NOBEL LECTURES

PHYSICS

1901 – 1921

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NOBEL LECTURES IN PHYSICS (1901–1921)

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Foreword

Since 1901 the Nobel Foundation has published annually *Les Prix Nobel* with reports from the Nobel award ceremonies in Stockholm and Oslo as well as the biographies and Nobel lectures of the Laureates. In order to make the lectures available to people with special interests in the different prize fields the Foundation gave Elsevier Publishing Company the right to publish in English the lectures for 1901–1970, which were published in 1964–1972 through the following volumes:

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Since the Elsevier series has been out of print for many years the Nobel Foundation has given World Scientific Publishing Company the right to publish these Nobel lectures, biographies and presentation speeches. The Nobel Foundation is very pleased that the intellectual and spiritual message to the world laid down in the laureates' lectures will, thanks to the efforts of World Scientific, reach new readers all over the world.

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Stockholm, March 1998

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Physics 1901

WILHELM CONRAD RÖNTGEN

«in recognition of the extraordinary services he has rendered by the discovery of the remarkable rays subsequently named after him»

Physics 1901

Presentation Speech by the former Rector of the National Archives C. T. Odhner, President of the Royal Swedish Academy of Sciences

Your Royal Highnesses, Ladies and Gentlemen.

The Royal Swedish Academy of Sciences received from Alfred Nobel the privilege of awarding two of the great Prizes which he founded in his will – the Prizes in those branches of Science which lay nearest his heart – those in Physics and Chemistry. Now that the Royal Academy of Sciences has received from its Committees their expert opinion on the suggestions sent in, as well as their own suggestions, it has made its decision, and as current President I am here to make it known.

The Academy awarded the Nobel Prize in Physics to Wilhelm Conrad Röntgen, Professor in the University of Munich, for the discovery with which his name is linked for all time: the discovery of the so-called Röntgen rays or, as he himself called them, X-rays. These are, as we know, a new form of energy and have received the name «rays» on account of their property of propagating themselves in straight lines as light does. The actual constitution of this radiation of energy is still unknown. Several of its characteristic properties have, however, been discovered first by Röntgen himself and then by other physicists who have directed their researches into this field. And there is no doubt that much success will be gained in physical science when this strange energy form is sufficiently investigated and its wide field thoroughly explored. Let us remind ourselves of but one of the properties which have been found in Röntgen rays; that which is the basis of the extensive use of X-rays in medical practice. Many bodies, just as they allow light to pass through them in varying degrees, behave likewise with X-rays, but with the difference that some which are totally impenetrable to light can easily be penetrated by X-rays, while other bodies stop them completely. Thus, for example, metals are impenetrable to them; wood, leather, cardboard and other materials are penetrable and this is also the case with the muscular tissues of animal organisms. Now, when a foreign body impenetrable to X-rays, e.g. a bullet or a needle, has entered these tissues its location can be determined by illuminating the appropriate part of the body

with X-rays and taking a shadowgraph of it on a photographic plate, whereupon the impenetrable body is immediately detected. The importance of this for practical surgery, and how many operations have been made possible and facilitated by it is well known to all. If we add that in many cases severe skin diseases, e.g. lupus, have been successfully treated with Röntgen rays, we can say at once that Röntgen's discovery has already brought so much benefit to mankind that to reward it with the Nobel Prize fulfils the intention of the testator to a very high degree.

No Lecture was delivered by Professor W. Röntgen.

Biography

Wilhelm Conrad Röntgen was born on March 27, 1845, at Lennep in the Lower Rhine Province of Germany, as the only child of a merchant in, and manufacturer of, cloth. His mother was Charlotte Constanze Frowein of Amsterdam, a member of an old Lennep family which had settled in Amsterdam.

When he was three years old, his family moved to Apeldoorn in The Netherlands, where he went to the Institute of Martinus Herman van Doorn, a boarding school. He did not show any special aptitude, but showed a love of nature and was fond of roaming in the open country and forests. He was especially apt at making mechanical contrivances, a characteristic which remained with him also in later life. In 1862 he entered a technical school at Utrecht, where he was however unfairly expelled, accused of having produced a caricature of one of the teachers, which was in fact done by someone else.

He then entered the University of Utrecht in 1865 to study physics. Not having attained the credentials required for a regular student, and hearing that he could enter the Polytechnic at Zurich by passing its examination, he passed this and began studies there as a student of mechanical engineering. He attended the lectures given by Clausius and also worked in the laboratory of Kundt. Both Kundt and Clausius exerted great influence on his development. In 1869 he graduated Ph.D. at the University of Zurich, was appointed assistant to Kundt and went with him to Würzburg in the same year, and three years later to Strasbourg.

In 1874 he qualified as Lecturer at Strasbourg University and in 1875 he was appointed Professor in the Academy of Agriculture at Hohenheim in Würtemberg. In 1876 he returned to Strasbourg as Professor of Physics, but three years later he accepted the invitation to the Chair of Physics in the University of Giessen.

After having declined invitations to similar positions in the Universities of Jena (1886) and Utrecht (1888), he accepted it from the University of Würzburg (1888), where he succeeded Kohlrausch und found among his collea-

gues Helmholtz and Lorenz. In 1899 he declined an offer to the Chair of Physics in the University of Leipzig, but in 1900 he accepted it in the University of Munich, by special request of the Bavarian government, as successor of E. Lommel. Here he remained for the rest of his life, although he was offered, but declined, the Presidency of the Physikalisch-Technische Reichsanstalt at Berlin and the Chair of Physics of the Berlin Academy.

Röntgen's first work was published in 1870, dealing with the specific heats of gases, followed a few years later by a paper on the thermal conductivity of crystals. Among other problems he studied were the electrical and other characteristics of quartz; the influence of pressure on the refractive indices of various fluids; the modification of the planes of polarised light by electromagnetic influences; the variations in the functions of the temperature and the compressibility of water and other fluids; the phenomena accompanying the spreading of oil drops on water.

Röntgen's name, however, is chiefly associated with his discovery of the rays that he called X-rays. In 1895 he was studying the phenomena accompanying the passage of an electric current through a gas of extremely low pressure. Previous work in this field had already been carried out by J. Plucker (1801–1868), J. W. Hittorf (1824–1914), C. F. Varley (1828–1883), E. Goldstein (1850–1931), Sir William Crookes (1832–1919), H. Hertz (1857–1894) and Ph. von Lenard (1862–1947), and by the work of these scientists the properties of cathode rays – the name given by Goldstein to the electric current established in highly rarefied gases by the very high tension electricity generated by Ruhmkorff's induction coil – had become well known. Röntgen's work on cathode rays led him, however, to the discovery of a new and different kind of rays.

On the evening of November 8, 1895, he found that, if the discharge tube is enclosed in a sealed, thick black carton to exclude all light, and if he worked in a dark room, a paper plate covered on one side with barium platinocyanide placed in the path of the rays became fluorescent even when it was as far as two metres from the discharge tube. During subsequent experiments he found that objects of different thicknesses interposed in the path of the rays showed variable transparency to them when recorded on a photographic plate. When he immobilised for some moments the hand of his wife in the path of the rays over a photographic plate, he observed after development of the plate an image of his wife's hand which showed the shadows thrown by the bones of her hand and that of a ring she was wearing, surrounded by the penumbra of the flesh, which was more permeable to the rays and there-

fore threw a fainter shadow. This was the first «röntgenogram» ever taken.

In further experiments, Röntgen showed that the new rays are produced by the impact of cathode rays on a material object. Because their nature was then unknown, he gave them the name X-rays. Later, Max von Laue and his pupils showed that they are of the same electromagnetic nature as light, but differ from it only in the higher frequency of their vibration.

Numerous honours were showered upon him. In several cities, streets were named after him, and a complete list of Prizes, Medals, honorary doctorates, honorary and corresponding memberships of learned societies in Germany as well as abroad, and other honours would fill a whole page of this book. In spite of all this, Röntgen retained the characteristic of a strikingly modest and reticent man. Throughout his life he retained his love of nature and outdoor occupations. Many vacations were spent at his summer home at Weilheim, at the foot of the Bavarian Alps, where he entertained his friends and went on many expeditions into the mountains. He was a great mountaineer and more than once got into dangerous situations. Amiable and courteous by nature, he was always understanding the views and difficulties of others. He was always shy of having an assistant, and preferred to work alone. Much of the apparatus he used was built by himself with great ingenuity and experimental skill.

Röntgen married Anna Bertha Ludwig of Zurich, whom he had met in the café run by her father. She was a niece of the poet Otto Ludwig. They married in 1872 in Apeldoorn, The Netherlands. They had no children, but in 1887 adopted Josephine Bertha Ludwig, then aged 6, daughter of Mrs. Röntgen's only brother. Four years after his wife, Röntgen died at Munich on February 10, 1923, from carcinoma of the intestine.

Physics 1902

HENDRIK ANTOON LORENTZ

PIETER ZEEMAN

«in recognition of the extraordinary service they rendered by their researches into the influence of magnetism upon radiation phenomena»

Physics 1902

Presentation Speech by Professor Hj. Théel, President of the Royal Swedish Academy of Sciences

Your Majesty, Your Royal Hignesses, Ladies and Gentlemen.

The Royal Swedish Academy of Sciences has decided to award this year's Nobel Prize for Physics to Professor Dr. Hendrik Antoon Lorentz of Leiden and Professor Dr. Pieter Zeeman of Amsterdam for their pioneering work on the connection between optical and electromagnetic phenomena.

Since the law of the conservation of energy was recognized as the first basic principle of modern physics, no realm of that science during the remarkable developments which have been based on this foundation has proved more fruitful than that which has had as its object the investigation of the connection between the phenomena of light and electricity.

Faraday, the great founder of the modern science of electricity, suspected this connection and devoted a great part of his experimental research to this very question. However, Maxwell was the first to take up Faraday's ideas again and develop them into a complete mathematical theory. According to this theory electrodynamic effects are transmitted through space at a finite speed and cause electrical currents, so-called displacement currents, even in non-conductors. Hence, every electrical current of periodically changing direction gives rise to an electrical wave motion, and light consists of just such a wave motion with an extremely short period.

This so-called electromagnetic theory of light of Maxwell's at first aroused comparatively little interest. Twenty years after its first appearance however it led to a scientific discovery which demonstrated its great significance in no uncertain manner. The German physicist Heinrich Hertz then succeeded in demonstrating that the electrical vibrations – which are generated under certain conditions when an electrically charged body is discharged – are propagated through the surrounding space in the form of a wave motion, and that the wave motion spreads at the speed of light and also possesses its properties. This gave a firm experimental basis for the electromagnetic theory of light.

In certain respects however Maxwell's theory of light was inadequate, in that it left individual phenomena unexplained. The greatest credit for the

further development of the electromagnetic theory of light is due to Professor Lorentz, whose theoretical work on this subject has borne the richest fruit. While Maxwell's theory is free from any assumptions of an atomistic nature, Lorentz starts from the hypothesis that in matter extremely small particles, called electrons, are the carriers of certain specific charges. These electrons move freely in so-called conductors and thus produce an electrical current, whereas in non-conductors their movement is apparent through electrical resistance. Starting from this simple hypothesis, Lorentz has been able not only to explain everything that the older theory explained but, in addition, to overcome some of its greatest shortcomings.

Alongside the theoretical development of the electromagnetic theory of light, experimental work also continued without interruption, and attempts were made to demonstrate in every detail the analogy between electrical wave motion and light. However, it was not sufficient to show a complete analogy between these phenomena; scientists wished far more to show that they were identical in nature, and to this end they attempted to demonstrate that magnetic forces act upon light in the same way as upon electric currents. It is this that Faraday was trying to prove, and the relevant experiments carried out by him led to the discovery of the rotation of the polarization plane of light by the effect of magnetic forces. His attempt to demonstrate the influence of magnetism on the radiation from a source of light – the last experiment with which Faraday was occupied – was, however, unsuccessful.

Professor Zeeman has recently succeeded in solving just this problem, which has up till now been the object of fruitless exertions on the part of many perspicacious research workers. Guided by the electromagnetic theory of light, Zeeman took up Faraday's last experiment, and, after many unsuccessful attempts, finally succeeded in demonstrating that the radiation from a source of light changes its nature under the influence of magnetic forces in such a way that the different spectral lines of which it consisted were resolved into several components. The consequences of this discovery give a magnificent example of the importance of theory to experimental research. Not only was Professor Lorentz, with the aid of his electron theory, able to explain satisfactorily the phenomena discovered by Professor Zeeman, but certain details which had hitherto escaped Professor Zeeman's attention could also be foreseen, and were afterwards confirmed by him. He showed, in fact, that the spectral lines which were split under the influence of magnetism consisted of polarized light, or in other words that the light vibrations are orientated in one particular way under the influence of the

magnetic force, and in a way which varies according to the direction of the beam of light in relation to this force.

For the physicist this discovery – the Zeeman effect – represents one of the most important experimental advances that recent decades have to show. For, through the demonstration that light is affected by magnetism in accordance with the same laws as vibrating electrically charged particles, clearly not only has the strongest support been given to the electromagnetic theory of light, but the consequences of Zeeman's discovery promise to yield the most interesting contributions to our knowledge of the constitution of spectra and of the molecular structure of matter. For these reasons the Swedish Royal Academy of Sciences has come to the conclusion that the discovery outlined here is of such great importance for the understanding of the connection between the forces of Nature and for the development of physical science that its recognition by the award of the Nobel Prize for Physics is justified. The Academy also bore in mind the great part which Professor Lorentz has played in the following up of this discovery through his masterly theory of electrons, which is moreover of the greatest significance as a guiding principle in various other realms.

Since the discovery in physics which the Royal Academy of Sciences wishes to recognize on this occasion represents the result of the most perspicacious research, both theoretical and experimental, the Academy considers that a division of the Nobel Prize for Physics between the two outstanding research workers, Professor Lorentz and Professor Zeeman, for their work on the connection between light and magnetism, is not only justified, but just.

HENDRIK A. LORENTZ

The theory of electrons and the propagation of light

Nobel Lecture, December 11, 1902

When Professor Zeeman and I received the news of the great honour of the high distinction awarded to us, we immediately began to consider how we could best divide our roles with respect to our addresses. Professor Zeeman was first to have described the phenomenon discovered by him, given the explanation of it, and given an outline of his later experimental work. My task should have been to consider rather more deeply our present-day knowledge of electricity, in particular the so-called electron theory.

I am more sorry than I can say that Professor Zeeman has been prevented by illness from undertaking the journey to Stockholm, and that therefore you will now only be able to hear the second half of our programme. I hope you will excuse me if under these circumstances I say only a little about the main theme, Zeeman's fine discovery. A short description of it, however, might well precede my further thoughts.

As is well known to you, Faraday even in his day discovered that magnetic forces can have an effect on the propagation of light. He showed in fact that in suitable conditions the vibrations of a beam of polarized light can be made to rotate by such forces. Many years later Kerr found that such a beam of light also undergoes similar changes when it is simply reflected from the polished pole of a magnet. However, it remained for Zeeman's talent to show that a magnetic field affects not only the propagation and reflection of light but also the processes in which the beam of light originates, that is to say that the rays emitted by a light source assume different properties if this source is placed in the gap between a magnetic north and south pole. The difference is shown in the spectral resolution of the light, when one is working with the type of light source whose spectrum consists of single bright lines – that is, with a coloured flame, an electrical spark, or a Geissler tube. To have a specific case before your eyes, imagine that my hands are the two poles, only much closer together than I am holding them now, and that the light source is between these poles, that is to say in the space immediately in front of me. Now if the spectrum of the light which shines on a point directly opposite me is investigated, there can be observed, instead of a single spectral

line such as can be seen under normal circumstances, a three-fold line, or triplet, whose components admittedly are separated from each other by a very small distance. Since each position in the spectrum corresponds to a specific frequency of light, we can also say that instead of light of one frequency the source is, under the influence of the magnetic field, emitting light of three different frequencies. If the spectrum consists of more than one line, then you can imagine that each line is resolved into a triplet. I must, however, add that the situation is not always as simple as this, and many spectral lines resolve into more than three components.

Before turning to the theory, I should like to remark that thanks to the speedy publication of research and the consequent lively exchange of views between scientists much progress must be considered as the result of a great deal of joint effort. Since it is expected of me, I am going to talk principally of my own ideas and the way in which I have come to them. I do beg of you, however, not to lose sight of the fact that many other physicists, not all of whom I can name in this short space of time, have arrived at the same or very similar conclusions.

The theory of which I am going to give an account represents the physical world as consisting of three separate things, composed of three types of building material: first ordinary tangible or ponderable matter, second electrons, and third ether. I shall have very little to say about ponderable matter, but so much the more about ether and electrons. I hope it will not be too much for your patience.

As far as the ether – that bearer of light which fills the whole universe – is concerned, after Faraday's discovery which I have already mentioned and also independently of it, many attempts were made to exploit the ether in the theory of electricity also. Edlund went so far as to identify the electric fluid with the ether, ascribing to a positively charged body an excess of ether and to a negatively charged one a deficiency of ether. He considered this medium as a liquid, subject to the Archimedean principle, and in this way succeeded in imputing all electrostatic effects to the mutual repulsion of particles of ether.

There was also a place in his theory for the electrodynamic attraction and repulsion between two metallic wires with electrical current flowing in them. Indeed, he formed a most remarkable conception of these effects. He explained them by the condition that the mutual repulsion of two particles of ether needs a certain time to be propagated from the one to the other; it was in fact an axiom with him that everything which occurs in Nature takes a

certain length of time, however short this may be. This idea, which has been fully developed in our present-day views, is found also in the work of other older physicists. I need only mention Gauss, of whom we know that he did not follow this up only because he lacked a clear picture of the propagation. Such a picture, he wrote to Wilhelm Weber, would be the virtual keystone of a theory of electrodynamics.

The way pioneered by Edlund, in which the distinction between ether and electricity was completely swept aside, was incapable of leading to a satisfactory synthesis of optical and electrical phenomena. Lorenz at Copenhagen came nearer the goal. You know, however, that the true founders of our present views on this subject were Clerk Maxwell and Hertz. In that Maxwell developed further and constructed a basis for the ideas put forward by Faraday, he was the creator of the electromagnetic theory of light, which is undoubtedly well known to you in its broad outline. He taught us that light vibrations are changes of state of the same nature as electric currents. We can also say that electrical forces which change direction extremely rapidly – many billions of times a second – are present in every beam of light. If you imagine a tiny particle in the path of a sunbeam, something like the familiar dust motes in the air, only considerably smaller, and if you also imagine that this particle is electrically charged, then you must also suppose that it is set into a rapid quivering movement by the light vibrations.

Immediately after Maxwell I named Hertz, that great German physicist, who, if he had not been snatched from us too soon, would certainly have been among the very first of those whom your Academy would have considered in fulfilling your annual task. Who does not know the brilliant experiments by which he confirmed the conclusions that Maxwell had drawn from his equations? Whoever has once seen these and learnt to understand and admire them can no longer be in any doubt that the features of the electromagnetic waves to be observed in them differ from light beams only in their greater wavelength.

The result of these and other investigations into the waves propagated in the ether culminate in the realization that there exists in Nature a whole range of electromagnetic waves, which, however different their wavelengths may be, are basically all of the same nature. Beginning with Hertz's «rays of electrical force», we next come to the shortest waves caused by electromagnetic apparatus and then, after jumping a gap, to the dark thermal rays. We traverse the spectrum far into the ultraviolet range, come across another gap, and may then put X-rays, as extremely short violent electromag-

netic disturbances of the ether, at the end of the range. At the beginning of the range, even before the Hertzian waves, belong the waves used in wireless telegraphy, whose propagation was established last summer from the south-west tip of England to as far as the Gulf of Finland.

Although it was principally Hertz's experiments that turned the basic idea of Maxwell's theory into the common property of all scientists, it had been possible to start earlier with some optimism on the task of applying this theory to special problems in optics. We will begin with the simple phenomenon of the refraction of light. It has been known since the time of Huygens that this is connected with the unequal rate of propagation of the beams of light in different substances. How does it come about, however, that the speed of light in solid, liquid, and gaseous substances differs from its speed in the ether of empty space, so that it has its own value for each of these ponderable substances; and how can it be explained that these values, and hence also the refractive index, vary from one colour to another?

In dealing with these questions it appeared once more, as in many other cases, that much can be retained even from a theory which has had to be abandoned. In the older theory of undulation, which considered the ether as an elastic medium, there was already talk of tiny particles contained in ponderable substances which could be set in motion by light vibrations. The explanation of the chemical and heating action of light was sought in this transmission of motion, and a theory of colour dispersion had been based on the hypothesis that transparent substances, such as glass and water, also contained particles which were set into co-vibration under the influence of a beam of light. A successor to Maxwell now has merely to translate this conception of co-vibrating particles into the language of the electromagnetic theory of light.

Now what must these particles be like if they can be moved by the pulsating *electrical* forces of a beam of light? The simplest and most obvious answer was: they must be electrically charged. Then they will behave in exactly the same way as the tiny charged dust motes that we spoke of before, except that the particles in glass and water must be represented, not as floating freely, but as being bound to certain equilibrium positions, about which they can vibrate.

