An interview with Louis de Broglie[1]

F. Kubli (1942 – 2023)

The following interview was preceded by an exchange of letters with Louis de Broglie, and was followed by correspondence and interviews which ultimately led to the writing of a thesis presented at the Polytechnicum ETH in Zurich. The interview took place at Neuilly, in the office of Louis de Broglie, who presented texts and books from his library during his explanations.

L. de Broglie: ... I worked with Mr. Dauvillier on the classification of X-ray lines which was then quite poorly managed. I published quite a few of things at that time. Then, at the same time, I went back to working on particles and waves. And I then published an article (Journal de Physique 1922) which was perhaps the first. Then, I published a few small things but which are not very important, and then three notes to the Comptes Rendus which are from the autumn of 1923, in which I laid down the first principles of wave mechanics.

And then here is my thesis, which was republished on the occasion of my 70th birthday. You will see that the essential idea of my thesis – I am very surprised to never see it cited – is that the frequency of the clock and the frequency of the wave do not transform in the same way. That was my starting point, because I had studied a lot the theory of relativity. After the war of 1914-1918, from 1919, I followed very well-taught courses at the Collège de France by Mr. Langevin. I have them up there, in the house, but it's not worth bringing them, because they're just class notes, – but that led me to study all the transformations very

[1]English translation of AFLB, **17**, n° 2, 1992, p. 111.

closely, especially of the theory of special relativity. I had also studied general relativity, but less. I was very imbued with special relativity, and it was special relativity that led me to my thesis, which is something you never say. This is an article that I published in 1926 a little after my thesis, which I used recently, in which I showed that a classic formula in geometric optics, called the Bouguer formula, is exactly the same as the formula

$$\lambda = h/p$$

As for Schrödinger's article, I became aware of it at the beginning of 1926, I had it before it was published, because he sent me preprints in advance. So very quickly, naturally, I thought about it, and I wrote an article in French, which appeared in the Journal de Physique. It was my first reaction after Schrödinger's work. I was trying to analyze the work from my point of view. There are two things that are interesting. The first thing is that I say right away that there is one thing that I don't like, and that is that it eliminates the corpuscle. And I give a reason: I say that I don't like it, because, for him, the corpuscle is the set of waves. Now, when we have a train of light waves which arrives on an atom, the wave train, we knew very well, can have dimensions up to the order of a meter, the atom has dimensions of the order of 10^{-8}cm. When the photon is localized, what happens, if it is the whole train of waves which was the photon, how can we imagine that the photon is localized in this very small space, if it isn't already located somewhere? I said it from the first article.

And then, another thing that I said several times was that I didn't understand configuration space. I say it at the end of this article. Schrödinger takes a configuration space, and I don't understand how. It's no longer a wave propagation. A configuration space doesn't exist. It's perhaps a correct mathematical representation, but it is no longer the representation of a physical phenomenon. Later, I added another criticism which is not there, but which is the following: If in the theory, following the interpretation of Copenhagen, it is said that the particles are not localized, I wonder how we can form a space with the coordinates of the particles. I have worked a lot on this and I am still working, to seek an interpretation consistent with the point of view that I'm developing now. At the beginning, I even wondered if it was true. Then, I didn't have the slightest doubt that it was true in the sense that with the configuration space we can calculate quantities of things, we can calculate the theory of hydrogen, the theory of helium, which is very beautiful, we have done what we call today quantum chemistry, that is to say the applications to chemistry, and there is no doubt that this formalism is exact. It is therefore not a question of doubting it, but it's a question of knowing if we can't interpret it differently than by saying that things happen in a configuration space. This is what I have done in recent years, and what I still do. It all ties into this.

This is a book I wrote with my brother. It was also published in German.

F. K.: Yes, I saw it at the library in Zurich.

L. de Broglie: I wrote this book while I was still working with my brother. I started to work less with him because I specialized in wave mechanics. It must have been between my thesis, which dates from 1924, and 1926, that we had the idea of writing a book together on the physics of X-rays. My brother was above all an experimenter. He didn't like theory very much, he hadn't studied it much. So we shared the work. It was understood that I would take the theoretical chapters and that he would take the experimental chapters. This is very clear when you know our two styles which are not quite the same. This meant that the book was written slowly because, when you write a book together, there are always tensions and that is to be expected. Finally it is dated 1928, but I am convinced that it was composed around 1926. We are used to dating the year which has not yet begun, so it could very well have been that the text was established towards the end of 1926. So there is something that I reread recently and which surprised me a lot: In the general consideration of the entry which is this, you can read it – So it's roughly from 1926 – on the next page which is curious, because basically I'm planning the laser.

F. K.: This is chapter 1, page?

L. de Broglie: I can show you exactly where it is. I say that in a source it seems that atoms can emit waves... photons which are in phase. I wrote this which is quite curious... it's the laser forecast. I have to tell you that I had almost forgotten about it. I re-read it a year or two ago, and I said to myself... Naturally I've paid a lot of attention to laser theory in recent years, but it's not the same thing! This was written at a very ancient time. Otherwise, I don't think there will be anything that would particularly interest you. My brother had done experimental things on X-rays and I was mainly concerned with the classification of the rays which I had done with Mr. Dauvillier and which, at that time, was not yet very well established.

