spoke too fast and without spelling things out.

Nevertheless, it was the tryptophan system which was still bringing forward arguments in favour of the same model ?

Yes. There were three arguments in favour : phage, lactose and tryptophan.

Et le tryptophane étudié par George Cohen.

And myself. Because I was the one who had the idea of telling Georges Cohen "you need to look for the regulatory gene and see how it works".

We had planned two types of mutations, or more precisely, we had two types of mutations that affected the three genes. There were zero-mutations and also constitutive mutations. And we believed that the zero-mutations were also in the operator. Which wasn't really true. In the end, it was a little more complicated than that because there was a... what is it called ? An effect of polarity, exactly, and that the mutations, at the beginning of the gene, on the N-terminal side, could in certain cases lead to a standstill of the synthesis in the entire system. What we had thought were I zero/0, inducers zero/0, actually were not. They were polarity mutants. There was the I- and the IS. The IS were extraordinarily... The IS were the super-repressed. And the IS have been extraordinarily useful because it was a gene, it was a dominant mutant. And it isn't easy to explain what dominant mutants are, unless you have polymers and you make a MES of the final structure. And everything with the IS was very interesting.

Specifically to show that it was a protein with many sub-units, the repressor and an allosteric protein.

It was the only way to explain it. And in fact, it happened to be once again exactly the same thing in phage. Because there are non inducible phages which are the exact same thing. Meaning that the inducible phage, is a phage whereas the repressor is crushed by induction, by more or less complicated things. And there were non inducible mutants who made a repressor, but the repressor wasn't affected by the inducer. It was a polymeric structure that no longer functioned normally.

With Monod we had reached, this is what I was mentioning earlier, the idea

that in phage the repression happened on 30 genes. So it either happened on the messengers... It needed to happen on a structure where the genes were combined and joined. It was either the messenger, or the DNA.

But the messenger itself hadn't been seen by anyone. At the time there were ideas that were.

Yes, that's true, I shouldn't be talking about the messenger at this point. It was either an intermediate... actually it was either an intermediate where all of the group's genes were represented, or it was the DNA itself.

Following that, Sydney and I started working together.

How did it happen ?

If I remember correctly, it's because you remembered Volkin and Astrachan's results showing that there were short-lived RNA molecules.

Yes, well I had completely forgotten Volkin and Astrachan, and so had Sydney. So we... I don't quite remember how it happened. But during a discussion at Sydney's, he had a room in his College. He was like any Englishman, they all have something in a College. He was in a very posh College, I don't remember which one. So he had a room there. And one day, we were having a discussion with Francis, who had arrived, where precisely I told them about the experiments on in-conductance, that we had done in Paris, which lead to the idea that there necessarily needed to be an intermediate between the gene and the protein, on which everything was represented. And it's following that... So we talked about it all and that's when they both leapt up, saying "It's Volkin and Astrachan.". I had completely forgotten about Volkin and Astrachan. I knew they existed but I had completely forgotten about them. They were the ones who had made the link. And so from then on, Sydney and I decided to go to Pasadena to do that experiment.

And it almost didn't work ?

We had exactly 30 days. Because he had engagements. Max Delbrück had invited him, No, in fact, Max Delbrück had invited me and Meselson had

invited him, that's it. And so we found ourselves over there. We decided that was what we were going to do, and we had exactly one month. We had to leave on a specific day. And for 20 days, it absolutely didn't work. Meaning that... the principle consisted in marking old proteins with sulphur and the new RNA with phosphor. And what we needed to show was that the new phosphor joined the old ribosome proteins. It absolutely didn't work for two or three weeks, until one day when we were hanging around on the beach, Sydney suddenly leapt up and said "It's the magnesium, we didn't put enough magnesium !". Because what we were looking at was inside cesium radians. And so obviously, the cesium was completed with the magnesium's divalent things, so much more magnesium was needed. We ran back to the lab, poured in tons of magnesium, and it worked.

And it was an experiment that showed that the ribosomes could basically join any messenger RNA.

That's right.

And at the same time, François Gros of the Jim Watson lab was showing the existence of the short-lived RNA.

Well, he went about it in a different way. He showed that... Volkin and Astrachan were working with phage. And they were trying to find, and they found, I think François was the one who found this, that when you briefly marked the RNA with growing bacteria, there was a sort of RNA that was very quickly marked, and that had the same composition as a basic DNA and which wasn't the same thing as ribosome. So it's through two different ways that we reached the same conclusion.

It was incredible. I know that, in Pasadena... I got there, I did three seminars. I did a seminar at Gunther Stent's who was at Berkeley. Afterwards I did a seminar at... I don't remember where, and then one at Max Delbrück's. And when I told Max Delbrück's about the hypothesis of a thing that at the time we called x, he threw his arms in the air, and left, that's how he was, he couldn't handle that much !

And what couldn't he handle ? An idea that didn't fall within the scope ?

Yes. An idea that seemed completely silly to him, that came from nowhere, that seemed completely useless. He really couldn't tolerate it. He was awful in seminars, he was unbelievable, sometimes he read his paper.

Which isn't really encouraging. But once the experiment was done, it's also that year that you are going to publish the big article in the "Journal of Molecular Biology". Everything worked together.

Yes. It was very quick. For two weeks people threw their hands up, but in the end everyone accepted it. Not only did everyone accept it but everyone was saying, which is typical, everyone was saying "I've already done it !".

In particular, the Brachet group in Brussels who said "The messenger RNA, we described it a long time ago !".

Them among others, yes.

We present the entire model. And then we did a series of things. Yes, there were two articles. Because I wrote my article on that. I don't remember what Sydney wrote. And Monod wrote a general thing, which were the teleonomic conclusions, as he called them.

Can you tell us what teleonomy is because it's... What was it ?

Well I think that it meant, it was to say, oriented towards an end, but without using the traditional word. Yes, that's it. So as not to speak of theology, he would speak of teleonomy. That's exactly it. That's the word that... yes, that's right.

We were the stars at Cold Spring Harbor.

Yes. At Cold Spring Harbor, we really were the stars, because we arrived with something fresh and we came out with that... Wait a minute, let me see... we weren't really the stars because Jim was also there with DNA. It was in 1958. This one was in 1961.

This one was in 1961. So when was Jim there ?

I really didn't understand his article, well I didn't really read very carefully, his *"Nature"* articles. But I attended a Cold Spring Harbor where Jim was explaining his model. I don't know if you remember Jim with his shirt hanging out...

I saw him when he was older, I didn't see him at that time.

There are photos of him with his shirt hanging out, his finger in the air to showwith his inimitable way of speaking... and at that point, his model was very clear.

But that must have been in 1953, don't you think ?

You're right, it was in 1953.

