KONINKLIJKE NEDERLANDSE AKADEMIE VAN WETENSCHAPPEN

BIJZONDERE VERGADERING DER AFDELING NATUURKUNDE

OP ZATERDAG 24 NOVEMBER 1962,

des namiddags te 3.30 uur,

voor de plechtige uitreiking van de Lorentz-medaille aan Prof. Dr. R. E. PEIERLS, hoogleraar aan de Universiteit van Birmingham.

Voorzitter: C. J. GORTER

Secretaris: Mw. C. H. MACGILLAVRY

De voorzitter opent deze bijzondere zitting.

Buiten een aantal leden der Akademie, zijn verschillende autoriteiten en verdere genodigden aanwezig.

De voorzitter spreekt de vergadering als volgt toe:

Dames en Heren,

Het verheugt mij bij deze plechtige vergadering, waarin voor de achtste maal de Lorentz-medaille wordt uitgereikt, verscheidene eregasten te mogen verwelkomen, te weten: de vertegenwoordiger van de Minister van Onderwijs, Kunsten en Wetenschappen, de Consul-Generaal van het Verenigd Koninkrijk van Groot-Brittannië en Noord-Ierland, tevens vertegenwoordigster van de Ambassadeur, de Commissaris van de Koningin in de provincie Noordholland, de vertegenwoordigers van het College van Burgemeester en Wethouders van Amsterdam, van de Universiteit van Amsterdam en van de Vrije Universiteit, zomede enige nakomelingen van Lorentz, ten dele vergezeld van hun echtgenoten. Ik moge in het bijzonder de waardering der Akademie uitspreken voor de aanwezigheid van Mevrouw G. L. DE HAAS–LORENTZ, die niet alleen via vader en echtgenoot door hechte banden vele jaren met de Akademie gelieerd was, doch ook door het aandeel, dat Mevrouw DE HAAS zelf in het wetenschappelijk onderzoek en het natuurkundig hoger onderwijs heeft genomen, gekwalificeerd is bij deze plechtigheid een ereplaats te bezetten.

De toekenning en uitreiking van de Lorentz-medaille is een aangelegenheid, die alle Nederlandse natuurkundigen ten nauwste aangaat en het is dan ook vanzelfsprekend, dat de meest vooraanstaande Nederlandse vakgenoten van LORENTZ en PEIERLS zijn uitgenodigd heden aanwezig te zijn. Het is verheugend, dat zovelen van hen de uitnodiging hiertoe hebben aanvaard en daardoor de nationale wetenschappelijke betekenis der Lorentz-medaille accentueren. Gaarne heet ik tevens van harte welkom de echtgenoten van de leden van de commissie voor de toekenning van de achtste Lorentz-medaille, die aan de uitnodiging tot het bijwonen van deze plechtigheid hebben gevolg gegeven.

Vervolgens richt de voorzitter zich in de Engelse taal tot Professor PEIERLS en zijn echtgenote, wier aanwezigheid de voorzitter op bijzonder hoge prijs stelt.

Hierna geeft de voorzitter het woord aan de heer H. B. G. CASIMIR die de redenen uiteen zal zetten, die ertoe geleid hebben de Lorentz-medaille aan Professor PEIERLS toe te kennen en die, namens de Akademie, de medaille aan de heer PEIERLS zal uitreiken.

De heer H. B. G. CASIMIR richt zich met de volgende woorden tot de begiftigde:

Dr. PEIERLS,

In 1931 the Lorentz Medal was awarded to Wolfgang Pauli; on the thirty-first of October Paul Ehrenfest made the presentation. I was Ehrenfest's assistant in those days and witnessed the event from the gallery. The memory of that occasion comes back to me, now that I have the privilege to confer the same honour upon the man who was then assistant to Pauli.

So let me read the opening lines of Ehrenfest's address. „Sie kennen den Platz den das Werk von H. A. Lorentz in der Entwicklung der Physik einnimmt. Auch persönlich haben Sie Lorentz kennen gelernt in seiner eigenartigen Wirkung auf die jüngeren Fachgenossen aller Nationen.

Sie müssten ihn aber auch noch in seinen Beziehungen zu Nederland und unserer Akademie gekannt haben! Dann würden Sie wissen wie viel hier bei uns mitklingt wenn wir den Namen Lorentz nennen. Der Gelehrte, dessen wissenschaftliche Leistung mit der Lorentz-Medaille geehrt werden soll, wird eben dadurch unserem Gefühl in besonderer Weise nahegebracht. Das durfte doch wohl in diesem Augenblick offen ausgesprochen werden."

