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Hendrik Antoon Lorentz's struggle with quantum theory

A. J. Kox

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Abstract A historical overview is given of the contributions of Hendrik Antoon Lorentz in quantum theory. Although especially his early work is valuable, the main importance of Lorentz's work lies in the conceptual clarifications he provided and in his critique of the foundations of quantum theory.

1 Introduction

The Dutch physicist Hendrik Antoon Lorentz (1853–1928) is generally viewed as an icon of classical, nineteenth-century physics—indeed, as one of the last masters of that era. Thus, it may come as a bit of a surprise that he also made important contributions to quantum theory, the quintessential non-classical twentieth-century development in physics. The importance of Lorentz's work lies not so much in his concrete contributions to the actual physics—although some of his early work was groundbreaking—but rather in the conceptual clarifications he provided and his critique of the foundations and interpretations of the new ideas. Especially in his correspondence with colleagues, such as Max Planck, Wilhelm Wien and Albert Einstein, he time and again tried to clarify the quantum principles and explore their consequences.

In this paper I will give an overview of Lorentz's work in quantum theory, including his informal contributions in discussions and correspondence. I will first discuss Lorentz's early work on radiation theory, in which he gives a derivation of a radiation law from classical electron theory. Then I will discuss Lorentz's 1908 Rome lecture

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in some technical detail, because the theory Lorentz develops in the lecture is a daring new application of Gibbs's statistical mechanics and because its outcome is that the most general classical theory one can think of leads inescapably to the Rayleigh–Jeans radiation law. A key element in this derivation is the theorem of equipartition of energy.

The Rome lecture was widely discussed, both in correspondence and in papers. As we will see, in the discussions with Lorentz and also in his later work three questions will keep coming back. The first one is the validity of the law of equipartition of energy and its possible modifications. A second theme, how to deal with the quantum discontinuity (also discussed in the lecture, although less explicit), would soon evolve in the specific question of where to localize the discontinuity: in the ether, in the interaction between matter and ether, in the resonators or elsewhere. A third theme, not explicitly present in the Rome lecture, but gaining much prominence in later work, is the matter of the light quantum. Do independent light quanta exist, and if so, how to reconcile their existence with typical wave phenomena such as interference? This last theme is in particular discussed in correspondence with Einstein.

The paper concludes with a discussion of the way Lorentz dealt with the parallel developments of matrix mechanics and wave mechanics. He carefully studied both theories but engaged with them in totally different ways: he extensively discussed and criticized wave mechanics, especially in correspondence with Erwin Schrödinger, whereas in the case of matrix mechanics he limited himself to trying to master the formalism and grasping its consequences. Although he had much respect and admiration for the work of the younger generation, he remained critical and to the end couched his quantum work in classical terms.

2 Early work on radiation theory

Lorentz's work on radiation theory is characterized by the same methodological consistency that we find throughout his work and that culminated in his mature theory of electrons in the first decade of the twentieth century. He bases himself on an ontology of particles and fields, or, to use his own terminology, on a strict separation of matter—consisting of charged and neutral particles—and ether. The latter acts as carrier of the electromagnetic action, caused by the presence of charged particles. Ether and matter are separate entities, that act on each other, but must be treated differently. The particles obey the laws of classical mechanics; the ether is governed by Maxwell's equations.[1]

The first paper I want to discuss dates from 1903, and has the title: "On the emission and absorption by metals of rays of heat of great wave-lengths" (Lorentz 1903). Lorentz bases himself on the theories of Paul Drude and Eduard Riecke for the electrical conductivity of metals, in which it is assumed that metals contain large quantities of freely moving electrons, that regularly collide with the metal atoms. Applying his electron theory to this model, Lorentz calculates the energy density of radiation emitted by the electrons by assuming that they only radiate when colliding with atoms,

[1] In Lorentz's early work the ether is still treated as a mechanical system, but gradually it loses all of its mechanical properties save one: its immobility. See Kox (1980) for further details.

limiting himself to the case of long wavelengths. In a dazzling show of mathematical prowess he finds that the radiation is distributed according to the Rayleigh–Jeans law (which, of course, was not yet known by that name in 1903) and also that it conforms to the long wavelength limit of Planck's brand-new radiation law:

$$f(\lambda, T) = \frac{8\pi kT}{\lambda^4}. \tag{1}$$

In this paper Lorentz makes his first comments on Planck's quantum hypothesis. He writes:

> […] the hypothesis regarding the finite "units of energy", which has led to the introduction of the constant h, is an essential part of the theory; also that the question as to the mechanism by which the heat of a body produces electromagnetic vibrations in the aether is still left open. Nevertheless, the results of Planck are most remarkable.

And later on, when comparing his result with Planck's work, he comments:

> There appears therefore to be a full agreement between the two theories in the case of long waves, certainly a remarkable conclusion, as the fundamental assumptions are widely different.

It is interesting to note that in his paper Lorentz characterizes Planck's theory in the following way:

> It will suffice to mention an assumption that is made about the quantities of energy that may be gained or lost by the resonators. These quantities are supposed to be made up of a certain number of finite portions, whose amount is fixed for every resonator; according to Planck, the energy that is stored up in a resonator cannot increase or diminish by gradual changes, but only by whole "units of energy", as we may call the portions we have just spoken of.

According to Thomas Kuhn, this is an "anomalous" reading of Planck's work, that Lorentz corrected later.[2] Closer study of Lorentz's published and unpublished utterances shows, however, that Lorentz never strayed very far from this interpretation. For instance, in his 1908 Rome lecture (see Sect. 3), he uses almost identical words, whereas Kuhn uses a quotation from a letter from Lorentz to Wilhelm Wien (to the effect that processes in the ether take place in a continuous way), which was written a bit after the lecture, to argue that Lorentz had dropped his earlier interpretation.[3] What Lorentz was uncertain about, as will be shown in Sect. 6 below, was where the discontinuity lay: in the interaction between ether and matter (i.e., the resonators) or in the interaction between the resonators and the other matter.

[2] See Kuhn (1978, p. 138).

[3] See Kuhn (1978, p. 194). The letter in question is dated 6 June 1908; it is reproduced in Kox (2008) as Letter 171. See also Sect. 4.1 below for a further discussion of the Lorentz–Wien correspondence.

3 The Rome lecture

From Lorentz's later work on radiation theory, it becomes clear that the paper discussed in the previous section sets the methodological stage for later developments along similar lines and that its critical tone about the mystery of the underlying mechanism in Planck's theory of energy elements remains an important theme.

The first important one of the later papers is the 1908 Rome lecture. It was delivered at a somewhat strange venue, namely, the 4th International Congress of Mathematicians in April 1908. Its impact has been considerable, if only because it made indisputably clear that Planck's energy elements were fundamentally foreign to classical mechanics and electrodynamics.

The lecture, entitled "The distribution of energy between ponderable matter and ether",[4] starts with a lengthy and very general discussion of the foundations of mechanics, kinetic gas theory, and electrodynamics, clearly meant for an audience of non-physicists. It is useful to follow Lorentz's reasoning in some detail, because it brings out the systematic way he has set up this paper.