This idea of small charged particles was otherwise by no means new; as long as 25 years ago the phenomena of electrolysis were being explained by ascribing positive charges to the metallic atoms in a solution of a salt, and negative charges to the other components of the salt molecule. This laid the

foundation of modern electrochemistry, which was to develop so rapidly once Prof. Arrhenius had expressed the bold idea of progressive dissociation of the electrolyte with increasing dilution.

We will return to the propagation of light in ponderable matter. The co-vibrating particles must, we concluded, be electrically charged; so we can conveniently call them « electrons », the name that was introduced later by Johnstone Stoney. The exact manner in which this co-vibration takes place, and what reaction it has on the processes in the ether, could be investigated with the aid of the well-known laws of electromagnetism. The result consisted of formulae for the velocity of propagation and the refractive indices, in their dependence on the one hand on the vibration period – i.e. on the colour of the light – and on the other hand on the nature and number of the electrons.

You will forgive me if I do not quote the rather complicated equations, and only give some account of their significance. In the first place, as regards the dependence of the refractive index on vibration period – that is, colour dispersion: in the prismatic spectrum and in rainbows we see a demonstration of the fact that the electrons in glass and water possess a certain mass; consequently they do not follow the vibrations of light of different colours with the same readiness.

Secondly, if attention is focussed on the influence of the greater or smaller number of particles in a certain space an equation can be found which puts us in a position to give the approximate change in the refractive index with increasing or decreasing density of the body – thus, for example, it is possible to calculate the refractive index of water vapour from that of water. This equation agrees fairly well with the results of experiments.

When I drew up these formulae I did not know that Lorenz at Copenhagen had already arrived at exactly the same result, even though he started from different viewpoints, independent of the electromagnetic theory of light. The equation has therefore often been referred to as the formula of Lorenz and Lorentz.

This formula is accompanied by another which makes it possible to deduce the refractive index of a chemical compound from its composition, admittedly only in rough approximation as was possible earlier with the aid of certain empirical formulae.

The fact that such a connection between the refraction of light and the chemical composition does exist at all is of great importance in the electromagnetic theory of light. It shows us that the power of refraction is not

one of those properties of matter which are completely transformed by the action of chemical combination. The relative positions of, and type of bond between, the atoms are not of primary importance as concerns the speed of propagation in a compound. Only the number of atoms of carbon, hydrogen, etc. is of importance; each atom plays its part in the refraction of light, unaffected by the behaviour of the others. In the face of these results we find it hard to imagine that the forces which bind an electron to its equilibrium position and on the intensity of which depends the velocity of light are generated by a certain number of neighbouring atoms. We conclude rather that the electron, together with whatever it is bound to, has its place within a single atom; hence, electrons are smaller than atoms.

Permit me now to draw your attention to the ether. Since we learnt to consider this as the transmitter not only of optical but also of electromagnetic phenomena, the problem of its nature became more pressing than ever. Must we imagine the ether as an elastic medium of very low density, composed of atoms which are very small compared with ordinary ones? Is it perhaps an incompressible, frictionless fluid, which moves in accordance with the equations of hydrodynamics, and in which therefore there may be various turbulent motions? Or must we think of it as a kind of jelly, half liquid, half solid?

Clearly, we should be nearer the answers to these questions if it were possible to experiment on the ether in the same way as on liquid or gaseous matter. If we could enclose a certain quantity of this medium in a vessel and compress it by the action of a piston, or let it flow into another vessel, we should already have achieved a great deal. That would mean displacing the ether by means of a body set in motion. Unfortunately, all the experiments undertaken on these lines have been unsuccessful; the ether always slips through our fingers. Imagine an ordinary barometer, which we tilt so that the mercury rises to the top, filling the tube completely. The ether which was originally above the mercury must be somewhere; it must have either passed through the glass or been absorbed into the metal, and that without any force that we can measure having acted upon it. Experiments of this type show that bodies of normal dimensions, as far as we can tell, are completely permeable to the ether. Does this apply equally to much larger bodies, or could we hope to displace the ether by means of some sort of very-large, very-fast moving piston? Fortunately, Nature performs this experiment on a large scale. After all, in its annual journey round the sun the earth travels through space at a speed more than a thousand times greater

than that of an express train. We might expect that in these circumstances there would be an end to the immobility of the ether; the earth would push it away in front of itself, and the ether would flow to the rear of the planet, either along its surface or at a certain distance from it, so as to occupy the space which the earth has just vacated. Astronomical observation of the positions of the heavenly bodies gives a sharp means of determining whether this is in fact the case; movements of the ether would assuredly influence the course of the beams of light in some way.

Once again we get a negative answer to our question whether the ether moves. The direction in which we observe a star certainly differs from the true direction as a result of the movement of the earth – this is the so-called aberration of light. However, by far the simplest explanation of this phenomenon is to assume that the whole earth is completely permeable to the ether and can move through it without dragging it at all. This hypothesis was first expressed by Fresnel and can hardly be contested at present.

If we wish to give an account of the significance of this result, we have one more thing to consider. Thanks to the investigations of Van der Waals and other physicists, we know fairly accurately how great a part of the space occupied by a body is in fact filled by its molecules. In fairly dense substances this fraction is so large that we have difficulty in imagining the earth to be of such loose molecular structure that the ether can flow almost completely freely through the spaces between the molecules. Rather are we constrained to take the view that each individual molecule is permeable. The simplest thing is to suggest further that the same is true of each atom, and this leads us to the idea that an atom is in the last resort some sort of local modification of the omnipresent ether, a modification which can shift from place to place without the medium itself altering its position. Having reached this point, we can consider the ether as a substance of a completely distinctive nature, completely different from all ponderable matter. With regard to its inner constitution, in the present state of our knowledge it is very difficult for us to give an adequate picture of it.

I hardly need to mention that, quite apart from this question of constitution, it will always be important to come to a closer understanding of the transmission of apparent distant actions through the ether by demonstrating how a liquid, for example, can produce similar effects. Here I am thinking in particular of the experiments of Prof. Bjerknes in Christiania*

* Now Oslo.

on transmitted hydrodynamic forces and of his imitation of electrical phenomena with pulsating spheres.

I come now to an important question which is very closely connected with the immobility of the ether. You know that in the determination of the velocity of sound in the open air, the effect of the wind makes itself felt. If this is blowing towards the observer, the required quantity will increase with the wind speed, and with the wind in the opposite direction the figure will be reduced by the same amount. If, then, a moving transparent body, such as flowing water, carries along with it in its entirety the ether it contains, then optical phenomena should behave in much the same way as the acoustical phenomena in these experiments. Consider for example the case in which water is flowing along a tube and a beam of light is propagated *within* this water in the direction of flow. If everything that is involved in the light vibrations is subject to the flowing movement, then the propagation of light in the flowing water will in relation to the latter behave in exactly the same way as in still water. The velocity of propagation relative to the wall of the tube can be found by adding the velocity of propagation *in* the water to the rate of flow of the water, just as, if a ball is rolling along the deck of a ship in the direction in which it is travelling, the ball moves relative to an observer on the shore at the sum of two speeds – the speed of the ship and the speed at which the ball is rolling on it. According to this hypothesis the water would drag the light waves at the full rate of its own flow.

We come to a quite different conclusion if we assume, as we now must, that the ether contained in the flowing water is itself stationary. As the light is partly propagated through this ether, it is easy to see that the propagation of the light beams, for example to the right, must take place more slowly than it would if the ether itself were moving to the right. The waves are certainly carried along by the water, but only at a certain fraction of its rate of flow. Fresnel has already demonstrated the size of this fraction; it depends on the refractive index of the substance – the value for water, for example, being 0.44. By accepting this figure it is possible to explain various phenomena connected with aberration. Moreover, Fresnel deduced it from a theoretical standpoint which, however ingenious it may be, we can now no longer accept as valid.

In 1851 Fizeau settled the question by his famous experiment in which he compared the propagation of light in water flowing in the direction of the beam of light with its propagation in water flowing in the opposite direction. The result of these experiments, afterwards repeated with the same

result by Michelson and Morley, was in complete agreement with the values assumed by Fresnel for the drag coefficient.

There now arose the question of whether it is possible to deduce this value from the new theory of light. To this end it was necessary first of all to develop a theory of electromagnetic phenomena in moving substances, with the assumption that the ether does not partake of their motion. To find a starting-point for such a theory, I once again had recourse to electrons. I was of the opinion that these must be permeable to the ether and that each must be the centre of an electric and also, when in motion, of a magnetic field. For conditions in the ether I introduced the equations which have been generally accepted since the work of Hertz and Heaviside. Finally I added certain assumptions about the force acting on an electron, as follows: this force is always due to the ether in the immediate vicinity of the electron and is therefore affected *directly* by the state of this ether and *indirectly* by the charge and velocity of the other electrons which have brought about this state. Furthermore, the force depends on the charge and speed of the particle which is being acted upon; these values determine as it were the sensitivity of the electron to the action due to the ether. In working out these ideas I used methods deriving from Maxwell and partly also relied on the work of Hertz. Thus I arrived at the drag coefficient accepted by Fresnel, and was able to explain in a fairly simple way most of the optical phenomena in moving bodies.

At the same time, a start was made on a general theory which ascribed all electromagnetic processes taking place in ponderable substances to electrons. In this theory an electrical charge is conceived as being a surplus of positive or negative electrons, but a current in a metallic wire is considered to be a genuine progression of these particles, to which is ascribed a certain mobility in conductors, whereas in non-conductors they are bound to certain equilibrium positions, about which, as has already been said, they can vibrate. In a certain sense this theory represents a return to the earlier idea that we were dealing with two electrical substances, except that now, in accordance with Maxwell's ideas, we have to do with actions which are transmitted through ether and are propagated from point to point at the velocity of light. Since the nature and manner of this transmission can be followed up in all its details, the demand that Gauss made for a theory of electrodynamics is fulfilled. I cannot spend any more time on these matters, but would like to mention that Wiechert at Göttingen and Larmor at Cambridge have produced very similar results, and that Prof. Poincaré has also contributed much to the development and evaluation of the theory.

I must also pass over many phenomena investigated in recent years, in which the concept of electrons has proved a useful guide, in order not to stray too far from the theory of the Zeeman effect.

When Prof. Zeeman made his discovery, the electron theory was complete in its main features and in a position to interpret the new phenomenon. A man who has peopled the whole world with electrons and made them co-vibrate with light will not scruple to assume that it is also electrons which vibrate within the particles of an incandescent substance and bring about the emission of light. An oscillating electron constitutes, as it were, a minute Hertzian vibrator; its effect on the surrounding ether is much the same as the effect we have when we take hold of the end of a stretched cord and set up the familiar motion waves in the rope by moving it to and fro. As for the force which causes a change in the vibrations in a magnetic field, this is basically the force, the manifestations of which were first observed by Oersted, when he discovered the effect of a current on a compass needle.

I will leave the explanation of triplets to Prof. Zeeman. I will confine myself to remarking that it is the negative electrons which oscillate, and that from the distance between the components into which the spectral line is resolved the ratio between the numerical value of the charge and the mass of these particles can be deduced. The results are in gratifying agreement with those which have been found in other contexts. The same or similar values for the ratio mentioned above have been found for the negative particles with which we are concerned in cathode rays.

A noteworthy aspect is the enormous size of the charge of these particles compared with their mass. A numerical example will give you some idea of this. Imagine that we had two iron spheres, each with a radius of one metre, situated ten metres apart, and that we gave each of them a surplus of our negative electrons of such a size that the mass of this surplus was the millionth part of a milligram. The spheres would then repel each other with a force equivalent to a weight of more than 80,000 kilograms and would therefore be able to reach a speed of many metres per second. I need hardly say that we are far from being able to make an experiment on this scale; we are not in a position to bring such a large number of electrons of one certain kind together on one body. If it were possible, we could carry out many interesting experiments which we can now only imagine. For instance, we could demonstrate the Zeeman effect on a simple pendulum. This can easily be made to swing in a circle, and if the bob is given an electrical charge the vertical component of the earth's magnetic field somewhat alters the period

of rotation, which is increased in one direction and reduced in the other. With the charges which we have at our disposal this difference is completely imperceptible, and Prof. Zeeman himself would be unable to observe the Zeeman effect on a pendulum.

Let us now turn from the relative sizes of charge and mass to their absolute values. We can at least give an estimate of these. If we combine the results to which Zeeman's experiments lead with those which can be deduced from the colour dispersion of gases, on the hypothesis that it is the same type of electrons which is under consideration in both cases, we come to the conclusion that the charge of an electron is of the same order of size as the charge of an electrolytic ion. The mass, however, is much smaller – about one eight-hundredth part of that of a hydrogen atom. J. J. Thomson at Cambridge has confirmed this result by a completely different method. At present we are not concerned with the exact value; the principal thing is that, as we have remarked before, the electron is very small compared with the atom. The latter is a composite structure, which can contain many electrons, some mobile, some fixed; perhaps it bears electrical charges which are not concentrated at single points but distributed in some other way.

Of the other magneto-optical phenomena I will only describe one in any greater detail. Soon after Zeeman had published his discovery the Russian physicists Egorov and Georgievsky found that a sodium flame situated between the poles of an electromagnet emitted partially polarized light – i.e., in its beams vibrations in a certain direction were present with a greater intensity than vibrations in the direction perpendicular to this. To describe this phenomena to you more exactly and at the same time to make clear how it is to be explained, I ask you to imagine once more that my hands are opposing magnetic poles and that the sodium flame is placed between them. Now if you were exactly opposite me, you would observe that the vertical electrical vibrations have a greater intensity than the horizontal ones.

This is connected with the fact that the flame has a certain thickness and that the beams emitted by the back half are partly swallowed up again as they pass through the front half. In accordance with a familiar rule, this absorption effect is strongest when all the incandescent particles in the flame are vibrating with the same period. It diminishes, and the flame therefore becomes brighter, as soon as this uniformity of the period of vibration is disturbed in any way. Now the magnetic field does this, in that instead of *one* common period of vibration it causes several to come into play. However, the in-

crease in illuminating power brought about in this way is restricted to the vertical vibrations in the flame that we are imagining. The horizontal vibrations of the electrons, from right to left and back again, are – it follows from the principles of the theory – not at all influenced by the magnetic field.

The conclusion therefore is that of the vibrations emitted only the vertical ones and not the horizontal ones are reinforced, which is the cause of the phenomenon we have observed.

I may add that this phenomenon is one of those magneto-optical effects which are most easily observed. The explanation given can also be put to the proof by the use of two flames instead of one, and an investigation of the absorption to which the light of the rear one is subject in the front one which has been situated between the magnetic poles.

Now that I have come to absorption, I must also consider the masterly and important theoretical considerations to which Prof. Voigt at Göttingen has been led by Zeeman's discovery. His theory differs from mine in that he always has in mind, not the emission of light, but its absorption. He explains the so-called *inverse* Zeeman effect – that is, the phenomenon that, when a strong white light is transmitted through the flame situated between the poles, instead of an absorption stripe we get a triplet of dark lines. On the basis of the parallelism between absorption and emission, it is possible to work back from this inverse phenomenon to the direct one.

Voigt does not refer to vibrating electrons; he is content to add appropriately chosen new terms to the equations which represent propagation in an absorbent medium. This method throws into relief the connection between the Zeeman effect and the rotation of the direction of vibration which was discovered by Faraday, and has other advantages, namely when vapours of rather high density, with correspondingly wider spectral lines, are concerned. Professor Zeeman will be able to give you an example of the effects of Voigt's theory.

However, any one who sets himself the task of drawing conclusions about the nature of the vibrations of electrons from these observations will, I think, prefer to choose the emission from very rarefied gases as an object of study. Here the radiation from the single molecules or atoms, undimmed by their effects on each other, is mirrored by the sharp lines in the spectrum. I followed this course in my later research, but came across considerable difficulties due to the fact that although the simple triplet frequently appears, in many cases there is resolution into more than three lines. This is a stumbling-block in the way of the theory. At all events it is easy to draw some general

rules about the state of polarization of the light beams corresponding to the different components – i.e., the shape and direction of their vibrations, but unfortunately I have hardly got any further.

As long as we have to deal only with resolution into three components, it is sufficient explanation to assume that each incandescent atom contains a single electron which can vibrate round its equilibrium position in all directions in the same way. This simple theory however leaves us high and dry as soon as the spectral lines split into more than three components in the magnetic field. It is obvious then that we must imagine atoms of more complicated structure, which are provided with electrical charges, and the parts of which are capable of making small vibrations, rather like the parts of an elastic resonant body. When I investigated the theory of such movements, which can be done without much difficulty, it became evident that such an arbitrary system would in general show no Zeeman effect at all.

However, no mathematical theory is necessary to perceive this and to find the condition necessary to bring about such an effect. Imagine a light source which shows a Zeeman triplet under the influence of a magnetic field. The three lines naturally cannot appear unless three types of vibration with slightly different periods are present in the particles of the light source. These periods however can only be different if the directions of movement or the shapes of the path in the three cases are not the same. We will say in short that we are dealing with three different *vibration patterns*, each with its own frequency, in the light source.

We will now gradually reduce the intensity of the magnetic field and finally let it disappear. As long as even a weak field is present, the three lines persist, only they draw nearer each other; the three vibration patterns thus always exist, but their frequencies approach a common limiting value, the frequency of the unresolved spectral line. In this way we come to suppose that even when we observe the latter, the three patterns of movement still exist, though without distinguishing themselves from each other by their frequency as is the case in a magnetic field. It can be expressed thus: the spectral line is already three-fold before the magnetic force comes into play, and this force has nothing else to do except, as it were, to push apart the three lines which originally coincided.

The same applies to a four-, five- or six-fold line, and you may rest assured that a spectral line will never resolve into six components unless, before the magnetic field is set up, each incandescent particle can vibrate in six different ways, that is, with exactly the same frequency.

Herein lies one necessary condition which is not quite so easy to fulfil. I could add a second condition for the appearance of clear-cut components of the spectral lines, but the one I have described should suffice to show that in the further development of the theory we cannot give free rein to our imagination. Instead, we are fairly limited in the choice of hypotheses. A suitable model of a vibrating atom would be an elastic spherical shell with a uniformly distributed electrical charge, whose surface is divided by nodal lines into a greater or lesser number of fields vibrating in different directions. However, I will not linger over the phenomena which appear in such models, for I fear I might wander too far from reality along these paths.

I have tried to delineate in broad outline how much – or it would be better to say, how little – the electron theory has achieved in the explanation of the new magneto-optical phenomena. If I were now to give an account of the experimental work, it would become clear that the experiments have made more considerable advances. The research workers have already made a start on comparing the different spectral lines of a chemical element with each other, with respect to their magnetic resolution, and on investigating the connection between this resolution and the regular relationships existing in spectra.

In this country, where the father of my worthy colleague Ångström, and Prof. Thalén have worked, and where Prof. Hasselberg continued his observations and measurements with indefatigable diligence, I hardly need to say how wonderful and rich a world these investigations into spectra have opened up to us. A world whose laws we are beginning to understand. It has become apparent that many line spectra are constructed according to a definite type; the lines are arranged in certain series, and in such a way that each series consists of lines which are distributed over the spectrum in accordance with a fairly simple law, and moreover there are relationships between the one series and the next. These relationships, in the clarification of which Prof. Rydberg and the German physicists Kayser and Runge have been particularly prominent, suggest a connection between the magnetic resolution of lines belonging to the same series. Such a connection has now in fact been confirmed. Runge and Paschen have found, in their investigation of the Zeeman effect in mercury, that all the lines of one series are resolved in exactly the same way.

I am convinced that the theory will only make significant progress when it also turns its attention not simply to one single spectral line but to all the lines of a chemical element. When once we succeed in building a theoretical

foundation for the structure of spectra, then and not before then will we be able to grasp successfully the more complicated forms of the Zeeman effect. It would be better to say: in the future, research into the regular relationships in the spectra and into the Zeeman effect must go hand in hand; thus they will be able to lead some day to a theory of light emission, the achievement of which is one of the greatest aims of present-day physics.

The electron theory also presents an enormous field of study outside the realm of magneto-optical phenomena. For one thing, the free-moving electrons, with which we are concerned in cathode rays and in some types of Becquerel rays, give rise to many interesting problems. I will single out only the important question of the so-called apparent mass of these particles. A definite magnetic field in the surrounding ether – and hence also a certain amount of energy in this medium – are inextricably connected with every movement of an electron; we can therefore never set an electron in motion without simultaneously imparting energy to the ether. To do this a great amount of work is necessary, and we must employ a greater force than if it were not necessary to set up this magnetic field. Calculation shows that the force required is the same as would be needed if the mass were somewhat greater than it is in reality. In other words, if we determine the mass in the usual way from the phenomena, we get the true mass increased by an amount which we can call the apparent, or electromagnetic, mass. The two together form the effective mass which determines the phenomena.

