

KONINKLIJKE NEDERLANDSE AKADEMIE VAN WETENSCHAPPEN

BIJZONDERE BIJEENKOMST

DER AFDELING NATUURKUNDE

op zaterdag 28 september 1974, des namiddags te 3.30 uur,

voor de plechtige uitreiking van de Lorentz-medaille
aan Prof. Dr. J. H. VAN VLECK, Harvard University, Cambridge, USA

Voorzitter: P. J. GAILLARD

Secretaris: J. LEVER

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

De voorzitter opent de vergadering en heet de aanwezigen welkom in het bijzonder Professor en Mevrouw Van Vleck, de Minister voor Wetenschapsbeleid, de Staatssecretaris van Onderwijs en Wetenschappen, de Consul-Generaal van de Verenigde Staten en de vertegenwoordiger van de Burgemeester van Amsterdam.

Hierna geeft de voorzitter het woord aan Prof. Dr. H. B. G. Casimir, die de keuze van de Commissie voor de Toekenning van de Lorentz-medaille zal toelichten en de heer Van Vleck de medaille zal overhandigen.

De heer Casimir spreekt de aanwezigen als volgt toe.

Hendrik Antoon Lorentz was a great physicist and a noble, kind and generous man. Awarding the medal that bears his name is for our Academy the most meaningful way to express our high regard for a theoretical physicist and our admiration for his work.

During the early award ceremonies the memory of Lorentz was still very much alive. In 1931 Ehrenfest in his address to Pauli could say: "But you should also have known him in his relations to the Netherlands and to our Academy. Dann würden Sie wissen wieviel hier bei uns mitklingt wenn wir den Namen Lorentz nennen". This is no longer quite true. Some of us have still met Lorentz, but very few of us have actually seen him at work. Of course, his name will not be forgotten and the results of his work are firmly woven into the web of physical theory, but by and by only a historian will be able to clearly distinguish the individual threads.

The former recipients of the Lorentz medal, Planck, Pauli, Sommerfeld, Debye, Kramers, London, Onsager, Peierls, Dyson and Uhlenbeck are as it were the pillars of a bridge between Lorentz and the present. But also here, time is doing its inexorable work. You and I will think of Kramers as of a friend that left us too soon, and we treasure the recollection of his mathematical skill, of the depth of his insight, and above all of his mildly ironical, yet very humane wisdom. To a younger generation he is just one of the great names of the past.

There is no tragedy in this, unless we want to regard our whole human existence, the whole march of history as a tragedy, but we do feel a faint nostalgia, like the one with which we remember an old garden, or a patch of wood, where we used to play as a child and which has now been absorbed into some urban development project. It is therefore a great satisfaction to all of us, that the man we considered most worthy to receive this year's Lorentz medal is also a man whose work is in several ways directly related to the work of Lorentz. Knowing your personal preferences in locomotion, I trust that you will not be displeased if I slightly elaborate my former metaphor by asserting that you provide us with a one-span railroad bridge—and a long one at that, a curious engineering feat—parallel to, and in many places cross-linked with, the multi-section traffic bridge supported by former medallists.

Your first major publication, your doctor's thesis, shows at once this parallelism. It deals with a problem that was independently and almost simultaneously also tackled by Kramers. It is a calculation of the energy of the fundamental state of the helium atom, for a model proposed by Kemble and on the basis of the "old" quantum theory, which means selecting a classically possible orbit by means of quantization rules. Your result for the energy expressed in atomic units was 5,53 whereas Kramers, who pushed the perturbation calculations even further, found somewhat later 5,5235. Since the experimental value is 5,8 this had to be regarded as strong evidence that the old quantum theory was entirely inadequate for dealing with many electron systems.