Lorentz first reviews the basics of radiation theory. From Kirchhoff's work it follows that a universal radiation law exists: the energy density of radiation in the wavelength interval $\lambda, \lambda + d\lambda$ at temperature T is given by:

$$F(\lambda, T)\,\mathrm{d}\lambda, \tag{2}$$

with F a function that is independent of the specific properties of the body that has produced the radiation.

Next Lorentz discusses the law of equipartition of energy, Boltzmann's work in kinetic gas theory, and the at that time not widely known statistical mechanics of Gibbs, as an alternative to Boltzmann's approach. He stresses that the phase space approach taken by Gibbs only works if the system under consideration can be described using Hamilton's equations, so that the ensemble behaves like an incompressible fluid (in other words, if Liouville's theorem holds for the ensemble density). He then introduces the ensemble density of the canonical ensemble in the form:

$$\varphi = C\mathrm{e}^{-E/\Theta} \tag{3}$$

and explains how one can determine macroscopic quantities from ensemble averages.

Lorentz now turns to electromagnetism. He argues that one needs to base this theory on a variational principle of the form:

$$\delta \int (L - U)\,\mathrm{d}t = 0 \tag{4}$$

[4] The lecture was published in different versions and in various places; see Lorentz (1908a) in the bibliography for more details.

with L the magnetic energy and U the electric energy. This becomes the standard Lagrangian for the electromagnetic field if one takes:

$$L = \frac{1}{2}\int \mathbf{H}^2 \, \mathrm{d}V \tag{5}$$

and

$$U = \frac{1}{2}\int \mathbf{D}^2 \, \mathrm{d}V \tag{6}$$

where $\mathbf{D}$ is the dielectric displacement and $\mathbf{H}$ the magnetic force. Note that, although the integrand in (3) has the form of a "standard" lagrangian, in this case the terms L and U do not have their usual meanings of kinetic and potential energy, respectively. It will become clear in the following why Lorentz is using this suggestive notation.

Having set the stage, Lorentz proceeds to consider the most general physical system he can think of. It consists of charged particles ('electrons'), neutral particles ('atoms'), and ether (i.e., electric and magnetic fields), enclosed in a rectangular box. The particles are all in motion; the electrons may be free or bound inside of atoms. This system can be described by four sets of generalized coordinates:

- $\{q_1\}$ for the neutral particles,
- $\{q_2\}$ for the charged particles,
- $\{q_3, q_3'\}$ for the electric field.

While $\{q_1\}$ and $\{q_2\}$ have a straightforward meaning, the 'coordinates' $\{q_3, q_3'\}$ have a more complicated interpretation. For each instant of time one can split the electric field into two parts: the first is the field that would exist if all charged particles were at rest at their positions q_2, while the second part obeys the source-free Maxwell equation $\nabla \cdot \mathbf{D} = 0$. This latter part can be expanded in a Fourier series of the modes that fit in the box; $\{q_3, q_3'\}$ are the coefficients appearing in this expansion. Thus, the the three components of $\mathbf{D}$ can be written as:

$$\begin{aligned}
D_x &= \sum_{u,v,w} (q_3\alpha + q_3'\alpha') \cos\frac{u\pi}{f}x \sin\frac{v\pi}{g}y \sin\frac{w\pi}{h}z \\
D_y &= \sum_{u,v,w} (q_3\beta + q_3'\beta') \sin\frac{u\pi}{f}x \cos\frac{v\pi}{g}y \sin\frac{w\pi}{h}z \\
D_x &= \sum_{u,v,w} (q_3\gamma + q_3'\gamma') \sin\frac{u\pi}{f}x \sin\frac{v\pi}{g}y \cos\frac{w\pi}{h}z.
\end{aligned} \tag{7}$$

Here f, g, h are the lengths of the sides of the box and u, v, w are integers running from 1 to ∞.[5] For each component and for each set $\{u, v, w\}$ three directions are defined, perpendicular to each other. One is determined by the vector $(\frac{u}{f}, \frac{v}{g}, \frac{w}{h})$, and the other two, corresponding to the two polarization states of the field, have the direction cosines

[5] The particular choice of sines and cosines is dictated by the boundary conditions in the box: the sides of the box are supposed to be perfectly conducting, which means that $\mathbf{D}$ is always perpendicular to the sides.

α and α', β and β', and γ and γ', respectively. The coefficients $\{q_3, q_3'\}$ (which, following Lorentz's usage, will be abbreviated to $\{q_3\}$ in the following) obviously depend on $\{u, v, w\}$ and on t.

The next step is to write the Lagrangian $L - U$ as a function of the generalized coordinates. Because Lorentz now considers a more general system than just electromagnetic fields, he includes kinetic energy terms in L and potential energy terms in U, in addition to the field terms. For U he finds

$$U = U_0 + \frac{1}{16} fgh \sum q_3^2 \tag{8}$$

with U_0 a function of the coordinates q_1 and q_2, accounting for the potential energy between the particles; the second term is $\frac{1}{2}\int \mathbf{D^2} dV$.

The term L consists in the first place of a part representing the kinetic energy of the electrons and of the neutral particles, denoted by L_0 and quadratic in $\dot{q}_1$ and $\dot{q}_2$. Further, to find the field part $\frac{1}{2}\int \mathbf{H^2} dV$ it has to be taken into account that magnetic fields are generated by moving charges as well as by changing electric fields. This gives rise to terms in $\mathbf{H}$ proportional to $\dot{q}_2$ and to $\dot{q}_3$, respectively, which leads to terms in L proportional to $\dot{q}_2^2$, $\dot{q}_3^2$, and $\dot{q}_2\dot{q}_3$. The terms proportional to $\dot{q}_2^2$ are absorbed in L_0; the explicit form of the part proportional to $\dot{q}_3^2$ is found by first calculating $\dot{\mathbf{D}}$ from (7) and then using $\nabla \wedge \mathbf{H} = (1/c)\dot{\mathbf{D}}$ to find $\mathbf{H}$. The final result is:

$$L = L_0 + \frac{fhg}{16c^2} \sum \frac{\dot{q_3}^2}{\pi^2(u^2/f^2 + v^2/g^2 + w^2/h^2)} + \sum_{ij} l_{ij} \dot{q_{2i}} \dot{q_{3j}}. \tag{9}$$

In the last term the index i refers to the individual electrons; the index j abbreviates the dependence of the quantities $\dot{q}_3$ on u, v, w and the direction cosines α, β, γ. Accordingly, the coefficients l_{ij} depend on u, v, w, α, β, γ and on the coordinates q_{2i}.

From this Lagrangian the Lagrange equations follow easily. Lorentz explicitly writes down the equations for q_3 and shows that they give rise to standing waves, with wavelength

$$\lambda = \frac{2}{\sqrt{u^2/f^2 + v^2/g^2 + w^2/h^2}} \tag{10}$$

Whenever there is a valid Lagrangian formalism, one can also write down Hamiltonian equations. That does not mean that Gibbsian statistical mechanics can be applied to the system under consideration yet. A major obstacle remains: because there are infinitely many terms in the sum (7) there are also infinitely many coordinates q_3, so that phase space would become infinitely dimensional, precluding the existence of a meaningful ensemble density. Lorentz's workround solution is to introduce what he calls "fictitious connections" ("liaisons fictives") in the ether by which standing waves with smaller wavelengths than some value λ_0 are excluded. As can be seen from (10) this means that an upper limit is imposed on the values taken by u, v, w. Lorentz justifies his condition by pointing out that one can make λ_0 as small as one wishes. To this "fictitious" system Gibbsian statistical mechanics is then applied.