Now the investigations published by Kaufmann and Abraham in the past year have shown that the apparent mass is by no means to be discounted. It certainly forms a considerable part of the effective mass, and there is a possibility that in the end we shall have to ascribe apparent mass only and never true mass at all to electrons.

The peculiar thing about this apparent mass is, moreover, that it is not constant, but depends on the velocity; consequently the study of the motion of the electron differs in many ways from ordinary dynamics.

It is hard to say if it will ever be possible to examine further with any success the question of the nature of an electron, which the research I have mentioned has already touched on. Meanwhile, even without ascertaining this, we can continue to test the basic assumptions of the theory in practice, and to draw from the properties of ponderable matter conclusions about the electrons it contains. The conductivity of electricity and heat by metals, thermoelectricity, permanent and temporary magnets, heat radiation and absorption, the optical, electrical and magnetic properties of crystals – all

these aspects promise us a rich harvest. And even farther fields are opening up to our view. If it is true, as had been concluded from optical experiments, that the dimensions of a ponderable body undergo a slight alteration as soon as it moves through the motionless ether, we must conclude that molecular forces are transmitted through the ether in a way similar to electrical effects, and that leads to the idea that these forces are basically of an electromagnetic nature and the material particles among which they exist are composed of electrons – or, at least, the electrical charges of these particles are not something accidental but something very significant, also where molecular forces are concerned.

Thus we hope that the electron hypothesis, as it is being taken up in widely different sectors of physics, will lead to a general theory embracing many aspects of physics and also of chemistry. Perhaps it will be itself completely transformed on the long journey; however, there can hardly be any doubt that our hypotheses about the connection of widely differing phenomena with electromagnetism will prove correct, and that hence, in so far as it relates to the nature of ponderable matter, that general theory will be an electrochemical one, as Berzelius already dimly foresaw and as he tried to demonstrate with the resources at his disposal.

This is admittedly a prospect of the distant future, and the individual scientist can scarcely hope to make any significant contribution to its achievement. As far as I am concerned, I would count myself fortunate if it fell to me, encouraged and spurred on as I am by the high distinction awarded to me by your Academy, to play a modest part in the solution of the problems which next present themselves to us.

I close with the warmest thanks for the attention with which you have listened to me.

Biography

Hendrik Antoon Lorentz was born at Arnhem, The Netherlands, on July 18, 1853, as the son of nursery-owner Gerrit Frederik Lorentz and his wife *née* Geertruida van Ginkel. When he was four years old, his mother died, and in 1862 his father married Luberta Hupkes. In those days the grade school did not only have school hours in the morning and in the afternoon, but also in the evening, when teaching was more free (in a sense resembling the Dalton method). In this way, when in 1866 the first highschool (H.B.S.) at Arnhem was opened, Hendrik Lorentz, as a gifted pupil, was ready to be placed in the 3rd form. After the 5th form and a year of study of the classics, he entered the University of Leyden in 1870, obtained his B.Sc. degree in mathematics and physics in 1871, and returned to Arnhem in 1872 to become a night-school teacher, at the same time preparing for his doctoral thesis on the reflection and refraction of light. In 1875, at the early age of 22, he obtained his doctor's degree, and only three years later he was appointed to the Chair of Theoretical Physics at Leyden, newly created for him. In spite of many invitations to chairs abroad, he always remained faithful to his Alma Mater. From 1912 onward, when he accepted a double function at Haarlem as Curator of Teyler's Physical Cabinet and Secretary of the «Hollandsche Maatschappij der Wetenschappen» (Dutch Society of Sciences), he continued at Leyden as Extraordinary Professor, delivering his famous Monday morning lectures for the rest of his life. The far-seeing directors of Teyler's Foundation thus enabled his unique mind to be freed from routine academic obligations, permitting him to spread his wings still further in the highest secluded realms of science, which are attainable by so few.

From the start of his scientific work, Lorentz took it as his task to extend James Clerk Maxwell's theory of electricity and of light. Already in his doctor's thesis, he treated the reflection and refraction phenomena of light from this standpoint which was then quite new. His fundamental work in the fields of optics and electricity has revolutionized contemporary conceptions of the nature of matter.

In 1878, he published an essay on the relation between the velocity of light

in a medium and the density and composition thereof. The resulting formula, proposed almost simultaneously by the Danish physicist Lorenz, has become known as the Lorenz-Lorentz formula.

Lorentz also made fundamental contributions to the study of the phenomena of moving bodies. In an extensive treatise on the aberration of light and the problems arising in connection with it, he followed A. J. Fresnel's hypothesis of the existence of an immovable ether, which freely penetrates all bodies. This assumption formed the basis of a general theory of the electrical and optical phenomena of moving bodies.

From Lorentz stems the conception of the electron; his view that his minute, electrically charged particle plays a rôle during electromagnetic phenomena in ponderable matter made it possible to apply the molecular theory to the theory of electricity, and to explain the behaviour of light waves passing through moving, transparent bodies.

The so-called Lorentz transformation (1904) was based on the fact that electromagnetic forces between charges are subject to slight alterations due to their motion, resulting in a minute contraction in the size of moving bodies. It not only adequately explains the apparent absence of the relative motion of the Earth with respect to the ether, as indicated by the experiments of Michelson and Morley, but also paved the way for Einstein's special theory of relativity.

It may well be said that Lorentz was regarded by all theoretical physicists as the world's leading spirit, who completed what was left unfinished by his predecessors and prepared the ground for the fruitful reception of the new ideas based on the quantum theory.

In 1919, he was appointed Chairman of the Committee whose task it was to study the movements of sea water which could be expected during and after the reclamation of the Zuyderzee in The Netherlands, one of the greatest works of all times in hydraulic engineering. His theoretical calculations, the result of eight years of pioneering work, have been confirmed in actual practice in the most striking manner, and have ever since been of permanent value to the science of hydraulics.

An overwhelming number of honours and distinctions from all over the world were bestowed on Lorentz. International gatherings were presided over by him with exceptional skill, both on account of his amiable and judicious personality and his masterly command of languages. Until his death he was Chairman of all Solvay Congresses, and in 1923 he was elected to the membership of the «International Committee of Intellectual Coopera-

tion» of the League of Nations. Of this Committee, consisting of only seven of the world's most eminent scholars, he became the President in 1925.

Through his great prestige in governmental circles in his own country, Lorentz was able to convince them of the importance of science for national production. He thus initiated the steps which finally led to the creation of the organisation now generally known under the initials T.N.O. (Dutch for Applied Scientific Research).

Lorentz was a man of immense personal charm. The very picture of unselfishness, full of genuine interest in whoever had the privilege of crossing his path, he endeared himself both to the leaders of his age and to the ordinary citizen.

In 1881 Lorentz married Aletta Catharina Kaiser, whose father, J. W. Kaiser, Professor at the Academy of Fine Arts, was the Director of the Museum which later became the well-known Rijksmuseum (National Gallery) of Amsterdam, and the designer of the first postage stamps of The Netherlands. There were two daughters and one son from this marriage. The eldest daughter Dr. Geertruida Luberta Lorentz is a physicist in her own right and married Professor W. J. de Haas, Director of the Cryogenic Laboratory (Kamerlingh Onnes Laboratory) of the University of Leyden.

Lorentz died at Haarlem on February 4, 1928.

PIETER ZEEMAN

Light radiation in a magnetic field

Nobel Lecture, May 2, 1903

As Professor Lorentz told you last December, immediately after hearing of the great and very honourable distinction awarded to us, we set to work to see how best to co-ordinate our two lectures. To my great regret I was unable to be present here for Professor Lorentz's lecture, but he was able to report to you on present electron theory from his viewpoint, only briefly touching on the experimental investigations which have occupied me in recent years. I hope that you will allow me therefore to emphasize these experimental investigations. Two fields of physics, light and magnetism, are combined in the subject of today's lecture, whose history dates only from the days of Michael Faraday. The wonderful discovery of the connection between light and magnetism, which he made in 1845, was the reward for an investigation carried out with indefatigable patience and tenacity. Today we call this connection the magnetic rotation of the polarization plane. Faraday succeeded in showing that the plane in which light oscillations take place, is rotated as soon as light passes through special magnetizable bodies along the lines of force. Faraday himself called his discovery the magnetization of light and the illumination of magnetic lines of force. His contempories did not understand this name, which perhaps corresponded more to what he was searching for than to what he found. Throughout his life his hopes, desires and yearnings led him to make repeated investigations into the connection between light, magnetism, and electricity.

The last experiment recorded in Faraday's laboratory notebook and ostensibly the last in his life, gives an indication of the extent to which his spirit was still occupied with the boundary region of possible phenomena.

It was on March 12, 1862, in the laboratory of the Royal Institution that Faraday carried out this experiment. The notes in his notebook, although not quite clear, leave no doubt that he was attempting to demonstrate by means of a spectroscope that magnetism has a direct effect on a light source. The result was however absolutely negative, and Faraday writes in his notebook «not the slightest effect demonstrable either with polarized or unpolarized light».

Perhaps it was because of this observation that Maxwell, at a meeting of the British Association in Liverpool on September 15, 1870, said of the light-radiating particles in a flame « that no force in nature can alter even very slightly either their mass or their period of oscillation », a statement which, coming from the mouth of the founder of the electromagnetic light theory and spoken with such intensity, must really surprise present-day physicists.

It was not simply out of a spirit of contradiction that I exposed a light source to magnetic forces. The idea came to me during an investigation of the effect discovered by Kerr on light reflected by magnetic mirrors.

When it is a question of splitting up the light of a luminous gas into very fine detail, the simple glass prism of Newton and Frauenhofer is of no use, and the physicist has recourse to the excellent aid which we owe to Rowland: the concave grating. Most physics institutes possess this polished metal mirror with a very large number of grooves, say 50,000 over a width of 10 cm scratched on by means of a diamond. A beam of compound light is no longer reflected by the lined surface in the ordinary way; instead each special kind of light follows its own path.

Of course the light source must be very restricted for the large number of beams corresponding to the various kinds of light to appear separately. This is ensured by placing the light source behind an opaque screen with a linear slit. The spectral image produced can be observed, and from the location and intensity of the linear-slit images we can determine how the different kinds of light in the light being studied are distributed on the basis of the period of oscillation and intensity. A further main advantage of Rowland's grating is that it is now no longer scratched on plane surfaces, but on spherical concave surfaces with a radius of say 3 metres, so that real images are produced of luminous lines without the need for the insertion of lenses. Moreover, photography has made it possible to fix these images and now provides us with a permanent record of each observed spectrum, which can be measured out at any time.

When we study the well-known Bunsen sodium flame by means of Rowland's grating, we see a spectrum consisting mainly of two separate sharp yellow lines, which in our grating lie about 1 mm from each other. We see that sodium radiation consists of two kinds of light, the periods of oscillation of which differ only very slightly (1 in 1000) from each other. We confined our attention exclusively to one of these two lines.

I must ask you now to go with me into the Physics Institute of Leiden University. In August, 1896, I exposed the sodium flame to large magnetic

forces by placing it between the poles of a strong electromagnet. Again I studied the radiation of the flame by means of Rowland's mirror, the observations being made in the direction perpendicular to the lines of force. Each line, which in the absence of the effect of the magnetic forces was very sharply defined, was now broadened. This indicated that not only the original oscillations, but also others with greater and again others with smaller periods of oscillation were being radiated by the flame. The change was however very small. In an easily produced magnetic field it corresponded to a thirtieth of the distance between the two sodium lines, say two tenths of an Ångström, a unit of measure whose name will always recall to physicists the meritorious work done by the father of my esteemed colleague.

Had we really succeeded therefore in altering the period of vibration, which Maxwell, as I have just noted, held to be impossible? Or was there some disturbing circumstances from one or more factors which distorted the result? Several of such might be mentioned.

We doubted the result. We studied the light source in the direction of the magnetic force, we perforated the poles of the magnet; but even in the direction of the magnetic lines of force we found that our result was confirmed. We also studied the reverse phenomenon, the absorption of light in sodium vapour, and this too satisfied our expectations. We then asked, do different substances behave in different ways? What happens when the magnetic force is raised to the maximum attainable values? How do different lines of the same substance behave? But before these questions could be answered, theory took over.

I was in fact able to verify experimentally some conclusions which followed from the theory of optical and electrical phenomena of my esteemed teacher and friend Professor Lorentz. This theory assumes that all bodies contain small electrically charged mass particles, «electrons», and that all electrical and optical processes are based on the position and motion of these «electrons». Light oscillations result from the vibration of the «electrons». On the basis of Lorentz's theory, if we limit ourselves to a single spectral line, it suffices to assume that each atom (or molecule) contains a single moving electron.

Now if this electron is displaced from its equilibrium position, a force that is directly proportional to the displacement restores it like a pendulum to its position of rest. In this model the electrons are represented by the red balls and the direction of the magnetic force by the arrows. Now all oscillatory movements of such an electron can be conceived of as being split up into

force, and two circular oscillations perpendicular to this direction rotating in opposite directions. In the absence of a magnetic field the period of all these oscillations is the same. But as soon as the electron is exposed to the effect of a magnetic field, its motion changes. According to well-known electrodynamic laws, an electron moving in a magnetic field is acted upon by a force which runs perpendicular to the direction of motion of the electron and to the direction of the magnetic field, and whose magnitude is easily determined. Here the rectilinear oscillation is not changed by the magnetic field, the period remains the same; on the other hand the two circular oscillations are subjected to new forces which, running parallel with the radius, either increase or decrease the original central force. In the first case the period of oscillation is reduced, in the second it is increased.

Now it is easy to determine the light motion to which this type of motion of the electrons will lead.

Let us consider first what happens in a direction running *perpendicular to the lines of force*. To the three electron motions there correspond three electrical oscillations, or in terms of the electromagnetic light theory three light oscillations of different periods. Thus the light source will emit *three*-colour light instead of the original *one*-colour light. Therefore, instead of the single non-polarized spectral line we shall see three separate lines when we place the light source in a magnetic field. The different directions of oscillation of the electrons affect the polarization state of the emitted light. The light of the middle component oscillates in parallel with, and that of the outer components perpendicular to the lines of force.

I will presently show you as an illustration a line which actually displays this behaviour postulated by Prof. Lorentz's theory.

But let us first consider the rays which run *parallel with the lines of force*. For this purpose I will rotate the model so that the arrow points in your direction. The opposite circular oscillations of the electrons excite two circularly polarized rays rotating in opposite directions, one having a longer and the other a shorter period of oscillation than the original spectral line. The original spectral line splits up under the action of the magnetic field into two components which are circularly polarized in opposite directions. The light source emits *two*-colour light.

I would now like to project for you two enlargements of photographs taken with the aid of Rowland's grating.* The lines are cadmium lines. In the first half of the picture you can see the unchanged line, and in the second

* A number of lantern slides were projected in the course of the lecture.

rectilinear oscillations occurring in the direction of the magnetic lines of half the line changed by magnetic forces, the so-called triplet, which we see in the direction perpendicular to the lines of force.

Secondly I will project for you a cadmium line observed in the direction of the lines of force. The first half of the picture shows the unchanged line, and the second half the double line or doublet produced by the magnetic forces.

You see how beautifully the consequences following from Prof. Lorentz's theory were confirmed by observation in these cases. I should point out, however, that at first some difficulty was experienced in observing the phenomena predicted by the theory, owing to the extreme smallness of the variations in the period of oscillation.

I have just said that the change was extremely small; but it could be said that it was unexpectedly large. The magnetic cleavage of the spectral lines is dependent on the size of the charge of the electron, or, more accurately, on the ratio between the mass and the charge of the electron. Let us see what the observations teach us. When Prof. Lorentz published his theory in 1895, no data were available from which to estimate the ratio between the mass and the charge of the light-exciting particles, and in this theory the ratio was left undefined. We can now calculate this ratio from the magnitude of the magnetic splitting of the spectral lines: it is 10^7 c.g.s. units per gram, a colossal number even for the physicist, since it is 1,000 times as great as the similar number which was known from electrolysis phenomena in the case of hydrogen atoms. This makes it most probable for the physicist that in the luminous particles only ca. 1/1,000th of the atom oscillates, and that the main mass of the atom remains virtually stationary. The oscillating electrons and the electrolysis ions were found to be not identical with each other; if they had been, the splitting of the spectral lines would have been only one thousandth of that observed, and then I should not have had the honour of addressing you in Stockholm today.

A further question must also be answered here and now, namely, are the oscillating particles positively or negatively charged?

We observed the doublet in the direction of the magnetic lines of force and studied the sign of the polarization. Then I suddenly resolved the problem: the oscillating electrons are *negatively* charged. We now know that cathode rays, which can occur in tubes filled with highly rarefied gases, are negative particles with the same high charge/mass ratio. We can conclude that that which vibrates in the light source is the same as that which travels in cathode rays.

We can hardly avoid recalling the two titles of Faraday's basic work: «Magnetization of light», «Illumination of lines of force» They appear to us to be almost prophesies, because we have now seen that light can in fact be magnetized, and according to Prof. Arrhenius's theory we have in nature itself, in the northern lights, an example of illumination of the magnetic lines of force of the Earth by the electrons escaping from the sun.

Nature gives us all, including Prof. Lorentz, surprises. It was very quickly found that there are many exceptions to the rule of splitting of the lines only into triplets. The French physicist Cornu was perhaps the first to observe that, contrary to the elementary theory, in some cases splitting into four lines, a quadruplet, occurs. In other cases splitting into five, six or even nine lines can be observed. In the line-rich spectrum of iron we find a whole selection of different forms. Very soon a number of physicists were working in the extended field; I need only name Becquerel, Cotton, Michelson, Preston, Righi, Runge, and Paschen. If I had more time at my disposal, I would gladly deal in greater detail with the work of the last-mentioned investigators. For the present, however, I must confine myself to projecting a cadmium line for you, which is split up into four lines, and negatives of a mercury line which has split up into nine components, and for which I am grateful to Prof. Runge. But despite this very complicated splitting-up, even when larger aids are used, the division into three groups of oscillations, two perpendicular to and one parallel with the lines of force, assumed in Lorentz's elementary theory, remains valid, as this photograph of the nonet shows.

It was natural that, soon after I had succeeded in splitting up lines, I should also study how the different lines behave in this respect. I was soon able to show by investigating the zinc lines that there are great differences in the splitting-up of different lines of a substance. Particularly great differences were found in lines belonging to different series, the discovery of which we owe to the lucid investigations of your countryman Prof. Rydberg, and in particular Professors Kayser and Runge.

I found very great differences in the lines of the different series, and it appeared that the splitting-up, contrary to the postulations of the elementary theory, expressed in the scale of oscillation frequencies, is *not* the same for all lines in the same magnetic field. We can conclude from this on the basis of Lorentz's theory that the charge/mass ratio is not the same for all electrons.

I would now like to talk about three separate phenomena, first a phenomenon which I have not been able to observe, secondly phenomena which I have hardly been able to verify, and thirdly a surprising phenomenon.

All the results which have been discussed so far have related to line spectra; but in the case of many bodies we also know of the existence of band spectra. Here a difference is found: the band spectrum displayed by iodine vapour or bromine vapour as an absorption medium at low temperatures, remains unchanged in a magnetic field; I personally have been unable to bring about any change in the extremely accurate images which Prof. Hasselberg has given us of the absorption spectra of bromine and iodine vapour, even with the strongest magnetic fields.

We are indebted to Prof. Voigt in Göttingen for a comprehensive theory of magneto-optical phenomena. The triplet you have seen today was absolutely symmetrical, as postulated in the elementary theory. Now on the basis of his theory Prof. Voigt was able to predict that as a result of the action of *weak* magnetic forces asymmetry should occur. According to him, the two external components should have different light intensities and be at different distances from the centre line. In the case of iron, zinc, and cadmium lines I was able to observe both asymmetries; because of their extreme smallness, however, I cannot demonstrate the phenomena in the projector.

However, another phenomenon, which will give you some idea of the scope of Voigt's theory, is more striking. In this phenomenon Faraday's magnetic rotation of the polarization plane and the magnetic splitting of the spectral lines, are intimately connected with each other.

The rotation of the polarization plane is extraordinarily small in all gases, thus also in sodium vapour. As Macaluso and Corbino found, it is only in the case of those colours which lie close to an absorption band of the vapour that the rotation is very great, of the order of 180°, and the rotation takes place in the *positive* direction, the direction of the current exciting the magnet.

What about the rotation inside the absorption band?

Prof. Voigt was able to predict that in the case of highly rarefied vapours the rotation must be negative in the zone between the two components of the doublet, i.e. opposite in direction to that outside the band, and also very great. I had the pleasure of confirming this theoretical finding in experiments on sodium vapour. Provided that the vapour is highly rarefied, the rotation in very strong fields between the lines of the doublet can rise to $-400°$.

To give you some idea of the distribution of the rotations, I will show you two negatives connected with this investigation.

The magnetic field is *not* set up.

The two dark vertical lines are the absorption lines of sodium vapour, the well-known D-lines. The reason why they are so broad is that the vapour

was very dense. The horizontal bands are interference bands, which were produced by means of a special device. They indicate the points where the direction of oscillation is the same. The directions of oscillation in each of the successive bands differ by 180°.