Perhaps these words are no longer entirely applicable. The number of physicists who knew Lorentz personally and experienced his powerful influence is dwindling. Yet there will always remain much in modern physics that derives from his contributions. This is clearly shown by the work of those who received the Lorentz Medal before you: Planck, Pauli, Debije, Sommerfeld, Kramers, Fritz London, Onsager. It is also shown by your own work which I shall now come to discuss.

In the midtwenties Heisenberg, Born and Jordan, Schrödinger, Dirac, and a few others, created the new quantum mechanics, a discipline that did not only confront physicists with unaccustomed mathematical formalism but also involved entirely new principles in the description of nature. Immediately after this major break-through there arose a small group of bright youngsters who took to these new theories like ducks to water, who mastered the novel and hardwon discipline with astonishing ease – possibly they even knew more quantum mechanics than classical mechanics – and who applied it to every type of atomic phenomenon thus conquering in glorious advance a range of problems completely inaccessible to classical theories. Of that group of youngsters you were a prominent member. You will forgive me – and here I quote an extremely flowery letter once written to you by a Burmese inventor – you will forgive me that I address your learned self in such a blunt manner. Yet the word bright youngster seems hardly out of place in connection with some one who was born in 1907, took his doctor's degree in 1929, and sent a first and very important paper to the ,,Zeitschrift für Physik" in December 1928. Your main field of activity was at first the theory of the solid state. I shall not relate in any detail your contributions to its mathematical formalism nor try to give a complete survey of all your results, but I want to stress the great importance and notable fruitfulness of several new ideas and principles that were introduced in the course of your work. There is first of all your explanation of the positive Hall effect. Let me briefly sketch the situation. The classical theory of the motion of electrons had been worked out in great detail by Lorentz; Bohr in his thesis gave it the finishing touch. But of course many problems had to remain unsolved until the advent of quantum mechanics. There was first of all the question: why do the electrons not give an appreciable contribution to the specific heat. The answer was given by Sommerfeld; it consisted in a straight-forward application of Fermi-Dirac statistics which in turn is an application of Pauli's exclusion principle. Next: how is it possible that electrons move so easily through metals although the atoms must give rise to considerable electric fields. Bloch solved this problem by showing the possibility of unperturbed propagation of electron waves in periodic structures. There remained another major riddle. Why is the Hall effect sometimes positive? More explicitly: when a magnetic field is applied in a direction perpendicular to a current there arises a transverse potential difference. Why does the sign of this transverse potential difference sometimes correspond to the motion of free positive particles all though we know for certain that the current is not carried by positive particles. This paradox has troubled Lorentz a good deal, for he was convinced that the current in a metal is carried by negative electrons even though the evidence in those days was less overwhelming than it is today. I quote two passages. The first one is taken from a lecture given before the Electrotechnische Verein in Berlin on December 20, 1904: ,,Bei dieser Sachlage wird nur eine eingehende Untersuchung des Halleffektes die Entscheidung bringen können . . . Ich halte es nicht für ausgeschlossen, dass es am Ende in dieser Weise gelingen wird von dem Halleffekt in Eisen Rechenschaft zu geben, ohne dass man zu freien positiven Elektronen seine Zuflucht zu nehmen braucht." The second one from a lecture three years later: ,,As to the Hall effect, which at first sight seemed to speak so strongly in favour of the two-fluid theory, we shall have to examine whether it cannot be accounted for by the motion of negative electrons only."

Of course, Lorentz was entirely right in his surmise that closer analysis of the motion of negative electrons would lead to an explanation of the positive Hall effect, but he could not suspect at the time that it would last more than twenty years and would require an entirely new type of theory before such an explanation could be given. Your theory was partly inspired by Heisenberg's work on almost-closed shells and may seem almost self-evident to a younger generation: a hole, an empty place in a nearly filled band, behaves as a positive particle. Yet the argument is in reality rather subtle because it involves two separate notions both of which are entirely non-classical and rather surprising: the notion of negative mass and the notion that an unoccupied state behaves as a particle. First one has to realize that electrons in states near the top of a band behave like particles with negative inertial mass: only then will an unoccupied place near the top of a band behave like a positive particle.

Dirac applied entirely analogous considerations to relativistic electrons in free space, in his theory of protons – which turned out to be a theory of positive electrons – but that was more than a year later. The positive hole in solids preceded the positive hole in empty space and that is as it should be, for a filled electron band is a more congenial notion than an infinite collection of electrons in empty space, which must in some devious way be normalized away in order to become unobservable. To such an extent has your theory become staple knowledge in solid-state physics and the bread and butter of semi-conductor manufacturers, that one often even forgets to mention your name in this connection!