Lorentz now returns to expression (3) for the ensemble density. He first notes that in L the term L_0 contains terms of the form $(1/2)m\dot{q_1}^2$. Because $E = L + U$, the exponential in (3) contains these terms as well. Taking the ensemble average of $(1/2)m\dot{q_1}^2$ using (3) (i.e., calculating the mean kinetic energy of a neutral particle), now gives $(1/2)\Theta$; for the three-dimensional motion of the particles this becomes $(3/2)\Theta$. Earlier in the paper, Lorentz has explained that one of the most important results of kinetic gas theory is that the mean kinetic energy of a moving molecule or atom is equal to αT, with T the temperature and α a universal constant (not to be confused with the direction cosine introduced earlier). Thus $(3/2)\Theta = \alpha T$ (or, in modern notation, $\Theta = kT$). In this way, Lorentz extends the standard interpretation of Θ for mechanical systems to his much more complicated system of particles and fields.

Now Lorentz turns to the second term in (8). Since the terms in this sum are quadratic in q_3, a calculation similar to the one above shows that in an ensemble average each term will contribute $(1/2)\Theta = (1/3)\alpha T$ to the mean electric energy. Taking into account the two polarization states represented by the coordinates q_3 and q_3', we get a total contribution by the q_3's of $2\alpha T/3$. This can also be interpreted as the contribution of one mode (u, v, w) to the mean electric energy.

Returning to the rectangular box, it is easy to see that the number of modes with wavelengths between λ and $\lambda + d\lambda$ that fit in the box (which is supposed to be sufficiently large) is equal to

$$\frac{4\pi}{\lambda^4} fgh \, \mathrm{d}\lambda. \tag{11}$$

Taking into account the earlier result for the mean electric energy per mode, Lorentz finds

$$\frac{8\pi\alpha T}{3\lambda^4} \, \mathrm{d}\lambda \tag{12}$$

for the energy density. Because the mean magnetic energy is equal to the mean electric energy, the total mean electromagnetic energy density is given by twice this expression, so that the radiation function (2) is now given by:

$$F(\lambda, T) = \frac{16\pi\alpha T}{3\lambda^4} \tag{13}$$

This is the Rayleigh–Jeans law.

In this way, Lorentz has generalized his earlier paper on long-wave radiation in metals, in which he first specified a mechanism by which radiation is generated and then explicitly calculated the energy-density of this radiation.

Lorentz of course realizes the consequences of his result. It is clearly in contradiction with the observed radiation curve, which shows a maximum when plotted as a function of λ. Moreover, it implies that in the case of a material body in equilibrium with the ether the latter will contain an infinite amount of energy and the energy will

become concentrated in ever shorter wavelengths.[6] This had already been concluded by Jeans. As Lorentz remarks:

> […] when Jeans published his theory, I had hoped that on closer inspection one could show that the theorem of equipartition of energy on which he based himself cannot be applied to the ether and that in this way one could find a true maximum of the function $F(\lambda, T)$. The preceding considerations seem to prove that this is not the case and that Jeans's conclusions will be inescapable unless the fundamental hypotheses of the theory are profoundly modified.[7]

This is a crucial conclusion. To summarize: Lorentz has shown that the validity of the equipartition theorem for material particles inescapably implies its validity for his more complicated mechanical–electromagnetical system (or, using his own words, for the ether). This then immediately leads to the Rayleigh–Jeans law.[8]

Is there a way to reconcile the experimental results with what he has found, Lorentz wonders. One thing is certain: for long wavelengths the law is satisfactory; the problem lies in the short-wavelength regime. A possible solution is that the maximum in the observed curve is an artefact of the experiment, perhaps due to the fact that the radiating bodies used in the experiments are not black for small wavelengths and thus radiate much less energy for these wavelengths than is assumed. In this way equilibrium between radiation and matter will take a very long time to set in—it is in fact never observed.

In the final paragraphs Lorentz emphasizes that he does not pretend to have provided the definitive solution to the radiation problem. The way one proceeds in theoretical physics, he argues, is to examine the relative likelihood of various hypotheses and theories for a given phenomenon and study the consequences that follow from those hypotheses. In the case of the Planck radiation law versus the Rayleigh–Jeans law, one has to conclude that the latter is very hard to bring in agreement with experiments, whereas Planck's law is in good agreement with them but requires a fundamental change in our thinking about electromagnetic phenomena. This is already clear if one looks at a freely moving electron emitting radiation with a continuous frequency spectrum. The question remains how to apply Planck's hypothesis of energy elements in this case. Lorentz concludes by expressing the hope that future experiments will provide firm evidence for one or the other of the two radiation laws.

[6] This is what Paul Ehrenfest dubbed the 'ultraviolet catastrophe'.

[7] "[…] lorsque Jeans publia sa théorie, j'ai espéré qu'en y regardant de plus près, on pourrait démontrer que le théorème de l'"equipartition of energy", sur lequel il s'était fondé est inapplicable à l'éther, et qu'ainsi on pourrait trouver un vrai maximum de la fonction $F(\lambda, T)$. Les considérations précédentes me semblent prouver qu'il n'en est rien et qu'on ne pourra échapper aux conclusions de Jeans à moins qu'on ne modifie profondément les hypothèses fondamentales de la théorie."

[8] As Einstein had already shown, given the validity of the equipartition law, the Rayleigh–Jeans law follows even without invoking the apparatus of statistical mechanics. See Sect. 4.2 below for details.

4 Reactions to the Rome lecture

4.1 Wilhelm Wien

Once the Rome lecture's contents became known among the physicists, it stirred up quite some emotion. Wilhelm Wien wrote to Arnold Sommerfeld on 18 May 1908:

> The lecture Lorentz delivered in Rome has disappointed me greatly. That he did not do more than present the old theory of Jeans without presenting a new point of view is in my opinion a bit shabby. […] This time Lorentz has not shown himself as a leader of science.[9]

Wien points out in rather strong terms that the question of the validity of Jeans's law—or rather its non-validity—is a purely experimental matter. He emphasizes that Lorentz's theoretical views on this point are irrelevant, because experiments show enormous deviations from Jeans's law in a region where one can easily establish how much the radiating body deviates from a black body.

Wien also communicated his objections to Lorentz himself, though in a rather more cautious way, in a letter dated 17 May 1908:[10]

> With much interest I have read the lecture you gave in Rome. I think that it is very useful to continue to keep all theoretical possibilities in mind. But I do not think that anyone who has ever done experiments in the field of radiation will admit that there is even the remotest possibility for the theory of Jeans to reach agreement with experience.[11]

He then points out that one can easily determine, by measuring their absorptive power, that the radiating bodies used in experiments depart at most a few percent from ideal black bodies and that the discrepancies of Jeans's law with experiment are so large that there is no way theory and experiment can be reconciled. The letter finishes with an admonition of sorts:

> I fear that further resistance and clinging to views that are too simple will be an impediment to the progress of science.[12]

[9] "Der Vortrag, den Lorentz in Rom gehalten hat, hat mich schwer enttäuscht. Daß er weiter nichts vorbrachte als die alte Theorie von Jeans ohne irgend einen neuen Gesichtspunkt hineinzubringen finde ich etwas dürftig. […] Lorentz hat sich diesmal nicht als ein Führer der Wissenschaft erwiesen." See Eckart and Märker (2000, nr. 132).