In 1927, I tried to create a theory where we conserve the wave and the corpuscle with the theory of guiding the corpuscle by the wave. In 1927, it was after Schrödinger 26 and probably even a little after Davisson and Germer. A little before the Solvay Congress. At the Solvay Congress Mr. Pauli said to me: "I read your article in The Journal of Physics is very interesting, but it's not true!" (Laughing) And today, I wonder... The theory was very difficult to develop, I realize that today. I was immediately stopped by difficulties of all kinds and I did not continue – for different reasons. Firstly, at that moment, I was hampered by circumstances completely foreign to science. My mother got sick, I took care of her, etc., it stopped me from working. Then in 28, I was appointed professor at the Sorbonne. I had never taught, I had given free lessons, and so I was obliged, from that moment on, to teach, and I said to myself: Teaching what I do, that doesn't work. Mathematically there are now all the works of Schrödinger, Dirac etc. that's what I need to expose. And I started to expose them. Little by little, I no longer continued in the path. I was on I was perhaps wrong, but it was partly due to the circumstances that took me, which forced me to occupy myself a lot with teaching, from that moment on.

If you want to ask me additional questions?

F. K.: You said that you still have notes that you took in Langevin's classes.

L. de Broglie: Yes, I can show you that, they are at the top, but I don't think it will interest you much, because it was a course that I was taking... it's not very well written.

F. K.: That doesn't matter, on the other hand we are looking for these lessons, because we can only have handwritten notes, and we have found little so far.

L. de Broglie: Mr. Langevin had, I don't know, whether I should say a fault or a quality, it was that he was extremely scrupulous. Every time he gave a lesson he said: I'm not happy with it, I have to start again.

F. K.: It's a lack!

L. de Broglie: Yes, yes,– only my lessons, I would like to show them to you, but they are not very well taken notes, and I don't know if you will be very interested in them. I don't believe that with notes like the ones I took we can very easily reconstruct a correct course. However, I had a surprise. You know that there has been a very big discussion about relativistic transformation formulas in thermodynamics. I believe that the theory given by Planck and von Laue is correct, and I have studied this a lot closely over the past few years. But there have been very important authors who have contested it. Well, I noticed that Mr. Langevin in his course, which I had forgotten, had given the theory of Planck-Laue! I knew it from Laue's book.

F. K.: What interests me is what you said in another interview, that you did not know at the time that Fermat's principle is a consequence of the wave equations by passing to the limit!

L. de Broglie: Yes – Perhaps I didn't know the demonstration. I knew well that we could deduce the equation of geometric optics from the equation of waves, but I don't know if I had the demonstration very present in my mind – which I later gave in a number of my books. It is likely that I did not know him, or that I only knew it in Schrödinger's time or thereabouts.

F. K.: And I believe that this demonstration is very important because Schrödinger took it up.

L. de Broglie: Yes, that's it, that's it. Of course. And then there was another thing that I didn't know... this, right:

F. K.: The Courant-Hilbert!

L. de Broglie: So I immediately bought that after Schrödinger's work, but I didn't know it. The theory of eigenvalues and eigenfunctions was given in a book by Poincaré, the book on propagation of heat. So in there I found it, but not under the title of eigenfunctions and eigenvalues, I don't know what he calls them anymore, there is something somewhere that is equivalent to that – he does not give them at all in a general form, but in a form applied to problems... – I have since reread this book, and I said to myself: But basically it is the theory of functions and eigenvalues – but it had not entered French teaching in a general form, the mathematicians knew it, but did not expose it as in the Courant-Hilbert.

F. K.: And then, regarding Hamilton's theory, in the Jacobi book that you studied, Jacobi did not mention the relationships between the principle of least action and the wave equations.

L. de Broglie: No, I don't think so, because there was a lot of astronomy, I read Jacobi's book.

F. K.: This was only taken up by Debye and Sommerfeld.

L. de Broglie: That's it! On the other hand in a book by Poincaré we found the kinetic theory of gases in a book called "Lessons on Cosmogonic Hypotheses". In it there was M. du Ligondès, who had tried to apply the kinetic theory of gases to problems of astronomy. Poincaré takes this opportunity to recall the kinetic theory of gases.

F. K.: Do you think that had some influence on you?

L. de Broglie: I had certainly read that book, but I had studied the kinetic theory of gases in many other ways. I had attended Lorentz's 1912 lectures, too. I certainly knew Boltzmann's book, which had been translated into French by M. Brillouin the father, and I came across Jeans' book a little later perhaps.