There follow several more publications on the applications of quantum theory, and on the correspondence principle and then, in 1926, your monograph on quantum principles and line spectra. In a way it shared the fate of Pauli's roughly simultaneous article in the *Handbuch der Physik*. It appeared at a moment when the new quantum mechanics had just been born. This new theory, together with the notion of the spinning electron, provided exact derivations of the semi-empirical rules of the older theory and made the celestial mechanics type of perturbation calculus superfluous. It must have been a curious experience to have to add—in proof—notes whose tenor could only be that the whole structure of the book would have to be modified.

I shall not try to describe the excitement of the early days of quantum mechanics. After all, I began my studies in 1926 and by the time I thought

(foolhardily perhaps) that I had sufficiently mastered the elements of mathematics and theoretical mechanics to venture a look at recent publications, the revolution was past its culmination point, although there was still a lot to do for a young theoretician.

You yourself have given a vivid description of those days in a lecture, delivered in 1963 on the occasion of receiving the Michelson Award; it is entitled *American Physics comes of Age*. In that lecture you argue convincingly that American theoretical physics reached maturity with the great breakthrough of quantum mechanics, when American theorists could start from scratch on equal terms with their European colleagues. The subsequent influx of European refugees did certainly help to reach pre-eminence but the essential turning point had been reached before. Allow me to disagree with you in one respect. In my opinion you have contributed far more to this coming of age through your own work, through the work of your pupils, through your many contacts everywhere in the scientific world than can be inferred from that speech. And I should like to draw attention to the fact that you were able to do important work on the quantum theory of line spectra and the correspondence principle although you were far from the centres where the action was, Copenhagen and Germany. *You had definitely come of age as a physicist before the dawn of quantum mechanics.*

In 1927 there appeared a series of three papers on dielectric constants and magnetic susceptibilities, a subject that would remain your principal, though not by any means your only field of work throughout your life. I shall not try to summarize these papers: that would amount to giving an introductory lecture on the theory of dielectric constants and magnetic susceptibilities. Let me just mention a few points. You give a very general derivation of the Langevin formula but you also show its limitations. Here the temperature independent paramagnetism—at first sight rather surprising—appears for the first time. You also discuss the special case of NO, where the apparent magnetic moment changes with temperature because there are two levels with a separation corresponding to about 170 °K. Your formula was accurately confirmed, by Wiersma at Leiden and by others. But even this was not sufficient to do away with the myth of the Weiss magneton, which survived for many more years. Do you remember the Strassburg conference just before the Second World War?

In 1932 you published your monograph on the theory of electric and magnetic susceptibilities. It was especially this by now classic book I had in mind when I said that your work is directly related to that of Lorentz. As a matter of fact your first chapter is entirely based on Lorentz's theory of electrons. Although similar ideas had been considered much earlier, Lorentz was the first to develop systematically a theory where the influence of matter on electric and magnetic phenomena is entirely explained in terms of atomic charges and currents, and he introduced the notion that the macroscopic electrical field strength and magnetic

induction are the averages of the microscopic fields. As you point out this is the logical starting point for your theoretical treatment.

Allow me a slight digression. In order to let the basic idea appear more clearly, Lorentz identified field strength and induction in vacuo. Einstein has once written that he considered this the greatest contribution of Lorentz to theory: that he freed us of the notion that also in vacuo there are two electric and two magnetic field quantities. From this point of view the mks system has both practical and didactical disadvantages. I would not ride this hobbyhorse of mine on this occasion, were it not that I happened to pass through Harvard when you had just received a letter from someone who urged that only the mks system should be used in the Harvard physics department, "like—said the letter—like in all the better physics departments in the country" or words to that effect. I remember the happy grin with which you reacted to that tactful remark, made to a department that included besides yourself Schwinger, Purcell, Ramsey and quite a few others.

Also the famous Lorentz-Van Leeuwen theorem, according to which there can be no magnetism in purely classical systems in thermal equilibrium is an important preamble to the quantum mechanical theory.

