OLD AND NEW QUESTIONS OF PHYSICS

BY

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SPEECH

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My Lords Curators, Ladies and Gentlemen teachers and students and Thou all, who honour this ceremony with Thy presence,

Dear listeners,

On this occasion of the inauguration of the new chair, named after Lorentz, I thought it would be appropriate to take the title of my speech from Lorentz as well and talk about: "Old and new questions of physics". This is meant in all modesty. You will not expect from me the mastery with which Lorentz, in his "*Alte und neue Fragen der Physik*", dealt with the burning questions of relativity and quantum theory at that time (i.e. 1910). But I can perhaps give you an impression of some recent problems, the development of which I have witnessed at close quarters. And I also hope to make you feel something of what Leiden physics, and especially the unforgettable trio Lorentz, Ehrenfest and Kramers, has meant to me personally.

Let me start by saying that the title should not suggest to you that physics consists of a number of loosely connected problems. The miracle of physics is the unity, the system and the great summarizing theories. In this system there are of course many gaps, which one can work on all one's life, but there is also a more or less clear boundary, a "frontier", and therefore there is always a central problem. In my student days, this was the problem of atomic structure and quantum mechanics; today, it is undoubtedly the problem of so-called elementary particles. What is the structure (if one can talk about it) of the last building blocks of matter: the electron, the proton and the neutron, and what is the connection with the many new, short-lived particles discovered in the last decade or so? I will therefore begin with some points of the development of this new question of physics. and I hope you will excuse the personal manner in which I will do this.

In the 1920s, the only elementary particles were the electron and the proton. Much had been written about their structure, especially concerning the electron, the Lorentz electron. Was this a tiny ball of negative electricity? By what forces, then, was it held together? Was the origin of its mass of an electromagnetic nature, caused by the entrainment of the electromagnetic field generated by the charge, or did the electron also have a "real" mass? All these questions, however important, were nevertheless at the limit of scientific attention at that time. The central problem was the structure of the atom, and for this the electrons could easily be regarded as charged mass points. The planetary model of the atom developed by Rutherford and Bohr was known to every student. We knew that the various chemical atoms consisted of a heavy nucleus, around which circled a number of electrons. Nor were we worried about the structure of the nuclei; they had to be composed of protons and electrons in one way or another. The burning question was, what were the laws governing the motion of the electrons? Bohr had taught us that these laws had to be very different from the well-known Newtonian mechanics, that there were discrete states to be selected from all possible mechanical orbits by the so-called quantum conditions, and that the motions of the electrons could suddenly change from one state to another (the so-called quantum jumps) in which electromagnetic radiation was then either absorbed or emitted. On this basis, one could determine, albeit mostly only qualitatively, the structure of atomic spectra, the chemical properties, the laws of X-rays, etc., could be understood. It was clear that one was at the beginning of a great new synthesis. But it was very difficult! Quantitatively, one really only understood the hydrogen spectrum. It seemed that one had to be well versed in theoretical mechanics - most theorists of my generation have one of the handbooks on the '*Mécanique Céleste*' in their bookcase - and that one also had to be well versed in the large experimental material collected on the atomic spectra. And this was indeed necessary, albeit that by going deeper, the final solution, quantum mechanics, turned out to be simple again.

Here in Leiden, Lorentz and Ehrenfest naturally took an active part in all these great developments, and as a student one was immediately immersed in them. The highlights of the week were Lorentz's Monday morning lecture and the Wednesday evening colloquium led by Ehrenfest. One had to go there even if one understood little of it, which was usually the case especially at the colloquium in the beginning. (At Lorentz's lecture, everything always seemed perfectly clear, even if one could often recount little of it!) But after a while one learned the "jargon", and thanks to the way Ehrenfest could summarize (which sometimes even made the speaker understand!) one soon gained a sense of confidence. On this occasion, I would like to mention the, in my opinion, unsurpassed training Ehrenfest gave his students. He actually always worked with one student every afternoon. The biggest sin was to say one understood something when that was not the case, and the miracle was that after a year one worked together almost as equals. Yes, you sometimes thought as a student that you actually knew better! Then one stood on one's own two feet, and had gained courage for one's own work. Courage is one of the main qualities the scientific researcher must have, and the method of

Ehrenfest is the only way I know of to instill it in a young student. Unfortunately, it requires not only the great didactic gifts of an Ehrenfest, but also the, in my opinion, ideal ratio of professors to students (i.e. one to one), which seems to be increasingly a thing of the past.