Now as soon as the magnetic field is set up we get the image now being projected. On each side of each of the D-lines the bands bend *upwards*, the more so the smaller the distance, because the rotation in the vicinity of the bands grows very rapidly and reaches almost 180° in the immediate neighbourhood of the bands. Within the bands a blurred band only is visible.

The phenomenon becomes far clearer once the vapour is highly rarefied. The bands bounding the components *rise* as before. At the same time, however, the inner band becomes detached; it has *fallen*, the rotation is negative. In our third image the rotation in the case of one of the D-lines is about −90°, in the other everything is more blurred, the rotation is about −180°.

Summarizing briefly the results of the tests described in the light of Lorentz's theory, it can be stated that firm support has been found for the assertion that electricity occurs at thousands of points where we at most conjectured that it was present. Innumerable electrical particles oscillate in every flame and light source. We can in fact assume that every heat source is filled with electrons which will continue to oscillate ceaselessly and indefinitely.

All these electrons leave their impression on the emitted rays. We can hope that experimental study of the radiation phenomena, which are exposed to various influences, but in particular to the effect of magnetism, will provide us with useful data concerning a new field, that of atomistic astronomy, as Lodge called it, populated with atoms and electrons instead of planets and worlds.

I count myself fortunate to be able to contribute to this work; and the great interest which the Royal Swedish Academy of Sciences has shown in my work and the recognition that it has paid to my past successes, convince me that I am not on the wrong track.

Biography

Pieter Zeeman was born on May 25, 1865, at Zonnemaire, a small village in the isle of Schouwen, Zeeland, The Netherlands, as the son of the local clergymen Catharinus Forandinus Zeeman and his wife, *née* Wilhelmina Worst. After having finished his secondary school education at Zierikzee, the main town of the island, he went to Delft for two years to receive tuition in the classical languages, an adequate knowledge of which was required at that time for entrance to the university. Taking up his abode at the house of Dr. J. W. Lely, conrector of the Gymnasium and brother of Dr. C. Lely (Minister of Public Works and known for initiating and developing the work for reclamation of the Zuyderzee), Zeeman came into an environment which was beneficial for the development of his scientific talents. It was here also that he came into contact with Kamerlingh Onnes (Nobel Prize in Physics for 1913), who was twelve years his senior. Zeeman's wide reading, which included a proper mastery of works such as Maxwell's *Heat*, and his passion for performing experiments amazed Kamerlingh Onnes in no small degree, and formed the basis for a fruitful friendship between the two scientists.

Zeeman entered Leyden University in 1885 and became mainly a pupil of Kamerlingh Onnes (mechanics) and Lorentz (experimental physics): the latter was later to share the Nobel Prize with him. An early reward came in 1890 when he was appointed assistant to Lorentz, enabling him to participate in an extensive research programme which included the study of the Kerr effect – an important foundation for his future great work. He obtained his doctor's degree in 1893, after which he left for F. Kohlrausch's institute at Strasbourg, where for one semester he carried out work under E. Cohn. He returned to Leyden in 1894 and became «privaat-docent» (extra-mural lecturer) from 1895 to 1897.

In 1897, the year following his great discovery of the magnetic splitting of spectral lines, he was called to a lectureship at the University of Amsterdam; in 1900 came his appointment as Extraordinary Professor. In 1908 Van der Waals (Nobel Prize in Physics for 1910) reached the retiring age of 70 and Zeeman was chosen as his successor, at the same time functioning as Director of the Physics Laboratory. In 1923 a new laboratory, specially erected for

him, was put at his disposal, a prominent feature being a concrete block weighing a quarter of a million kilograms, erected free from the floor, as a suitable platform for vibration-free experiments. The institute is now known as the Zeeman Laboratory of Amsterdam University. Many world-famous scientists have visited Zeeman there or worked with him for some time. He remained in this dual function for 35 years – on numerous occasions refusing an invitation to occupy a Chair abroad – until in 1935 he had to resign on account of his pensionable age. An accomplished teacher and of kind disposition he was much loved by his pupils. One of these was C. J. Bakker, who was from 1955 until his untimely death in an aircraft accident in 1960 the General Director of the Centre Européen des Recherches Nucléaires (CERN) at Geneva. Another worker in his laboratory was S. Goudsmit, who in 1925 with G. E. Uhlenbeck originated the concept of electron spin.

Zeeman's talent for natural science first became apparent in 1883, when, while still attending the secondary school, he gave an apt description and drawing of an aurora borealis – then clearly to be observed in his country – which was published in *Nature*. (The Editor praised the meticulous observations of «Professor Zeeman in his observatory at Zonnemaire»!)

Zeeman's main theme of investigation has always concerned optical phenomena. His first treatise *Mesures relatives du phénomène de Kerr*, written in 1892, was rewarded with a Gold Medal from the Dutch Society of Sciences at Haarlem; his doctor's thesis dealt with the same subject. In Strasbourg he studied the propagation and absorption of electrical waves in fluids. His principal work, however, was the study of the influence of magnetism on the nature of light radiation, started by him in the summer of 1896, which formed a logical continuation of his investigation into the Kerr effect. The discovery of the so-called Zeeman effect, for which he has been awarded the Nobel Prize, was communicated to the Royal Academy of Sciences in Amsterdam – through H. Kamerlingh Onnes (1896) and J. D. van der Waals (1897) – in the form of papers entitled *Over den Invloed eener Magnetisatie op den Aard van het door een Stof uitgezonden Licht* (On the influence of a magnetization on the nature of light emitted by a substance) and *Over Doubletten en Tripletten in het Spectrum teweeggebracht door Uitwendige Magnetische Krachten* (On doublets and triplets in the spectrum caused by external magnetic forces) I, II and III. (The English translations of these papers appeared in *The Philosophical Magazine*; of the first paper a French version appeared in *Archives Néerlandaises des Sciences Exactes et Naturelles*, and in a short form in German in *Verhandlungen der Physikalischen Gesellschaft zu Berlin.*)

The importance of the discovery can at once be judged by the fact that at one stroke the phenomenon not only confirmed Lorentz' theoretical conclusions with regard to the state of polarization of the light emitted by flames, but also demonstrated the negative nature of the oscillating particles, as well as the unexpectedly high ratio of their charge and mass (e/m). Thus, when in the following year the discovery of the existence of free electrons in the form of cathode rays was established by J. J. Thomson, the identity of electrons and the oscillating light particles could be established from the negative nature and the e/m ratio of the particles. The growing number of observations made by other investigators on studying the effects of using various substances as light emitters – not all of them explicable by Lorentz' original theory (the so-called «anomalous Zeeman effect» could only adequately be explained at a later date, with the advent of Bohr's atomic theory, quantum wave mechanics, and the concept of the electron spin) – was assembled by him in his book *Researches in Magneto-Optics* (London 1913, German translation in 1914). Not only has the Zeeman effect thrown much light on the mechanism of light radiation and on the nature of matter and electricity, but its immense importance lies in the fact that even to this day it offers the ultimate means for revealing the intimate structure of the atom and the nature and behaviour of its components. It still serves as the final test in any new theory of the atom.

Already in his second communication Zeeman expressed the opinion that the accepted existence of strong magnetic fields on the surface of the sun could be verified, since these should alter spectral lines derived from the celestial body. (It is typical of Zeeman to extend physical concepts into the realm of celestial phenomena.) In a letter to him (1908) the astronomer G. E. Hale, Director of Mount Wilson Observatory, corroborated this opinion by means of photographs which indicated that in solar vortices the spectral lines indeed appeared to be affected by magnetic fields. Even the theoretical prediction concerning the probable interrelationship between the directions of polarization and those of the magnetic fields was subsequently confirmed by Hale.

With regard to Zeeman's activities outside the field of the magnetic splitting of spectral lines, mention should first be made of his work on the Doppler effect in optics and in canal rays (laboratory tests). A second field of study was that on the propagation of light in moving media (justification of the existence of the Lorentz-term in the Fresnel drag coefficient). Other investigations were those into the influence of the magnetic moment of the nucleus on the hyperfine structure of spectral lines. He also succeeded, with

J. de Gier, in discovering a number of new isotopes (^{38}Ar, ^{64}Ni, amongst others) by means of Thomson's parabola mass spectrograph. Zeeman's predilection for testing fundamental laws also found expression in his verification – carried out with an accuracy of $< 1 : 10^7$ – of the equality of heavy and inert masses.

Zeeman was Honorary Doctor of the Universities of Göttingen, Oxford, Philadelphia, Strasbourg, Liége, Ghent, Glasgow, Brussels and Paris. He was also a member or honorary member of numerous learned academies, including the very rare distinction of Associé Etranger of the Académie des Sciences of Paris. He was also member and Chairman of the Commission Internationale des Poids et Mesures, Paris. Appointed member of the Royal Academy of Sciences of Amsterdam in 1898, he served as the Secretary of the Mathematical-Physical Section from 1912 to 1920. Among the other distinctions may be mentioned the Rumford Medal of the Royal Society of London, the Prix Wilde of the Académie des Sciences of Paris, the Baumgartner-Preis of the Akademie der Wissenschaften of Vienna, the Matteucci Medal of the Italian Society of Sciences, the Franklin Medal of the Franklin Institute of Philadelphia, the Henry Draper Medal of the National Academy of Sciences of Washington. He was also made a Knight of the Order of Orange-Nassau and Commander of the Order of the Netherlands Lion.

Outside his field of study Zeeman showed much interest in literature and the stage. An entertaining host, he loved to invite his collaborators and pupils to dine with him at his home, an event preceded by a learned talk in his study and followed by a gathering in the family circle.

Zeeman married Johanna Elisabeth Lebret in 1895; they had one son and three daughters. During the last year of his professorship he suffered from ill-health. He died after a short illness on October 9, 1943.

Physics 1903

ANTOINE HENRI BECQUEREL

«in recognition of the extraordinary services he has rendered by his discovery of spontaneous radioactivity»

PIERRE CURIE

MARIE SKLODOWSKA-CURIE

«in recognition of the extraordinary services they have rendered by their joint researches on the radiation phenomena discovered by Professor Henri Becquerel»

Physics 1903

Presentation Speech by Dr. H. R. Törnebladh, President of the Royal Swedish Academy of Sciences

Your Majesty, Your Royal Highnesses, Ladies and Gentlemen.

The last decade's record of development in the physical sciences is outstanding for the discoveries, as unexpected as they are impressive, that have been made. The Royal Academy of Sciences has been accorded the task of beginning to carry out the noble intentions expressed by Alfred Nobel in his will dating from this fruitful period for the physical sciences. The great discovery, to which the Academy of Sciences resolved to award the Nobel Prize of 1903 for Physics, marks a stage in this brilliant expansion, while it is at the same time strictly linked to the discovery which won the very first Nobel Prize for Physics.

Further to the discovery of Röntgen rays, the question was raised whether they could not also be produced in other conditions than those in which they had been first observed. During the course of experiments in this field, Professor Henri Becquerel achieved results that not only contained an answer to this question, but led to a new discovery of the first order.

When electricity is discharged through a tube filled with highly rarefied gas, the phenomenon of radiation occurs in this tube. This has been called cathode radiation, which when it meets an object in its turn produces the rays discovered by Röntgen. It often happens that these cathode rays also give rise to phenomena of light, called fluorescence and phosphorescence, in objects they encounter. Now it is this circumstance which occasioned Becquerel's experiments. He asked himself if the bodies whence the phosphorescent rays emanate, after having been subjected for a longer or lesser time to the action of ordinary light, would not likewise emit Röntgen rays. To solve this problem, Becquerel made use of the well-known property of Röntgen rays to affect a photographic plate. Having placed aluminium foil upon a sensitive plate, he laid glass laminae on it with the phosphorescent materials that were under study, contending that if the photographic plate was affected through the aluminium foil, this could only take place by means of rays which like those of Röntgen had the property of passing through metals. Carrying his research further, Becquerel found that the sensitive

plate bore images from certain substances, in particular from all the salts of uranium. He thus demonstrated that these substances emit rays of a special nature, distinct from ordinary light. Tests continued and he established an even more extraordinary fact, namely that this radiation is not in direct relation to the phenomenon of phosphorescence, that phosphorescent materials as well as those which are not can give rise to this radiation, that previous lighting is never necessary for the phenomenon to occur, and lastly that the radiation in question continues with invariable force to all appearances without its origin being traced to any of the known sources of energy. This was how Becquerel made the discovery of *spontaneous radioactivity* and the rays that bear his name. This discovery revealed a new property of matter and a new source of energy, the latter of puzzling origin. It goes without saying that a discovery such as this was bound to excite the liveliest interest in the scientific world and give birth to a whole host of new investigations with the aim of making a thorough study of the nature of the Becquerel rays and determining their origin. It was at this point that M. and Mme. Curie undertook the most comprehensive and systematic research into this topic, examining the majority of simple substances and a large number of minerals to find if possible new substances with the remarkable properties of uranium. The first discovery in this field was made at approximately the same time by the German Schmidt and by Mme. Curie, both of whom found that thorium possesses radioactive properties to about the same degree as uranium.

During research, scientists have made full use of the property of the Becquerel rays, to make conductors of electricity out of bodies that are not so in normal circumstances. As a result, if rays of this kind fall on an electroscope charged with electricity, it will discharge more or less quickly, according to the greater or lesser activity of these rays which make the air round the electroscope a conductor. The electroscope has thus to some extent played the same part in respect of radioactive materials as the spectroscope in the search for new elements. M. and Mme. Curie, having thus found with the aid of the electroscope that the radioactive properties of the mineral pitchblende were more marked than those of uranium, came to the conclusion that pitchblende must contain one or more new radioactive substances. By breaking up pitchblende into its chemical components and examining, again with the aid of the electroscope, the radioactivity of the products that were obtained, they at last managed by means of a series of solutions and precipitates to isolate the materials that were distinguished by radioactivity of extraordinary intensity. Some idea of the prodigious work these results entail

may be formed from the fact that it takes 1,000 kg of raw material to produce a few decigrams of these active substances. Of these, *polonium* was discovered by M. and Mme. Curie, *radium* was also discovered by them in collaboration with Bémont, and *actinium* by Debierne. Of all these materials, at least radium has been shown to be a simple substance.

Becquerel had already shown by the study of radiation in uranium some of the most important properties of these rays. It was however only by means of the highly radioactive substances we have just mentioned that it became possible to carry out more comprehensive research into the Becquerel rays and in certain respects to amend findings on them. Foremost among scientists who carried out this programme, we find both Becquerel and also M. and Mme. Curie.

Becquerel radiation resembles light in several respects. Propagation is rectilinear. Like certain light wavelengths, it has a strong photochemical action, causes phosphorescence, etc. Yet it differs from light in certain essentials, for example by its property to pass through metals and many other opaque bodies, by the intensity with which it brings about the discharge of bodies charged with electricity, and lastly by the absence of phenomena of reflection, interference and refraction, characteristic of light. In this the Becquerel rays are exactly similar to Röntgen rays and cathode rays. It has been found all the same that Becquerel radiation is not homogeneous, but is composed of different kinds of rays, some of which like those of Röntgen are not deflected by magnetic or electric forces, while others are, like cathode rays or Goldstein rays. Like Röntgen rays, Becquerel rays have a very strong physiological effect; thus, for example, they attack the skin, affect the eye, etc.

Lastly, certain of the radioactive substances have a special property which bears no direct relation to their beams. This is to make temporarily radioactive all the bodies in their neighbourhood, by producing a radioactive emanation which communicates the radioactive property to the surroundings.

It is thus beyond doubt that Becquerel rays are in strict relation to Röntgen rays and cathode rays. The modern theory of electrons, which is used to explain this last form of radiation, has also been applied with the greatest success to explain the Becquerel rays.

We could close our account of the discoveries of Becquerel and the Curies at this point, as what we have covered is the main result of research they undertook early in 1903 and which is consequently to be considered in de-

ciding the award of the Nobel Prize for 1903. Surely the discoveries we have outlined are important enough to deserve such a prize. These discoveries have taught us that special forms of radiation that were only known hitherto by electric discharges through rarefied gas are natural phenomena of wide occurrence. We have gained knowledge of a property of matter quite new to us, the capability of emitting, spontaneously as it seems, these marvellous rays. We have gained new methods, infinitely superior in subtlety to any we had in this sphere, to examine under certain conditions the existence of matter in nature. Finally we have found a new source of energy, for which the full explanation is not yet forthcoming. It will evidently give rise to new research of the very highest value in physics and in chemistry.

The discoveries of Becquerel and the Curies in their own right herald a new era in the history of the physical sciences. Now we can only touch on the magnificent experiments carried out in this domain last year by Curie who discovered in radium the great spontaneous development of heat, together with the findings of Rutherford and Ramsay on the release of helium by radium, discoveries that are bound to be of great importance for the physicist and for the chemist alike. The promise for the future stemming from Becquerel's discovery seems near full realization.

The discoveries and research of Becquerel and of M. and Mme. Curie are closely bound up with each other; the latter were of course co-workers. The Royal Academy of Sciences did not think it right to distinguish between these eminent scientists, when it came to awarding the discovery of spontaneous radioactivity a Nobel Prize. The Academy thus deemed it equitable to share the Nobel Physics Prize for 1903 by awarding one half to Professor Henri Becquerel for the discovery of *spontaneous radioactivity*, and the other half to Professor and Madame Curie for great merit, of which they have given proof in work on rays first discovered by Henri Becquerel.

Professor Becquerel. The brilliant discovery of radioactivity shows us human knowledge in triumph, exploring Nature by undeflected rays of genius that pass through the vastness of space. Your victory serves as a shining refutation of the ancient dictum, *ignoramus – ignorabimus*, we do not know and we shall never know. It breeds the hope that scientific toil will succeed in conquering new territories and this is mankind's vital hope.

The great success of Professor and Madame Curie is the best illustration of the old proverb, *coniuncta valent*, union is strength. This makes us look at

God's word in an entirely new light: «It is not good that the man should be alone; I will make him an help meet for him.»

Nor is that all. This learned couple represent a team of differing nationalities, a happy omen for mankind joining forces in the development of science.

With sincere regret that these two prize-winners are prevented by commitments from being with us, we are fortunate in having in their stead the distinguished Minister, M. Marchand, representing France who has most kindly consented to receive the prize awarded to his fellow-countrymen.

ANTOINE H. BECQUEREL

On radioactivity, a new property of matter

Nobel Lecture, December 11, 1903

The subject which I propose to speak to you about has become, in only a few years, so vast that in order to deal with it in a single lecture I should have to confine myself to listing the main facts following the chronological order of their discovery. But as M. and Mme. Curie are to describe to you their fine work on radium, I will simply summarize the subject and give some account of my own research.

At the beginning of 1896, on the very day that news reached Paris of the experiments of Röntgen and of the extraordinary properties of the rays emitted by the phosphorescent walls of Crookes' tubes, I thought of carrying out research to see whether all phosphorescent material emitted similar rays. The results of the experiment did not justify this idea, but in this research I encountered an unexpected phenomenon.

Of all the phosphorescent materials, uranium salts seemed particularly suitable for the investigations, because of the exceptional structure indicated by the harmonic series of the bands making up their absorption and phosphorescence spectra. Thus I placed sheets of double sulphate of uranium and potassium on photographic plates enveloped in black paper or protected by a sheet of aluminium and exposed them to light for several hours. On developing the plates, I found that the uranium salt had emitted rays which reproduced the silhouettes of the crystalline sheets through the black paper and various screens of metal or thin glass laid on the plates.

Under these conditions the phenomenon could be ascribed to a transformation of solar energy, like phosphorescence, but I soon recognized that the emission was independent of any familiar source of excitation, such as light, electricity or heat.

We were thus faced with a spontaneous phenomenon of a new order. Figure 1 shows the first print, which revealed the spontaneity of the radiation emitted by the uranium salt. The rays passed through both the black paper which enveloped the plate, and a thin sheet of copper in the shape of a cross. Figure 2 shows the radiograph of an aluminium medal; the non-uniform absorption of the radiation by the different thicknesses of metal reveals the

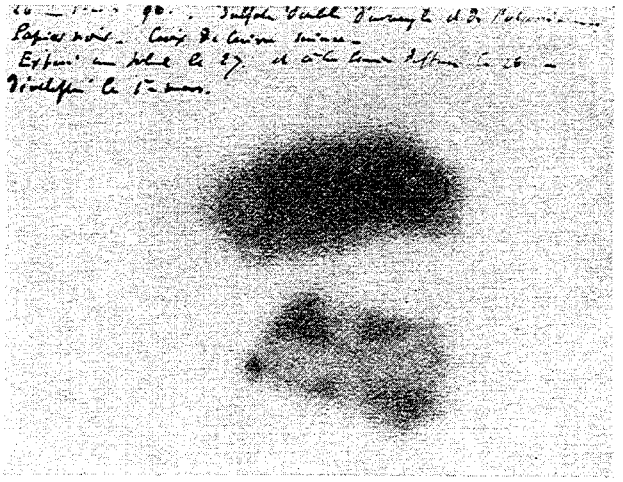


Fig. 1.

effigy. As the uranium salts used had been prepared a very long time beforehand, it was to be supposed that the intensity of the phenomenon was independent of time, and hence that the emission was constant. All the later experiments have shown that the activity of uranium does not diminish appreciably with time.