From then on you have continued to make important contributions to almost every branch of the theory of magnetism: paramagnetism and diamagnetism, paramagnetic relaxation and resonance, optical rotation in magnetic media, ferromagnetism and ferromagnetic resonance, anti-ferromagnetism. Let me mention a few of the highlights.

First of all, you and your coworkers carefully investigated the energy-levels of ions of the iron group and of the rare earths in the electric fields prevailing in crystals, thus elaborating in great detail the theoretical approach of Bethe. Then you studied the influence of dipole-dipole coupling. The famous Lorentz field can be regarded as corresponding to a first approximation. Your treatment is based on an expansion of the partition function in powers of $1/T$. Even here, there is a similarity to the work of Lorentz, who often preferred expansion in a power series to more general but less perspicuous formalisms. It might be said that your straightforward and systematic approach has the advantage that it tells us the truth and nothing but the truth. Unfortunately it does not give us the whole truth, because the series in $1/T$ converge only asymptotically and break down near singularities. Being myself of a rather lazy disposition, I must confess to a weakness for stopping the expansion after the first term. This gives us a specific heat proportional to $1/T^2$ which in turn leads to elegant thermodynamic formulae. You were also able to show that the Onsager model is a better approximation than the Lorentzfield.

As to paramagnetic relaxation I should like to mention specifically your later work on the so called "phonon bottleneck". No one will forget your very illuminating statement that there are only very few phonons

on speaking terms with the magnetic transitions in the spin system; at low temperatures these phonons interact only very slightly with the others.

In studying paramagnetic resonance you use again a power series expansion to calculate the second and fourth moment of the line profile. On the basis of this seemingly formalistic approach you were able, together with C. J. Gorter, to point out the remarkable phenomenon of exchange narrowing. You show that an exchange interaction gives no contribution to the second moment of the intensity distribution in a resonance line, but it does give a contribution to the fourth moment, which thus gets much larger than it would be for a Gaussian distribution. But this means that the peak of the distribution must be sharper than for the Gaussian curve.

I mentioned exchange interaction. You were among the first to use the now well-known expression, involving the scalar product of two spin vectors. In 1934 you devoted a lengthy paper to a further development of such vector methods, that are originally due to Dirac.

But let me not go into further details of your work, let me rather try to describe some of its main characteristics. First of all, it is always eminently sound: the physical ideas behind the calculations are clear and clearly stated. Secondly your work is thorough, accurate and straightforward. You are not afraid of long computations and intricate mathematics: if a frontal attack seems to be the most logical approach then you do not look for cunning detours. Thirdly, you keep in touch with experiments. You have been inspired by their results and inspired new experiments. It may surprise some people that this should be specifically mentioned, but I think it should, not only because this trait has made you many friends in this country, but also for another reason. Just as there exists in modern art a current of non-figurative painting, there would appear to exist to day a current of non-figurative theory, of abstruse and esoteric mathematics without much connection with reality. It seems to me there is a danger in this, especially for that class of theorists who under the guidance of a man like yourself might be able to do a workmanlike piece of research related to experimental findings, but who lack the genius that is required to create valuable new theory out of one's own ideas. After all, even Einstein himself became far less fertile as soon as he began to rely on formal mathematical constructions rather than on concrete physical ideas like he did in his earlier days.

Of course the same characteristics can also be found in your other, non-magnetic work, about which I want still to say a few words.

In the field of atomic spectra I should like to mention the paper with Abragam on the Zeeman effect in the microwave spectrum of atomic oxygen, where with your usual thoroughness and precision you work out all the corrections that should be taken into account before one can arrive at an exact comparison of experiment with the higher order quantum electro-dynamical corrections to the magnetic moment of the electron. I mention also two papers – with Weisskopf and with Margenau

respectively—on the pressure broadening of spectral lines because they are a direct continuation of work by Lorentz.