[10] See Kox (2008, Letter 170).

[11] "Ich habe mit grossem Interesse den Vortrag gelesen den Sie in Rom gehalten haben. Ich glaube dass es durchaus zweckmässig ist alle theoretischen Möglichkeiten dauernd im Auge zu behalten. Aber ich glaube nicht, dass irgend jemand, der jemals experimentell auf dem Gebiete der Strahlung gearbeitet hat, Ihnen zugeben wird, dass für die Theorie von Jeans auch nur die entfernteste Möglichkeit besteht mit der Erfahrung in Übereinstimmung zu kommen."

[12] "Ich fürchte daher, dass ein längeres Sträuben und Festhalten an zu einfachen Vorstellungen hemmend auf den Fortschritt der Wissenschaft wirken kann."

Revisions of fundamental ideas, Wien continues, have always led to great progress in science, provided that they forced themselves upon us, and this is one of those occasions.

Lorentz was quick to realize that he had been in error. In his reply to Wien of 6 June 1908[13] he admits his mistake, thanking Wien for pointing it out to him.[14] In his letter to Wien Lorentz emphasizes that he had not meant to claim that he had proven the correctness of Jeans's law. At the same time, Wien's letter has convinced him that the experimental bodies are indeed close enough to an ideal black body to unambiguously disprove Jeans's law. He adds a very simple reasoning that makes the invalidity of this law even more striking: a simple calculation shows that, were Jeans's law the right

[13] Kox (2008, Letter 171).

[14] That Lorentz was quite embarrassed about his error becomes clear from a letter to Pieter Zeeman of 20 August 1908. (The letter is in the Zeeman Archive in the Noord-Hollands Archief, Haarlem, The Netherlands.) He wrote: "To finish, I have to tell you that I have been very unfortunate in a certain respect with my Rome lecture. You know the theory of Jeans. For years I had broken my head about it, and about radiation theory in general, and I had always some hope that, if one stayed with the usual foundations of electron theory, one could escape from the theorem of "equipartition of energy" and could show that there had to be a real maximum (inversely proportional to T) in the radiation function. The question whether one can apply the theorem in question to a system consisting of ether, electrons, and atoms comes down to whether one is allowed to use the methods of Gibbs's statistical mechanics and this in turn depends on whether one can put the equations that describe the phenomena in the form of Hamilton's equations of motion. When I had found that this is indeed the case, it did not seem an unsuitable subject for a mathematical congress, and so I explained there how one arrives at Jeans's formula, starting from the generally accepted principles. I then also spoke of the well-known way in which Jeans wants to explain the contradiction between his theory and the observations, but unfortunately I overlooked (I don't quite understand how) that one cannot enter that road without getting into conflict with phenomena that are very well known and simple. Thus, instead of concluding "only Planck's theory is feasible", I expressed myself at the end as if a decision between this theory and Jeans's should still be sought. W. Wien […] has written to me about this right away, and I have conceded the case immediately. […] I am, of course, extremely sorry that I was wrong, but I could do nothing else but admit it, because spoken words, too, cannot be undone." ("Ik moet U eindelijk schrijven dat ik met mijne voordracht te Rome in zeker opzicht ongelukkig ben geweest. Gij kent de theorie van Jeans. Daarover en over de stralingstheorie in het algemeen had ik mij jaren lang het hoofd gebroken en ik had altijd eenige hoop dat men, als men zich aan de gewone grondslagen der electronentheorie hield, aan het theorema der "equipartition of energy" zou kunnen ontkomen, en zou kunnen aantoonen dat er een werkelijk maximum der stralingsfunctie (omgekeerd evenredig met T) moest zijn. De vraag of men het bedoelde theorema op een stelsel bestaande uit aether, electronen en atomen mag toepassen, komt hierop neer of men van de methoden der statistische mechanica van Gibbs gebruik mag maken, en dit hangt er weer van af of men de vergelijkingen die de verschijnselen beschrijven, in den vorm der bewegingsvergelijkingen van Hamilton kan brengen. Toen mij nu gebleken was dat dit inderdaad mogelijk is, leek mij dat geen ongeschikt onderwerp voor een mathematisch congres, en zoo heb ik daar uiteengezet hoe men inderdaad, van de algemeen aangenomen grondbeginselen uitgaande, tot de formule van Jeans komt. Ik heb toen ook gesproken van de bekende wijze waarop Jeans de tegenspraak tusschen zijn theorie en de waarnemingen wil verklaren, maar ongelukkigerwijze heb ik over het hoofd gezien (ik begrijp zelf niet hoe) dat men dien uitweg niet kan inslaan zonder met zeer bekende en eenvoudige verschijnselen in strijd te komen. Dientengevolge heb ik, in plaats van te concludeeren: "alleen de theorie van Planck is houdbaar", mij aan het slot van de voordracht uitgelaten alsof er nog een beslissing tusschen deze en die van Jeans zou gezocht moeten worden. W. Wien […] heeft mij er dadelijk over geschreven en ik heb hem de zaak dadelijk toegegeven. […] Het spijt mij natuurlijk zeer dat ik mij vergist heb, maar ik kon niet anders doen dan het erkennen, want ook gezegde woorden nemen geen keer.")

one, a bar of silver should emit enough radiation in the visible spectrum to be visible in the dark at room temperature.[15]

Thus, Lorentz concludes, only Planck's theory—which he claims to admire very much—remains and the considerations in his Rome lecture show that the standard electron theory needs to be supplemented in one way or another to solve the radiation problem. Defending himself against Wien's veiled accusation of standing in the way of the progress of science, Lorentz stresses that he too believes that bold new hypotheses lead to progress in physics and he praises the quantum hypothesis as one of those new ideas. But, he adds, if we adopt Planck's theory, we immediately encounter a serious problem. Elaborating on his final remarks in the Rome lecture, Lorentz argues as follows. If we assume that equilibrium between ether and matter is brought about by Planck's resonators, which can only absorb or emit energy in discrete quantities, we introduce a mechanism for which equipartition is no longer valid. But if our system also contains free electrons, for the equilibrium between these and the ether the equipartition law should be valid. This means that two different equilibria exist within one system, which is in conflict with the second law of thermodynamics.[16]

4.2 Einstein

Another reaction to the Rome lecture was more positive. On 13 April 1909 Albert Einstein wrote:

> I have to tell you how much I admire the beauty of your derivation of Jeans's law. I cannot think of any serious objection to this derivation. Reading your paper has been a real event for me.[17]

At first sight it seems puzzling that Einstein praised Lorentz for his derivation of a radiation law he was convinced to be wrong, but if we look a little more closely at Einstein's work we can understand his reaction. In the paper from 1907 in which he developed his quantum theory of specific heats (Einstein 1907), Einstein had made a very strong case for a total revision, not just of radiation theory, but also of what he called 'molecular mechanics'. A key role in his argument was played by the equipartition law. His reasoning went as follows. Planck has shown, on purely classical grounds, that the interaction between his oscillators and a radiation field leads to the expression

$$U_\nu = \frac{c^3}{8\pi\nu^2}\rho_\nu \tag{14}$$

[15] Lorentz would use the same example in an addendum to some of the printed versions of his Rome lecture.