In obtaining these first results I noticed that the radiation of uranium discharged electrically-charged materials located some distance away, and this phenomenon provided a second method of studying the new rays. The pho-

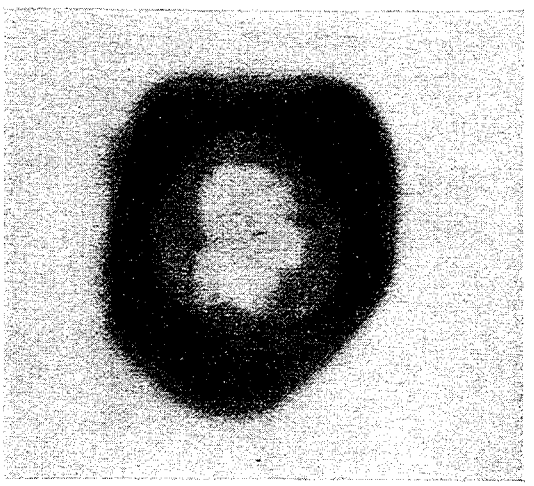


Fig. 2.

tographic method was primarily a qualitative one, the electrical method gave numerical data, and the early measurements revealed the constancy of the radiation with time.

The two methods showed that all uranium salts, whatever their origin, emitted radiation of the same type, that this property was an atomic property connected with the element uranium, and that metallic uranium was about three and a half times as active as the salt used in the first experiments.

A sphere of charged uranium, which discharges spontaneously in the air under the influence of its own radiation, retains its charge in an absolute vacuum. The exchanges of electrical charges that take place between charged bodies under the influence of the new rays, are the result of a special conductivity imparted to the surrounding gases, a conductivity that persists for several moments after the radiation has ceased to act.

These fundamental properties of the radiation emitted by uranium were verified later by numerous investigators; of these I will only mention Rutherford, who established that the conducting properties of the gases through which the radiation of uranium passes, are completely equal to the ionization caused by other factors.

During the course of these first experiments, the observation of a number of phenomena that were hitherto unexplained led me aside from the path to which my later experiments were to bring me back. Various samples of phosphorescent calcium sulphide resting on small plates of glass and covered with a small bell-jar of glass had been laid on a photographic plate protected by a sheet of aluminium 2 mm thick, as shown in Fig. 3. The print developed after 48 hours (Fig. 4) revealed silhouettes of the plates of glass, reproduced with the details which would have been produced by refraction and total

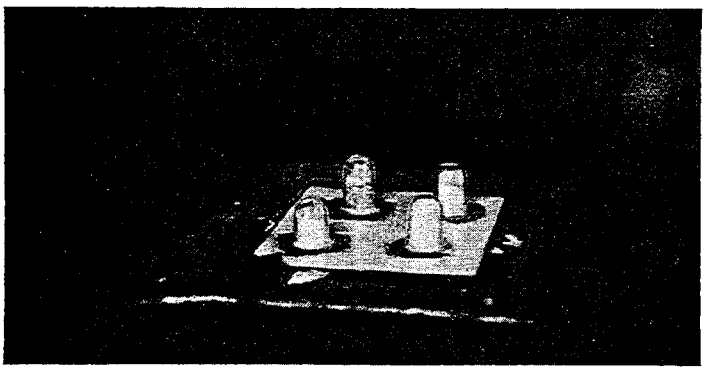


Fig. 3.

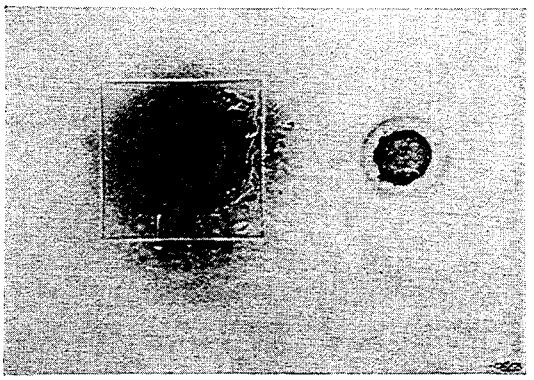


Fig. 4.

reflection of the light rays. Although at the same time Troost and Niewenglowski observed the emission of penetrating rays by phosphorescent materials, the above experiment could not be reproduced; the cause of the momentaneous appearance and the disappearance of the activity of these products is unknown. But since, in the experiments with uranium, the silhouettes of all the screens put in between are edged with white lines outlining projected shadows, and similar to those of the print which we have just mentioned, I was led to attribute the properties of light to the radiation from uranium, while all later experiments have shown that this radiation cannot be reflected or refracted like light rays.

In 1898 Schmidt and Mme. Curie observed almost simultaneously that thorium has properties similar to those of uranium; these properties were studied by Owens and by Rutherford. Mme. Curie, after measuring, by means of the ionization of air, the radioactivity of a large number of minerals containing uranium or thorium, noted the remarkable fact that some minerals were more active than metallic uranium. After having verified the fact that the activity accompanied the uranium molecule in its various combinations, M. and Mme. Curie concluded from this that there must be present in the minerals mentioned a substance more active than uranium, and they set about isolating it. They treated one of the most active of these minerals, Joachimsthal pitchblende, and first separated from it active bismuth which they supposed contained a new substance, polonium, and then soon afterwards they obtained extremely active barium, associated with a new element, radium. These products were obtained by fractional crystallizations in conjunction with electrometer readings. The activity of the products in-

creased with their richness in new elements. The activity of pure radium is about one million times greater than that of uranium.

Giesel has succeeded in obtaining very active preparations, and Debierne has studied a product intimately associated with thorium, which he has called actinium.

Of these different preparations, radium alone has the characteristics which we associate with simple substances; it has an emission spectrum formed of lines not belonging to any other known element, and the molecular weights of radium salts increase with their radium content.

The intensive radiation of radium excites the phosphorescence of different materials and restores the ability of crystals to phosphoresce under heat, when they have lost this as a result of a previous rise in temperature; moreover, radium salts become luminous spontaneously under the influence of their own radiation.

The first samples of polonium and radium that M. and Mme. Curie were good enough to lend me, revealed a striking difference in the nature of the radiation emitted by each of these substances. They were put into small paper cylinders closed at the bottom by very thin flakes of mica or aluminium, and the cylinders were placed on a photographic plate. The print in Fig. 5 shows that the radium radiation has easily passed through the various envelopes while the polonium radiation has not penetrated the wall of the paper cylinder; thus the polonium rays are only slightly penetrating.

Towards the end of 1899, first Giesel and then Meyer and von Schweidler observed that the radiation of active preparations is deflected by a magnetic

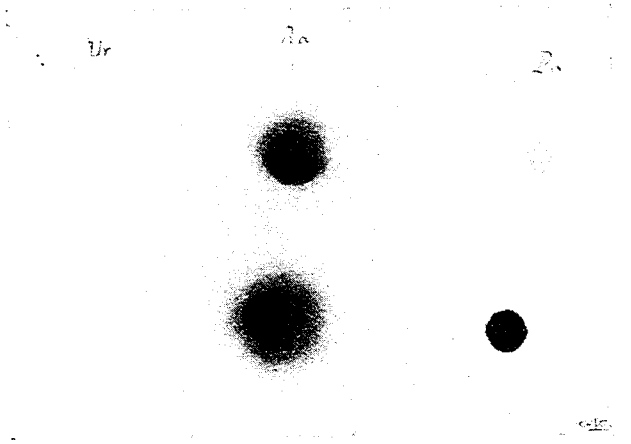


Fig. 5.

field. At the same time, unaware of these experiments, I noted the same in the case of radium radiation, after first having noted that in a non-uniform field the radiation is concentrated on the poles, as are cathode rays in the experiment of Birkeland. Figures 6 and 7 show some pictures which were obtained in the following way:

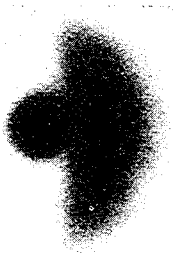


Fig. 6.

Fig. 7.

Several grains of radio-active matter are put on a photographic plate enveloped in black paper and placed horizontally between the poles of a magnet; on developing the plate after a few moments' exposure, it will be seen that beside the mark indicating the position of the active source a strong impression has been produced due to the radiation reflected by the field and collected on the plate, on one side only.

Almost at once I realised that the rays emitted by polonium were not deflected under the above experimental conditions and that there are two kinds of rays, one highly deflected by a magnetic field, and the other apparently not deflected. When studying the emission from radium, M. and Mme. Curie observed the simultaneous presence there of the two types of rays and saw that the rays from polonium, in common with those of the same type emitted by radium, were increasingly absorbable according as they had passed through a greater thickness of absorbent material: the in-

Fig. 8.

verse occurs in the case of the heterogeneous beams of the other rays. The photograph in Fig. 8 shows the two types of rays known nowadays as α-rays for those emitted by polonium, and β-rays for the magnetically deflectable part of the radiation from radium. I further found that thorium emits the two types of rays and that, even in a vacuum, uranium emits only β-rays (Fig. 9), not excluding the existence of much less active, non-deflectable rays.

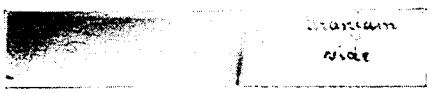


Fig. 9.

There is, in fact, a third type of rays, γ-rays, not deflected by a magnetic field, attention to which was first drawn by an experiment conducted by Villard and which seem analogous to X-rays.

The action of a magnetic field enables the various components of the radiation from radioactive substances to be separated and analysed.

β-Rays behave like cathode rays; it may be assumed, as has been for the latter, that they are made up of masses m carrying at velocity v negative charges e. In a uniform magnetic field of intensity H the trajectories normal to the field must be circular paths, the radius R of which is given by the relation $RH = (m/e)v$.

For an original direction making an angle α with the lines of force, the trajectories are helices which wind on cylinders of radius $R \sin\alpha$. I have verified the various geometrical consequences of this similarity.

The beam of β-rays is made up of an infinity of rays with trajectories having different radii of curvature; the magnetic field disperses them as a prism disperses the light rays of various colours. A photographic plate without black paper wrapping can be placed in, and parallel to, a uniform, horizontal magnetic field, for instance, then on the plate a small lead dish containing a few grains of radiferous barium forming a source of very small diameter. The radiation is directed at the plate and produces an image on just one side. If small strips of different substances – paper, aluminium or various metals – have been placed on this side it will be found that in the diffuse image which represents a kind of spectrum there are rays with varying penetration which, under each screen, give images whose limits are different and which constitute absorption spectra. It will be seen in the following that the image is mainly caused by the secondary radiation which originates at the face where the incident rays emerge. Figure 10 is an example of the photo-

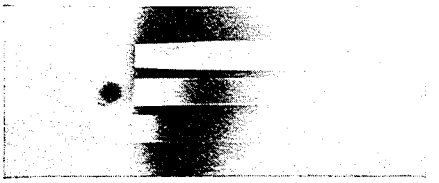


Fig. 10.

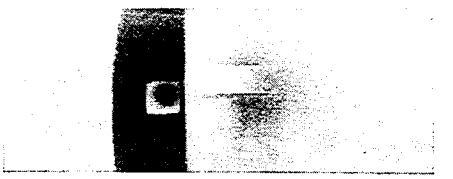


Fig. 11.

graphs obtained; the screens are: a strip of black paper, a strip of aluminium 0.1 mm thick, and a platinum foil 0.03 mm thick. To secure what might be termed a pure spectrum, i.e. such that each point on the plate is struck by a single beam, all of whose trajectories have the same curvature, the radiation from the point source must be made to pass through a narrow aperture. The result is the same as the foregoing (Fig. 11). The photograph shows, moreover, an image produced by the secondary rays emitted by the inside face of a lead semi-cylinder which covered the source and in which was pierced the small aperture in question. Finally, when the radiation is gathered through a second small aperture, the result is a single ray.

Each single ray may be defined by the value of the product *RH* corresponding to it. In the above tests, the *RH* values of the active rays ranged from 600 to about 2,500.

A more complete analysis of the radiation can be made by applying the following general method:

The active material is placed at the bottom of a deep, narrow groove in a small block of lead to produce a very thin vertical beam issuing from a linear source a few millimetres long. This system is placed in, and parallel to, the uniform field of a magnet. A photographic plate is then arranged above the

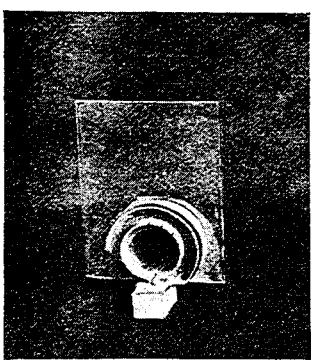


Fig. 12.

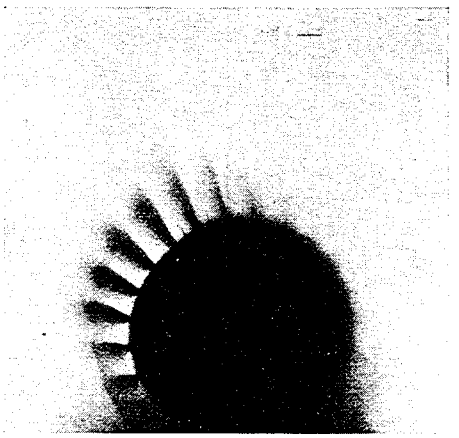


Fig. 13.

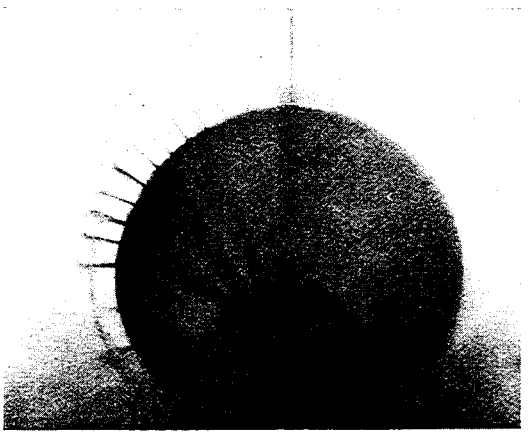
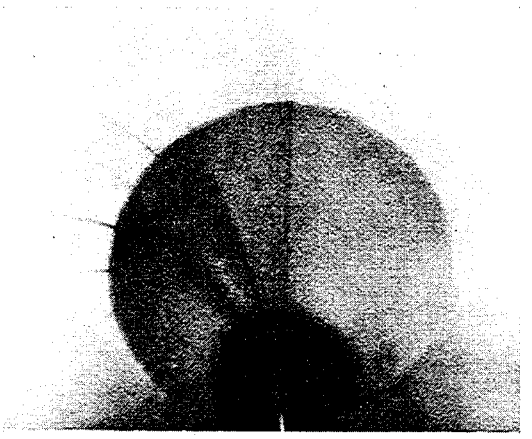


Fig. 14. ↑ ↓ Fig. 15.



source, at right angles to the field and intercepting the deflected beam. The very oblique rays reaching the plate produce thereon an image which differs very little from the actual trajectory of a skimming ray emitted normally to the field. On the photographic plate are placed a number of screens pierced by small slits normal to the plate; these then allow either portions of pure spectra or single rays to pass. To eliminate the light emitted by the source, the latter is covered with a thin aluminium foil. In practice the screens are fixed by means of an adhesive to a glass plate which serves to press them against the photographic plate. Figures 12, 13, 14 and 15 show the arrangement of the screens and some of the photographs obtained by this method. For two of these photographs an aluminium foil 0.1 mm thick, concentric with the pierced screen, was mounted beyond the pure spectra. It can be seen that the least deflectable rays pass through this foil as if it were not there; other more deflectable rays produce, on emerging, secondary rays which are photographically more active than the incident rays, and lastly the most deflectable rays do not pass through the aluminium foil and produce at the entry face secondary rays which give an intense image. The general image within the contours of the screens is also attributable to secondary radiation. The magnetic field had an intensity of 859 c.g.s. units. In these tests the α-rays were blocked but the γ-rays give straight line images revealing a discontinuity between these rays and the less deflectable β-rays; for these latter the product RH is about 10^4. In general the not very deflectable β-rays are very penetrating and the rays which are highly deflected are also very readily absorbed. These tests illustrate the advantages of this method for analysing the effects produced for each single ray.

When applied to secondary radiation this method has shown that the rays were deflected by a magnetic field in the same direction as the cathode rays.

While I was conducting these experiments, M. and Mme. Curie demonstrated that the β-rays from radium actually carry negative electrical charges; the bodies which receive the radiation become negatively charged while the source itself becomes positively charged. For this double phenomenon to be observed, all the conductors and the source itself must be completely surrounded by insulating materials, e.g. paraffin, or be placed in a vacuum.

On the other hand I have shown that the β-rays from radium were deflected by an electrostatic field. When F is the intensity of this field, the trajectory of a single ray, as characterized by the quantities m, e and v defined above, is a parabola with parameter $(m/e)(v^2/F)$, and the size of this parameter combined with that of the radius of curvature of the same ray's trajectory in

a known magnetic field enables m/e and v to be determined. The experiment performed with the apparatus in Fig. 16 provided the image shown in Fig. 17 which reveals the electrical deflection by the shadow projected by a vertical screen normal to the field. By combining for a single beam the crossed electrical and magnetic deflections, Kaufmann has made much more precise measurements than the ones that can be deduced from the earlier experiments. His measurements showed him that the ratio e/m was a function of the velocity v which, for the least deflectable β-rays, tends towards the speed of light. Interpretation of this fact in terms of Max. Abraham's concepts suggests that the mass of the electrons is at least in part, if not entirely, the outcome of electromagnetic reactions – a result which prompts fresh ideas about the nature of the inertia of matter.

Apart from the β-rays, identical with cathode rays, α-rays make up an important part of the radiation from active substances. I mentioned earlier how I had observed them for the first time with polonium, and how their apparent non-deflectability and the peculiarities of their low penetrability

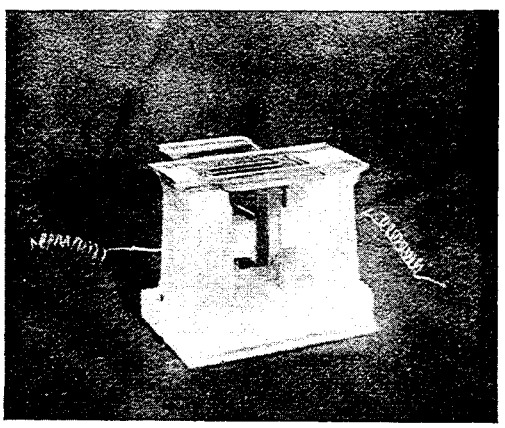


Fig. 16.

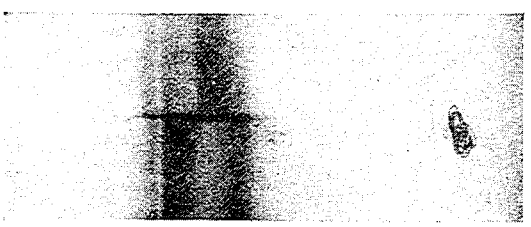


Fig. 17.

had caused them to be classified apart. By means of a very delicate electrical experiment, Rutherford discovered that the α-rays could be deflected very slightly by a very strong magnetic field, and that the deflection was in the opposite direction to that of β-rays. They hence behave as if carrying positive electrical charges and appear identical with Goldstein's « Kanalstrahlen ».

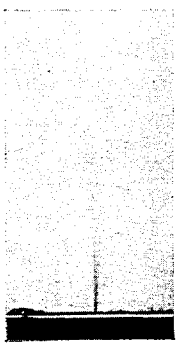


Fig. 18.

Using the above arrangement I recorded photographically the trajectory described in a magnetic field by α-rays from radium and the rays from polonium which are identical. Figure 18 shows a photograph (enlarged) obtained in a magnetic field of 10,000 c.g.s. units. The two concurrent paths each correspond to a direction of the magnetic field which has been reversed in the middle of the exposure. They do not present any trace of dispersion, which allows us to regard the active beam as homogeneous. It is noted, moreover, that the radius of curvature calculated for the different points of the trajectory continues to increase as the path in the air increases. This phenomenon can be attributed to the fact that the positive charges, with a velocity which is ten times less than that of the β-rays and a real or supposed mass a thousand times larger, attract the neutral molecules of the air, or are discharged progressively in the ionized air.

As the α-rays are very absorbable, they appear in consequence to constitute the most active part of the radiation when this is measured by the ionization of the air in the neighbourhood of the source. These rays are also the most active in exciting the phosphorescence of zinc blende, and of diamond, whereas barium platinocyanide becomes equally luminous under the influence of the α- and β-rays and the phosphorescence of the double sulphate of uranium and potassium is particularly excited by the β-rays. The curious effect of Sir W. Crookes' spinthariscope should be attributed to the

α-rays, and this effect seems due to the cleavages accompanied by flashes identical with those produced when various crystals are fractured.