Molecular spectra might well be called your second major field of activity. But I do not want to go in detail. Let me rather relate one curious episode in which I was slightly involved myself. In 1934 Eckart published a paper in which he arrived at the surprising conclusion that the moments of inertia that determine the energy levels of a rotating molecule are quite different from those derived classically from the geometrical configuration. I had been dealing with a related problem in my thesis and was at first rather alarmed. However, by a somewhat sloppy argument, based on a contact transformation, I was able to convince myself that the moments of inertia of a molecule are really its moments of inertia. I wrote Eckart about this, but in the mean time you had tackled the question much more thoroughly. Now allow me to read a few lines from a note in your paper:

The difference between Casimir's method and mine is mainly superficial. The utilization of the contact transformation is the more elegant procedure, while perturbation theory is more explicit in showing how orders of magnitude enter in removal of the Eckart paradox . . .

I am quoting this passage not only because it gives me some personal pleasure, but because it illustrates one more characteristic of all your work: your carefulness and generosity in acknowledging the work of others. I remember that when someone complained that the heroic days of theoretical physics were over you replied by saying "physics is still an honourable profession". It will remain an honourable profession as long as there are physicists like yourself.

Here I stop my survey of your work, although I have said nothing about your work on the chemical bond, nothing about your work on cohesion in metals and so on.

But before I finish I have a few other things to say. I think that it would embarrass both Abigail and yourself if I were to speak at length about your generous hospitality, if I were to enumerate all the many acts of kindness towards Dutch physicists in distress and otherwise, so I shall not even try. But, on behalf of all of us: thank you.

Dear Van, this has been a very incomplete and very superficial summary, but how could one do justice in half an hour to half a century of work by a great physicist, and a hard working man at that? But I can sum up my summary in very few words: I know you are proud of the Dutch ancestry of the Van Vlecks: SO ARE WE.

Nadat de heer Casimir de medaille aan de heer Van Vleck heeft overhandigd, wenst de voorzitter de heer Van Vleck namens de Akademie van harte geluk met de hem toegekende onderscheiding.

Het woord is hierna aan de heer Van Vleck:

Dr. Casimir, Members of the Academy, Ladies and Gentlemen,

It is a great honor, and a particular personal pleasure to me, to receive the medal bearing the name of Lorentz, a name revered by every physicist. I like to recall that in 1922 I heard the three lectures he gave at Harvard, and I was thrilled to have the opportunity of shaking his hand. At the time I was only a graduate student. I am now 75 years old, Lorentz was two generations older than I, and very possibly I will be the last Lorentz medalist to have ever seen him.

In preparing my talk today I ran into difficulties. I had a title all selected, "My scientific links with the Netherlands", which is my theme today, but later I found that no title is used for the medalist's reply. Also I discovered that my links were so many that when I wrote up my remarks they consumed about twice the allotted time. So I condensed considerably and am submitting a manuscript for printing somewhat longer than what I will actually say.

My scientific ties with the Netherlands have been more sustained than with any other European country. I make this statement only after considerable reflection, for I also treasure my association with the physicists of England and France. However, they do not embrace as long an interval of time, nor have they influenced my work so often. There are three Dutch physicists, Gorter, Kramers, and Kronig, whose research interests have often been unusually close to my own. Consequently we often duplicated each other in the results of our investigations, sometimes published independently, sometimes unpublished to avoid duplication. I cannot emphasize too strongly that it was never a case of cut-throat competition such as is described in Watson's book "Double Helix" about the race to find the structure of the DNA molecule, but rather an unavoidable overlap when we were thinking along similar lines. In the case of Gorter, however, our results were more apt to be in series than in parallel, as he is also an experimentalist whereas I am a mere theorist. Kronig was not a native-born Dutchman nor was Ehrenfest but both men spent their entire professorial careers in Holland, and so are properly associated with the Netherlands.