F. K.: And Planck's book?

L. de Broglie: Ah yes, yes, Planck's book, of course, but Planck had created the theory of black radiation, he did not create kinetic theory. He talks about it, naturally. Ah, yes, the book on black radiation I had certainly read it before the war of 14. Also its thermodynamics, but which is completely classic. And so, what else was there in this line of ideas – ah, yes: Gibbs' book, too, which I had read.

F. K.: This is also seen in your thesis –

L. de Broglie: Yes yes. Gibbs' book, which is a little more difficult to read than Boltzmann's book because it is less physical, but it was quite interesting too. And then Mr. Langevin gave lectures on the kinetic theory of gases, he published this by exception, I can show you. Ah, here are the articles by Paul Langevin. These are notes that he had made especially in the Comptes Rendus and the famous articles on magnetism... There is in there, somewhere, the conference that he had given, in “Physics for twenty years". It’s in there that there is a conference, somewhat a popularization conference, before the physics society on statistical thermodynamics. There you go, I also read it in 1912. It’s the chapter which is entitled “The physics of the discontinuous”. Yet another perfectly dated memory, this one, because it can’t be later than 1912, I’ll tell you why. In this last year, which I spent working, it was therefore in the spring of 1912, Henri Poincaré gave a conference, for a private company, on the kinetic theory of gases. And I was there. And that’s the only time I saw him! And it is certainly before July 1912, because he died in July 1912, it must be in March 1912.

F. K.: I spoke with Mr. Fierz, and he advised me to look for an article by Henri Poincaré which could have guided you...

L. de Broglie: Ah, let’s see. Towards the end of his life he published an article on quanta.

F. K.: But this is something that was also done by Ehrenfest, who also proved that Wien’s law requires quanta.

L. de Broglie: Maybe, yes.

F. K.: But I don’t think this is a new idea about quanta.

L. de Broglie: I don’t think so. In the last thoughts of Henri Poincaré there is an article on this. He did this towards the end of his life after the 1911 Solvay Congress, which he had attended, and he showed that if the balance of radiation is preserved, it is incompatible with continuous physics and that it was necessary to introduce an element of discontinuity. But it didn’t bring anything very decisive, it must have been surprising at that time, when we believed that we could perhaps continue to have continuous physics.

F. K.: But all the same, when you were young, it may have guided you a little...

L. de Broglie: Yes, it must have interested me, certainly, because I read all of Poincaré’s books at that time. This one appeared after his death. The last thoughts are posthumous things.

F. K.: Do you believe that the lack of knowledge of the Courant-Hilbert book and of what we said about geometric optics could have hindered you?

L. de Broglie: It may have hindered me, yes, obviously. I clearly saw the relationship between Fermat's principle and Maupertuis' principle, because Fermat's principle is an optical principle and Maupertuis' principle is a mechanical principle, therefore which fits into Hamilton-Jacobi's theory, but maybe I haven't gone deep enough to see it.

F. K.: On the other hand, the Schrödinger equation takes place in a fictitious space and ...

L. de Broglie: Yes, we can first take it in a physical space for a particle, but the fictitious space has never satisfied me, and I have done great work on this. In particular one of my students, Mr. Andrade e Silva, did a doctoral thesis to show how we can interpret this with our ideas, that is to say that everything happens in a physical space and it is only a certain representation in the configuration space. It is relatively easy, relatively only, in the case of particles which are of different nature. What is more difficult is when the particles are of the same nature, to find the Bose-Einstein statistics and the Fermi-Dirac statistics. But we are still working on it, I started working on it again with him.

F. K.: I believe that the idea of the double solution was already in your mind before Schrödinger's work?

L. de Broglie: Ah yes, I made some little notes in which I developed this idea. After Bohm's work I published a book which is entitled: "Will quantum physics remain indeterminate?" So there, I did a series of things with Jean-Pierre Vigier, and I republished there the few notes to the Comptes Rendus that I had made between my thesis and the article of 27. Here is one, for example, which is dated 26. There, I was trying in the case of light – in short, it was the theory of the double solution, the theory of guidance that I gave there, but in a somewhat restricted form, because I applied it to light quanta, but we already see that it's the same idea...

F. K.: But do you think that, before Schrödinger's work, you had already thought of an idea of guidance? In the thesis it is not explicitly stated...

L. de Broglie: Yes, if you look at my thesis, you see – no, it is not explicitly said, but I had the idea that the particle moved while remaining in phase with its wave, which clearly indicates that there is a trajectory which is fixed by the wave. I didn't say it perhaps in the form of guidance, but you see here, in an older work, I said it (Nov 24, C.R. 179) – even this one, which is a bit earlier, and in it I say it... it is at the time of my thesis. I passed my thesis in November 24, only it's not in my thesis, because it was already printed. And there, you see, what I say gives a bit guidance ideas...