There are many things that are potential Nobel prize winners. So it needs to get noticed a little more than the others. And actually, it really caused a stir. The whole world got excited about this model, and way too much. Meaning that they... people from almost everywhere, from every discipline, be it the guys who studied protein synthesis in rat liver, or anything, the model was applied to everything and anything. So after four or five years people couldn't stand to talk about it, and the model was rejected by everyone.

Something of a pendulum effect ?

Exactly.

There's a negative period. Yes, there was a period when people didn't believe in it at all.

There was a negative period. There's a negative period. Yes, there was a period when people didn't believe in it anymore. And eventually, it came back. And then there were these two guys, I don't remember their names, who were also working on regulation... and who were making models.

Britton and Davidson.

That's it. Britton and Davidson, who were making completely eccentric models, and who were very succesful, but who were completely eccentric, who didn't have any experimental basis. But very successful.

So that wasn't doing you any good.

Not really, because we found it so crazy that it didn't really bother us ! Have you read them ?

I've seen them. I had discussions with Americans, it's strange, these models have kept a certain aura.

And I wonder why because they don't have any links with any experimental data. It's very strange.

I think that Davidson had a very strong position.

Yes, that's it. And he also talked a lot, he attended every conference, he flooded conferences with his theory.

But there's absolutely none of it left.

In my opinion, there was nothing there to start with.

And when did the Operon model basically make a comeback- was once again recognised ? Was it from the genes development ?

I'm not exactly sure. There was an eclipse that lasted a few years and then it came back. But what happened was that there had been an excess, an excess in one way which lead to an excess in the other. And it became normal again after a couple of years. But for a very long time, the regulation models and the ones of negative things, the people that were working in the regulation of superior organisms in particular, were never referring to our stuff. It was completely...

Yes, and in particular embryologists like...I don't know, Conrad Waddington or even Boris Ephrussi were...

No, Waddington came over... he came over to Paris especially to see what we were doing. He was an odd guy, did you ever meet Waddington ? Very intelligent and a little strange. He sort of looked like Churchill, he was the type of guy...but he came over to- I think he was giving conferences, of which I've forgotten the name, at Columbia, there was a series of quite famous conferences... and he was supposed to do that, so he came over to see if what we were doing could be applied to his regulation in the superiors.

And I think he applied it a little at the start.

Yes.

And afterwards, he became...

He became against it. But that was a little too easy. I think that there was an excess of people who were in favour of it, which lead to an excess of people who were against it. And it took a long time to put the record straight. But very often, even now, when people do the history of it all they don't refer to it, they don't talk about it.

No. And even the development genes, they sometimes have names, we speak of master gene, when we could very well be speaking of regulator genes. But it's a new nomenclature. Nowadays, in post-genomics people do refer to themselves very often to your model. It has became a sort of icon of post-genomics, in some ways. Which is interesting. But in France, it's your model that basically helped to discover molecular biology. Yes. It played a key role..

Yes.But that, that's French stupidity. Yes, exactly. Genetics, that's incredible. The first genetics chair was Ephrussi's. There was no genetic chair for years, not in Medicine or at the Science Faculty. And the first genetics chair was Ephrussi's. And the first microbiology was André Lwoff's but even later.

As for biochemistry, apart from Fromageot, it was still quite traditional. Well there were some.

Well there were some. There was Gabriel Bertrand who was Monod's precursor. Did you ever meet Gabriel Bertrand ? A very old man with a goatee,

who took very small steps and who had the office... Monod was given the... Trefouel had given his department to Monod, but not the office, which was... So Monod was waiting for him to get sick to get the office...

It is Gabriel Bertrand who, in enzymes, told that that was the metal who was important and not the protein...

Yes.

It's quite an unbelievable discovery. For the evolution of biochemistry.

It's quite an unbelievable discovery. And for student's education, it's was not bad.

We were told by a Swedish journalist, the week before the Nobel prize, which I think took place on the 15th of October or something like that. The previous Saturday, a Swedish journalist had called Monod and had come to see him. And Monod rang me so I could be there as well. She said "It seems that you have won the Nobel prize ?" We said "we'll see". And that's it. And so we asked her "are there two or three of them ?" She said "I don't know". Apparently, she didn't know if there was a third one or not. But that quickly became clear.

And when you got the Nobel prize, what did it represent ?

It represents an avalanche. We weren't famous at all, no one knew who we were. There weren't any French students. There were American, German, Turkish, Chinese students but no French ones. And practically no one knew this laboratory. At the time, Monod had been a lecturer at the Faculty for a while, but very few students had known about it. And so, it was a sort of avalanche. Suddenly, at noon that day, the day the prizes were announced, we saw journalists turning up from all over the world, we were completely knocked out. Well it was... it was terrible.

And afterwards, in the years that followed, it still allowed the development of molecular biology.

Yes, it was its main virtue. Yes, it enabled us to get money, it allowed us to do... what was it called... the DGRST. There was a molecular biology concerted action which enabled the construction of various buildings, including this one, and four or five molecular biology buildings. Yes, it definitely sort of unblocked the system.

And to have students who must have then been hurrying over.

Yes, the students rushed over. Obviously. That's a very French thing to do.

Going for the winning team in a way.

Yes, but mainly the total absence of anything current. Because, actually it started in the fifties. We can say that, molecular biology started with Watson-Crick. The physicists had it on their mind when they said "the molecules really do need to do something and therefore biology must be molecular". That was Delbrück, Crick and those people. But the real demonstration was Watson-Crick. It showed that a molecule could account for absolutely incredible properties in biology. But between 1953 and approximately 1960, not much happened here.

There were, I don't know, maybe a maximum of 20 guys. And so there were meetings here and there. I remember several meetings. I think that the Rockefeller foundation had given a group constituted of Ephrussi, OleMaale, did you ever meet Ole Maalu ? He was a Dane, a very charming guy, very friendly. And then, there was Ephrussi, Maale who were the first geneticists... and probably Cavalli the Italian... had given some money to these guys do to a series of conferences. There was one in Denmark, there was one in Pallanza in Italy and then there was another one I don't remember where. And there were 15 or 20 guys, always the same ones. It was a very small... Roughly there were Delbrück's guys... there was the Delbrück-Luria group, Jim who came from it because he was Luria's student. In England there was Crick and Sydney. And there was us here. And that's it.

Quite a change compared to nowadays.

And everything happened by post or phone.

We knew roughly six months ahead of publication what was being written, what everyone was doing. It was rather exceptional for a while.

You spoke of golden age when referring to that time.

Yes, that's more or less it. But I mean there was... I particularly remember that story... I told you that there was a meeting at Sydney's with Francis. And it took place in Sydney's room in his college - Kings, or something like that. And they asked me to summarise what we were doing in Paris. And the conclusion was "there needs to be something, an intermediary x". And that's when they leapt up and said... Well. But about three weeks or a month earlier, I had made the exact same speech, exactly the same one, in Denmark, in Copenhagen, with the same guys, and not one of them bat an eyelid. That's how it was !