If there were several students, Ehrenfest strongly encouraged them to work together. And so it happened that in 1925 I came into closer contact with Goudsmit. I had been in Italy for some years, and although I had done the doctoral examination, I didn't really know anything. Goudsmit had specialized early on, had already published several articles on atomic spectra, of which he was a connoisseur, and he was given the task by Ehrenfest of bringing me up to date. Goudsmit did so with great dedication. Throughout the summer of 1925, three afternoons a week, he told me everything he knew. It turned out that we complemented each other well, and we therefore went ahead and tried to digest the latest articles by Bohr, Pauli, Landé and others We did not understand much of it, and sometimes it seemed to us pure abracadabra, a kind of magic game with the quantum numbers.

We now know that the obscurity of the theory of atomic structure at that time was due to two causes. First, the true mechanics, quantum mechanics, was not known. This required a revision of the foundations of natural description, as thorough as proved necessary for the theory of relativity. This happened in the great "breakthrough" of 1926 through the work of Heisenberg. Schrödinger and Dirac. The second, less fundamental cause was the incompleteness of the planetary model of the atom. Apart from the "annual" movement of the electrons around the nucleus, it must be assumed that the electrons also exhibit a "daily" rotation. This is the electron's spin hypothesis.

Goudsmit and I arrived at this idea by studying a paper by Pauli, in which the famous exclusion principle was formulated, and in which four quantum numbers were assigned to the electron for the first time. This was done *entirely* formally; no representation was attached. It was a mystery to us; we were so familiar on the one hand with the proposition that each quantum number corresponded to a degree of freedom, and on the other hand with the idea of the point electron that apparently had only three degrees of freedom, that we did not know our way around the fourth quantum number. Only if the electron was really a sphere that could still rotate did it become comprehensible to us. The fourth quantum number was then linked to this rotation. A bit more technically, it was clear to us, that we had to assume:

1. each electron rotated such that it possessed half a quantum unit ($\frac{1}{2}\hbar$) angular momentum, and
2. each electron therefore also possessed a magnetic moment, whose ratio to angular momentum was two times greater than the well-known $e/2mc$, which applied to orbital motion.

The first assumption was necessary to explain that only two orientations of the axis of rotation relative to the plane of orbital motion were possible. Due to the magnetic moment, these two orientations would have somewhat different energy and this would then be the origin of the alkali doublets. We knew that the dependence of the doublet splitting on the nuclear charge would then be convenient, and we thought that the fine structure of the hydrogen spectrum was also in harmony with our picture, although we did not see it in detail because the calculation seemed very difficult to us. The second assumption (the so-called gyromagnetic anomaly) was needed to explain the anomalous Zeeman effect. We found a bit later in an old piece by Abraham (to which Ehrenfest drew our attention) that for a rotating sphere with surface charge, the factor of two needed could really be understood classically. This encouraged us, although our enthusiasm was greatly dampened when it turned out that for the rotation required, the velocity at the surface of the electron had to be many times that of light!

I remember that we came up with most of this one afternoon late in September 1925. We were somewhat excited, but did not *think of* publishing anything. It seemed so speculative and bold, that something had to fail, especially also because Bohr, Heisenberg and Pauli, our great authorities, had never proposed anything like this. But of course we told Ehrenfest. He was immediately impressed, especially, I believe, by the vivid nature of our supposition, which was so entirely in his line. He made us aware of all sorts of things (among others, that in 1921 A. H. Compton had already suggested the idea of a rotating electron as a possible explanation for the natural unity of magnetism) and finally said that it was either very important, of nonsense, and that we should just write a short communication for *Naturwissenschaften* and give it to him. He concluded with: "*und dann werden wir Herr Lorentz fragen*" [and then we should ask Lorentz]. This happened. Lorentz received us with his well-known great kindness, and he was full of interest, though I believe also a little skeptical. He would think about it. And really, already the next week he gave us a whole manuscript written in his beautiful handwriting with long calculations on the electromagnetic properties of rotating electrons. It went a bit too high for us, but it was clear, that if one took the picture of the rotating electron seriously, seen from the classical point of view, there were serious difficulties. One of them (later also noted by Fermi) was, that the magnetic energy was so large that with the known size of the electron due to the equivalence of mass and energy the electron would have greater mass than the proton. or if one stuck to the known mass the electron should be thought of as large as the whole atom. Either way, it seemed nonsense! Goudsmit and I both thought, it might be better if we published nothing for the time being. But when we mentioned this to Ehrenfest, he replied:

“Ich habe ihren Brief schon langst abgesandt; Sie sind beide jung genug om sich eine Dummheit leisten zu können"! [I alread sent off your Letter; You are young enough to permit yourself a foolishness!] Our communication appeared in late October, and it immediately attracted attention. Some days after it appeared, we already received a letter from Heisenberg, in which he wrote that with our model the doublet splitting in the alkali spectra looked good apart from a factor of two, and he asked our opinion on this difficulty. We had not yet derived Heisenberg's formula ourselves, but when we knew it could be done, we succeeded after hard work, and we got the same wrong extra factor two. This difficulty was clarified some months later by Thomas, and has since been called the Thomas factor two. In late November, Bohr came to Leiden for the celebration of Lorentz's golden doctorate, and we spent many hours talking to him then. Bohr was not worried about the difficulties associated with the classical description of the spin, and he was actually immediately convinced. He urged us to discuss precisely the fine structure of the hydrogen spectrum in particular, and he helped us to draft a second 'Letter to the Editor' (this time to Nature), which consequently showed strong characteristics of his style. It appeared in January 1926, and since then the idea of spin has been widely accepted. Pauli was one of the last to accept it, although it was only a model to make the four quantum numbers he had postulated understandable.

I have gone into such detail about the discovery of the electron spin, partly because my memories of it may be of some historical interest, but also because it showed that the elementary particle (for it soon turned out that the proton too had a spin ½ ħ) had more attributes than just mass and charge. It is a first indication of the problem of the structure of the elementary particle, which certainly could not be understood on the basis of classical theory.

The further development I will only very briefly outline. In 1926 came the great synthesis of quantum mechanics. It was such a profound experience that for my generation it defined the whole conception of science, and one always hopes for a repetition of such an explosion of insight! The idea of the spin of the electron was adapted to quantum mechanics by Pauli in 1927 and spin was thereby given a complementary interpretation viz. as the polarization of matter waves. This led Dirac in one of the most profound treatises of this century to the so-called relativistic theory of the electron, which explained the spin of the electron in a sense. It led to the prediction of the positron, which was discovered soon after (in 1932) by Anderson and Blackett. In the same year, the neutron was found by Chadwick, thus increasing the number of elementary particles to four already. To this was added before the war the meson and perhaps the neutrino, while after the war the number rose rapidly and is already about 20. It would take me much too far if I wanted to tell you the history of discovery, indeed just the properties of all these new particles. Let me suffice to say that all these particles have a more or less short lifespan – the only stable ones remain the electron, the proton and perhaps the neutrino –, that there is a genetic link between the particles that points to new selection rules, and that one gets the impression that one can distinguish three families of particles and also three types of interaction. In short, it is clear that one is facing an entirely new world. The research is almost entirely in the hands of the experimenters. I think most theorists would agree that the present methods of quantum theory have lost their power. If one knows the properties of the elementary particles, one can more or less describe their further behaviour, but one lacks the predictive power regarding these properties. Unfortunately, experimental work is very expensive. Although one has learned a lot and will learn much more from cosmic ray research, surely the future lies with high-energy machines. And that is what is called million dollar physics in America. But there is no escaping it, and anyone who has visited the cosmotron laboratory in Brookhaven, for instance, will have felt the fascination that emanates from all those groups of enthusiastic physicists working around such a monster machine at the frontiers of science.

On a recent phase of the development of quantum mechanics I would like to elaborate, partly because for a while it seemed to be the dawn of a new synthesis (and perhaps it is!), but also to bring out more clearly the role that Kramers played in this, which may not have been sufficiently appreciated. My focus here is on the so-called renormalisation programme of field theory. The application of quantum theory to the electromagnetic field is almost as old as quantum theory itself. It was already necessary for the derivation of Planck's radiation law. However, its systematic application did not occur until 1927 in Dirac's radiation theory. This quantum electrodynamics gave the mathematical description of the photon-wave dualism of the electromagnetic field. To express the dualism material particle-the Broglie wave, one had to analogously consider the wave equations of matter as classical field equations and apply quantum theory to them. In this description, the interaction of electrons with the electromagnetic field became the interaction of two quantized fields. Through the work of Pauli, Weisskopf, Yukawa et al, this method was extended so that to each elementary particle a quantized field could be added that depended on the nature (charge, mass, spin) of the particle. Around 1940, this seemed to be the final form of quantum mechanics.