F. K.: And Schrödinger's idea of superimposing waves to form a corpuscle which is the wave train?

L. de Broglie: I completely agree, but on the condition of saying that what exists is the wave train, it is not the individual waves. This seems almost obvious to me from a physical point of view. If you consider a vibrating string, it can vibrate with the harmonics; but if you excite it any way, it will have a very complicated movement. Besides, what exists is the superposition: if you can photograph it, you don't see the components. The components are a mathematical artifice. You can isolate them by a process, for example if you fix certain points of the rope, but then the rope is no longer in its initial state. For me, in a superposition, the components do not exist. What we do in short, when we measure an exact quantity, Mr. Andrade e Silva has also studied it a lot, in quantum physics, we isolate a component, but it did not exist before. And it is precisely this which characterizes the notion of measure in the interpretation of the double solution.

F. K.: But are the wave train and the corpuscle two different things, although related, or are they the same thing?

L. de Broglie: First I talked about the theory of the pilot wave. I took a wave and then a corpuscle which was forced to follow this wave as if it were a law imposed on it. But I came to the idea quite quickly, already at the time of my article in 27, where I had found everything, that the corpuscle was incorporated into the wave, that it is a point of great intensity of the wave, and that's why he is always in phase with his wave, because he is part of it. – And then this idea seemed to me to be the best idea; the first, the pilot wave, is, if you like, a way of saying things more simply to people who want to see things broadly. But the deep idea, for me, was the idea that the corpuscle was a point, a kind of singular point, a singular region in the wave.

F. K.: Is this a wave group singularity? Is it before the thesis or after?

L. de Broglie: There, see, I have the idea that it is a singularity in the group of waves. It's in the C.R. of November 17, 24. I passed my thesis on November 25, 1924!

F. K.: This seems very interesting to me, because the idea of a singularity is different from Schrödinger's ideas.

L. de Broglie: Ah, yes indeed.

F. K.: In your books that you have just published, the idea of a singularity is linked with the proper mass of photons – you correct Maxwell's equations –

L. de Broglie: Ah yes, the photon wave equations? For this to work well, you need to take a small mass. But this is already in my thesis, the idea of a small mass of the photon.

F. K.: And in the thesis, have you ever thought about a connection with singularities?

L. de Broglie: I don't know, I wouldn't dare say that – that there was a connection – in any case, the connection with the variation of mass is very recent, it's thermodynamics, it was after 1960. I had the idea that I develop here, that we must consider that the proper mass is variable and that this is what gives a particular guidance in the case where there is a superposition of waves. When we do not have a monochromatic wave, we have a variation in mass which causes the particle to follow the guidance trajectory. It works very well, and it goes with the Planck-Laue theory, in its relativistic form, it's a sort of consequence.

F. K.: But this idea of a photon's proper mass somehow connects the photon with the electron and...

L. de Broglie: that's it, that's it, because, I was guided towards wave mechanics by Einstein's theory of light quanta. So I started with photons and electromagnetic waves. My idea was that what is true for photons and electromagnetic waves, must be true for the electron and other particles. So, in one case, the particle was missing: in the case of light, since Fresnel, we only saw the wave, we no longer saw the particle, and we managed to make a synthesis between the two. In the case of the electron, it's the opposite, we have the particle, but not the wave, we have to introduce the wave, that's the idea I had at that moment.

F. K.: But the idea of a proper mass of the photon – I don't think anyone else had the idea of a proper mass...

L. de Broglie: No. However, much more recently, in 1950, Mr. Schrödinger made an article saying that there is no contradiction between the introduction of a proper mass of the photon and the theory of black radiation. And that, I had seen it, I think I can show it to you too, I had discussed it in all my books on light, and again recently, in 1949, I had discussed the whole question of proper mass, and I had shown that there was no contradiction with Planck's law. Because one might believe that there is a contradiction. And then, Messrs. Schrödinger and Bass, in the meantime (between the first edition of my book and the second) published an article where they said the same thing. (Proc. Roy. Soc. (A) N 1182, vol 232). They did not read what I had written. I wrote to Schrödinger, and he said to me: Yes, we are in complete agreement. And that he did not know that I had already said it. I say in "Wave mechanics of the photon and quantum field theory" (1949) exactly the same thing. Here I say that we can draw a kind of objection from black radiation. I discuss it and I come to the conclusion that there is no objection, that it can be raised. And so here, I put in the second edition (1957) a note which was not in the first, in which I say that Schrödinger had examined this question at the same time as me.

F. K.: But at the time of your thesis, what guided you, I think if we admit a proper mass...

L. de Broglie: The idea of the proper mass of the photon comes first of all from the fact that, when we give the photon a zero proper mass, we start from the formula