Crick was the brightest of all the molecular biologists who ever existed in the world. He was astonishing. He was so astonishing that he would even explain to everyone what they were doing. So that people understood the importance of their own work he would explain it to them. Which people didn't always appreciate. There are some who don't like things to be done that way. But he was extraordinarily bright. He made a speech, it must have been in 1960, on the synthesis of proteins...

I think that it was in 1957, the conference where he spoke, in particular, about the code, wasn't it ? The idea of code ?

That's right, the idea of code, the idea of ; what are those things called,

that... adaptors, yes.

He had a very surprising opinion on that. In comparison to the confusion, the mess that was the synthesis of proteins, Crick's speech was extraordinarily simplifying, he brought things into perspective, he showed where we needed to look for something and where we shouldn't be looking for anything. And he was very surprising. Very surprising.

And you say that nevertheless, at the start, when you saw the Watson-Crick model, you knew Watson, and everyone was saying

he was the author.

Yes. I thought Crick was one of Watson's appendages. Until he got here. He came over for a seminar. And then, it was clear that he wasn't a by-product.

And afterwards, towards the end of his life, Francis Crick turned towards the development of the nervous system, and all that. Did you follow what he did later on ?

First he did... I think he did a little development.

Yes, he had worked with Peter Lawrence.

That's right. And he also worked with Sydney. Because Sydney and Francis were together, in a room in Cambridge, and they were hard at work there because they were both very very bright. Sydney is an extraordinarily bright guy, full of ideas, never stops talking. I think he must have gone on for 20 hours without stopping. He's extraordinary in his own way.

Seymour Benzer is also a really extraordinary person. Seymour came over for a year, quite early on, soon after I arrived at the Pasteur Institute. We were in the same lab. And for more or less a year, he arrived in the morning, said "hi !" and we didn't hear from him all day. And in the evening, he would say "bye !". That's all we would hear from him all day. But he was surprising. Because he's a guy who, you talk to him, you ask him a question, no answer, he doesn't say anything. And then, four days later he comes back and he's got to the bottom of the problem. He's also really outstanding in his own way.

Jean Weigle, he's in a class of his own. Weigle was a physics lecturer in Geneva. He decided, when he was 40, that at 50 he would stop. That it was inconceivable for a physics lecturer to be older than 50 years old. So, he was stopping. And when he stopped he started travelling the world. He had money so he travelled. He ended up in Pasadena at Delbrück's laboratory. He was fascinated by phage, and so he came back to Geneva with Lambda, K12 and everything else, all the gear. And he was very very friendly, he was a very warm guy, very nice. He died very early, soon after he started working on phage.

Leo Szilard, he's just like a bumblebee who goes from one flower to the next and fertilises just about anything. Szilard is also extraordinary, an incredible, surprising man. He had 10 ideas a minute. He had ideas about everything. I must have forgotten now, but he had, I have somewhat forgotten his ideas, but there were things to make money, to do experiments, to...

I know that he was very active in anti nuclear weapon movements, wasn't he ?

Yes, he was very active. He was one of the guys who... He went to see Einstein, he took him by the arm, and took him to see... Who was it then ?

Roosevelt.

He's the one that set the whole thing off, yes. But he was very surprising.

Aaron Novick was one of Szilard's physics students. And when Szilard had had enough of atomic bombs and physics, he got into biology, they both got into biology. Their first work was something Monod had been doing on his side, which was a continuous growth device, you know, bactogene. They called it bastostat and Monod called it bactogene but it was the same thing. And they did it independently. Szilard was incredible. He came in, sat you down, and sat opposite you with his notebook, he started asking questions, he wrote down all your answers. And a year later, he would say "that day at that time, you said this, is it still true ?"

Jim Watson, he's... Well you know Jim Watson ?

I know him. But I know him as Head of...

He's an extraordinary person. Actually, he's probably the one that did the most molecular biology of all the system. He's the one who dragged Crick into it. And he's the one who... as well as the human genome. He's incredible. But at the same time, he's completely crazy. When you see him, he's very surprising.

And you also said that he was the one who read his paper during, him as well since...

Yes. That was during a conference, the Copenhagen conference that I was talking about earlier. Every time someone was making a speech he would open his paper. Which means that when he went up to do his speech, everyone took a paper out.

I was absolutely convinced that... which is odd... Luria and Delbrück, that was something classic, it was an incredible experiment. Do Luria and Delbrück mean anything to you ?That really was an incredible experiment, from a conceptual point of view as well the way in which it was done and everything. And so, they were very famous. In the lab, we talked about Luria and Delbrück all the time. And I don't know why, but I imagined Delbrück to be the short fat one and Luria the tall dark Italian. But it's the exact opposite. Luria wasn't fat but he was short and dark, whereas Delbrück was a tall blond. It's always surprising to find out that people aren't like you imagined them to be.

And, in your opinion, those were things that were so established that you already imagined the people...

Yes, well in the lab, that's all we talked about. Luria and Delbrück... until the day we saw them.

And you worked with Salvador Luria at Pasteur ?

Yes, he came over to Monod's lab a few times for a few months. So he was a short Italian, very dark, with a bad temper, screaming, very politically committed. Whereas Delbrück was a cold and calm guy.

Gunther Stent was one of Delbrück's students, like most of those guys. And Elie Wollman had gone to the United States for two years at Delbrück's lab. And Elie and Gunther Stent became very good friends, they worked together on something completely uninteresting, which was the role of tryptophan in the absorption of T4 phage. Apparently, T4 phage doesn't get absorbed if there isn't any tryptophan. So they worked on that for two years. It didn't make much of an impact. And so Elie and Gunther were very good friends and Gunther often came over. And in fact, Gunther invited Elie to Berkeley for a year. And it's during the year that Elie spent in Berkeley that we really

did things with Monod. And so when he came back, we told him "come work with us". And he said no. It wasn't worthy of him.

And afterwards, Gunther Stent is sort of going to be opposed to the Operon ?

Yes, he did the Operon, but he didn't do it at the DNA level, but at the level ofbut he was always against it. We can almost say that he was regularly opposed to every theory that came out, and we can say that we could pretty much bet that the truth was more or less the opposite of what he was saying. Almost every time. It wasn't "that reliable", but almost.

And in fact afterwards, in the years that follow, I know that in development biology, he's also going to be opposed to gene development.

Yes, he always ventured off the beaten tracks. But he was very very nice. He talked quite a lot. He's a philosopher now, it suits him well, he had a philosophical mind. He's a philosophy lecturer. He gives philosophy lectures at Berkeley.

And when you speak of that time, you often insisted on the pairs of scientists.