Next I recall your remark that the lattice vibrations in a body through which an electric current is passing are not necessarily in thermal equilibirum. According to Bloch's theory, electrons in a perfectly periodic crystal move without being scattered. Thermal vibrations disturb the periodicity, give rise to sattering and hence to resistance. In Bloch's papers it is tacitly assumed that the thermal vibrations are in equilibrium. You pointed out that this is not necessarily so: the electric current may be accompanied by a distribution of lattice waves that travel preferentially in the same direction, somewhat like a marching brass band surrounded by a crowd of children that on the average move along. Recent work on the phonon drag in semi-conductors has completely vindicated your ideas. On entering the next village both the brass band and the children reorganise. At a junction both the electron current and the lattice waves contribute to the Peltier effect. I may also add that in recent theories on paramagnetic relaxation we meet with a similar situation.

Equally important is your famous Umklapprozess. Since in a periodic lattice wave vectors (measured in units of the reciprocal lattice) have only a meaning modulo 2π, a change of the wave vector by almost 2π corresponds in reality only to a slight change, and in collision processes the sum of the wave vectors is not necessarily conserved but may change by n times 2π. The idea occurs in your work on electrons in metals and also in your famous paper on conduction of heat in insulating crystals. The ,,Umklapprozess'' is characteristic of the discrete lattice: in a continuum there exists no ,,Umklappung''. In a continuum there exists always an integral in the form of a pseudo momentum – of lattice vibrations in an insulator, of lattice vibrations plus electrons in a conductor. This integral corresponds to the translational invariance of the Hamiltonian. But in a discrete system there is no rigorous translational invariance and the ,,Umklapprozess'' destroys the integral.

I shall be short about your other major field of activity, nuclear physics. I am not a very competent judge, but it appears to me that in some ways the nucleus did not provide as happy a hunting ground to the theoretician as did the atom and the solid state; and reading your own lucid expositions of the subject has not removed this impression. What is lacking is the sense of exhilaration over the new tools that provide a ready answer to many formerly unsolvable riddles. The tools are still there, essentially unchanged, but their application requires now much hard work and cumbersome calculations, and even the most ambitious calculations have to be confined to models that reveal only part of the truth.

Meanwhile the exact nature of nuclear forces is still unknown and it is doubtful whether we shall ever be able to derive it from an analysis of nuclear data: we may have to wait for the results of high-energy physics.

Thus in retrospect, the early days of quantum mechanics may well appear as the true old times ,,when every morning brought a noble chance and every chance brought out a noble knight''. But however this may be, you yourself were certainly not corrupted by the good custom of your early success and you have attacked this new field with undaunted energy and with your usual skill and perspicacity.

Let me cite one example: your work with Yoccoz on collective motion in the shell model and the continuation of this work in a recent paper with Thouless. It is important because it endeavours to reconcile the idea of rotational states of the nucleus as a whole with the shell model; it is quite a mathematical tour de force and it illustrates the peculiar difficulties of nuclear theory. In an atom electrons encircle the nucleus, their orbits can be labelled by quantum numbers and retain to a large extent their individuality even though they perturb one another. Rotation of the atom as a whole is not a meaningful concept. In a molecule the nuclei of the constituent atoms are almost fixed with respect to one another and we can now speak about rotation of the molecule. I mention in passing that separating off the rotation is slightly tricky.

In the nucleus the situation is ambiguous. The shell model meets with a measure of success, notably in explaining the so-called magic numbers, but the interactions between orbits are very strong. It is not possible to localize particles inside the nucleus the way we can localize atoms in a molecule, yet the idea of rotation of the nucleus as a whole has been extremely useful in explaining γ-spectra. Under these circumstances a straight-forward canonical transformation is of little use. The procedure that enabled you to find equations for rotation without such a complete transformation would appear to be a significant advance.

Throughout your scientific career you have also made contributions to the general formalism of quantum mechanics or elucidated special points: I mention your early papers with Landau, your work on dispersion theory and on the use of complex eigenvalues.

Professor Peierls, you are not only an outstanding scientist, you are also a great teacher. The work carried out at Birmingham under your guidance covers nearly all aspects of theoretical physics from dislocations to high-energy physics. You have greatly contributed to the scientific life and the scientific manpower of England, the country that with wise foresight welcomed you to its shores thirty years ago.

Let me now on behalf of the Royal Academy of Sciences of the Netherlands hand you the Lorentz Medal.

I trust that you will accept it in the spirit in which it is offered: as a well-earned distinction and a token of our admiration and our friendship.

De voorzitter wenst de heer PEIERLS namens de Akademie geluk met de hem toegekende onderscheiding en stelt hem in de gelegenheid een woord tot de aanwezigen te richten.