$$W = \frac{m_0 c^2}{\sqrt{1-\beta^2}} \quad (= h\nu)$$

and we make both m_0 tend towards 0, β towards 1 ($v = c$) by saying that in the limit it cancels out. We arrange for the quotient to have the value $h\nu$. I always found it artificial, I said to myself: Mathematically it's good, but physically it must not be true – that we make both the proper mass tend towards 0. So I said to myself: why we would not take a very small proper mass, we would have all the advantages of the proper mass, which would remain, although the proper mass is very small. The objection is that the speed of light will depend on the frequency. But if the proper mass is small, it must depend extremely little, this is a question which I have dealt with several times because there is the question of non-dispersion in a vacuum. Thus, astronomers see a star that is obscured: it reappears. When it reappears, if the violet radiation went faster than the red radiation, the star would appear violet before it was red. But this effect does not exist for stars, and I verified this for a long time, even perhaps at the time of my thesis, in any case since, certainly, I had spoken with astronomers. Then this implies that the proper mass is less than 10^{-45}g. And, on the other hand, considerations that I had developed with Ms. Tonnelat, in particular, led us to wonder if this proper mass would not be - but this is much more hypothetical - linked to the radius of the universe. Then the calculation would show that it is equal to 10^{-65}g. Finally, in any case, I think it is essential to say that it must be less than 10^{-45}g. If it was above, we'd know. Then there were some other objections: I was told that the speed of light is the invariant of the theory of relativity and there must be a well-defined value of the speed of light. But this is not true, there must be a well-defined speed limit, but nothing prevents a particle of extremely small mass from having a speed which is not quite this one. And naturally all the reasoning we make about light signals will remain true at least over distances of the order of the radius of the universe.

F. K.: So it is above all the mathematical process, the passage to the limit which…

L. de Broglie: That's it, that's it. I talked about it in my thesis, I don't know what I said exactly, but I explicitly said why I took the non-zero proper mass. And so in this case, what is also amusing and which amused me a lot in my thesis, is that if we redo the theory of the change of frequency by reflection on a moving mirror, it works by itself from the mechanical point of view as from the electromagnetic point of view, the two cases are absolutely connected. But when we take zero proper mass we can only do it from the electromagnetic point of view, we can no longer do it from the mechanical point of view.

F. K.: This doesn't work with strictly zero mass?

L. de Broglie: Ah, no, because it requires a small shift, precisely, of $h\nu$.

F. K.: I thought that Schrödinger, in an article from 1922, had proven that reflection from a moving mirror…

L. de Broglie: Ah yes, if we do a purely wave theory, that's fine, but I find the same result by the processes of conservation of energy and momentum. In short, to find the same result, by this process, you need a non-zero proper mass.

F. K.: This is very interesting, because, in quantum field theory we have to return to these limit calculations and I believe that we admit a very small mass to avoid certain difficulties...

L. de Broglie: Yes, there are authors, even those who do not at all agree with my interpretation, who return to the idea that it could have a small proper mass.

F. K.: Only to avoid these passage to the limit calculations?

L. de Broglie: Yes, of course.

F. K.: So this is the starting point for admitting that this mass exists?

L. de Broglie: Yes, I think, it is firstly because of this passage to the limit which I have always found a little strange and not very pleasant, and also because I made, in my thesis, a whole series of calculations by purely mechanical processes giving the same result in relativity theory as by a wave mechanical process, and which show that the two cases correspond exactly. You also saw in my thesis, at the end, that I was the first to find Bose-Einstein statistics – only in a form that was not clear enough, perhaps, for us to see that it was something new, but I gave it before Bose!

F. K.: I believe that you have, in short, redone the idea of the mass point which we also use for statistical calculations of the ideal gas.

L. de Broglie: Yes, that's it. In the article I wrote in 22 on black-body radiation I also introduced a proper mass, but then I moved to the limit. But in my notes of 1923 I used the non-zero proper mass.

F. K.: And it's before the thesis and it's still for the reasons you told me?

L. de Broglie: Yes, that's it.

F. K.: This seems very important to me, because it...

L. de Broglie: Yes, that shows you the way…

F. K.: The path followed by your thought.

L. de Broglie: Yes, yes.– I did a lot of other work then, which is not very well known. I wrote a whole book on electronic optics in which I gave a whole theory that is exposed quite rarely, I believe, the theory of what we call Eikonal functions. In the case of electronic optics it is more complicated than in the case of ordinary optics because, there, in ordinary optics, there are five aberrations and I believe that there are seven in electronic optics. Now I also started with discussions on the general ideas of wave mechanics, and in particular I found reasons to suppose that the potentials are physical quantities, which goes with the non-zero proper mass. And at the beginning I gave some examples, that is some reasons, for admitting this. So, I demonstrated a formula which shows that in general we give a formula which is inaccurate to within 50 % – which is not bad – I said to myself that I must be wrong, all the other authors give it. Mr. Dupouy, who is a specialist in electronic optics, gives it in his little book on electronic optics. And then one day I received a letter from a former student, who lives in Grenoble, and he had written a little book on electronic optics in which there is an article where he said that this formula that we find everywhere is 50 % inaccurate. I told him: But I said it in my book! (Laugh)

F. K.: So, regarding singularities, are you familiar with Mie's 1912 paper?

L. de Broglie: Ah, yes, of course. I even did a course on it in which I tried to see what was in Mie's article – I didn't publish this course, because I did 2 or 3 courses there when I started teaching, but I wasn't sure enough to publish them. But I studied Mie's article a little.