It was a time, when people often worked in pairs. Yes, I think that until the war, they worked alone. Now you need 20 or 25 people to work on the genome. But at that time, at the birth of molecular biology, there were couples, there were pairs. There was Watson and Crick, there was Luria-Delbrück, there was Jacob-Monod, etc. There was Meselson and Stahl. It's funny, but that's how it was, there were pairs. But I believe that working alone is boring, it's much more fun to work with someone, it's more cheerful, it goes much quicker.

In the end, It wasn't the Pasteurian laboratory who isolated the repressor ?

No.

Did you regret it ?

Yes, I didn't really appreciate it. I thought that the geneticists had worked much harder than the chemists. And that the chemists, whose job it was to isolate the repressor hadn't isolated it. Actually, the people that isolated the repressor used one or two tricks, such as obtaining mutants that did much more than the normal strain, the wild-type, because obviously, the quantity of repressor per cell is very small. And we could expect that it would be very small. So they had better isolate the mutants that were doing too much. And they found some, which simplified the isolation of the repressor.

But in the end, the isolation ?

Well, it wasn't us. Yes, it was even indispensable. But that's why it would have been good if it had been us.

On the other hand, you go, very quickly after working on the Operon, develop the Replicon model with Sydney Brenner.

With Sydney, yes. Yes, because we... it happened on a beach... Me, I had already knit with this story for some time because I found that during the recombination systems, that it is by conjugation or transfer by phage, when a piece of DNA was transferred, a piece of DNA say, was transferred by any of these mechanisms in a recipient cell, this piece of DNA did not replicate. It only replicated under certain conditions and when it had a certain segment of the phage chromosome for example or any other ... So the conclusion of this story is that there had to be a site that allowed replication or not, and compared to the operator's system, I built this thing with the replicator and this difference was supposed to be positive. I had arguments to say "there must be a gene that makes a protein, a product, that acts on a site, that allows the replication of DNA, for example, or any ...", that's it.

So that positive regulation in fact, for you, that was not a problem.

No.

The idea that there could be positive regulations alongside negative regulations, which annoyed Jacques Monod.

But Jacques Monod had a spirit, he was made in such a way that nature must be rational but have the same rationality as him. Which was not absolutely obvious. That's why, for example, he wanted all regulations to be negative. Since we had found 3 systems one after the other, phage, lactose, and tryptophan were all negative regulations, so nature was o-negative regulator. Which was not absolutely obvious a priori.

You believe much more, you, basically, to the diversity and inventiveness of nature.

I have always believed in Do It Yourself (DIY). DIY, that is to say, to use the stuff as they are and take an old thing to make a new one.

This is an idea that you will develop in the 70s.

Later, yes. Because evolution is a bit like that. Evolution takes a structure, and it uses it, not just in the way it was used in the beginning but to do other things at once. And that, it looks like DIY.

And it's a concept that has been very successful, everyone is talking about DIY today.

Yes, but I think it was a very reasonable idea. It annoyed a lot, because the idea that we were the product of DIY, people did not like that much. But in fact, it's still a very useful notion because that's how it works.

What I mainly wanted was to change material. I wanted to have something... instead of bacteria, I wanted an organism that had eyes, that looked at you and that had a soul. And bacteria don't really have souls. And so there were a lot of discussions. Because the question was "if we want to go on to superior organisms, which one ?". So two of my friends, Seymour Benzer and Sydney, had already taken to plunge. Seymour was working on... drosophila

and its behaviour in particular.

Yes, that's right. And Sydney had chosen the small warm. So I asked Sydney if I could borrow his small worm, which he did with disgust. He lent it to me, but he wasn't very happy that I was working with it. But I didn't really enjoy

working with the small worm. Which means that I didn't work with it for very long. And I thought drosophila was the perfect system, a tremendous system because of genetics, because of the possibility of, of really... But I thought that importing drosophila to Pasteur, with a sufficiently big enough group so as to be able to do something, wasn't very reasonable. Whereas the mouse, which is an organism on which bacteria, viruses and everything is tested... it was perfectly reasonable to do a little mouse genetics and do it with a mouse. Hence the mouse. Which, obviously, didn't allow me to do as much as with drosophila. But nevertheless, I thought it was much more reasonable to work with mice at Pasteur than with drosophila.

And for the eyes and the soul. The mouse is better !

Yes, of course. It's even better than roundworms Roundworms have souls like everybody else !

It seemed obvious to me, in the same way that we had been able to do things with bacteria, which was because we had brought together people from completely different horizons and education to work on one organism, which was Escherichia Coli K12 and its phage, that if we wanted to start working on superior organisms, the same thing needed to be done. So we could then start talking about which superior organism to pick, but eventually, for a variety of reasons, the mouse was the best one. Hence the idea of the mouse facilites at the Institute. People then thought I was crazy or... because why choose mice rather than drosophila, rather than man, rather than anything else. And they also thought "he wants to have an institute, which he would manage". If there was something in which I had no interest, it was to manage an institute. And eventually, it failed because of that. People didn't follow, which is silly because if we had done it, we would have been ten years ahead of everyone else.

Nevertheless, in the molecular biology department in which you have settled, the mouse had quite an important place.

Well yes, because eventually I bent the system. But then I had a long talk with Monod, because it wasn't easy at the start. Especially... what was his name, Oudin who found it completely stupid, and who thought the only normal organism was the rabbit. Because serum is made from rabbits, and

that having 5000 mice in a corner of the Pasteur Institute was completely unreasonable. So I had absolutely Homeric talks with him, especially in the large lecture theatre at Pasteur, where I tried to prove that we needed to do genetics, which in his opinion really wasn't obvious, and if we wanted to do genetics, it wasn't rabbit genetics but mouse genetics that was needed. And eventually, Monod agreed with me and Oudin wasn't very happy. And we transformed practically all Pasteur's animal houses into mice houses.

I don't remember who hired him *, who found him, yes, I think it was Fauve. He was Fauve's friend. Did you know Fauve ? Who was in Garches and then came here. He was the only one who was used to working with mouse-type organisms ; he had been working on them for years. He knew what it was like to inject a mouse, whereas we were completely inapt. He recruited Jean-Louis who was an incredible recruit. He immediately understood the importance of the project, and he got down to work, with a lot of difficulty, because of Oudin. Oudin wanted Fauve to give him rabbits. And Fauve didn't want to.

But nevertheless, you had a mice animal house which was at the time one of the best in France.

I had mouse house because I stole it from Oudin. It was the animal house which was initially supposed to be a rabbit one. So I went to talk to him for three hours with Monod who totally understood, and who said "all right, we'll work on the mouse". To Oudin's great displeasure. Did you know Oudin ?

Yes, but I had trouble understanding the concepts he used.

I had a fantastic discussion with him, I don't know if you remember it, in the middle if the big lecture theatre. Because we were talking about the way we worked. I told him "we can't work if we don't start with a theory". He said "sir, I don't form theories, I do experiments".