My scientific links with the Netherlands began in 1910, when I was 11 years old. My father, a professor of Mathematics at the University of Wisconsin was on his sabbatical and we spent about a fortnight in Holland. A year earlier he had hired as instructor on his staff, Arnold Dresden, who had just taken his Ph.D. at the University of Chicago. Dresden was born in Amsterdam but moved to America for graduate study. He was back in Amsterdam for the summer vacation of 1910, I presume to see his parents. This was a happy coincidence for the Van Vleck family, as Dresden was very hospitable in taking us around to see things off the beaten path for Americans, since tourism was not then a well-organized industry.

After sixty-four years, I still recall a visit to the dunes near Haarlem,

the cheese market at Alkmaar, the slums of Amsterdam, and the crowded, smoky, third-class railway cars in which Dresden took us for reasons of economy. Things were different when fifty years later I spent a term as Lorentz professor. The slums in Amsterdam were replaced by modern blocks of apartments—I wish we had done as well in the United States—, there was no third-class, and instead modern electric trains.

Little did I realize in 1910 that only six years later, as a freshman at the University of Wisconsin, I would be taking my first course in mathematics at the then college level from the same Arnold Dresden.¹⁾ My early contacts with Dresden gave me my first intimation that American science has a great debt to those who emigrated from the Netherlands to the U.S.A. Dresden had a respected and useful career in American mathematics, serving for a long period of time as secretary of the American Mathematical Society and professor of Mathematics at Swarthmore after he left Wisconsin.

I will not attempt to enumerate the many scientists of Dutch origin who have contributed to American physics, often with the highest distinction. Among them are my friends Bloembergen, Goudsmit, and Uhlenbeck, all “correspondents” of this Academy, whereas I am a “foreign member”, my 17th century ancestry does not qualify me as a native-born Dutchman! Not being in the social sciences, I will not attempt to analyze in depth the reasons for the successes of the Hollanders who have moved to America. Besides native ability and pleasant personality, one quality that has helped is linguistical skill, in particular familiarity with English. This was not as wide-spread in 1910 as now. I remember Dresden then explaining to a native in some rural place which we visited that the Van Vleck family couldn't converse with him because they didn't speak Dutch. That surprised him, Dresden said, because, although he realized there were other languages, he thought everybody, anywhere, could at least speak Dutch in addition. During my sojourn in the Netherlands fifty years later I almost fell into the same kind of fallacy in reverse. It was always a shock to me when I found some one who couldn't speak English.

After 1910, my next academic contact was in 1914 when my father received an honorary degree from the University of Groningen ²⁾ on the

¹⁾ In his autobiography, “Scene of Change”, Warren Weaver (for many years Director of Science of the Rockefeller Foundation) tells how he also took courses as an undergraduate from Dresden, whom he characterizes as “A splendid undergraduate teacher . . . a real scholar, a gifted linguist, and an able and enthusiastic musician”.

²⁾ In 1934, when I moved to Harvard from Wisconsin, I met Prof. W. P. Allis, a physicist at M.I.T. We didn't recall that we had already met twenty years earlier at Groningen when our fathers received honorary degrees simultaneously. I learned this information from my mother. She had taken both of us children out for a ride during one of the scientific sessions.

occasion of its 300th anniversary. I recall the presence of Queen Wilhelmina and the impressive academic rites. On the way home to the U.S.A., just before the outbreak of World War I, I remember father telling me there was a very distinguished Dutch astronomer on board our ship, the Noordam. Presumably I was introduced to him. At any rate I recall his name. It was Kapteyn!

In 1922 I completed my doctoral thesis at Harvard under Prof. Kemble. The subject was the calculation in the old quantum theory of the binding energy of a crossed-orbit model of the helium atom which he proposed. Independently of Kemble, Niels Bohr suggested a similar model and persuaded Kramers³⁾ to make the same type of calculation. As a result both Kramers and I published our computations unbeknownst to each other and both obtained practically the same answer, needless to say, not in agreement with experiment; one more indication that the so-called old quantum theory was not the real thing despite the spectacular quantitative success in 1913 of the Bohr atomic model of the hydrogen spectrum.