[16] Not long after the exchange with Wien, Lorentz submitted (Lorentz 1908b), a response to (Lummer and Pringsheim 1908), a critical paper by the experimentalists Ernst Lummer and Otto Pringsheim. In it he retracts his remarks on the validity of the Rayleigh–Jeans law in the Rome lecture without any reservation. He also reiterates the argument about two different equilibrium states first formulated in the letter to Wien.

[17] "Ich muss Ihnen meine Bewunderung ausdrücken über die Schönheit Ihrer Ableitung des Jeans'schen Gesetzes. Gegen diese Ableitung wüsste ich keinen ernst zu nehmenden Einwand. Die Lektüre Ihrer Abhandlung ist für mich ein Ereignis." See Kox (2008, Letter 185).

with U_ν the mean energy of an oscillator with frequency ν and ρ_ν the energy density of radiation of the same frequency. Assuming that equipartition holds for the oscillators, we have $U = kT$. Substituted in (14) this gives the Jeans radiation law, which, as Einstein points out, is only valid for large values of T/ν. Einstein's conclusion is that even for a system of oscillators we have to modify the equipartition law because it leads to serious contradictions.[18] So, what Lorentz had done was to provide in effect a much stronger basis for the argument that equipartition was the problem, and this must have pleased Einstein.[19]

In Lorentz's reply, a lengthy letter dated 6 May 1909,[20] as well as in a letter to Wien from 12 April 1909[21] it becomes clear that his thinking about the quantum problem has evolved and that he now accepts the need for energy elements:

> I no longer have doubts that only with the help of the hypothesis of energy elements one can arrive at the correct radiation-law.[22]

In his reply to Einstein Lorentz also admits that one cannot do without energy elements. He reiterates some of the points already discussed in his earlier correspondence with Wien and Planck: the problem of the existence of two equilibrium states in a system containing free electrons as well as oscillators and the question of where to localize the discontinuity.

Lorentz uses the occasion of his letter to Einstein to discuss another important quantum issue that had only been touched upon briefly in his earlier correspondence: how to deal with the light quanta, postulated by Einstein in 1905:

> I find it hard to subscribe to the view that light quanta retain a certain individuality even during their propagation, as if one were dealing with point-like quantities of energy or at least energy quantities concentrated in very small spaces.[23]

He now works out his objections in detail, emphasizing in particular the problems a localized light quantum poses in explaining interference and the resolving power of telescopes. He concludes that a light quantum must have a length of at least dozens of centimeters in order to account for the observed possibility of interference with phase differences of millions of wavelengths. From the fact that the resolving power of a telescope gets better with increasing aperture, he then infers that light quanta should have a larger lateral extension than the aperture of any telescope. For an aperture of, say, 50 cm this leads to an estimate for the lateral extension of at least 5,000 cm^2

[18] He then proceeds to show how one can modify the structure of phase space, namely by quantizing it, to obtain Planck's law from a statistical mechanical calculation.

[19] One might wonder why Lorentz did not refer to Einstein's 1907 paper in his Rome lecture.

[20] Kox (2008, Letter 189).

[21] Kox (2008, Letter 184).

[22] "Ich zweifle jetzt gar nicht mehr daran, dass man nur mit Planck's Hypothese der Energieelemente (die man übrigens noch in verschiedener Weise auffassen kann) zu der richtigen Strahlungsformel gelangt." Lorentz to Wien, 12 April 1909.

[23] "Ich kann mich aber schwerlich der Meinung anschliessen, dass die Lichtquanten auch während der Fortpflanzung eine gewisse Individualität behalten, als ob man es mit 'punktförmigen' oder jedenfalls in sehr kleinen Räumen konzentrierten Energiemengen zu tun hätte."

(because light quanta that do not hit the telescope right in the middle of the objective should still cover the full opening). Lorentz would raise these objections time and again in later papers as well as in correspondence with colleagues.

Einstein was pleased with Lorentz's letter. To his former collaborator Jakob Laub he wrote on 19 May 1909:

> I am presently carrying on an extremely interesting correspondence with H. A. Lorentz on the radiation problem. I admire this man like no other: I might even say, I love him.[24]

Einstein takes the objections raised by Lorentz very seriously, emphasizing not so much their points of disagreement but rather where they agree. He suggests that the discrepancy between the behavior of free electrons and resonators should be resolved by a suitable generalization of Planck's hypothesis, which is simply too narrow if only applied to monochromatic resonators. He also denies that he subscribes to the idea of discrete, independent point-like light quanta, but instead suggests a picture in which these quanta are singularities that are surrounded by a vector field whose strength decreases with increasing distance. The field energy is then related somehow to the number of these singularities.

4.3 An objection by Van der Waals Jr.

A more technical problem with Lorentz's approach in the Rome lecture was pointed out by Johannes Diderik van der Waals Jr. (at the time professor at the University of Groningen but soon to be his famous father's successor in Amsterdam). He objected that Lorentz's formalism only works when one assumes that the electrons have material mass, in addition to their electromagnetic mass.[25] This was a very serious assumption, because a consensus had more or less been reached that electrons only possess an electromagnetic mass. Especially Walther Kaufmann's experiments on the ratio of charge to mass of electrons seemed to indicate that they have no material mass.[26]

Van der Waals's reasoning went as follows: if the electrons lack mechanical mass, accelerations no longer occur in the equations of motion. This circumstance leads to relations between the coordinates q_2 and $\dot{q_2}$, so that these are no longer independent. Because of this a canonical ensemble cannot be formed, so that the whole edifice

[24] "Mit H.A. Lorentz habe ich gegenwärtig eine überaus interessante Korrespondenz über das Strahlungsproblem. Ich bewundere diesen Mann wie keinen andern, ich möchte sagen, ich liebe ihn." (Klein et al. 1993, Doc. 161).

[25] See Van der Waals (1909).