It was very difficult at the time. It was obvious that drosophila and even roundworms were much simpler. But mainly drosophila, because there were 50, 60, 70 years of genetics, mutants absolutely everywhere, rearrangements, translocations, this and that, it was fantastic. And effectively it was with

∗. Jean-Louis ?

drosophila that the most important things were done. But it wasn't easy to import drosophila over here. Whereas the mouse, was sort of the support for everything that was to be done...

Basically, you had anticipated the mouse, but the tools only arrived 10 or 15 years after you took that decision.

Yes. But well, it was reasonable.

My idea was that if we went from bacteria to more complicated organisms, the two systems were needed. Meaning having the organism and cell culture. There weren't that many because the famous small worm, we can't put his cells in cultures. And in my opinion, that was almost a necessity. Hence the reason for the mouse. Because on mice cells we really could do a lot of things. And teratoma in particular. The day I discovered teratoma, I jumped for joy. Boris Ephrussi is the one who made me discover it. He was the one working on it, he didn't do much with it by the way. But nonetheless, he extracted two or three strains which were used worldwide. I've forgotten their name, but Boris' strains were very famous.

F9s ?

Among others, yes. That's right.

PCC3s, PCC4s ?

No, we were the ones who extracted those. The PCC was us. It was Paris and I don't remember what. P was for Paris. Edwige was the one who extracted those strains.

And was it a delight to work on Teratocarcinoma cells, to see inactivation in the box.

It was incredible. If you put that in a box, there are cells that look like nothing. All of a sudden, a few cells aggregate, differentiate themselves and start beating, it's incredible to see a beating with a rhythm which is the cardiac rhythm of fifty cells, which are aggregated in the middle of the box, it's extraordinary. It makes you feel like God.

And in the end, the ES cells that we speak so much of today, they are practically identical, to those cells. So, what people are doing today is what you were already doing 30 years ago. Domesticate them in a way.

Absolutely. But I don't think that they realise it. Because they are doing it on human cells, and obviously it much more dignified to do that on human cells.

It's difficult, It's difficult, yes.

But a lot of the things that are done with it nowadays, we were doing them with mice cells.

Even the compounds used like retinoïc acid, were compounds that you were already using at the time to master inactivation.

Absolutely.

And from your work with the mouse, what will you remember ? What were your biggest contributions and your biggest regrets ?

First of all, I'll remember that it's a lot more difficult than bacteria. It's incredible how much more difficult than bacteria. But that nevertheless, with cells, you can do an enormous amount of things in vitro. And what is necessary, at least that's what I believe to have understood, was to work with both, meaning the cells and the entire mouse, go from one to the other and back. I think that in the end it was a good thing. And eventually, all it did was confirm that the idea of mouse facilites was the most reasonable thing to do.

Do you think that France would be in advance on the ES cells nowadays, if you had received more support at the time ?

Probably, yes. I think so, yes. ES cells have become a philosophico-religious problem. So it's difficult to talk about it.

We might talk about it later when we focus on ethical problems.

Maybe.

And is there anything that you regret, an experiment that you may not have done or that you could have done at the time but didn't, or that others have done with the mouse.

More than likely, but I have forgotten.

I was thinking of what Beatrice Menz did when she managed to reconstitute

Yes, Yes. That was one of the things we wanted to do early on, and Beatrice Menz did it. There was another guy that was very good. What was his name ? He was Beatrice Menz's rival. I've forgotten his name. He was the first one to do it, Beatrice Menz did it afterwards.
Extracting cells and re-injecting them in morula or in blastocyst, and making it appear in the mouse, that was indeed a magnificent experiment. We sort of failed to do that.

At the beginning of the eighties, genetic engineering brought the tools that allow the characterisation of genes, which until then had been difficult since it had been almost impossible to isolate the gene until the beginning of the eighties. At the time, how did you feel about the arrival of genetic engineering ?

Bad, because it happened in a slightly complicated context.
At the time there were constant arguments. I don't know if you remember. I didn't really like that. Which means that at the beginning, I didn't really get into genetic engineering. I waited for it to calm down a little. Well there were four or five guys that were very good here. There was one whose name I've completely forgotten, who was very bright. Alain Rambach, yes. What happened to him ?

I think that he manages a biotechnology company.

He was very clever and quite unbearable. There were many like that, unbearable and clever. The time really called for it. There were... what was the

other one called, the Canadian that came and went, who was really impressive in his own way. He was very clever and unbearable.

I don't remember the Canadian. I remember Pierre Tiollais who wasn't unbearable at all.

No Pierre Tiollais wasn't unbearable, not at all. No, no, it was another one whose name I forget. It doesn't matter. Yes, that's right.

I don't know and who is one of the first to push the techniques on genetic engineering.

Yes.

But in the end, it's in the middle of the eighties that, for the mouse, it's really going to produce tools for isolating genes.

What mainly came as a shock, was to discover they were the same ones everywhere. That was something staggering. Since then, we have completely accustomed ourselves to the idea. But at the time, realising that the genes that constitute a man are the same as those that constitute a drosophila, that wasn't obvious. That whole thing really surprised me.

The bricolage concept sort of enables its explanation.

Yes, absolutely. But what's important to know is, once you've accepted it, it's all right. But at first, it's still very surprising.

Until then, it was believed that the development of the mouse or of the drosophila were going to be different genes. Identical principles maybe, but completely different genes ?

I started off... When I started doing biology, which was in the mid fifties, the idea was that the molecules that composed drosophila, weren't the same molecules as the ones that composed a cow. And it was because a cow was made of cow molecules that it was a cow. And that's how it was for quite a while. And it's only little by little, that we noticed that certain molecules, for instance haemoglobin, were the same everywhere, and little by little, this

idea grew and spread, and eventually we noticed that all organisms were made with the same molecules. That was the height of bricolage, yes.

And specifically with the paxis genes, which control the formation of the eye.

For instance, yes. The formation of all eyes, everywhere.

That's also something truly extraordinary and unexpected, unpredictable.

Totally unpredictable but astonishing. Confirming the wonderful concept that is bricolage.

The idea of making sequences wasn't something I particularly enjoyed. I thought it needed to be done, that we would do it, but that we had time, that there was no rush. And that it wasn't worth rushing to make sequences. It was obvious we would be making sequences and that we would end up making the human sequence. Since making the human sequence we haven't done much. We will be making some, but it takes time. And it was the idea I had at the beginning. Meaning that it needed to be done but that I'd rather the others did it.

And post-genomics, which is largely talked about these days, how do you, that is to say the new technologies that would allow the observation of the organism in a global manner, protein networks. This, I consider that it will be for future generations, but not mine.

In regards to the transcriptome, people rediscover your intuitions, because they look at the coregulated genes as a whole, and in the end, they fall back on what you imagined in the sixties.

Yes. It was predictable. It's a good representation of the idea, yes.