In 1923, I spent a summer vacation in Europe with my parents. Our itinerary included Copenhagen. I called on Bohr at his laboratory and he persuaded me that when we were in Holland I should by all means look up Kramers in Apeldoorn where he was supposed to be visiting his aunt. When I reached there I discovered that he had gone to a resort north of Bergen aan Zee or Schoorl. A day or two later I succeeded in reaching him by a combination of train, steam tram and walking. Kramers was most cordial. This was the beginning of a friendship that lasted until his passing in 1952. I'm so glad that Kramers received the Lorentz medal shortly before his untimely death.

In 1924 I was an assistant professor at the University of Minnesota. On an American trip, Ehrenfest gave a lecture there which I heard. Conversely, strange to say, I gave a lecture with Ehrenfest in the audience. He always wanted to hear what was going on, and said he would like to hear a colloquium by a member of the staff. I was selected to give a talk on my "Correspondence Principle for Absorption", which I was writing up for publication in the Physical Review. Incidentally the same calculations were made independently and practically simultaneously by Slater at the suggestion of Kramers, both of whom were then at Copenhagen. Because of the appearance of my paper in the Physical Review their results were never published. I remember Ehrenfest being surprised at my being so young a man. The lengthy formulas for perturbed orbits in my publication on the three-body problem of the helium atom had

³⁾ Kramers was a name known to me even before 1922, as he published his calculations on the intensities of spectral lines in the Proceedings of the Danish Academy a year earlier. It is a commentary on the lack of interest in the quantum theory in those days that Harvard did not have the Proceedings, and I had to go to the American Academy of Arts and Sciences to get a copy and cut the pages in it.

given him the image of a venerable astronomer making calculations in celestial mechanics.

The quantum mechanical revolution metamorphosed physics in 1926. Numerous applications suggested themselves. One was to the dielectric constant of a diatomic molecule of the HCl type, where the classical value $1/3$ in the constant of proportionality was restored, replacing the absurd values found in the old quantum theory. Papers demonstrating this fact were published practically simultaneously by Mensing and Pauli, by Manneback, by Kronig, and by myself.

My earliest publication on magnetic susceptibilities in 1926 had perhaps the worst scientific blunder I ever made. I stated that Pauli's classical formula for the diamagnetic susceptibility also applied to molecules in quantum mechanics. This is untrue because only in the monatomic centrosymmetric case is the orbital angular momentum devoid of non-diagonal elements. It was Kronig who wrote me straightening me out. My mistake was a stupid one, as I had previously included non-diagonal elements in the dielectric calculations I have mentioned.

In 1928 I moved from Minnesota to Wisconsin and there a post-doctoral fellow with me, S. C. Wang, set up the secular determinant for the energy levels, and also the selection rules, for the asymmetrical top in quantum mechanics, a subject of considerable interest over the years to chemists because it helps in deriving the moments of inertia from band spectra. I persuaded Wang that it was simpler to use matrix than wave mechanical methods. Kramers and Ittman had already published two papers on the structure of the wave equation for the asymmetrical top. They had formulas for the secular equation similar to Wang's, which they were planning to publish, but the appearance of his paper made this superfluous. At that time I was interested in molecular spectra. Kronig's paper on λ -doubling fascinated me, though at first I found it hard to understand, and it led me to extend his formulas to more general cases inclusive of spin.

In 1930 I had a Guggenheim fellowship. After landing in England in February, where my wife and I spent a month, we crossed over to Holland. We met De Haas, Fokker, and a young student Gorter for the first time. Also we journeyed to Groningen to see Kronig and to Utrecht to see Kramers.