[26] See, e.g., Miller (1981, chapter 1.11), for a discussion of these experiments. See also the following statement by Lorentz in section 32 of Lorentz (1909b), his 1906 lectures at Columbia University: "[…] with a view to simplicity, it will be best to admit Kaufmann's conclusion that the negative electrons have no material mass at all." And in section 34 he generalizes his view in the following way: "I for one should be quite willing to adopt an electromagnetic theory of matter and of the forces between material particles. […] Therefore, […] all forces may be regarded as connected more or less intimately with those which we study in electromagnetism." These forces, Lorentz argues, include molecular forces and gravitation. See McCormmach (1970) for more on this 'electromagnetic world picture.'

of Lorentz's reasoning collapses.[27] That Lorentz took this objection very seriously becomes clear from his correspondence. He mentions the objection in letters to Einstein[28] and to Arnold Sommerfeld,[29] adding in both cases that he has been unable to find a solution—except the obvious one of postulating that there is a material mass, but that it is vanishingly small. The most detailed discussion is found in correspondence with Van der Waals himself, in particular in a letter of 7 April 1909. Here Lorentz introduces the assumption that in the system under consideration the speeds of the electrons are small as compared to the speed of light. For vibrating electrons, this means that their amplitudes are small compared to the wavelength of the radiation and for non-vibrating ones that their state of motion changes noticeably over distances that are small with respect to the wavelength of the radiation. Thus, Lorentz's assumption once again amounts to a condition on the wavelengths allowed in the system under consideration. On his assumption, Lorentz shows, the coordinates become independent again. Still, he must have felt uncertain about his supplementary condition, because he did not follow up on his plans to publish his ideas, in spite of an announcement in the letter to Van der Waals that he would do so. Only in 1911, in his lecture at the first Solvay Conference did Lorentz openly speak out on this point (see Sect. 5).

It is interesting that in the correspondence with Van der Waals, nor in his Solvay lecture does Lorentz counter Van der Waals's objection by simply introducing a small material mass for the electrons and working out the consequences. In a letter to Van der Waals of 19 November 1908 Lorentz does raise this possibility, but he then rejects it. His argument is that unforeseen problems might arise in the limiting case of zero material mass, which one would have to consider if only electromagnetic mass should exist—and, as we saw, Lorentz believed this to be the case. As he puts it: "In any case, it is much safer to directly consider electrons without material mass."[30]

5 The first Solvay Conference

The first Solvay Conference, which was held in Brussels from 30 October to 3 November 1911, offered Lorentz the opportunity to express himself on the crisis in physics that the emerging quantum theory had caused. In his opening address as chairman of the meeting he showed himself far from optimistic:

> At the moment we have the feeling that we are at a dead end, with the old theories showing themselves more and more powerless to pierce the darkness that surrounds us from all sides. […] What will be the result of these meetings? I dare not predict it, not knowing what surprises may be in store for us. But, as

[27] If in phase space the coordinates and their time derivatives are no longer independent, the incompressibility condition $\sum \partial \dot{s_i}/\partial s_i$, with s_i the coordinates of the phase space, is no longer satisfied, so that the ensemble is not stationary.

[28] Lorentz to Einstein, 6 May 1909 (Kox 2008, Letter 189).

[29] Lorentz to Sommerfeld, 23 November 1908 (Kox 2008, Appendix, Letter 178a).

[30] "In elk geval is het veel veiliger, rechtstreeks electronen zonder materieele massa te behandelen."

> it is wise not to count on surprises, I admit that it is very likely that we will contribute only little to the immediate progress.[31]

Lorentz's lecture at the Solvay meeting (Lorentz 1912b), dealt with the same subject as his Rome lecture, but its main point, the problem of the validity of the theorem of equipartition of energy in the theory of radiation, now appears already in its title. Lorentz accepts the failure of classical theory to account for the radiation experiments from the outset and points out how Planck's hypothesis of energy elements has been more successful and has even found "unexpected verifications" ("verifications inattendues"). But he finds it still useful to reiterate his Rome derivation of the Rayleigh–Jeans law to bring out clearly what the problems are with the classical approach. As he puts it:

> Before starting the discussion of Planck's hypothesis, it is perhaps useful to become aware of the shortcomings of the old theories.

Lorentz's approach is now more systematic: he first reproduces the simple derivation of the Rayleigh–Jeans law on the basis of the equipartition law that was earlier given by Einstein (see Sect. 4.2), and then immediately poses his central question:

> Is there a way to escape from the equipartition theorem, either in general, or in in its application to the problem that we are occupied with?[32]

Lorentz then essentially repeats the calculation of the Rome lecture, although with one important difference: whereas in the Rome lecture Lorentz glosses over his introduction of the "fictive connections" that put a lower limit on the allowed wavelengths, and in fact proceeds as if this limit does not exist, he now emphasizes this cutoff as essential to avoid the problem of zero material mass of the electrons. He does add, however, that the restriction on the possible wavelengths does not resolve the discrepancy between his outcome (the Rayleigh–Jeans law) and experiments, as the experimental differences show themselves also at larger wavelengths than the extremely small ones excluded here. In the end, however, the conclusion of the Solvay lecture is the same as the one drawn in the Rome lecture: the equipartition theorem is incompatible with the observed form of the radiation curve. Obviously, after the critical reactions to the Rome lecture, Lorentz no longer tries to find a way out by doubting the validity of the radiation measurements.

6 The discontinuity

In addition to the problem of equipartition of energy and the validity of the Rayleigh–Jeans law, another important point of often confused discussion emerged in the correspondence with Wien, Planck and Einstein following the Rome lecture. It is

[31] "Nous avons maintenant le sentiment de nous trouver dans une impasse, les anciennes théories s'étant montrés de plus en plus impuissantes à percer les ténèbres qui nous entourent de tous côtés. […] Quel sera le résultat de ces réunions? J n'ose le prédire, ne sachant pas quelles surprises peuvent nous être réservées. Mais, comme il est prudent de ne pas compter sur les surprises, j'admettrai comme très probable que nous contribuerons pour peu de chose au progrès immédiat." (Lorentz 1912a, pp. 7–8).

[32] "Y a-t-il moyen d'échapper, soit au théorème de l'équipartition en general, soit à son application au problème qui nous occupe?"

the question where one should localize the discontinuity—if there is one. In a letter to Planck, for instance, Lorentz maintains that processes that take place in the ether are continuous in character, agreeing with Planck on this point.[33] Two months later, he admits that one cannot do without light quanta, but immediately adds his standard objections against the individuality of these quanta. He now claims that h might be a constant of the ether: the Maxwell equations are valid, but links between groups of vibrations limit the number of degrees of freedom of the ether, a limitation that finds its expression in the occurrence of Planck's constant.[34] Two months later Lorentz speculates again that h has something to do with the particles that produce radiation.[35] His final view, expressed in 1912, seems to be that the interaction between ether and resonators is continuous, whereas the energy exchange between ordinary matter, such as atoms, and resonators is somehow quantized.[36]

7 The light quantum

After his initial discussion with Einstein (see Sect. 4.2) Lorentz kept arguing against the independent existence of light quanta in his correspondence and in later publications. He was of course not alone in his objections: in fact, the large majority of physicists rejected the existence of light quanta on grounds similar to the ones put forward by Lorentz.[37]

Let me briefly mention two later publications in which Lorentz expresses himself strongly on this point. The first is a paper from 1909, with the title "The hypothesis of light quanta" (Lorentz 1909a). Here Lorentz first gives an elegant derivation of Planck's law, using the now well-known combinatorial expression to count the number of ways to distribute p identical elements over n indistinguishable resonators. He then discusses the light quantum hypothesis and its successful application to phenomena such as Stokes's law for phosphorescence[38] and the photoelectric effect.[39] He also discusses experiments by Stark on canal rays that seem to support the light quantum hypothesis. The discussion is, as always, fair and thorough, but he concludes:

[33] Lorentz's letter is lost, but its contents may be partially reconstructed from Planck's reply of 24 March 1909 (Kox 2008, Letter 187).