And you make a few very striking distinctions, for example, day science and night science, which is an idea that you really developed. There are basically two forms of science. There's night science which is...

There's the science that gets published and the science that is dreamt or appears in nightmares. That's the idea. Meaning that I think that the majority of ideas come to you at night. When waking up with a jump, sweating, etc. And while dreaming and having nightmares, that's what night science is. But all those ideas don't go on to be written, they don't appear in articles. In articles, everything is simple, clear and straightforward...

And is it normal to distinguish the two so much ?

I don't know if it's normal or not. That's just how I see things. But I think it must be very different. That's how I worked. Meaning that very often at night, I would wake up thinking of experiments we had done, or that we were going to do. And generally, what we think of at night has nothing to do with the reality of the real. It's simply... but eventually it's quite useful because it still allows a few ideas that would not come up on their own to emerge in a rather pure logical reflection.

It's an idea and diversity generator ?

Yes, if you want, that's it, yes. Afterwards, what exceeds need to be trimmed and brought backand the day science, is the one that can be seen appearing in decent articles, meaning what is allowed to be thought, to be shown.

There's also, you also distinguish between warm and cold science. Cold science being the one we read in books. And warm science being the one we do.

It's sort of the same thing.

But at the same time, there's the psychological aspect because the one that really interests you is warm science.

Yes, but all that is obviously psychological. We can hardly get rid of our psychology. Yes, yes that's right. But I think that it's true that you wake up at night with ideas that, most of the time, are wrong. Personally, I had reached the point of having a pen and paper next to my bed, mainly so as not to miss anything, because by morning you don't remember any of it. And

actually, most of these ideas that occur at night, are fit for the dustbin, but once in a while, there are one or two that emerge. So I had the paper and pen by my side, in order to quickly write things down on paper. Actually, I didn't write much, but I still believe that at night, we're not in a similar intellectual and psychological state as the one we are in during the day. Well, at least that's how I am.

As if unexpected connections were made.

If you like, that's right.

Actually, we always say that people are equal when faced with illness. But it isn't true. They are different when faced with illness. They aren't equal in front of therapy, if you will. But identity and equality aren't the same thing at all. Identity is biology and equality is culture. And it's because people are different that the notion of equality is necessary.

But I think that it's a very dominant confusion, for example with cloning.

I agree, but I find it silly. Everyone's talking about it. Equality when faced with this and that. It's not an equality, it's a difference.

And the problems with stem cells which was also largely talked about, what do you think about it ?

I think that it's still a fantastic tool and that it needs to be handled with precaution.

But I don't think we can go without stem cells. What we need is simply to set barriers on the exercises which we can carry out with stem cells. But it is still an incredible tool.

Towards the end of the 1960s, I was a little fed up with doing genetics of bacteria, so I wanted to start changing, I wanted to have time to think, and I asked myself the question "why is what do we do ?". So that led to the story of biology, but what I tried was not to read the stories of biology that others had done, but to make my own story by looking at what people had

do, exactly.

The original works, the works.

Yes that's it. Well, that was the idea.

And that, today, you would do it the same way, or you had...

Today, I will not do it *(laughs)*.

But have your ideas changed ? Has the history of biology changed ?

No, no, my ideas have not changed, my ardor, if you like, my eagerness to read people and to write, has changed, that's all. In other words, I have aged. I spent a lot of time at that. Finally, it was not useless, because we learn a lot. It's not easy, how the stuff came, who did what, etc., that's very interesting. And for that, it is necessary to avoid reading the ready-made stories of the biology, which in general are quite badly done.

Which were very traditional, right ? Which was a story of discoveries that accumulated ... But yet when your book came out, it was said that you were deep in the new epistemological history of science, for example, your book was similar to those of Foucault, saying that we found the same breath at the time.

Yes, but it's not entirely wrong. I liked Foucault well. I thought Foucault was a very smart guy. He had lots of ideas. It is not false.

Except that Foucault has sometimes been reproached for not always referring to the original works.

Ah, it's very simple, I saw how it worked, it rushed on the preface and the first 10 pages of the preface. And there, he was decortifying the preface in an extraordinary way and he came out that. It was absolutely amazing and extremely effective.

Okay, but the rest, however, escaped his attention a little.

The rest, he did not care.

And it is a book that has remained, because today still, it remains a reference for the history of the discipline.

Well, I think they sell a lot of them. I think there are some courses where we recommend reading this, which always amazes me a bit because it's still quite old now, in 1970, it's been 30 years, 40 years.

There has been no work with a similar ambition since then.

It is possible.

Except maybe Ernst Mayr, who has oriented him more towards the history of genetics. And in the book *The game of the possibilities*, it is there that you have divulged a little this notion of do-it-yourself.

So *The game of possibilities*, that's very simple, it was ... I was invited to lecture on the west coast, I do not remember where, so I wrote the texts of these three conferences, which are become *The game of possibilities*. It was the time when I was doing DIY, and I put it in there, yes, it was one of the chapters of the book.

And then there was your autobiography *The Statue Within*.

It was something else.

And *The mouse, the fly and the man*...
No, there I took lectures that I was doing, yes, it was not a real book. It was reuse of leftovers.

Yes, but there was something new about the development of biology since the Nobel Prize.

A little. It was less a book than the others. *The game of possibilities*, I like, I think it's a little book, it's not too long, it's not too boring, it's pretty well removed. It was the best of the series, I think. The first is a little heavy, you

can learn things but it's a bit hard to read. It's dense, very dense !

I didn't know Georges Canguilhem, I met him then. And the first time I saw him, he told me something extraordinary. He said "if I'd met you earlier I would have written less rubbish". It was very surprising. Because he wrote a lot of rubbish about genetics, among other things. I don't know if you...

Yes, Yes I read it. At one point he even came very close to Lysenkoism.

Yes, he didn't understand any of it, absolutely nothing. And I found it touching that he would say that to me. Because, nonetheless, he is a very successful man, who educated generations of philosophers, and who is a very likeable and good guy. He was a member of the Résistance. He was a very good guy. Absolutely in his favour, but it amazed me, if you like. It surprised me that a guy of his stature and calibre would say that to me.