The third kind of rays, γ-rays, are characterized by their great penetrability and their non-deflectability in a magnetic field. In the attached illustration (Fig. 19) made by the method described earlier, the α-rays have been stopped near their source, the β-rays are deflected by the magnetic field, whilst the γ-rays and the light emitted by the radium salt form a rectilinear beam which falls upon a quartz prism. The luminous rays are deflected, whereas the γ-rays leave a trace which can be followed without deflection not only past the prism but even through the prism itself.

The great penetrability of the γ-rays and also of the less deflectable β-rays means that neither the ionization of the air nor the photographic plate can give an exact idea of their intensity, since they pass through the gases and the silver salts without being absorbed.

If fairly thick metal plates are placed in their path, these rays are transformed and, either on the entry face or on the exit face, give rise to secondary rays which are more absorbable, so much so that the effect observed immediately behind these screens is more intense than if this screen did not

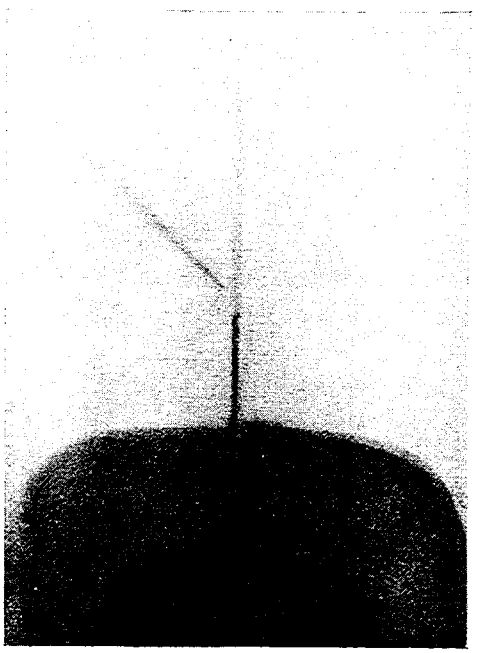


Fig. 19.

exist. This transformation reminds one of the effect produced by placing a fluorescent screen in the path of a beam of invisible radiations.

A photographic plate receiving the radiation of radium filtered by lead 1 cm thick is affected more under a strip of lead 1 mm thick than in the regions not covered by this screen. Figure 20 shows the effect produced by the radiation leaving through the walls of a lead parallelepiped after passing through 5 to 12 mm of metal.

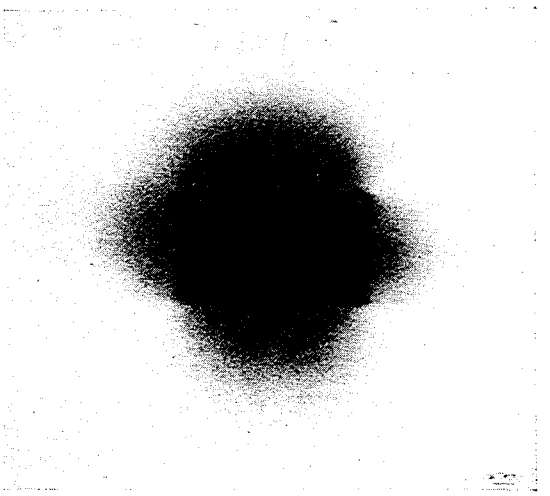


Fig. 20.

These secondary phenomena can account in part for the appearance of cast shadows on the edges of all the more or less transparent screens placed on the photographic plates.

Uranium and polonium emit penetrating rays which appear to be identical with γ-rays.

Radiation of radioactive substances produces various chemical actions; it acts upon the substances used in photography, and the α- and β-rays are the most active in this respect; it colours glass violet or brown, and the alkaline salts are coloured yellow, violet, blue or green. Under its action paraffin, celluloid and paper turn yellow; white phosphorus is transformed into red phosphorus. This transformation has been noted with β-rays, but it is probable that α-rays are equally active. Ozone is produced in the air around active bodies. Not only gases but also liquid dielectrics (petroleum, liquid air) and insulating solids like paraffin are ionized when they have been penetrated by radium radiation, and they preserve their conductive properties for a few

moments after the radiation has ceased to act. Giesel has observed that an aqueous solution of radium bromide continuously liberates oxygen and hydrogen at the rate of approximately 10 cm^3 per gram per day. According to Ramsay and Soddy these gases would also contain helium.

M. Curie has discovered moreover that radium salts continually emit heat; pure radium would emit approximately 80 calories per gram per hour. Curie and Dewar observed that in plunging a tube containing radium into liquid hydrogen there was a continuous release of hydrogen gas; with 0.7 g of radium bromide 73 cm^3 of hydrogen were obtained per minute.

Various physiological effects have been observed with radium rays; they excite phosphorescence in the interior of the eye; when an active product is brought near to the temple, a sensation of light is perceived. They act upon the epidermis and profoundly disorganize the skin, as do X-rays. The effect is produced without any sensation being felt at first and it only develops after several weeks; it then produces more or less deep lesions which can take several months to heal and which leave scars. At present an effort is being made to utilize this action in the treatment of lupus and cancers.

Radium rays have an active effect on the nerve centres and can then cause paralysis and death; they seem to act with particular intensity on living tissues in the process of evolution.

Up to now we have only spoken of the radiation which is transmitted through glass, mica, opaque bodies and metals. In the emission of radioactive bodies there is another phenomenon of a different nature which appears to be intimately connected with radioactivity, if it is not the primordial phenomenon. Thorium and radium emit energy in a particular form; the resultant activity is propagated in the form of an active vapour which has been called *emanation* and which is arrested by any covering, however thin, which is impermeable to gases.

This emanation seems to settle upon all bodies in order to make them radioactive, but the activity disappears when the latter are no longer under the influence of the active source, even when they are in a closed chamber. These facts were discovered simultaneously at the end of 1899 by Rutherford for thorium, and by M. and Mme. Curie for radium. Rutherford when studying the activity of thorium saw that besides the ordinary radiation there was an effect produced by an *emanation* comparable with an active vapour. This latter is deposited on all bodies, principally on those which are negatively charged, and it makes them momentarily active.

At the same time M. and Mme. Curie discovered that under the influence of radium the bodies become temporarily active; this *induced* activity persists for some time after the bodies have been removed from the radium; in the open air it diminishes by about one half in half an hour.

The phenomenon is produced with regularity and with great intensity in a closed space; the induced activity is then the same on all the bodies; it is independent of the nature of the gas and of the pressure within the chamber, but the activation is no longer produced if a vacuum is constantly maintained by removing the gases which are liberated as soon as they are produced. Solutions of radium salts produce activation with greater intensity than the solids.

The water of crystallization extracted from the active salts, or the water separated from an active solution by a semi-permeable celluloid wall, become strongly radioactive. The bodies activated by the radium produce the same effects as radium; the glass walls activated by the emanation emit a penetrating radiation which passes through them and makes them luminous, whilst the activating solution can only emit feeble radiation.

The activating property is diffused gradually through the gases inside a closed chamber, through capillary tubes or imperceptible fissures; the bodies become more active as the volume of gas at their surface is greater.

A gas which has been kept near to radium and has acquired the property of making solid bodies radioactive is itself radioactive, but it only emits rays with very little penetrating power which will not pass through a glass wall. When it is removed from the radium it continues to emit rays and to cause radioactivity. Its activity from this double point of view diminishes by half during each successive period of four days and finally becomes extinct. This period of four days is a time constant characteristic of radium emanation.

Air charged with emanation produces phosphorescence of various substances (glass, zinc blende, etc.) and this phenomenon allows various striking experiments to be conducted with relation to the propagation of emanation.

Radium emanation behaves like a gas from many points of view. M. and Mme. Curie have shown that it divides like a gas between two intercommunicating gas chambers and that it is diffused in the air in accordance with the diffusion law for gases; its diffusion coefficient in air appears to be close to that of carbonic acid.

Rutherford and Soddy have discovered that the emanation condenses at the temperature of liquid air. All experiments lead one to regard the emanation as a material gas. However, the hypothesis of the existence of such a gas

is based solely on the radioactive manifestations, and contrary to what happens with ordinary matter, the emanation disappears spontaneously in a sealed tube which encloses it.

Ramsay and Soddy have recently found that by enclosing the emanation from radium in a sealed tube and studying the emanation spectrum, the spectrum of helium could be seen progressively appearing, which was not observable at first. The writers explain these facts by a transformation that would be half complete in four days, and fully complete in 28. Without resorting to the hypothesis of the transformation of matter, while one is still waiting for a more complete demonstration of such an important fact, it would be possible to explain the facts by agreeing that the helium already exists in the gases of the emanation in the sealed tube, but that the special state of the emanation prevents the lines of the helium spectrum from appearing. The latter would gradually appear as the emanation is transformed; and it was observed that the activity of the emanation was reduced by half by the end of four days.

Further, it is relevant to quote a striking method of activation which leads one to make certain reservations about the conclusions to be drawn regarding the presence of new elements in radioactive preparations. Every inactive substance which is put into solutions of uranium, radium or thorium, and then precipitated out, becomes radioactive, but slowly loses its radioactivity. This fact was first noted by Curie and Giesel; the latter activated bismuth in this way.

With uranium, a trace of barium precipitated as the sulphate becomes noticeably more active than uranium; when activated in this way barium, like uranium, gives off only β-rays. After precipitation, the uranium salt, which is collected as a solid, is less active than before. This reduction in activity can be carried further by repeating the process, but the products gradually regain spontaneously their former activity.

The temporary reduction in activity after dissolution is a general phenomenon. Radium salts, when they are taken out of solution again are less active than before they were dissolved; their activity then increases over several months before reaching a maximum. They give off heat following this increase in activity.

According to Debierne, barium activated by actinium can be separated from inactive barium; it fractionates as radiferous barium chloride, the least active portion being least soluble in water and hydrochloric acid. Debierne obtained by this method a product which was one thousand times more ac-

tive than uranium. Activated barium behaves like a false radium, but differs from true radium in lacking the characteristic spectrum, and in losing its activity with time.

The spontaneous recovery of radioactivity by substances in which it has been reduced may be explained in terms of the substances themselves trapping their emanation, either on the active molecules, or on the inert molecules associated with them.

Following on this research, various workers have found more examples of radioactivity, sometimes as trace elements in metals, sometimes in natural phenomena.

Elster and Geitel discovered that atmospheric air shows to a slight extent the same properties as activated gases. By stretching long negatively-charged wires in the air they were able to recover traces of activated substances on them. Gases trapped in enclosed spaces, and air extracted from the ground or near waterfalls, show these properties, as do gases extracted from certain types of water.

To sum up, the radioactive substances whose nature is now well established are: uranium, thorium, radium, and polonium; actinium can be added, although very little information has been published about this last product. Reservations must be made about various other products obtained by Giesel, and about a preparation of active bismuth or active tellurium obtained electrolytically by Markwald.

Uranium gives off β-and γ-rays; it does not give off an emanation in air, but the activation which it produces in solution can be explained as the effect of an emanation.

Thorium and radium give off α-, β- and γ-rays, and an activating emanation in gases.

Polonium does not give off β-rays. It gives off α- and γ-rays, but loses its activity with time.

Actinium is said to possess a remarkable activation power.

Besides uranium and thorium, only radium has characteristics which enable it to be considered as an element with properties related to, but distinct from, those of barium. However, it is worth noting that this substance is never found, even as a trace element, in ordinary barium minerals, and that it is only met with in uranium minerals, where it is found with barium. This fact may well have a significance which will become clear to us later.

Radioactive substances, especially radium, give off energy in all the known

forms: heat, light, chemical reactions, electrical charges, γ-radiation. They seem to maintain the same state indefinitely, and the source from which they derive the energy they give off escapes us.

Among the hypotheses which suggest themselves to fill the gaps left by current experiments, one of the most likely lies in supposing that the emission of energy is the result of a slow modification of the atoms of the radioactive substances. Such a modification, which the methods at our disposal are unable to bring about, could certainly release energy in sufficiently large quantities to produce the observed effects, without the changes in matter being large enough to be detectable by our methods of investigation.

In this scheme, there would still be scope to wonder whether the transformation of the atom comprises a slow, spontaneous evolution, or whether it is the result of the absorption of external radiation beyond the range of our senses. If such a radiation were to exist, one could still picture the radioactive substances transforming it without themselves being altered. So far no experiment has confirmed or invalidated these hypotheses.

Biography

Antoine Henri Becquerel was born in Paris on December 15, 1852, a member of a distinguished family of scholars and scientists. His father, Alexander Edmond Becquerel, was a Professor of Applied Physics and had done research on solar radiation and on phosphorescence, while his grandfather, Antoine César, had been a Fellow of the Royal Society and the inventor of an electrolytic method for extracting metals from their ores. He entered the Polytechnic in 1872, then the government department of Ponts-et-Chaussées in 1874, becoming *ingénieur* in 1877 and being promoted to *ingénieur-en-chef* in 1894. In 1888 he acquired the degree of *docteur-ès-sciences*. From 1878 he had held an appointment as an Assistant at the Museum of Natural History, taking over from his father in the Chair of Applied Physics at the Conservatoire des Arts et Métiers. In 1892 he was appointed Professor of Applied Physics in the Department of Natural History at the Paris Museum. He became a Professor at the Polytechnic in 1895.

Becquerel's earliest work was concerned with the plane polarization of light, with the phenomenon of phosphorescence and with the absorption of light by crystals (his doctorate thesis). He also worked on the subject of terrestrial magnetism. In 1896, his previous work was overshadowed by his discovery of the phenomenon of natural radioactivity. Following a discussion with Henri Poincaré on the radiation which had recently been discovered by Röntgen (X-rays) and which was accompanied by a type of phosphorescence in the vacuum tube, Becquerel decided to investigate whether there was any connection between X-rays and naturally occurring phosphorescence. He had inherited from his father a supply of uranium salts, which phosphoresce on exposure to light. When the salts were placed near to a photographic plate covered with opaque paper, the plate was discovered to be fogged. The phenomenon was found to be common to all the uranium salts studied and was concluded to be a property of the uranium atom. Later, Becquerel showed that the rays emitted by uranium, which for a long time were named after their discoverer, caused gases to ionize and that they differed from X-rays in that they could be deflected by electric or

magnetic fields. For his discovery of spontaneous radioactivity Becquerel was awarded half of the Nobel Prize for Physics in 1903, the other half being given to Pierre and Marie Curie for their study of the Becquerel radiation.

Becquerel published his findings in many papers, principally in the *Annales de Physique et de Chimie* and the *Comptes Rendus de l'Academie des Sciences.*

He was elected a member of the Académie des Sciences de France in 1889 and succeeded Berthelot as Life Secretary of that body. He was a member also of the Accademia dei Lincei and of the Royal Academy of Berlin, amongst others. He was made an Officer of the Legion of Honour in 1900.

He was married to Mlle. Janin, the daughter of a civil engineer. They had a son Jean, b. 1878, who was also a physicist: the fourth generation of scientists in the Becquerel family.

Antoine Henri Becquerel died at Le Croisic on August 25, 1908.

PIERRE CURIE

Radioactive substances, especially radium

Nobel Lecture, June 6, 1905

Allow me, first of all, to tell you that I am happy to speak today before the Academy of Sciences which has conferred on Mme. Curie and myself the very great honour of awarding us a Nobel Prize. We must also tender you our apologies for being so tardy in visiting you in Stockholm, for reasons quite outside our control.

I have to speak to you today on the properties of the *radioactive substances*, and in particular of those of *radium*. I shall not be able to mention exclusively our own investigations. At the beginning of our studies on this subject in 1898 we were the only ones, together with Becquerel, interested in this question; but since then much more work has been done and today it is no longer possible to speak of radioactivity without quoting the results of investigations by a large number of physicists such as Rutherford, Debierne, Elster and Geitel, Giesel, Kauffmann, Crookes, Ramsay and Soddy, to mention only a few of those who have made important progress in our knowledge of radioactive properties.

I shall give you only a rapid account of the discovery of radium and a brief summary of its properties, and then I shall speak to you of the consequences of the new knowledge which radioactivity gives us in the various branches of science.

Becquerel discovered in 1896 the special radiating properties of *uranium* and its compounds. Uranium emits very weak rays which leave an impression on photographic plates. These rays pass through black paper and metals; they make air electrically conductive. The radiation does not vary with time, and the cause of its production is unknown.

Mme. Curie in France and Schmidt in Germany have shown that thorium and its compounds possess the same properties. Mme. Curie also showed in 1898 that of all the chemical substances prepared or used in the laboratory, only those containing uranium or thorium were capable of emitting a substantial amount of the Becquerel rays. We have called such substances *radioactive*.

Radioactivity, therefore, presented itself as an atomic property of uranium

and thorium, a substance being all the more radioactive as it was richer in uranium or thorium.

Mme. Curie has studied the minerals containing uranium or thorium, and in accordance with the views just stated, these minerals are all radioactive. But in making the measurements, she found that certain of these were more active than they should have been according to the content of uranium or thorium. Mme. Curie then made the assumption that these substances contained radioactive chemical elements which were as yet unknown. We, Mme. Curie and I, have sought to find these new hypothetical substances in a uranium ore, *pitchblende*. By carrying out the chemical analysis of this mineral and assaying the radioactivity of each batch separated in the treatment, we have, first of all, found a highly radioactive substance with chemical properties close to bismuth which we have called *polonium*, and then (in collaboration with Bémont) a second highly radioactive substance close to barium which we called *radium*. Finally, Debierne has since separated a third radioactive substance belonging to the group of the rare earths, *actinium*.

These substances exist in pitchblende only in the form of traces, but they have an enormous radioactivity of an order of magnitude 2 million times greater than that of uranium. After treating an enormous amount of material, we succeeded in obtaining a sufficient quantity of radiferous barium salt to be able to extract from it radium in the form of a pure salt by a method of fractionation. Radium is the higher homologue of barium in the series of the alkaline earth metals. Its atomic weight as determined by Mme. Curie is 225. Radium is characterized by a distinct spectrum which was first discovered and studied by Demarçay, and then by Crookes and Runge and Precht, Exner, and Haschek. The spectrum reaction of radium is very sensitive, but it is much less sensitive than radioactivity for revealing the presence of traces of radium.

The general effects of the radiations from radium are intense and very varied.

Various experiments: Discharge of an electroscope. – The rays pass through several centimetres of lead. – A spark induced by the presence of radium. – Excitation of the phosphorescence of barium platinocyanide, willemite and kunzite. – Coloration of glass by the rays. – Thermoluminescence of fluorine and ultramarine after the action of radiation from radium on these substances. – Radiographs obtained with radium.

A radioactive substance such as radium constitutes a continuous source of energy. This energy is manifested by the emission of the radiation. I have

also shown in collaboration with Laborde that radium releases heat continuously to the extent of approx. 100 calories per gram of radium and per hour. Rutherford and Soddy, Runge and Precht, and Knut Ångström have also measured the release of heat by radium, this release seems to be constant after several years, and the total energy released by radium in this way is considerable.

The work of a large number of physicists (Meyer and Schweidler, Giesel, Becquerel, P. Curie, Mme. Curie, Rutherford, Villard, etc.) shows that the radioactive substances can emit rays of the three different varieties designated by Rutherford as α-, β- and γ-rays. They differ from one another by the action of a magnetic field and of an electric field which modify the trajectory of the α- and β-rays.

The β-rays, similar to cathode rays, behave like negatively charged projectiles of a mass 2000 times smaller than that of a hydrogen atom (electron). We have verified, Mme. Curie and I, that the β-rays carry with them negative electricity. The α-rays, similar to the Goldstein's canal rays, behave like projectiles 1,000 times heavier and charged with positive electricity. The γ-rays are similar to Röntgen rays.

Several radioactive substances such as radium, actinium, and thorium also act otherwise than through their direct radiation; the surrounding air becomes radioactive, and Rutherford assumes that each of these substances emits an unstable radioactive gas which he calls *emanation* and which spreads in the air surrounding the radioactive substance.

The activity of the gases which are thus made radioactive disappears spontaneously according to an exponential law with a time constant which is characteristic for each active substance. It can, therefore, be stated that the emanation from radium diminishes by one-half every 4 days, that from thorium by one-half every 55 seconds, and that from actinium by one-half every 3 seconds.

Solid substances which are placed in the presence of the active air surrounding the radioactive substances, themselves become temporarily radioactive. This is the phenomenon of *induced radioactivity* which Mme. Curie and I have discovered. The induced radioactivities, like the emanations, are equally unstable and are destroyed spontaneously according to exponential laws characteristic of each of them.

Experiments: A glass tube filled with emanation from radium which was brought from Paris. – Discharge of an electroscope by the rays from the in-

duced radioactivity. – Phosphorescence of zinc sulphide under the action of the emanation.