F. K.: Did you already study this article before your thesis?

L. de Broglie: Mie's article – let's see – ah, I can't tell you if I had studied it – when is it?

F. K.: from 1912.

L. de Broglie: 1912? I'm not sure I read it.

F. K.: You also cite an article by Bateman in your thesis...

L. de Broglie: Wait – it's not an article, it's a book – I might have it here – where did I cite it?

F. K.: He wrote a book about...

L. de Broglie: I'm sure I read it – but I don't remember quoting it.

F. K.: It's cited in your thesis, but it's an article –

L. de Broglie: Yes, yes, it's an old article in which he considered singularity solutions, I believe.

F. K.: Yes, I think so too.

L. de Broglie: Where do I get this book – it's not a very big book – it's been a long time since I looked at it. I don't know where to find it –

F. K.: I have a little book at home (Electrical and Optical Wave-Motion) which was also published by Dover – on these models of the electron, I believe, and there it talks about singularities.

L. de Broglie: Ah, from whom?

F. K.: From Bateman.

L. de Broglie: But that must be it! It's this one! It's almost certainly the one, yes.

F. K.: I believe he builds a model of the photon on electromagnetic considerations, and he talks about singularities.

L. de Broglie: Yes, yes, I had studied that, but I wasn't able to come to an agreement with my ideas while telling myself: there may be a relationship, but I wasn't very sure of having seen it…

F. K.: You established, in your thesis, a mathematical link between the two principles of Fermat and Maupertuis. Are the ideas already moving towards the double solution?

L. de Broglie: Ah, I think they were already going towards the double solution, since, at the same time, in my thesis, I was a little embarrassed because I had the impression that people were completely skeptical about what I was doing. So I didn't want to be too assertive and I had to include the things that seemed the most certain to me. The others, I put them in the *Comptes Rendus*. But, you know how it happened: I started to show my thesis to my brother. My brother was an experimentalist and he said, yes, that's good, only it's very theoretical, you should do a whole experimental part on it. So I told him: But I can't, I'm not an experimenter, I'll only be able to reproduce what other people have already done. Finally I published the article like this. Which shows you that there was already some reluctance. Then I went to see Mr. Langevin. Mr. Langevin developed electromagnetic theory extensively, but he did not believe in photons, at least at that time. Later he used them. He said: "Oh, photons, you know, we don't really know if they exist, etc." I could see that he wasn't very excited about my thesis. And he was embarrassed, because I asked him to judge it, and that's where he took Einstein's advice, as I recounted in my note on Einstein – and Einstein replied: "Yes, it's very interesting." So, Langevin said: "If the thesis pleases Einstein very much, that means that he may be right." This means that I was not moving forward on very sure ground at that time. I was still very young and it was the first time that I had done something important. I had written small articles. And so, obviously, I may not have said everything in my thesis which I could have said.

F. K.: But this phase wave is somehow already considered as an electromagnetic wave?

L. de Broglie: Yes, but in the phase wave, you see, there is the word "phase" which is curious – I put "phase" because I said to myself: the amplitude that we usually considered is not accurate, there must be the corpuscle somewhere. A constant amplitude, for example in a plane electromagnetic wave, it does not contain the corpuscle, so what is true is the phase, the amplitude... : that's why I took a phase wave. And in the theory of guidance it is the phase which guides – the amplitude ultimately gives by its square only the probability of presence – according to the law of continuity. And then, obviously, I used a slightly strange language, which was abandoned by myself afterwards, I no longer called it a phase wave, which came from what I said to myself: I want to affirm that the phase is exact, I cannot affirm that the amplitude as it is usually constituted is exact.

F. K.: So Rayleigh's idea; I believe that Thomson already speaks of "pulses" in X-rays... and...

L. de Broglie: Ah, yes. It's not exactly the same thing –

F. K.: Is that something else?

L. de Broglie: It seems to me – I haven't studied this theory much, but I think I didn't attach much importance to it because I didn't know, well – I don't think it's a good way.

F. K.: So phase waves were a different thing from ordinary electromagnetic waves?

L. de Broglie: Ah, but for me the phase wave exists for the electron, that is not the electromagnetic wave. The phase wave for the photon could be the electromagnetic wave, but for the electron it is certainly not an electromagnetic wave, it is a generalization. This is the view I still have. I still have the idea that the electromagnetic wave is indeed the wave which corresponds to the photon, but it is a wave which does not have the same characteristics as the wave which corresponds to the electron.

F. K.: Sommerfeld's book: "Atombau und Spektrallinien" influenced many physicists. Léon Brillouin talks about it...

L. de Broglie: Naturally, but I knew about it very early because my brother was very interested in it, because of the X-rays. So I looked at it a lot, with him or alone, and I had learned a lot from Sommerfeld's book.

F. K.: The wave/corpuscle dualism – today we look at it with the eyes of the Copenhagen school...

L. de Broglie: Yes, of course.

F. K.: But for you, at the time of your thesis, as still today, this duality is something else?

L. de Broglie: Yes, I believe that in Einstein's idea it was something else, at the time he wrote his articles, we could see it very clearly.