When I got there, I had found it difficult to settle somewhere. Because I had met Terouanne once. Terouanne was the director or assistant director of the CNRS. He was quite an astonishing guy in his own way. So there was the CNRS, Terouanne. There was the INSERM, which wasn't called INSERM at the time, it was the National Hygiene Institute. And the guy that ran it, what was his name ? I don't remember. And so when I arrived, I went to see him. And I said to him "right", because my little speech went as follows "Right, I haven't done anything, I fought in the war, I have problems with it, I would like to do biology, can I work here ?". So most guys threw me out straight away, Terouanne in particular. He... no, I would tell them "I wanted to do genetics". And he says to me "personally, I'm not interested in genetics". Fine. And then eventually, Trefouel to whom I had made the same speech, said yes and took me on, all because of my friendly face and I don't even know why. Because I hadn't done anything with biology, I didn't have any ideas apart from the fact that we probably needed to do bacterial genetics, which horrified most people, by saying "there are no genes, no DNA, etc.". And that's it. But in the end, he was very nice, he gave me a grant. The only problem with the grant was that I needed to find a place in the institute. My mind was already made up, I wanted to work with Lwoff and nowhere else. And I went to see Lwoff who said "I don't have any room for you". The following month I went to Lwoff, and he said "I don't have any room for

you". It went on like that until May or June. And in June, I needed to find something for the next year and I was really struggling to find someone who could take me on. And eventually, in June, he said to me "you know, we have found the induction of the prophage. Would you be interested ?". I told him "I'm dying for it !". And in the end, he took me on. I came out of there, I went straight to a bookshop to see what induction and prophage were. I found something incomprehensible on induction, but prophage didn't exist.

We haven't really talked about the Pasteur Institute where your scientific career was made.

Completely.

So firstly there's the discovery of the Pasteur Institute that you write about in *The Statue Within* a placed filled with traditions and history ?

All you need to see is the crypt. It's extraordinary, have you been to see it. Yes. You have to admit that it's astonishing.

It's surprising, yes. A blend of scientism and religiosity.

Absolutely. Amazing. And what is surprising, I don't know if it still takes place and if you've done it, but it's the annual visit on the anniversary of what I think is Pasteur's death, where the whole of the institutes parades in front of the tomb, one by one. That is also incredible, particularly in such a place.

But really, those were traditions which were both rich, but at the same time could be a little unproductive sometimes.

Yes, of course. But at the start there were incredible guys.

For example ?

There was a guy who came... When I got there, one of my first discoveries at the Pasteur Institute, was to find a guy who got there at noon, cooked his steak on the Bunsen burner and left at quarter past one. That was his day

at the Pasteur Institute. No problem. Nobody paid attention to it. That's what was good. Absolutely. The good side of the place. It's changed a lot. It was something, it was so calm, you could do whatever you wanted. And also do nothing, we could do as we wanted at that time in the Pasteur Institute.

There weren't any laboratory controls, audits, nothing like that ?

Right at the beginning, when I arrived, right at the beginning, no. It really changed afterwards. But at the beginning, there was hardly any control. The whole institute was suspicious of the Lwoff-Monod group. They were completely separated, taking refuge in a... the proof that they were suspicious of Lwoff, is that he was given the attic up there. And that he was confined in his attic. That was also quite surprising. And a lot was needed for him to..., it took a Nobel prize for it to change. It's quite astonishing. In the fifties and sixties.

And who was behind the change ?

Well, there were three guys who brought about the revolution. There was Wollman, there was Virat, which probably doesn't mean anything to you, he was a veterinary surgeon, and there was a third one, Panthier, Jean-Jacques'father. Those were the three guys who plotted and brought about the revolution at Pasteur. Meaning that they... It was boiling. The director was Trefouel, who was very nice, but who was still really extraordinary. Trefouel was a member of the Academy, there weren't many, nobody from our group, neither Lwoff nor Monod had anything to do with the Academy. But since we wanted our notes at the Academy, we needed an academician to present the note. So, we would go to see Trefouel. We would bring a note on bacteriophage to Trefouel. He would say "it's absolutely fantastic, it's exactly like the chemistry of I don't know what".

You couldn't really see the link.

No. We would say "Yes, yes, you're right". And he would take our note and bring it to the Academy.

But nevertheless, in 1960, Mr Trefouel continued to run the Pasteur Institute, but in a slightly lax way. I mean that a serious

readjustment was needed.

Yes, because he had absolutely no idea about modern biology. He was more interested in biochemistry. He did some very good things. He focussed on sulfonamides and things like that, which was very good. But biology isn't limited to sulfonamides. As I was telling you earlier, when we went to see him, and we brought him a note, he would say "It's like sulfonamides !". Elie and I both struggled to keep a straight face. We would get there with our note, we did them together, and he'd say "it's fantastic, it's like sulfonamides". And then, we couldn't look at each other ! No to laugh, absolutely.

And how did these three people go about with the revolution ?

So the revolution was... I don't remember the details, but...

Was there a big personality change at the institute ? We know that Monod is going to arrive in 1971.

Yes, but that's later on. Well, there mainly were those three guys. I don't remember the details. The three who wanted to change the institute. And I had been assigned the task of going to see Lwoff to ask him if he would be director. So I went to see Lwoff, I didn't have to say a single thing, he was director, he considered himself to be director. And that was also a problem. And in the end, he didn't become director, so he left, he went to the Curie Institute. Yes, he didn't like it. Because he had he was an absolutely wonderful person, but he had his friends and his enemies. He would do anything for his friends, and everything against his enemies. And so, he didn't hesitate to go around the institute saying "those guys need to thrown out !" when he was about to become director. So people didn't really appreciate it.

When I started at Lwoff's laboratory, there was an incredible team of technicians. I've forgotten their names, which is a real shame. But there were two guys and three women in the service, who were extraordinary, extraordinarily devoted, efficient and kind. And it lasted that way for a while. Nowadays, it's too big, there are too many people.

But it still lasted for, because at the beginning of the molecular biology building, it was still the same people.

Yes, the same ones, who died one by one. I've forgotten their names. There were two guys who were absolutely tremendous.

I don't remember the guys. I remember the women. There was Louise, Célestine, Germaine who were in Monod's lab.

That's it. They were all Bretons. That's true.

And you, you had technicians with you, in particular Martine Tallec, with whom you worked ?

Yes. Before Martine, I had, I don't remember what her name was, who is now François Gros'wife. Who was very good, very efficient. I had two at one point, I had Martine and her and it went tremendously well. Because we needed to subculture hundreds of complicated things what we stuck on dark agar which gave blue or red depending whether it was lactose+ or lactose-. And there, they were fantastic. They spent entire days culturing strains, gathering them, looking at what they were...

And all that in a perfectly accurate way. You could completely trust them ?

Yes, yes. I trusted them more than myself. Much more. Martine was incredible. She was fantastic. François'wife was a little more "distinguished". She was a little more reserved. But Martine worked hard and was very efficient. She was fantastic.

Afterwards she went on to work with Hubert Condamine.

That's right. I think they really liked each other.

Things were difficult until Mrs Veil came into office. And then Monod, who was the new director, got on really well with her, and they created a new system. It's partly because of her that the Pasteur Institute was revived. It was revived, because since Pasteur, the Pasteur Institute was self-financed. The whole world financed it, the Brazilian emperor, the... What was that cavalry regiment called ? The very famous cavalry regiment that, I forget,

The Bengal Lancers I think, who brought their contribution. And that's how it all began. And it went very well until more or less the war. From the war onwards, it all completely changed because things were more and more expensive and there was less and less money. And so Monod, who had become director, and Simone Veil who had become Health Secretary. And so together they concocted a system whereby the State gave half of the budget and the Pasteur Institute somehow managed to get the other half.