I spent most of my fellowship in Germany but a walk which I took with Kramers along one of the dunes (or more likely canals, as it was winter) was by far the most rewarding experience of my entire European visit. He told me of the possible importance of the crystalline electric fields in connection with magnetism, and advised me to read Bethe's paper on the group theory of energy levels in crystalline fields. As a result I was stimulated to work on the crystalline field theory of magnetism when I returned to the United States. In 1932 I was fortunate in having two very able post-doctoral students, Penney and Schlapp, who worked

out many details of this theory, which has been important both in solid state physics and inorganic chemistry. Penney was then "Bill", but he is now Lord Penney. When I lectured before the Royal Society of London in 1968 with Lord Penney in the audience, I showed an original slide of his 1932 calculations. For diversion, I told the audience how the Wisconsin soccer team would beat everybody when he played with them. I now have a sequel to this. Before coming to the United States, Penney had also been a post-doctoral fellow with Kronig at Groningen. In virtue of what I learned when I visited the Kronigs in Switzerland last year, I can now inform the Royal Academy of the Netherlands that Penney occasionally played with the Groningen University soccer team, and so his presence was appreciated in certain athletic as well as scientific circles in both our countries. Also a recent letter from Penney tells me that it was Kronig who advised him to come to Wisconsin on his Commonwealth Fellowship. So again I am indebted to Kronig.

In 1932, 1936, 1938, I made summer trips to Europe and in each case I included Leiden on my itinerary. Sometimes some of the key figures were away. To see the Kramers in 1932, my wife and I travelled to Veere in Zeeland. I do remember that in 1938, Gorter was in residence, newly married. He and his attractive wife Lilla took me to tea at Kasteel Oud Wassenaar, which I liked so much that I made it my home when I was Lorentz professor in 1960. In the early thirties, Gorter's interests were mainly in static magnetic susceptibilities but after his discovery of paramagnetic relaxation and dispersion in 1936, much of his research effort was focussed on the dynamic aspects of paramagnetism. He aroused my interest in this subject. In 1938 the Leiden experimentalists were particularly perplexed because caesium titanium atom relaxed so rapidly that no dispersion effects were detected. This appeared to contradict Kramers' theorem that the energy levels are doubly degenerate even in a static electric field when there is an odd number of electrons, a remarkable theorem that he told me about in 1930 and which was basic to many of my subsequent calculations on paramagnetic salts.⁴⁾ I took it as a challenge to explain away the paradox presented by the titanium

4) A French physicist, Jean Becquerel, made long visits to Leiden during the summers in the 1930's. For him it was a sort of pilgrimage to Mecca, for his primitive laboratory in the Jardin des Plantes in Paris lacked low-temperature facilities. At Leiden he was able to make his favorite experiments on the paramagnetic rotary dispersion of rare earth crystals, some of which he inherited from his father. He passionately tried to interest all theoretical physicists in his experiments, and succeeded with Kramers and with me, as both of us independently published papers on the closely related aspects of the Faraday effect in rare earths. I once asked Kramers in the 1930's what his research had been concerned with recently and he replied "That d— tysonite" of Becquerel's. It should, however, be said to the credit of Becquerel, that he was perhaps responsible for focusing so much interest on magnetic problems. Kramers' theorem on double degeneracy, for instance, may have been one consequence.

alum and so did Kronig. We both discovered that the explanation is found in the presence of certain terms that manifest themselves because the problem was a dynamic rather than static one. Kronig published his results before I had my manuscript ready. However, I still could write a paper because I had certain terms which are present besides those discovered by Kronig.

During World War II most of my research was concerned with radar and radar counter-measures. I showed that there was a troublesome absorption line for radar at about $1\frac{1}{4}$ cm wave length because of water molecules in the atmosphere. To calculate its intensity, I used the same secular determinants for the asymmetrical top that Kramers and Ittman and my student Wang had developed in 1928. Also I showed that at $\frac{1}{2}$ cm, a wave length so short it was never used, there would be even stronger absorption because of the so-called rho-type triplets of the oxygen molecule, whose theory was pioneered by Kramers in 1927. None of us who worked in molecular spectra in the 1920's dreamed that two decades later some of the results might have military significance, and four decades later important applications to radioastronomy and astrophysics.