Finally, according to Ramsay and Soddy, radium is the seat of a continuous and spontaneous production of helium.

The radioactivity of uranium, thorium, radium and actinium seems to be invariable over a period of several years; on the other hand, that of polonium diminishes according to an exponential law, it diminishes by one-half in 140 days and after several years it has almost completely disappeared.

These are all the most important facts which have been established by the efforts of a large number of physicists. Several phenomena have already been extensively studied by them.

The consequences of these facts are making themselves felt in all branches of science:

The importance of these phenomena for *physics* is evident. Radium constitutes in laboratories a new research tool, a source of new radiations. The study of the β-rays has already been very fruitful. It has been found that this study confirms the theory of J. J. Thomson and Heaviside on the mass of particles in motion, charged with electricity; according to this theory, part of the mass results from the electromagnetic reactions of the ether of the vacuum. The experiments of Kauffmann on the β-rays of radium lead to the assumption that certain particles have a velocity very slightly below that of light, that according to the theory the mass of the particle increases with the velocity for velocities close to that of light, and that the whole mass of the particle is of an electromagnetic nature. If the hypothesis is also made that material substances are constituted by an agglomeration of electrified particles, it is seen that the *fundamental principles of mechanics* will have to be profoundly modified.

The consequences for *chemistry* of our knowledge of the properties of the radioactive substances are perhaps even more important. And this leads us to speak of the source of energy which maintains the radioactive phenomena.

At the beginning of our investigations we stated, Mme. Curie and I, that the phenomena could be explained by two distinct and very general hypotheses which were described by Mme. Curie in 1899 and 1900 (*Revue Générale des Sciences*, January 10, 1899, and *Revue Scientifique*, July 21, 1900).

1. In the first hypothesis it can be supposed that the radioactive substances borrow from an external radiation the energy which they release, and their radiation would then be a secondary radiation. It is not absurd to suppose that space is constantly traversed by very penetrating radiations which cer-

tain substances would be capable of capturing in flight. According to the recent work of Rutherford, Cooke and McLennan, this hypothesis seems to be useful for explaining part of the extremely weak radiation which emanates from most of the substances.

2. In the second hypothesis it can be supposed that the radioactive substances draw from themselves the energy which they release. The radioactive substances would then be in course of evolution, and they would be transforming themselves progressively and slowly in spite of the apparent invariability of the state of certain of them. The quantity of heat released by radium in several years is enormous if it is compared with the heat released in any chemical reaction with the same weight of matter. This released heat would only represent, however, the energy involved in a transformation of a quanty of radium so small that it cannot be appreciated even after years. This leads to the supposition that the transformation is more far-reaching than the ordinary chemical transformations, that the existence of the atom is even at stake, and that one is in the presence of a transformation of the elements.

The second hypothesis has shown itself the more fertile in explaining the properties of the radioactive substances properly so called. It gives, in particular, an immediate explanation for the spontaneous disappearance of polonium and the production of helium by radium. This theory of the transformation of the elements has been developed and formulated with great boldness by Rutherford and Soddy who state that there is a continuous and irreversible disaggregation of the atoms of the radioactive elements. In the theory of Rutherford the disaggregation products would be, on the one hand, the projectile rays, and on the other hand, the emanations and the induced radioactivities. The latter would be new gaseous or solid radioactive substances frequently with a rapid evolution and having atomic weights lower than that of the original element from which they are derived. Seen in this way the life of radium would be necessarily limited when this substance is separated from the other elements. In Nature radium is always found in association with uranium, and it can be assumed that it is produced by the latter.

This, therefore, is a veritable theory of the transmutation of elements, although not as the alchemists understood it. The inorganic matter would necessarily evolve through the ages and in accordance with immutable laws.

Through an unexpected consequence the radioactive phenomena can be important in *geology*. It has been found, for example, that radium always

accompanies uranium in minerals. And it has even been found that the ratio of radium to uranium is constant in all minerals (Boltwood). This confirms the idea of the creation of radium from uranium. This theory can be extended to try to explain also other associations of elements which occur so frequently in minerals. It can be imagined that certain elements have been formed on the spot on the surface of the Earth or that they stem from other elements in a time which may be of the order of magnitude of geological periods. This is a new point of view which the geologists will have to take into account.

Elster and Geitel have shown that the emanation of radium is very widespread in Nature and that radioactivity probably plays an important part in *meteorology*, with the ionization of the air provoking the condensation of water vapour.

Finally, in the *biological sciences* the rays of radium and its emanation produce interesting effects which are being studied at present. Radium rays have been used in the treatment of certain diseases (lupus, cancer, nervous diseases). In certain cases their action may become dangerous. If one leaves a wooden or cardboard box containing a small glass ampulla with several centigrams of a radium salt in one's pocket for a few hours, one will feel absolutely nothing. But 15 days afterwards a redness will appear on the epidermis, and then a sore which will be very difficult to heal. A more prolonged action could lead to paralysis and death. Radium must be transported in a thick box of lead.

It can even be thought that radium could become very dangerous in criminal hands, and here the question can be raised whether mankind benefits from knowing the secrets of Nature, whether it is ready to profit from it or whether this knowledge will not be harmful for it. The example of the discoveries of Nobel is characteristic, as powerful explosives have enabled man to do wonderful work. They are also a terrible means of destruction in the hands of great criminals who are leading the peoples towards war. I am one of those who believe with Nobel that mankind will derive more good than harm from the new discoveries.

Biography

Pierre Curie was born in Paris, where his father was a general medical practitioner, on May 15, 1859. He received his early education at home before entering the Faculty of Sciences at the Sorbonne. He gained his Licenciateship in Physics in 1878 and continued as a demonstrator in the physics laboratory until 1882 when he was placed in charge of all practical work in the Physics and Industrial Chemistry Schools. In 1895 he obtained his Doctor of Science degree and was appointed Professor of Physics. He was promoted to Professor in the Faculty of Sciences in 1900, and in 1904 he became Titular Professor.

In his early studies on crystallography, together with his brother Jacques, Curie discovered piezoelectric effects. Later, he advanced theories of symmetry with regard to certain physical phenomena and turned his attention to magnetism. He showed that the magnetic properties of a given substance change at a certain temperature – this temperature is now known as the Curie point. To assist in his experiments he constructed several delicate pieces of apparatus – balances, electrometers, piezoelectric crystals, etc.

Curie's studies of radioactive substances were made together with his wife, whom he married in 1895. They were achieved under conditions of much hardship – barely adequate laboratory facilities and under the stress of having to do much teaching in order to earn their livelihood. They announced the discovery of radium and polonium by fractionation of pitchblende in 1898 and later they did much to elucidate the properties of radium and its transformation products. Their work in this era formed the basis for much of the subsequent research in nuclear physics and chemistry. Together they were awarded half of the Nobel Prize for Physics in 1903 on account of their study into the spontaneous radiation discovered by Becquerel, who was awarded the other half of the Prize.

Pierre Curie's work is recorded in numerous publications in the *Comptes Rendus de l'Académie des Sciences*, the *Journal de Physique* and the *Annales de Physique et Chimie*.

Curie was awarded the Davy Medal of the Royal Society of London in

1903 (jointly with his wife) and in 1905 he was elected to the Academy of Sciences.

His wife was formerly Marie Sklodowska, daughter of a secondary-school teacher at Warsaw, Poland. One daughter, Irène, married Frédéric Joliot and they were joint recipients of the Nobel Prize for Chemistry in 1935. The younger daughter, Eve, married the American diplomat H. R. Labouisse. They have both taken lively interest in social problems, and as Director of the United Nations' Children's Fund he received on its behalf the Nobel Peace Prize in Oslo in 1965. She is the author of a famous biography of her mother, *Madame Curie* (Gallimard, Paris, 1938), translated into several languages.

Pierre was killed in a street accident in Paris on April 19, 1906.

No Lecture was delivered by Mme. Curie.

Biography

Marie Curie, *née* Sklodowska, was born in Warsaw on November 7, 1867, the daughter of a secondary-school teacher. She received a general education in local schools and some scientific training from her father. She became involved in a students' revolutionary organization and found it prudent to leave Warsaw, then in the part of Poland dominated by Russia, for Cracow, which at that time was under Austrian rule. In 1891, she went to Paris to continue her studies at the Sorbonne where she obtained Licenciateships in Physics and the Mathematical Sciences. She met Pierre Curie, Professor in the School of Physics in 1894 and in the following year they were married. She succeeded her husband as Head of the Physics Laboratory at the Sorbonne, gained her Doctor of Science degree in 1903, and following the tragic death of Pierre Curie in 1906, she took his place as Professor of General Physics in the Faculty of Sciences, the first time a woman had held this position. She was also appointed Director of the Curie Laboratory in the Radium Institute of the University of Paris, founded in 1914.

Her early researches, together with her husband, were often performed under difficult conditions, laboratory arrangements were poor and both had to undertake much teaching to earn a livelihood. The discovery of radioactivity by Henri Becquerel in 1896 inspired the Curies in their brilliant researches and analyses which led to the isolation of polonium, named after the country of Marie's birth, and radium. Mme. Curie developed methods for the separation of radium from radioactive residues in sufficient quantities to allow for its characterization and the careful study of its properties, therapeutic properties in particular.

Mme. Curie throughout her life actively promoted the use of radium to alleviate suffering and during World War I, assisted by her daughter, Irène, she personally devoted herself to this remedial work. She retained her enthusiasm for science throughout her life and did much to establish a radioactivity laboratory in her native city – in 1929 President Hoover of the United States presented her with a gift of $ 50,000, donated by American friends of science, to purchase radium for use in the laboratory in Warsaw.

Mme. Curie, quiet, dignified and unassuming, was held in high esteem and admiration by scientists throughout the world. She was a member of the Conseil du Physique Solvay from 1911 until her death and since 1922 she had been a member of the Committee of Intellectual Co-operation of the League of Nations. Her work is recorded in numerous papers in scientific journals and she is the author of *Recherches sur les Substances Radioactives* (1904), *L'Isotopie et les Éléments Isotopes* and the classic *Traité de Radioactivité* (1910).

The importance of Mme. Curie's work is reflected in the numerous awards bestowed on her. She received many honorary science, medicine and law degrees and honorary memberships of learned societies throughout the world. Together with her husband, she was awarded half of the Nobel Prize for Physics in 1903, for their study into the spontaneous radiation discovered by Becquerel, who was awarded the other half of the Prize. In 1911 she received a second Nobel Prize, this time in Chemistry, in recognition of her work in radioactivity. She also received, jointly with her husband, the Davy Medal of the Royal Society in 1903 and, in 1921, President Harding of the United States, on behalf of the women of America, presented her with one gram of radium in recognition of her service to science.

For further details, *cf.* pp. 79 and 80. Mme. Curie died at Savoy, France, after a short illness, on July 4, 1934.

Physics 1904

Lord RAYLEIGH
(JOHN WILLIAM STRUTT)

«for his investigations of the densities of the most important gases and for his discovery of argon in connection with these studies»

Physics 1904

Presentation Speech by Professor J. E. Cederblom, President of the Royal Swedish Academy of Sciences

Your Majesty, Your Royal Highnesses, Ladies and Gentlemen.

The Royal Academy of Sciences has decided that the Nobel Prize for Physics for the present year is to be awarded to Lord Rayleigh, Professor at the Royal Institution, London, for his investigations on the density of the most important gases, and for his discovery of argon, one of the results of those investigations.

Among the problems in physico-chemical science that have more especially taken up the attention of scientists, the nature and composition of atmospheric air has always held a prominent position. For centuries this problem has been the object of both keen enquiry and extensive experimental investigation, consequently its history affords a very striking picture of the gradual development of those sciences in their entirety, closely connected as it is with the progress made in the various departments of physics and chemistry. The retarding influence, which in former times was continuously exercised not only by incorrect opinions that had become firmly established but also by insufficient experimental groundwork, is plainly observable, and this explains the fact that during the seventeenth century the solution of the problem was not, and could not, be arrived at by such scientists as Boyle, Mayow, and Hales; it was only obtained a hundred years later, after the discoveries of Priestley, Black, Cavendish, and above all Lavoisier, in a manner which not only then, but up to quite a recent date, was considered final.

Under such circumstances it is but natural that the discovery of a new component of the air, one that is present to the considerable amount of about *one* per cent, excited great and justifiable astonishment. How was it possible, people asked, that in the face of all the improvements in both physical and chemical methods of observation of the present day this gas should for so long have remained unobserved? The answer to this question lies not only in the strange indifference to chemical investigations by which the age is characterized, but also in the investigations on the physical properties of atmospheric gases not having then reached that high degree of

accuracy which Lord Rayleigh has since succeeded in attaining. This is specially the case in determining densities. It has been shown that nitrogen, when separated from the air, is invariably heavier than when produced from its chemical compounds. As the difference is no less than one half per cent, there is no doubt as to the existence of this divergence, since the accuracy of the weigher was such that the possible fault could only be 1/50 thereof. Since between these two kinds of nitrogen – on the one hand the atmospheric, on the other that obtained from chemical compounds – there is a definite difference in density, the question arose: What could be the cause of this peculiar state of things? All the circumstances of the investigation which might be supposed to have any influence in this respect having been carefully examined, and their influence being found insufficient to explain the difference observed, there remained, in Lord Rayleigh's opinion, but one possibility, viz. that the atmospheric nitrogen was not a simple element, but was a combination of pure nitrogen and some new, hitherto unknown, heavier gas. If so, this gas could be isolated in some way or other. The methods, physical or chemical, available for this isolation were already known in principle, and the problem now was to obtain the new gas not only in the purest form possible, but also in a sufficient quantity to allow of a thorough investigation of its essential properties. These both difficult and tedious tests have been carried out conjointly by Lord Rayleigh and Sir William Ramsay, and have resulted not only in completely proving that the new gas occurs in a ready state in the air, but also in establishing a thorough knowledge of its chief physical and chemical characteristics.

The time at my disposal does not permit of my giving a detailed account of these questions, interesting and important as they undoubtedly are, but I venture to call attention to the fact that besides the great importance always adherent to the proving of the existence of a new element, this one is of special interest owing to the purely physical investigations on which it is based, investigations which – embracing not only nitrogen but several other important gases – are characterized by a delicacy and precision that is very rarely met with in the history of physical research. Considering also that to the discovery of argon we may trace one of the causes of Sir William Ramsay's brilliant discovery of helium and the other so-called « noble gases », which followed shortly after, we may truly aver that Lord Rayleigh's work is of that fundamental character that the award to him of the Nobel Prize in Physics must be greeted with sincere and fully justified satisfaction, more especially since this section of his work is but a single link in a long chain

of remarkable investigations with which from various points of view he has enriched Physical Science, and which are of such a nature that they will ensure him a prominent position in its history for all time to come.

Lord RAYLEIGH

The density of gases in the air and the discovery of argon

Nobel Lecture, December 12, 1904

The subject of the densities of gases has engaged a large part of my attention for over 20 years. In 1882 in an address to the British Association I suggested that the time had come for a redetermination of these densities, being interested in the question of Prout's law. At that time the best results were those of Regnault, according to whom the density of oxygen was 15.96 times that of hydrogen. The deviation of this number from the integer 16 seemed not to be outside the limits of experimental error.

In my work, as in the simultaneous work of Cooke, the method of Regnault was followed in that the working globe was counterpoised by a dummy globe (always closed) of the same external volume as itself. Under these conditions we became independent of fluctuations of atmosphere density. The importance of this consideration will be manifest when it is pointed out that in the usual process of weighing against brass or platinum weights, it might make more apparent difference whether the barometer were high or low than whether the working globe were vacuous or charged with hydrogen to atmospheric pressure. Cooke's result, as at first announced, was practically identical with that of Regnault, but in the calculations of both these experimenters a correction of considerable importance had been overlooked. It was assumed that the external volume of the working globe was the same whether vacuous or charged to atmospheric pressure, whereas of course the volume must be greater in the latter case. The introduction of the correction reduced Cooke's result to the same as that which I had in the meantime announced, viz. 15.88. In this case therefore the discrepancy from Prout's law was increased, and not diminished, by the new determination.

Turning my attention to *nitrogen*, I made a series of determinations, using a method of preparation devised originally by Harcourt, and recommended to me by Ramsay. Air bubbled through liquid ammonia is passed through a tube containing copper at a red heat where the oxygen of the air is consumed by the hydrogen of the ammonia, the excess of the ammonia being subsequently removed with sulphuric acid. In this case the copper serves

merely to increase the surface and to act as an indicator. As long as it remains bright, we have security that the ammonia has done its work.

Having obtained a series of concordant observations on gas thus prepared I was at first disposed to consider the work on nitrogen as finished. Afterwards, however, I reflected that the method which I had used was not that of Regnault and that in any case it was desirable to multiply methods, so that I fell back upon the more orthodox procedure according to which, ammonia being dispensed with, air passes directly over red hot copper. Again a series in good agreement with itself resulted, but to my surprise and disgust the densities obtained by the two methods differed by a thousandth part – a difference small in itself but entirely beyond the experimental errors. The ammonia method gave the smaller density, and the question arose whether the difference could be attributed to recognized impurities. Somewhat prolonged inquiry having answered this question in the negative, I was rather at a loss how to proceed. It is a good rule in experimental work to seek to magnify a discrepancy when it first presents itself, rather than to follow the natural instinct of trying to get quit of it. What was the difference between the two kinds of nitrogen? The one was wholly derived from air; the other partially, to the extent of about one-fifth part, from ammonia. The most promising course for magnifying the discrepancy appeared to be the substitution of oxygen for air in the ammonia method, so that *all* the nitrogen should in that case be derived from ammonia. Success was at once attained, the nitrogen from the ammonia being now 1/200 part lighter than that from air, a difference upon which it was possible to work with satisfaction. Among the explanations which suggested themselves were the presence of a gas heavier than nitrogen in the air, or (what was at first rather favoured by chemical friends) the existence in the ammonia-prepared gas of nitrogen in a dissociated state. Since such dissociated nitrogen would probably be unstable, the experiment was tried of keeping a sample for eight months, but the density was found to be unaltered.

On the supposition that the air-derived gas was heavier than the « chemical » nitrogen on account of the existence in the atmosphere of an unknown ingredient, the next step was the isolation of this ingredient by absorption of nitrogen. This was a task of considerable difficulty; and it was undertaken by Ramsay and myself working at first independently but afterwards in concert. Two methods were available – the first that by which Cavendish had originally established the identity of the principal component of the atmosphere with the nitrogen of nitre and consisting in the oxidation of the

nitrogen under the influence of electric sparks with absorption of the acid compounds by alkali; the other method was to absorb the nitrogen by means of magnesium at a full red heat. In both these ways a gas was isolated of amount equal to about one per cent of the atmosphere by volume and having a density about half as great again as that of nitrogen. From the manner of its preparation it was proved to be non-oxidizable and to refuse absorption by magnesium at a red heat, and further varied attempts to induce chemical combination were without result. On this account the name *argon* was given to it. The most remarkable feature of the gas was the ratio of its specific heats, which proved to be the highest possible, viz. 1.67, indicating that sensibly the whole of the energy of molecular motion is translational.

Argon must not be deemed rare. A large hall may easily contain a greater weight of it than a man can carry.

In subsequent investigations Ramsay and Travers discovered small quantities of new gases contained in the aggregate at first named argon. Helium, originally obtained by Ramsay from clevite, is also present in minute quantity.

Experiments upon the refractivity and viscosity of argon revealed nothing specially remarkable, but the refractivity of helium proved to be unexpectedly low, not attaining one-third of that of hydrogen – the lowest then known.

As regards the preparation of argon, it is advantageous to begin with *liquid air*, for preparing which a plant is now to be found in many laboratories. It should perhaps be remarked that «liquid air» is something of a misnomer. What is liquified is not the whole of the air supplied or even a fair sample of it. The oxygen, being less volatile, is contained in undue proportion, and this excess increases as evaporation proceeds. Argon, being intermediate in volatility, may be expected to increase relatively to the nitrogen, though it decreases relatively to the oxygen. A number of analyses for oxygen and argon as evaporation proceeded are appended (Table 1); they relate to the *vapour* arising from the liquid and not to the liquid itself. The oxygen, expressed as a fraction of the whole, varied from 30 to 98 per cent. From 43 to 90 per cent of oxygen, the argon, as a percentage of the whole, scarcely varied from 2.0.

The experiment under the head 98 per cent is not quite comparable with the others. The last entry, corresponding to 100 per cent of oxygen, is theoretical and does not represent any actual experiment.

Table 1.

Percentage of oxygen	*Percentage of argon*	*Argon, as a percentage of nitrogen and argon*
30	1.3	1.9
43	2.0	3.5
64	2.0	5.6
75	2.1	8.4
90	2.0	20.0
98	0.76	33.0
100	0.00	100.0

The above numbers show that a great advantage may be obtained by starting with liquid air. Something depends upon the procedure to be adopted for eliminating the nitrogen. Upon a moderate scale and where there is a supply of alternating current, the method of oxidation, as in the analyses, is probably the more convenient. In this case it may be an advantage to retain the oxygen. If the oxygen content be about 60 per cent, as in the third experiment, the proportion is about sufficient to oxidize the nitrogen. We may compare this with the mixture of atmospheric air and oxygen which behaves in the same manner. In the latter case the proportion of argon would be reduced from 2.0 per cent to about 0.4 per cent, so that the advantage of using the liquid air amounts to about *five* times. In an arrangement that I have described for oxidizing nitrogen upon a large scale, the mixed gases were absorbed at the rate of 20 litres per hour.