F. K.: So you are of the opinion that we could establish a physical reality there...

L. de Broglie: Yes, of course.

F. K.: Many physicists talk about the isolation of theorists in France at that time.

L. de Broglie: First of all, yes, there were not very many of us in France, in theory, but then I followed the Langevin seminars for several years after the war of 14. There were still around thirty people who attended. There was Léon Brillouin, who was Langevin's deputy at the Collège de France, and then there were many others, I even see some of them still from time to time. There were mathematicians, like Georges Darmois, who did things that guided me, about whom I spoke in a certain place, because he studied solutions to singularities in general relativity. In Einstein's idea, they represented matter –

F. K.: Was this before your thesis?

L. de Broglie: It was later, in 1927. Darmois wrote an article in the *Mémorial des Sciences Mathématiques* where he summarized conferences he had given in the United States on the theory of general relativity, and then he had the idea that singularities are trapped in geodesics. There is a tube of geodesics, and in the center, we don't really know what's going on, but we know that there is something like a singularity. So that's exactly the idea of the double solution, we have guidance lines, there is a place that traps something, we don't really know what's happening, but where a large amplitude comes from. So I showed in some of my presentations, I insisted a lot on this, on the relationship that there was, which had also been clarified by Mr. Lichnerowicz.

F. K.: Have you discussed your idea with Mr. Darmois?

L. de Broglie: With Mr. Darmois? No, because Mr. Darmois was a professor in the provinces, he came to the Langevin seminary, but we didn't see him much. I didn't have the opportunity to speak to him, but I spoke about it with Léon Brillouin etc. and with Langevin himself. I must say that all the same I worked quite isolated, this is certain, not completely, but still enough.

F. K.: Did Mr. Brillouin defend your thesis?

L. de Broglie: Mr. Brillouin recognized the interest of my thesis, and he especially developed the mathematical side, but I had benefited a lot from certain books by Mr. Brillouin, in particular a book that he published on the theory of quanta, in which he showed the relationship between thermodynamics and quantum theory, and I read this book a lot, it taught me a lot.

F. K.: Is it by Marcel Brillouin?

L. de Broglie: No, it's the son. Marcel Brillouin, the father, had an idea that I cited somewhere, which has a vague resemblance. He wondered if in the atom there wasn't a point that describes something, and if it wasn't a question of a bit of resonance, if you like, that's a bit of the idea, but it was very vague, he had an article on it, but very vague.

F. K.: You mention it – I also believe that Max Jammer in his book (*The Conceptual Development of Quantum Mechanics*) talks about it – but it's a rather vague resemblance...

L. de Broglie: Yes. I even think I cited it to please Léon Brillouin, (laughing) who said to me: My father did something similar. So I said yes, there is some resemblance, and I'm going to quote it.

F. K.: At the time when Mr. Brillouin said to you: My father did something similar – so you had already developed your ideas –

L. de Broglie: Yes, I think it was after my idea. Perhaps Mr. Brillouin, the father, had sent me his article which I did not know, where I could see that in fact there was a resemblance – but it is very vague.

F. K.: But then we can't talk about "precursor"?

L. de Broglie: I don't really believe that we can't say that he is a precursor. There is a vague analogy.

F. K.: So we can't say that Mr. Brillouin guided you!

L. de Broglie: Ah, no, I didn't work with him at all.

F. K.: And Mr. Léon Brillouin who was in Germany – could he have helped you?

L. de Broglie: Mr. Léon Brillouin had been in Germany in 1912 with Sommerfeld – I did not know him at that time – I knew him during the war because he was also in the military telegraphy service as me, but he was not with me, he was in another department. So I saw him there, but very little. I especially saw him after the war when I took courses and meetings by Langevin, I believe he was Langevin's adjunct, he was his assistant. He was young then, so he was still there, and I saw more of him. Then I was with him at the Henri Poincaré Institute because when we created the I.H.P. we did a chair and a master's degree in theoretical physics. And as Mr. Léon Brillouin was in teaching before me, and he is also a little older than me, he received the chair and I received the master's degree. This lasted about two years. And then Mr. Brillouin the father was retired from the Collège de France and then his son came to tell me: "Listen, I would like to replace my father at the Collège de France, for the following reason, that my father is used to come to his office at the Collège de France, and if it's a new professor, he will be very annoyed, so if it's me, it wouldn't bother me at all." So I said to him: "Well , listen, go to the Collège de France, I stay at the Institut Henri Poincaré." That's how I stayed at the Henri Poincaré Institute. I would have done better to go to the Collège de France, because it was better. Especially at the end of my career at the Henri Poincaré Institute, I was overwhelmed by the number of students, the number of theses, etc., which created discomfort, because with the multiplicity of things. On the other hand, he no longer had to take care of the students…

F. K.: Do you think that Mr. Léon Brillouin could have given you some advice?

L. de Broglie: I don't think – no – he was more oriented towards mathematical formalism – especially in the physicist sense – while remaining a physicist, but finally – you read his book on quantum theory – he was very good, but these were not at all the questions of wave mechanics, simply the formalism of quantification in the form of Bohr-Sommerfeld, this was before the theories of my thesis.