An amusing episode occurred during the war when there was a committee meeting to discuss anomalous properties of the propagation of radar waves over the ocean. To explain this behavior, a distinguished British astronomer suggested that the atmosphere might have a strange dependence on the index of refraction on frequency because of salt particles in the air. I showed that this was nonsense by invoking the so-called Kramers-Kronig relations connecting the real and imaginary parts of the electric susceptibility. These relations showed unequivocally that the proposed frequency dependence was such that the atmosphere would be completely opaque to all radar!

In 1947, we at Harvard were very pleased to have Gorter as visiting professor for the summer term. He stressed that it was hard to understand why the absorption lines in certain paramagnetic salts were higher at the center and narrower than one expected from the broadening caused by the dipolar fields from other ions. Then one morning Gorter and I met each other and each had the explanation — the so-called phenomenon of exchange narrowing. We had arrived at it in different ways. I used a mathematical model based on the so-called method of moments. He had a more physical picture in how the spin waves associated with exchange spoil the coherence of the dipolar fields. So we wrote a paper together describing our conclusions reached by different routes.

The physicists in Holland showed remarkable vigor and morale in giving their research programs new momentum after the hardships of the war. It was with admiration and pleasure that I attended the conference on magnetism at Amsterdam in 1948, and on spectroscopy at radiofrequencies, also at Amsterdam, in 1950.

After the 1948 conference we crossed the Atlantic on the same ship

as the Kronigs (the last of my eight crossings on the Holland-America Line). Kronig was visiting professor at Johns Hopkins University and afterwards he told me that he thought America was a more attractive country than when he left it in 1927. I am not convinced that this was a completely objective reading, for in 1927 he was a bachelor while in 1948 he was accompanied by his charming wife Grethe.

In 1951 I was asked to write an article for a special memorial issue of Reviews of Modern Physics in honor of John Tate's 25 years as editor of the Physical Review. I felt it absolutely essential that I contribute because of Tate's kindness to me in my early career, but was dejected because I lacked any appropriate research idea. I had just been made Dean of Applied Science at Harvard and most of my time was consumed by administration. Fortunately I happened to remember the anomaly in sign in the commutation relations of the angular momentum of molecules when referred to moving rather than fixed axes. This had been first discovered by Klein. The underlying theory was elaborated and made more lucid by Casimir in his doctoral thesis in 1931. It occurred to me that the Klein-Casimir results had not been adequately exploited in their application to the various coupling situations in diatomic molecules. So fortunately, thanks to Casimir, I was able to come up with a satisfactory paper.

In 1960 I had the honor of being Lorentz professor at Leiden. It was a pleasant sojourn. As compared with the early pre-war days, the number of physicists competent in both the theoretical and experimental aspects of magnetism, had expanded even more than the number of automobiles. I made many new acquaintances in this field as well as renewing old ones. Most of my old friends were in high administrative positions. Gorter was director of the Kamerlingh Onnes Laboratory; van den Handel was editor-in-chief of Physica, Casimir was a managing director of Philips and Kronig was Rector Magnificus of Delft University. In my inaugural address I expressed the worry that the building program at Leiden had not kept up with the growth in physics. It is a pleasure to find that there are now new laboratories and this is no longer true.

There is a tradition regarding the last piece which is always played at the final concert of the late-spring early-summer season, so-called Pops concert series, of the Boston Symphony Orchestra. You might guess it would be Sousa's "Stars and Stripes Forever" — instead it is "Valerius". It is called by us the Netherlands Prayer of Thanksgiving. By the same token I will end my talk by sounding as its final note an expression of thankfulness both for the Lorentz medal and for the contacts which I have had over the years with the scientists of the Netherlands.

De voorzitter dankt de heer Van Vleck voor zijn toespraak en sluit de bijeenkomst.