It's true that the sulfonamides, for example, didn't bring anything to the Pasteur Institute.

Nothing, it's an incredible scandal. The sulfonamides, something that worked for a very long time. I think that all they brought to the Institute was a technician for Trefouel. It's really quite incredible. That's one of the reasons why Trefouel was fired, amongst others. It was unbelievable. Everything went to Rhône-Poulenc, because at the same time he worked for Rhône-Poulenc.

Yes, he was a consultant at Rhône-Poulenc. And the seventies also saw a sort of separation between precisely the production of vaccines etc.

That's right. Until then it was completely mixed up. There were labs, for example salmonella labs, there was a room for research, a room for production. And there were quite a few labs like that in the Institute where production was done. Until they were separated. I think it was Monod who separated all that and who sent everything to Garches.

That made that production was realised in a better way.

Yes, obviously, because the guys that were making huge buckets of bacteria product in their lab in the middle of the institute weren't very happy. Yes, from then on it became much more industrial.

He was a universal mind. He knew everything about biology, he knew all about literature, philosophy, he played the piano. And once in a while, he would disappear for eight days to go play the piano somewhere. He was a very very surprising person.

He was one of your closest colleagues in the mouse adventure ?

Yes, he was very much in favour of it. And unfortunately, he passed away. But intellectually he was very very bright, really remarkable. He knew everything about literature, philosophy, he was fantastic. He was a Normale graduate, he studied literature. He was a good Normale graduate ! Are you a Normale graduate ? They're not all good. Some are unbearable. There's a little of everything like everywhere. But he was particularly friendly. I felt tremendous pain seeing Hubert go. Terrible. François Cuzin, yes, a very good Normale graduate, very efficient. And what were they called the little... ? Episomes. He was really good with episomes. He's the one who found the majority of things, the F, the factors, etc. Yes, that's right, the F, etc. Yes, he was very bright. I think he's retired now. Yes, but he's in Nice now. He's a powerful lord in Nice. But there weren't many of Hubert's quality. Hubert was exceptional. Absolutely exceptional. Yes, he was very very interesting. He had ideas about everything, he knew a lot of things. And his passing truly saddened me.

Science, people don't know it and don't understand it. And people hate what they don't know and don't understand. They are always ready to take down the things they don't know. It's a simple psychological principle. And also, I don't think it bears any importance. That's very French. Because I don't think that's true in the United States. I think that the recruitment of young researchers is the same as it always was. I think it's essentially French. It's important to say that in France, literature is what people find interesting. The people who are valued are always writers. Scientists are considered as just a load of rubbish.

When you had the Nobel, you were considered as an intellectual.

Not really as an intellectual. I don't think that scientists in France are considered as intellectuals. That's exactly what I meant earlier. The class of so-called intellectuals consists of philosophers, writers, possibly painters. But definitely not scientists. Scientists, no way. I think it's a typical French thing, that in France we like literature and philosophy. And that's what is valued. In fact, when the Nobel prizes are announced, most of the time the papers vaguely talk about scientists, but develop in great detail the literature prize, etc. It's clear. In France, we don't get excited at all about science. We get excited about literature, painting, etc. I don't know. I think that it's deeply

anchored in the French soul. I don't know what could be done to change this.

And the current scientific movement which took place last spring. You took part, a little, in the debate.

A little, yes.

What do you think about it ? Are the complaints justified ?

Yes, I think so. I don't think the government understands that science is essential to foresee the future. That's how the future is made. And that consequently, if we cut ourselves off from it, we can't prepare for tomorrow. It's a rare stupidity, but that's how it is. So we do need to get a move on and explain to the government that it will be useful for tomorrow.

But then there's the famous problem of the reform of science, of the institutions.

Yes, well... reforms and debates. I haven't been able to stand them for quite a while. We've seen too many of them, of debates and meetings which didn't change anything, or which provide something but you waste a great amount of time, there are always the same guys telling the same stories. There are the smooth talkers who... I really don't enjoy them.

Today we often talk about the precaution principle. What do you think about it ?

Not much, because I think it's mainly a way of protecting governments, to be sure that they protect themselves from their own possible mistakes. I think that there needs to be some precaution, but making a principle of it isn't reasonable.

Some say that it could threaten scientific knowledge.

Partly. You could freeze everything with it, if you wanted. Because obviously, by precaution, you can't touch anything. It could blow up in your face.

It sort of leads to ethical questions. You wrote quite a lot on ge-

netics, the place it can have, or the bad use that can eventually be made of genetics in regards to racism, etc. What do you think about it ? Is there a risk that genetics be used for that purpose ?

There's a risk and the Nazis showed it to us. That's clear. All that has been around for a while... The idea of separating people according to their abilities, etc., it's ancient history. It was pushed to such an extent by the Nazis, but it's ancient history. Which reappears more or less everywhere, as soon as the Nazi story clarifies itself.

And do you not think that modern technologies, in vitro reproduction, and all that, brings up a particular problem, or provides an additional risk ?

I think so. But we need to be careful. Meaning that we shouldn't be doing just anything. I think that... we can't block everything, but we need to be careful. We need to be careful not to do just anything. I was thinking of, what is it called... ? Duplication... Cloning... it's a sort of hanging threat. And what I found particularly incredible, was on the day we started speaking of cloning, the ukase of politics saying "never, never. We are banning it". But cloning a man doesn't seem like something that will be done tomorrow, so I don't see the point of it.

No ? Or for some people with very...

Yes, there are lunatics everywhere. But the importance of cloning people still doesn't seem very obvious.

Interview pour la collection "La mémoire du Collège de France" (Editions Montparnasse)

Je pense que... Vous savez, ça, c'est un problème qui sera éternel, c'est le problème de la recherche de la connaissance et de l'application de cette connaissance. Depuis toujours, disons, quand on passe de l'âge de pierre à l'âge de fer, avec le fer, on peut faire des couteaux ; avec ce couteau, on peut peler sa pomme ou le planter entre les épaules de son meilleur ami. Donc ça, ça a toujours existé, ça a des conséquences de plus en plus importantes. La recherche de connaissance me paraît exactement une des caractéristiques fondamentales de l'esprit humain, la connaissance et la recherche de connaissance, et fatalement, toute connaissance nouvelle a des applications qui peuvent être soit bonnes, soit mauvaises. Alors ce qui n'est pas clair, ce qui a mal été décidé, et mal mis au point, c'est "quelle est la mécanique, quelle est la structure, qui décide du bien et du mal, dans ce domaine ?". Mais autrement, je ne me sens pas particulièrement responsable d'avoir fait des découvertes sur les mécanismes des gènes si vous voulez.