At this time, Kramers was writing the second part of his major textbook on quantum mechanics and in it he naturally had to account for the aforementioned turn of the theory specially with regard to quantum electrodynamics. It was a real struggle! I can still hear him sigh, that he just could not understand why Dirac's theory of radiation was so good, even though it did not sufficiently distinguish between the electron's own field and the external field, even though it did not account for the infinite electromagnetic mass. and even though its correspondence with the classical theory of the electron was completely unclear. Kramers, with his deep knowledge of both classical and quantum theory, found especially this last point highly unsatisfactory. He was also earlier and more aware of the imperfections of the theory than the majority of his contemporaries. Quantum electrodynamics *was* imperfect; one had to calculate with tact to avoid bumping into the infamous divergences, but most physicists (myself included) did not worry much about this.

The matter is unfortunately too technical to go into details and I can only tell you the external history. In his textbook, published in 1938, Kramers could only give another form to the quantum theory of the radiation field, which at least made it clear why in many cases Dirac's theory gave the right answer. However, he was aware that this form was not equivalent to Dirac's theory and that it might lead to deviations, which could be tested experimentally. During the war, he continued to work on this constantly, and so when the first of a series of conferences on field theory took place in America at Shelter Island in spring 1947, Kramers was able to fully recite his ideas at least in the unrelativistic form. It made a very big impression, especially also because at the same conference Lamb told about his measurements, which showed for the first time definitively that there were deviations in the fine structure of the hydrogen spectrum from the existing Dirac's theory.

One might have expected that the refinement of the theory as begun by Kramers would be able to account for these deviations. This expectation was completely fulfilled. By Schwinger, Feynman and Dyson, Kramers' ideas were taken up with vigour, and at the following conferences (Pocono 1948, Oldstone 1949) the complete relativistic theory could already be recited. The so-called Lamb shift was completely explained. It is one of the great successes of post-war physics, where one does not know what to admire more, the sophistication of the experiment or the power of the theory. Added as a second great success was Schwinger's prediction that the gyromagnetic ratio of the electron had to deviate slightly from the value two postulated by Goudsmit and me, a prediction confirmed experimentally by Kusch and Foley. In short, it seemed, as I said, the dawn of a new synthesis. It therefore inspired a whole generation of young theorists just as quantum mechanics did in 1926. Unfortunately, the great expectations were somewhat disappointed. In particular, its application to meson fields proved to be extremely difficult, and although one cannot yet say for sure, that for the meson field the theory and experiment are in contradiction, a rather large dose of optimism is needed to maintain confidence in the theory. Moreover, as I mentioned earlier, as far as elementary particles are concerned, the theory has a descriptive character, and therefore loses much of its predictive power. It is clear that one has made a considerable step forward, but it is also clear that much is still missing from the theory. By splitting off the infinities – and that this can be done in an unambiguous way for the electromagnetic and some other fields is one of the main achievements of the theory – one has already cut off the questions about the structure of the elementary particles. With this problem, in my opinion, one can only move forward by careful groping speculation on the basis of the experiment. The recent work of Pais and

Gell-Mann provides an example of this. One can think differently about it – I myself have a lot of sympathy for it – but surely it is certain that the future synthesis, for which we have been waiting for so long, will not come merely through the expansion of the mathematical apparatus of present-day field theory. but that new physical thoughts are needed. And to find these, the young theorist will not lack a certain courage and also naivety. Perhaps he can learn from the history of the discovery of the spin, which I have just outlined.

Finally, I would like to discuss an old question of physics that I have been working on in recent years. It is the so-called condensation problem. Can one understand exactly why a gas below a certain critical temperature can always be condensed to a liquid by compression? It is a question from statistical physics, i.e. the part of physics whose task is to explain the macroscopic properties of matter from its known assumed molecular constitution. Most of the gaps in the system of physics are formed by the unsolved problems of statistical physics. These include the well-known problems of superconductivity and phenomena in liquid helium, but they also include the much less obvious questions on the explanation of so-called phase transitions. Why does a solid melt at a sharply defined temperature; why does a vapour condense at a sharply defined density, etc.; one can easily multiply the number of such questions, especially if one also considers magnetic and other types of transitions. Let me emphasize here that for almost none of these commonly known phenomena exists a rigorous explanation, even though the basis from which one must start has been known for more than 50 years. Of course, this is not to say that one does not have qualitative explanations, which are often of great value in statistical physics, in contrast to the problem of elementary particles, however, in my view the task of the theorist lies in the development of exact methods, in the elaboration of the mathematical apparatus. At least for me, the great charm of statistical physics lies in its connections with areas of mathematics with which one otherwise rarely comes into contact.