[34] See Planck's summary in Planck to Lorentz, 16 June 1909 (Kox 2008, Letter 192).

[35] See Kox (2008, Letters 194 and 195) (drafts for a letter dated 30 July 1909, now lost).

[36] See Lorentz (1912c). In this paper Lorentz tries to explore in a purely classical way the consequences of the hypothesis that somehow during collisions between resonators in a solid and the atoms of a surrounding gas energy is only exchanged in discrete quanta. He is motivated by Einstein's successful theory of specific heats, in which quantized monochromatic oscillators are the building blocks of solids.

[37] Typical is the assessment given by Planck, Nernst, Rubens and Warburg in their proposal to award Einstein a salaried membership of the Prussian Academy of Sciences: "That he might sometimes have overshot his target in his speculations, as for example in his light quantum hypothesis, should not be counted against him too much." ("Daß er in seinen Spekulationen gelegentlich auch einmal über das Ziel hinausgeschossen haben mag, wie z.B. in seiner Hypothese der Lichtquanten, wird man ihm nicht allzuschwer anrechnen dürfen.") (Klein et al. 1993, Doc. 445).

[38] The light emitted in a phosphorescence process is always lower in frequency than the light absorbed.

[39] These two processes were also discussed by Einstein as evidence for his light quantum hypothesis in Einstein (1905).

> This is all without doubt very striking, but nevertheless it seems to me that on closer inspection grave objections to the light quantum hypothesis arise.[40]

Lorentz then reiterates his reasoning about the size of the quanta and the paper ends with the conclusion:

> What has been said may be sufficient to show that there can be no question of light quanta that remain concentrated in small spaces and remain always undivided during their propagation.[41]

A few years later, during a discussion at the September 1913 meeting of the British Association for the Advancement of Science, Lorentz repeats his rejection of the existence of light quanta:

> Now it must, I think, be taken for granted, that the quanta can have no individual and permanent existence in the ether, that they cannot be regarded as accumulations of energy in certain minute spaces flying about with the speed of light (Lorentz 1913, p. 381).

Eventually, though, Lorentz had to modify his views, especially once Einstein's explanation of the photoelectric effect had been confirmed by Robert Millikan's experiments. But he remained concerned because the problem of how to reconcile the existence of light quanta with phenomena like interference remained unsolved.

In 1921 Lorentz finally saw a way out through an idea first formulated by Einstein in a discussion they had in Leiden. Einstein never published his idea, but he refers to it obliquely in a paper from 1921.[42]

The first elaboration of Einstein's idea came in a letter from Lorentz to Einstein of 13 November 1921, written by Lorentz to make sure he had understood Einstein correctly.[43] The mechanism he outlines is clearly inspired by De Broglie's postulated pilot waves: whenever radiation is emitted, this radiation consists of two parts, which Lorentz calls energy radiation and interference radiation. The latter carries no energy, but has a wavelike character (one might think of ordinary electromagnetic waves with infinitesimally small amplitude). It paves the way, so to speak, for the quantized energy radiation that follows it. The idea is that the light quanta are no longer completely free in their motion; where they can go, and how many can go to a certain spot is dictated by the 'intensity' of the interference radiation. Let us take the double-split experiment as an example. The interference radiation creates the well-known interference pattern on the screen behind the slits, but because the radiation carries no energy we cannot see it. The number of light quanta that land on the screen is determined by this interference pattern: the higher the 'intensity' in a spot on the screen, the more light quanta

[40] "Dies alles ist ohne Zweifel sehr auffallend, aber trotzdem will mich dünken, dass bei näherer Betrachtung ernste Bedenken gegen die Hypothese der Lichtquanten aufsteigen."

[41] "Das Gesagte dürfte genügen, um zu zeigen, dass von Lichtquanten, die bei der Fortbewegung in kleinen Räumen konzentriert und stets ungeteilt bleiben, keine Rede sein kann."

[42] See Einstein (1921). In this paper Einstein proposes an experiment to determine whether canal rays have a wave-like or a particle-like structure. The reasoning behind the experiment turned out to be flawed: see Janssen et al. (2002, Doc. 68, note 5).

[43] Kox (2008, Letter 371).

reach that spot. Where the 'intensity' is zero, no light quanta arrive. Thus the familiar interference pattern is made visible by a succession of individual quanta.

The Einstein–Lorentz idea had little resonance with other physicists. But Lorentz remained charmed by it: he included a more elaborate form, including the interaction of two beams, in lectures given in 1922 at the California Institute of Technology (Caltech),[44] as well as in lectures at Cornell University in the fall of 1926.[45]

8 Wave mechanics and matrix mechanics

While in the years after 1911 quantum physics made huge progress, with the breakthrough achieved by Bohr in 1913 and the subsequent development of what is now known as the 'old quantum theory' by Sommerfeld and others, Lorentz remained skeptical. As late as 1925 in a lecture at the Société Française de Physique he summarized his misgivings in the following way:

> All this [i.e. quantum theory] is of great beauty and importance, but unfortunately we do not understand it. We do not understand Planck's hypothesis on the oscillators, nor do we understand the exclusion of non-stationary orbits and we do not see how in Bohr's theory the light is eventually produced. For, admittedly, the mechanics of quanta, the mechanics of discontinuities, still has to be made.[46]

It would take another 2 years before the first steps were taken towards a true quantum mechanics. At the end of 1925 Werner Heisenberg published the ground-breaking paper in which he developed the formalism of matrix mechanics and in the first months of 1926 Erwin Schrödinger published his wave mechanics.[47] It is interesting to compare Lorentz's reactions to the two new approaches. From his correspondence, in particular with Paul Ehrenfest, it becomes clear that he studied Heisenberg's original paper and the further development of matrix mechanics by Heisenberg, Born, Jordan, and Dirac. He even lectured in Leiden on matrix mechanics, as early as the fall of 1926.[48] But he never engaged with matrix mechanics in the way he did with wave mechanics. There is no correspondence on technical points with the authors mentioned earlier, and in his correspondence with Ehrenfest, for example, Lorentz seeks out Ehrenfest's help and opinion rather than trying to extend the theory.

[44] See Lorentz (1927, secs. 50–53).

[45] A set of mimeographed lecture notes of the Cornell lectures is preserved in the Caltech Archives.

[46] "Tout cela est d'une grande beauté et d'une extrême importance, mais malheureusement nous ne le comprenons pas. Nous ne comprenons ni l'hypothèse de Planck sur les vibrateurs, ni l'exclusion des orbites non stationnaires et nous ne voyons pas, dans la théorie de Bohr, comment, en fin de compte, la lumière est produite. Car, il faut bien l'avouer, la mécanique des quanta, la méchanique des discontinuités, doit encore être faite." (Lorentz 1925).

[47] See Heisenberg (1925) and Schrödinger (1926a,b); see also, for instance, Jammer (1966) or Mehra and Rechenberg (1982–2001, vols. 2 and 5), for historical overviews of the development of matrix mechanics and wave mechanics.