F. K.: But this isolation still handicapped you, compared with the work that was done in Göttingen or Copenhagen.

L. de Broglie: I don't think so. Isolation – I've thought about this a lot – has pros and cons. It has the disadvantage that we don't know enough things, there are things that escape you. It has the advantage that we can be much more original, because we are not guided by the idea of saying yourself, what I think there would not please everyone.

F. K.: Maybe your idea needed some isolation, because it's a very fundamental change.

L. de Broglie: Yes, it depends on the characters too. There are people who like to choose ideas, there are others who like more personal reflection.

L. de Broglie: I can show you Léon Brillouin's book.

F. K.: The Brillouin book that you cite in your thesis?

L. de Broglie: There it is, it is from 22. It was a very well done presentation, moreover, very clear, it was a presentation of Bohr's theory, of quantum theory as it existed before my thesis. This stopped with Sommerfeld's theory. He wrote a very nice book on tensor calculus and its applications to physics, but it was more mathematics, if you like.

F. K.: I think I asked you the essentials. So, in summary, if we want to understand the progression of your thought, we must first of all look at the ideas of the double solution.

L. de Broglie: Yes, of course.

F. K.: In an interview with L. Brillouin that the Americans did about your thesis, he talked about your ideas of the double solution, but they deleted it, suggesting that it was not interesting...

L. de Broglie: Yes –

F. K.: This is something that surprised me. He also talks about a memory – that one day you were in front of a table with elementary particles – alpha and beta lines, which are deflected in a magnetic field in different directions, and he says that you said: we must have something in common between these particles – and that was the starting point. Is this a fair idea?

L. de Broglie: It is perhaps possible that I made such a presentation, because I made presentations at Langevin, which he attended. I don't remember exactly anymore.

F. K.: He also talks about the idea of Nadelstrahlung.

L. de Broglie: I still wonder if there is not something true in the Nadelstrahlung – not in the form of an isolated straight line, but of a train of waves which would be emitted in a small cone. .

F. K.: But this idea of Nadelstrahlung is also the line you followed?

L. de Broglie: I'm not sure about the idea of Nadelstrahlung, but there is something that could be introduced into my theory... it's something I don't dare say yet at the moment... I have talked about it often because I have students with whom I exchange ideas on the hypotheses that I have, and so I am free to say what I want, but I have never written it down. There are some reasons to think that it may be like that, because, if it is not like that, there is an objection that has been made for a long time. You have a star in which there is an emission of a wave, then this wave will become spherical larger and larger, and then it will reach the earth, then the photon could manifest itself there, but there will be almost zero intensity. So, we say: It's probability – it's not the wave train, it's just probability. But we can wonder if there is not a process which is not contained in the usual linear theory of the propagation of light, and which would be precisely like this. What would be isotropic would be the set of a very large number of emissions, but not a specific emission. It's possible.

F. K.: Perhaps these notes that you took during Langevin's classes, you don't think that would be of interest?

L. de Broglie: I don't think this would interest you very much – because these are notes that I wrote quickly – but I myself was surprised to find demonstrations of relativistic thermodynamics there, but anyway, it's very classic, there is absolutely nothing very original in this course. He was very clear, he was very good. His students tried to get him to write these lessons, but they never could. He always said: I'll think about it again...

F. K.: But it's a shame, because these seminars are very important for theoretical physics in France.

L. de Broglie: Of course. He was pretty much the only one who knew about quantum physics and relativity –

F. K.: But unfortunately, he didn't keep his manuscripts –

L. de Broglie: I don't think so. It would be his children who would have it. But they would have published it.

F. K.: It would be very interesting if you could give your course essays!

L. de Broglie: Look, I'll watch it again to see, but I think it's a bit shapeless, we write quickly, there are skipped sentences, things like that...

F. K.: But at least that would give something. otherwise, nothing remains.

L. de Broglie: I'll look. There are several notebooks... there are even the dates, when I wrote it down – between 1919 and 1925, roughly, I even continued a little bit after my thesis. The only qualification I make is that it was not an essay.

The interview ends here. The main ideas were discussed later in several letters. Parts of the interview were cited in the thesis of interviewer F.K. (1970).

References

[1] Kubli, F. (1970): *Louis de Broglie und die Entdeckung der Materiewellen.* Archive for History of Exact Sciences **7**,26.

[2] Kubli, F. (1973): *La thèse de 1924.* Dans: *Louis de Broglie, sa conception du monde physique,* Gauthier-Villars, Paris

[3] Kubli, F. (1975): *A propos du 50ème anniversaire de la mécanique ondulatoire.* Rev. Hist. Sci. **28** 97.