But let me come back to the condensation problem. Its qualitative solution was already given in 1873 in Van der Waals' famous dissertation as a consequence of the well-known equation of state. It took a long time to escape the grip of this thesis. Van der Waals's physical ideas were so clear, and the agreement with experiment, though quantitatively never exact, was qualitatively so beautiful, that not much was left to be desired.

The modern phase of the problem began when Kamerlingh-Onnes abandoned the Van der Waals' equation of state to describe the experimental material and used instead the systematic so-called virial development. He was well aware that the successive virial coefficients were a measure of the interaction of molecules in pairs, triplets, etc., and that therefore the relationship with intermolecular forces could be formulated much more precisely. The general theory of virial coefficients was developed by Ursell and Mayer around 1930. When with this came the insight that a phase transition (like condensation) could only be understood mathematically as a limit property of the state integral – a point which to my knowledge was first noted by Kramers at the Van der Waals congress in 1937 – the condensation problem could at least be formulated mathematically exactly. When I was still in Utrecht, I did this together with Kahn. It turned out that condensation was possible and, at sufficiently low temperatures, also quite probable, but the final solution still seemed a long way off.

For me, the stimulation to think about the condensation problem again came from an article by Kirkwood. In it, it was deduced on an approximate assumption that there was a phase transition even if there were *only* repulsive forces between molecules. Physically, this seemed highly paradoxical, and I just couldn't believe it. It seemed important to me to see whether one could rigorously prove or rigorously refute Kirkwood's claim for the following reason. It is well known that one can bring helium to the solid state far above the critical point by compression. One even gets the impression that this will be possible at *any* temperature if only the pressure is made high enough. Something similar has been found for water dust, so it may be a general property, valid for all gases. If so, then the explanation must rest on a general property of intermolecular forces. The sharp repulsion, operating at small distances *is* such a general property, and so perhaps Kirkwood's phase transition could explain the always possible condensation of the fluid to the solid phase.

Unfortunately, the matter is again too technical to explain in detail to you the results of the attempts, which I have made with Riddell and Ford in this direction. Let me suffice to say that we have still not managed to prove nor disprove Kirkwood's suspicion, but we have made progress, so I am rather optimistic that the matter will be decided soon. It might also be interesting to mention that the mathematical problems that emerge all have to do with the theory of so-called linear graphs, a theory already started by Cayley. In my opinion, it is the mathematical counterpart of the physical theory of phase transitions, and one has here perhaps another example of the harmony between mathematics and physics that is so dear to my heart.

First of all, I would like to express my respectful thanks to *Her Majesty the Queen* for my appointment as Extraordinary Professor at this university.

My Lords Curators. I feel it a great honour, that you have agreed to nominate me as the first occupant of the Lorentz Chair. The idea of alternating visiting professorships in theoretical physics seems to me very happy. Especially in America, but also here, there is so much as it were floating information, which is unpublished or poorly published. that a personal contact is of great value. I will do my best to start this new venture on the right foot.

I would further like to express my gratitude to the Trustees of the Lorentz Fund, who, in part thanks to support from industry, have made this Chair possible; to the Regents of the University of Michigan for giving me leave to accept this appointment, and to the Department of State for awarding me a Fulbright travel grant.

Ladies and Gentlemen Professors. Although I will only be in your midst for a short time, I would still like to tell you how delighted I am to spend some time teaching here, and thereby perhaps get closer contact with some of you. Leiden University is not only my alma mater, but I have always been connected to it by family ties, as it were, first through my uncle C. C. Uhlenbeck and now through my brother E. M. Uhlenbeck. The only thing that makes me wistful is the memory of my teacher Ehrenfest and my friend Kramers. How highly I rate their merits has, I hope, been shown by my speech. What they have meant to me humanly I find it hard to put into words.

Dear Gorter and dear De Groot. I know that this chair came about through your initiative. I can only say: I hope that I can be of some use for you and your collaborators. I myself will certainly learn a lot from the new work in Leiden and from the contact with Dutch physicists.

Dear Fokker. You are about the only one of the old guard of Dutch physicists, to whom we all owe so much, who is still in office. It is a great pleasure for me that we will be colleagues at least for a short time and will see each other regularly.

Dear Casimir. We have known each other since you came to study with Ehrenfest as a young student, and we have often had occasion, both here and in America, to renew our friendship. I hope this time will bring such an opportunity again.

Ladies and Gentlemen Students. Officially, you will have little to do with me, and I would therefore like to offer just one piece of advice. A visiting professor is someone who has thrown off the responsibilities of his office in his own university, and who therefore has very little to do. Make use of that!

I have spoken.