[48] See his lecture notes in nrs. 289, 574, and 576 in the Lorentz Archive in the Noord-Hollands Archief, Haarlem, The Netherlands.

With wave mechanics the situation was different. On 30 March 1926 Schrödinger sent Lorentz the proofs of his first two papers on wave mechanics (Schrödinger 1926a,b), asking Lorentz for his comments.[49] On 27 May the 72-year-old Lorentz replies with a letter of nine densely written pages,[50] from which it becomes clear that he has thoroughly analyzed Schrödinger's papers. Not surprisingly, Schrödinger's approach appealed to him, as being more 'anschaulich' than the much more abstract matrix mechanics. Lorentz writes that he had very much enjoyed studying the papers, but that he has also found some problems that, in his view, might be unsurmountable:

- It will be very difficult to give a physical interpretation of Schrödinger's wave functions ψ, because they are complex quantities in a high-dimensional configuration space;
- A calculation has shown that wave packets formed from such wave functions cannot represent stable particles, because of their rapid spreading.

These problems cast doubt in particular on one of the basic ideas behind wave mechanics: the extension of Hamilton's old analogy between mechanics and geometrical optics.[51] As Lorentz put it:

> Your idea that the change which our dynamics must undergo will be similar to the transition from geometrical optics to wave optics sounds very enticing, but I have doubts about it.[52]

In his reply[53] Schrödinger tries to counter Lorentz's objections. He suggests that one needs to look at $\psi^*\psi$ (with ψ^* the complex conjugate) to find a physical interpretation of the wave function and suggests that this quantity is related to the electrical charge density.[54] He also refers to a note that he has included and in which he shows that that for a harmonic oscillator stable wave packets can be constructed, indicating that perhaps there is still hope for free or bound electrons to be represented by packets.[55]

[49] See Kox (2008, Letter 405). Schrödinger later also sent the proofs of his third paper, in which he shows the formal equivalence of wave mechanics and matrix mechanics; see *ibid.* Letter 412.

[50] Kox (2008, Letter 412).

[51] In short: Hamilton had shown that there is a formal analogy between mechanics and geometrical optics, in the sense that the Hamilton–Jacobi formalization of mechanics can be translated into the eikonal formalism of geometrical optics. Since geometrical optics is the short-wavelength limit of wave optics, one might wonder whether classical mechanics is the limiting case of some wave theory which is analogous to wave optics. In Hamilton's days there was no reason to suppose that mechanics might have any kind of wave-like character, but when Schrödinger was looking for a quantum theoretical generalization of classical mechanics, trying to find a wave-like theory was a plausible way to proceed. In fact, his derivation of the Schrödinger equation in Schrödinger (1926b) takes this approach. See also Goldstein (1950) for more on the optical–mechanical analogy and Joas and Lehner (2009) for Schrödinger's inspiration by the analogy.

[52] "Ihre Vermutung, dass die Umwandlung, welche unsere Dynamik wird erfahren müssen, dem Übergange von Strahlenoptik zu Wellenoptik ähnlich sein wird, klingt sehr verlockend, aber ich habe doch Bedenken dagegen."

[53] Kox (2008, Letter 413).

[54] Not long afterwards Schrödinger had to abandon this interpretation.

[55] The included note is a copy of the manuscript of Schrödinger (1926b).

Lorentz replied as quickly as 19 June with a letter of 19 large pages full of calculations.[56] He essentially crushes Schrödinger's hopes with a new and detailed analysis of the behavior of wave packets in wave mechanics. In his usual gentle way he concludes that there is little hope left for Schrödinger's theory:

> If we have to give up the wave packets and with it one of the fundamental ideas of your theory, the transformation from classical mechanics to a wave mechanics, something would be lost that would have been very beautiful. I would be extremely pleased if you could find a way out.[57]

Lorentz believed to have shown Schrödinger's theory to be untenable—at least from a purely classical point of view. As we know, Schrödinger did not give up. Complete new interpretations of the wave equation and the wave function were needed to give physical meaning to the theory. But Lorentz had gone as far as he could go: reaching further beyond the boundaries of classical physics was too much for him.[58]

9 Conclusion

From the discussions in the preceding sections a few key points have emerged, both concerning Lorentz's concrete contributions and his approach to quantum theory:

- Lorentz's way of thinking about the radiation problem was strongly influenced by his views on the constitution of matter and on the interaction between matter and ether, views that had led to his eminently successful electron theory.
- The conclusion of his Rome lecture that classical theory necessarily leads to the Rayleigh–Jeans law and that the discrepancy between the experimental radiation curve and classical theory can only be removed by a radical new element has served as an important clarification of the radiation problem, in particular about the role of the equipartition theorem.
- It is fair to say that, after the clarification provided by the Rome lecture, Lorentz did not come up with any new ideas but kept repeating his objections. He proceeded as he always did in physics: with great caution, and with impressive technical mastery. In this particular case he was perhaps more critical and cautious than in his earlier, purely classical work, because of his desire to keep as much of classical theory intact as possible. In specific cases, such as the discussion with Schrödinger, this led to useful clarifications, but real progress in quantum theory had to come from representatives of a younger generation, who embraced the quantum hypothesis as something new and unavoidable and made daring applications of it.

[56] Kox (2008, Letter 416).

[57] "Indes, wenn wir die Wellenpakete aufgeben müssen und damit einen der Grundgedanken Ihrer Theorie, die Umwandlung der klassischen Mechanik in eine undulatorische, so würde damit etwas verloren gehen, das sehr schön gewesen wäre. Es würde mich sehr freuen, wenn Sie hier einen Ausweg finden könnten."

[58] In spite of his misgivings, Lorentz lectured on wave mechanics (as well as on matrix mechanics) while visiting Cornell University in the fall of 1926 and the California Institute of Technology in the first months of 1927. See Footnote 45; see also Lorentz to Schrödinger, 21 January 1927 (Kox 2008, Letter 420), for evidence that Lorentz covered the same material at both institutions.

– To the very end, and true to his status as classical physicist *par excellence*, Lorentz kept thinking in purely classical terms and tried to adhere as closely as possible to classical theory in his work on quantum theory.[59]

As will have become clear from the preceding discussion, Lorentz also remained true to himself in another more general way: in his capacity to objectively evaluate and appreciate points of view of others, without giving up his own, strong convictions.[60] Although he made no secret of his personal preferences for specific approaches, he never rejected alternatives out of hand and was willing and able to accept new concepts, such as the light quantum, when the evidence was overwhelming. Still, in the end, he was and remained a classical physicist.

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[59] Examples, not mentioned in the text or only mentioned in passing are Lorentz (1912c) (see Footnote 36), and Lorentz (1928), a paper delivered at the 1927 Como conference, with the revealing title: "On the rotation of an electron that circles around a nucleus."

[60] We owe this insight to Russell McCormmach, who first formulated it in his article on Lorentz in the *Dictionary of Scientific Biography*. The best-known known instance of this attitude is found in his view of the theory of relativity: Lorentz was a great admirer of Einstein's work, lectured extensively on the theory, but never gave up on the existence of the electromagnetic ether and thus of a privileged reference frame (see Kox 2008 for a discussion).

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