De heer PEIERLS maakt hiervan gaarne gebruik en spreekt de vergadering als volgt toe:

It is difficult to find words to express the many things I want to say, but at least it is clear where to start, and that is to thank you, Mr. President, and your colleagues in the Academy, for having chosen me for this distinguished award. When I first heard about this I received the news with great pleasure and enormous surprise, and, until a few minutes ago, I found it hard to believe that I should be regarded as having merited this honour. However, after listening to Professor CASIMIR's persuasive exposition of all the important things I seem to have done, I have almost come to believe that there might be a case.

The award gives me such great pleasure, not only because you have chosen my name for this distinction, and not only because it places me amongst the group of outstanding men who have received this award before me, but particularly because of its association with the name of Lorentz.

I belong to the post-Lorentz generation, and although I was already trying to become a physicist during his lifetime, I never had the privilege of meeting him. However, I had a reflected impression of his personality through the attitude of my teacher, Pauli, whose respect, as you know, was not bestowed easily. Listening to the manner in which Pauli used to refer to Lorentz, one received a clear impression of his greatness and his charm, and much of this is evident to any physicist familiar with his work.

As Casimir has already reminded you, we of the post-Lorentz generation were singularly fortunate in the period in which we started our work. The formulation of quantum mechanics had just provided the clues to the explanation of practically all atomic phenomena. One only had to look at any one of the unsolved problems which nobody else had yet had time to study in the light of the new ideas, to find yet another answer which fitted into the pattern.

I doubt whether physics will ever again go through such a stage. It is conceivable that, in our lifetime, a new break-through in elementary particle physics might lead to a similar situation, but it does not seem to me likely that such a development could be simple enough to be comparable with the early days of quantum mechanics.

Of the many lessons we can learn from Lorentz and his work, an important one in my opinion is that physics is essentially one subject whose different parts are inter-linked. If one wanted today to discuss the work of Lorentz and the ideas that have developed from it, one might almost have to call two (or more) meetings of completely different audiences, one of solid state physicists, one of field theorists, and perhaps more. Lorentz approached his problems not from interest in specific fields of application or in specific techniques of study, but from what they could teach us about the fundamental laws and the clarification they could provide about the nature of the physical world.

Today it is harder than ever before to avoid specialization. It is surely impossible today to know all details of the work going on in all fields, but it is possible to be aware of the crucial questions, the growing points and the principles in the major fields. If we try to do so, we are following the example set by Lorentz.

It is interesting to try and imagine the reaction of Lorentz to present-day physics if he were with us today. He would certainly be interested in the theory of solids and the electron theory of metals, and he would wish to see how the many fascinating questions, which he formulated so clearly, have found their answers, and how this understanding had led to many interesting and powerful applications.

I agree with Casimir that he might not be very interested in the theory of atomic nuclei, which by its nature remains a complex and complicated field, though it contains many problems of fundamental interest, whose solution forms an essential part of our understanding of nature.

But he would most certainly be concerned with field theory and the study of elementary particles, which is a direct continuation of his work on the nature of the electron, the only well-established elementary particle in his time. I doubt whether we have today a satisfactory understanding even of the electron problem. While great progress has been made, allowing us to account for many aspects of the relativistic behaviour of electrons, there may still be new points, perhaps unimportant quantitatively, but essential to the understanding of principles, that elude us even today.

But Lorentz was also willing to apply the physicist's skill to problems of practical value in the struggle with the hazards of nature. We remember his great contribution to the problem of the dykes and its importance for the damming of the Zuiderzee. After my journey to this meeting I cannot help dreaming of another Lorentz who, in a similar spirit, might show us the way of dealing with fog.

It remains, Mr. President, to thank you and the Academy for inviting my wife and me to be present on this occasion, to Professor Casimir for his kind and flattering words, and to the many distinguished members of the audience for being here. It is a particular pleasure to me to see Mrs. de Haas–Lorentz here. One little piece of work I did, which Casimir did not have time to mention and which in itself is of no great importance, gave me great pleasure because it related to the beautiful experiments of Professor de Haas which show the surprising resonances of metallic electrons in a magnetic field, which can tell us so much about the dynamics of their motion and which connect in such a beautiful way the skill of low temperature experiments, like those of de Haas, with Lorentz's picture of the motions of electrons in metals.

Once more, Mr. President, to you and to all many thanks.

De voorzitter zegt de heer Peierls dank voor zijn vriendelijke woorden en spreekt zijn erkentelijkheid uit jegens de leden van de commissie en in het bijzonder jegens de heer Casimir voor de door hen in het belang van de Akademie verrichte werkzaamheid. Hierna sluit de voorzitter de vergadering.

De heer Peierls en zijn echtgenote begeven zich hierna naar de koffiekamer, waar degenen, die de bijzondere vergadering hebben bijgewoond in de gelegenheid worden gesteld hem te complimenteren.