In the alternative method the nitrogen is absorbed by magnesium or calcium. In this case it is necessary first to remove the oxygen; but oxygen is so much more easily dealt with than nitrogen that its presence, even in large proportion, is scarcely an objection. On this view and on the supposition that liquid air is available in large quantities, it is advantageous to allow the evaporation to proceed to great lengths. A 20 per cent mixture of argon and nitrogen (Experiment 5) is readily obtained.

Although the preparation of a considerable quantity of argon is rather an undertaking, there is no difficulty in demonstrating its existence with the most ordinary appliances. By the use of a specially shaped tube and an ordinary induction-coil actuated by a small Grove battery, I was able to show the characteristic spectrum of argon at atmospheric pressure, starting with 5 cc only of air.

Another question relating to the gases of the atmosphere has occupied my attention – namely the amount of free hydrogen. It may be remembered that M. A. Gautier, as the result of very elaborate investigations, announced that the amount of free hydrogen reached 2/10,000, in addition to variable quantities of hydrocarbons dependent upon the locality where the air was collected. My own earlier observations related to the visibility of the C-line in sparks taken through carefully dried air at atmospheric pressure. This line could never be entirely eliminated, but with precaution could be made somewhat faint. When (country) air to which 1/5,000 of hydrogen had been added was substituted for the pure air, the visibility of C was markedly increased; and the difference was such that one might easily believe that the proportion of hydrogen actually operative had been doubled. This conclusion would be in precise accordance with M. Gautier, could we assume that the smaller quantity of hydrogen really accompanied the air. But other observations rendered this assumption extremely doubtful.

In the first place the visibility of C with ordinary air was not perceptibly diminished by passage of the air over red-hot copper oxide. It may be argued that this reagent is not competent in moderate length to remove the last traces of hydrogen from air, even though the air be passed over it in a slow stream. I found, however, on a former occasion that hydrogen purposely introduced into nitrogen could be so far removed in this way that the density remained sensibly unaffected, although 1/10,000 of residual hydrogen might be expected to manifest itself.

Moreover, when air purposely contaminated as above with 1/5,000 of hydrogen was passed over the copper oxide, the additional hydrogen appeared to be removed, the visibility of C reducing itself to that corresponding to untreated air.

Subsequently further spectroscopic experiments were made in which the platinum points were replaced by points of aluminium; and the evidence could hardly be reconciled with Gautier's view without coincidences of little *a priori* probability.

I was accordingly induced to make determinations by direct combustion. Ten litres of fresh *country* air dried as thoroughly as possible were passed over copper oxide at a red heat. The gain in weight of a phosphoric tube through which the air subsequently passed indicated an amount of free hydrogen corresponding to only about one-seventh of that recorded by Gautier, even though no allowance be made for the possible presence of hydrocarbons.

A defective gain of weight can hardly be explained as due to faulty manipulation. The important question is as to the efficiency of the copper oxide. Did my furnace tube allow the main part of the free hydrogen to pass unburnt? The question is one that can hardly be answered directly, but I may say that variations of temperature (within moderate limits) did not affect the result.

What it is possible to examine satisfactorily is the effect of small additions of hydrogen to the air as collected. In my later experiments the added hydrogen was only 1 cc, that is 1/10,000 by volume, or half the quantity originally present according to M. Gautier. The hydrogen was first diluted in a gas pipette with about 100 cc of air and allowed time to diffuse. The 10-litre aspirating-bottle being initially full of water, the diluted hydrogen was introduced at the top and was followed by 10 litres of air from the open, after which the mixture stood overnight, precautions which had been found sufficient to ensure a complete mixture in the spectroscopic work. The results showed an *additional* gain of 0.00072 grams, very nearly the full amount (0.00075) corresponding to the 1 cc of added hydrogen. We may say then that the copper oxide was competent to account for a small *addition* of hydrogen to air.

Another branch of my work upon gases has relation to the law of pressure, especially at low pressures. Under these circumstances the usual methods are deficient in accuracy. Thus Amagat considers that under the best conditions it is not possible to answer for anything less than 0.01 mm of mercury. By the use of a special manometer I was able to carry the accuracy at least 50 times further than Amagat's standard, and thus to investigate with fair accuracy the effect of pressures not exceeding 0.01 mm in total amount. Boyle's law was fully verified, even in the case of oxygen, for which C. Baur had found anomalies, especially in the neighbourhood of 0.7 mm pressure.

More recently I have made determinations of the compressibility of gases between one atmosphere and half an atmosphere of pressure. For this purpose two manometric gauges, each capable of measuring half an atmosphere, were employed. The equality of the gauges could be tested by using them *in parallel*, to borrow an electrical term. One of the gauges alone would thus serve for half an atmosphere, while the two combined *in series* gave the whole atmosphere. In combination with these gauges volumes in the ratio of 2 : 1 were needed. Here again the desired result was arrived at by the use of two equal volumes, either alone or in combination. Any question as to

the precise equality of the two volumes is eliminated in each set of observations by using the two single volumes alternately. The mean result then *necessarily* corresponds to the half of the total volume, except in so far as the capacities of the vessels may be altered by change of pressure.

The annexed Table 2 gives a summary of results for the various gases.

Table 2.

Gas	*B*	*Temperature*
O_2	1.00038	11.2
H_2	0.99974	10.7
N_2	1.00015	14.9
CO	1.00026	13.8
Air	1.00023	11.4
CO_2	1.00279	15.0
N_2O	1.00327	11.0

Here $$B = \frac{pv \text{ at } \frac{1}{2} \text{ atmos.}}{pv \text{ at } 1 \text{ atmos.}}$$

the temperature being the same at both pressures and having the value recorded. That B should be less than unity in the case of hydrogen and exceed that value for the other gases, is what was to be expected from the known behaviour at higher pressures.

The principal interest of these results is perhaps to calculate corrections to ratios of densities, as found at atmospheric pressure, so as to infer what the ratios would be in a state of great rarefaction. It is only under this condition that Avogadro's law can be expected to apply accurately, as I pointed out in 1892 in connection with oxygen and hydrogen.

In the case of nitrogen and oxygen, the correction is not important, and the original comparison of densities* is sensibly unaffected. According to this method the atomic weight of nitrogen is 14.01, in opposition to the 14.05 found by Stas.

* Rayleigh and Ramsay, *Phil. Trans.*, (1895).

Biography

John William Strutt, third Baron Rayleigh, was born on November 12, 1842 at Langford Grove, Maldon, Essex, as the son of John James Strutt, second Baron, and his wife Clara Elizabeth La Touche, eldest daughter of Captain Richard Vicars, R. E. He was one of the very few members of higher nobility who won fame as an outstanding scientist.

Throughout his infancy and youth he was of frail physique; his education was repeatedly interrupted by ill-health, and his prospects of attaining maturity appeared precarious. After a short spell at Eton at the age of 10, mainly spent in the school sanatorium, three years in a private school at Wimbledon, and another short stay at Harrow, he finally spent four years with the Rev. George Townsend Warner (1857) who took pupils at Torquay.

In 1861 he entered Trinity College, Cambridge, where he commenced reading mathematics, not at first equal in attainments to the best of his contemporaries, but his exceptional abilities soon enabled him to overtake his competitors. He graduated in the Mathematical Tripos in 1865 as Senior Wrangler and Smith's Prizeman. In 1866 he obtained a fellowship at Trinity which he held until 1871, the year of his marriage.

A severe attack of rheumatic fever in 1872 made him spend the winter in Egypt and Greece. Shortly after his return his father died (1873) and he succeeded to the barony, taking up residence in the family seat, Terling Place, at Witham, Essex. He now found himself compelled to devote part of his time to the management of his estates (7000 acres). The combination of general scientific knowledge and acumen with acquired knowledge of agriculture made his practice in estate management in many respects in advance of his time. Nevertheless, in 1876 he left the entire management of the land to his younger brother.

From then on, he could devote his full time to science again. In 1879 he was appointed to follow James Clerk Maxwell as Professor of Experimental Physics and Head of the Cavendish Laboratory at Cambridge. In 1884 he left Cambridge to continue his experimental work at his country seat at Terling, Essex, and from 1887 to 1905 he was Professor of Natural Philosophy in the Royal Institution of Great Britain, being successor of Tyndall.

He served for six years as President of a Government Committee on Explosives and from 1896 to 1919 he was Scientific Advisor to Trinity House. He was Lord Lieutenant of Essex from 1892 to 1901.

Lord Rayleigh's first researches were mainly mathematical, concerning optics and vibrating systems, but his later work ranged over almost the whole field of physics, covering sound, wave theory, colour vision, electrodynamics, electromagnetism, light scattering, flow of liquids, hydrodynamics, density of gases, viscosity, capillarity, elasticity, and photography. His patient and delicate experiments led to the establishment of the standards of resistance, current, and electromotive force; and his later work was concentrated on electric and magnetic problems. Lord Rayleigh was an excellent instructor and, under his active supervision, a system of practical instruction in experimental physics was devised at Cambridge, developing from a class of five or six students to an advanced school of some seventy experimental physicists. His *Theory of Sound* was published in two volumes during 1877–1878, and his other extensive studies are reported in his *Scientific Papers* – six volumes issued during 1889–1920. He has also contributed to the *Encyclopaedia Britannica.*

He had a fine sense of literary style; every paper he wrote, even on the most abstruse subject, is a model of clearness and simplicity of diction. The 446 papers reprinted in his collected works clearly show his capacity for understanding everything just a little more deeply than anyone else. Although a member of the House of Lords, he intervened in debate only on rare occasions, never allowing politics to interfere with science. His recreations were travel, tennis, photography and music.

Lord Rayleigh, a former Chancellor of Cambridge University, was a Justice of the Peace and the recipient of honorary science and law degrees. He was a Fellow of the Royal Society (1873) and served as Secretary from 1885 to 1896, and as President from 1905 to 1908. He was an original recipient of the Order of Merit (1902), and in 1905 he was made a Privy Councillor. He was awarded the Copley, Royal, and Rumford Medals of the Royal Society, and the Nobel Prize for 1904.

In 1871 he married Evelyn, sister of the future prime minister the Earl of Balfour, and daughter of James Maitland Balfour and his wife Blanche, the daughter of the second Marquis of Salisbury. They had three sons, the eldest of whom was to become Professor of Physics at Imperial College of Science and Technology, London.

Lord Rayleigh died on June 30, 1919, at Witham, Essex.

Physics 1905

PHILIPP EDUARD ANTON VON LENARD

«for his work on cathode rays»

Physics 1905

Presentation Speech by Professor A. Lindstedt, President of the Royal Swedish Academy of Sciences

Your Majesty, Your Royal Highnesses, Ladies and Gentlemen.

The Royal Swedish Academy of Sciences has decided to give this year's Nobel Prize for Physics to Dr. Philipp Lenard, Professor at the University of Kiel, for his important work on cathode rays.

The discovery of the cathode rays forms the first link in the chain of brilliant discoveries with which the names of Röntgen, Becquerel and Curie are connected. The discovery itself was made by Hittorf as long ago as 1869 and therefore falls in a period before that which the Nobel Foundation is able to take into account. However, the recognition which Lenard has earned himself by the further development of Hittorf's discovery (which is becoming of increasing importance) shows that he too deserves the same reward as has already come to several of his successors for work of a similar nature.

Cathode rays are a phenomenon which occurs when electricity is discharged in a rarified gas. If an electric current is led through a glass tube containing rarified gas, certain radiation phenomena appear both in the gas and around the metal wires or poles through which the current is carried. These phenomena change in form and nature if the gas contained in the tube is rarified even further. At a given low pressure of gas, rays are emitted from the negative pole, called the « cathode », which are invisible to the naked eye but which can be observed through certain peculiar effects. This is due to the fact that when these rays hit the walls of the glass tube, or other obstacles in their path, they cause them to glow or fluoresce and are able to bring objects against which they are directed to a glowing heat. Like rays of ordinary light they propagate in straight lines, but they differ in that they can be deflected from their straight path by means of a magnet.

The general characteristics of these cathode rays had been known a long time, although not sufficiently to clarify their true nature. Twenty years ago two basically different concepts were prevalent. According to one concept, which was supported especially by German physicists, cathode rays consisted as do normal rays of light, of undulatory motion in the ether. According to the other concept, which was mainly popular among English sci-

entists, cathode rays were supposed to consist of particles which were ejected from the cathode and were charged with negative electricity. The decision for one or other of these theories rested on the results of experimental research. These experiments, however, were greatly impeded by the fact that one seemed to be restricted to phenomena within the glass tube itself, since the cathode rays ended at the wall of the tube. The question of whether they could at all exist outside the tube remained unanswered.

These were the circumstances prevailing when Lenard began his work on cathode rays in 1893. He started from a fact which had been observed by his great and prematurely deceased teacher Heinrich Hertz: that these rays were able to pass through thin metal plates which had been introduced into the discharge tube. At Hertz's suggestion he utilized this fact in an attempt to lead the rays out of the tube. He used for this a tube which was not wholly made of glass but terminated at one place in a very thin aluminium plate. As the cathode rays reached Lenard's «aluminium window», it was found that they passed through it and continued their course in the air outside the tube. This constituted a discovery which was to have the most far-reaching consequences, above all for the study of the radiation phenomena themselves. It became possible to study cathode rays under much simpler and more convenient experimental conditions than before, and also to separate observations on conditions needed for the production of the rays in the tube from questions concerning their propagation and other characteristics.

Lenard found first of all that the rays coming through the aluminium window possessed the same characteristics as those previously noted in rays inside the tube, i.e. that they cause fluorescence, can be deflected by a magnet and so on. He further proved that cathode rays have certain chemical effects such as causing impressions on photographic plates, ozonizing air, making gases conducting through so-called ionization, etc. It was also discovered that these rays pass unimpeded through empty space but that in gases they are subject to diffusion which increases with the density of the gas; and, moreover, that bodies in general differ in permeability, as their absorptive power bears a direct relationship to their density. Cathode rays proved to be carriers of negative electricity even in empty space and they could be deflected from their path by both magnetic and electrical fields. Finally, Lenard showed that there are various types of cathode rays, differentiated amongst other things by the fact that they are deflected by magnets, to a greater or lesser extent. He also found that the formation of one or other type of ray is determined by the degree of gas rarification in the discharge tube.

When Lenard began his work on cathode rays he approached the concept of their nature from the German viewpoint noted above, whereby the rays are explained as being vibrations in the ether. Through the results of his work which we have just briefly described and in particular through the discovery that cathode rays are influenced by electrical fields, this view became untenable. He now came closer to the English view, put forward mainly by Crookes, that the rays are composed of particles which are ejected by the cathode and are bearers of negative electricity. Since then, however, this theory has had to be modified in several significant details in order to reconcile it with phenomena which have been brought to light through the work of Lenard and others. It was shown, for example, that these particles which, according to Crookes, are ejected from the cathode – the so-called «electrons» – must have a considerably smaller mass than chemical atoms, that the velocity of these electrons can come to about one-third of the speed of light, but that there are also cathode rays which are considerably slower: the various types of cathode rays are in fact explained by the different speeds with which they are ejected from the cathode. In his more recent work Lenard has been able to produce cathode rays with relatively slow speed, rays which are formed through the influence of ultraviolet light on bodies charged with negative electricity. This has also served to explain an important phenomenon noted by other research workers.

The research by Lenard, only a very brief report of which is given here, has been followed by a series of valuable studies by other scientists as well. Development of the theoretical basis for the theory of electrons has gone hand in hand with the experimental work. The study of electrons, their characteristics and their behaviour in relation to matter has been given a sounder basis through these researches on cathode rays and has been gradually developed into one of the foremost theories of modern physics by Lenard himself and by other workers. This theory is in fact not only important for the explanation of cathode rays and other closely related phenomena – the electron theory with its concepts on the constitution of matter has become of the most fundamental importance for the sciences of electricity and of light and for both the physicist and the chemist.

It is clear that Lenard's work on cathode rays has not only enriched our knowledge of these phenomena, but has also served in many respects as a basis for the development of the electron theory. Lenard's discovery that cathode rays can exist outside the discharge tube, in particular, has opened up new fields of research in physics. It gave an impetus to the search for other

thus far unknown sources of similar rays, and the revolutionary discoveries by past Nobel Prize winners – Röntgen, Becquerel and the two Curies – and by other scientists which have followed can well be considered the fruits of this impetus and links in the history of development of one and the same science.

Because of the overall importance of Lenard's work, and because of its scientific value and pioneering nature, the Royal Swedish Academy of Sciences has decided to award him the Nobel Prize in Physics for the year 1905.

PHILIPP E. A. VON LENARD

On cathode rays

Nobel Lecture, May 28, 1906

I am pleased to fulfil my obligation as a Nobel Prize winner to talk to you here on cathode rays. I assume that you would prefer me to tell you what others could not tell you. I shall describe to you the development of the subject – which also embraces recent theories concerning electricity and matter – as it has appeared to me, on the basis of my own experience.* This will give me a welcome opportunity of showing on the one hand how my work has depended on that of others, and on the other how in one or two points subsequent, or more or less contemporary, work by other investigators is related to mine. Thus – using the simile which you, my esteemed colleagues of the Academy of Sciences, have used at the head of your member's diploma** – I shall now speak not only of the fruits but also of the trees which have borne them, and of those who planted these trees. This approach is the more suitable in my case, as I have by no means always been numbered among those who pluck the fruit; I have been repeatedly only one of those who planted or cared for the trees, or who helped to do this.

In the time at my disposal I can deal at length with only a few aspects of my work in the field under discussion.

The start takes me back 26 years to Crookes. I had read his lecture on «radiating matter» (5)*** – his term for cathode rays**** – and was greatly impressed by it. You are all acquainted with the tests he made. Here Fig. 1 is one as a reminder: the glass tubes with highly rarefied air; the negatively charged plate or cathode (*a*) on which the rays are produced; a cross (*b*) in the path of the rays, and here the shadow of the cross (*d*) thrown by the rays

* In this paper I have tried hard to put into their historical perspective all the publications which in my opinion have made basic contributions to knowledge, even if they came to my notice too late for them to influence my work.

** Coat of arms: gardener planting young trees, with the motto «For our descendants».

*** The numbers in brackets refer to the bibliography at the end; here «p.» gives the page number in the case of voluminous publications.

**** After Faraday, Hittorf (2) and then Goldstein (4) had already produced and progressively studied «glow rays» or «cathode rays». But Crookes made more progress than these workers, because he carried out experiments at a higher vacuum.

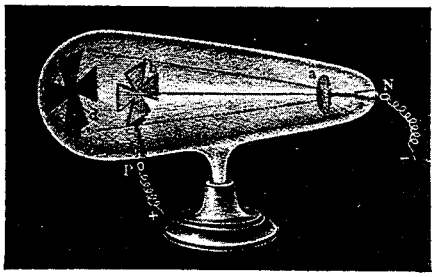


Fig. 1.

onto the phosphorescent glass wall. The shadow moves when a magnet is brought near; this is a sign that the cathode rays – unlike light rays – are bent in a magnetic field.

I have always attached great importance in my work to the problem of isolating the phenomenon being studied from interference sources, irrespective of the difficulties that this entails, an approach already adhered to by Crookes in his work. For it was he who produced these cathode rays in a pure form as never reached before, and showed that the phenomena concerned are of a very special type differing from other discharge phenomena through their attractive simplicity. The real nature of «radiating matter» or «the fourth state of aggregation», as he called it, was then beyond my comprehension, just as it must, we may now be sure, have been beyond his. But I readily shared his enthusiasm when he said: «Here, I believe, are the ultimate realities.» And we were right: that is why I stand here today!

My interest in these matters found no direct expression during my student days. Electrical gas discharges were not considered a suitable object of study for beginners, and rightly so. But even mature investigators achieved nothing really significant in this field in the years following Crookes' work. They did not obtain any results that in themselves opened new vistas, and so far as purity of experimental conditions was concerned they hardly progressed beyond Crookes' work.

It was only later, when I was assistant to Quincke in Heidelberg, that I had the opportunity and the facilities for building a mercury air pump capable of giving very high rarefaction – then by no means a standard item of equipment in physics institutes – and for carrying out tests myself on cathode rays. I wanted to advance as directly as possible, and thought how fine it would be, in particular, to bring these rays from the tube out into the open air; it would then be possible to carry out direct experiments with them. To do this it was necessary to fit into the wall of the tube an airtight seal that would

allow the rays to pass through. Now radiating *matter* would not readily pass through airtight seals, but might not Eilhard Wiedemann be right in assuming that cathode rays were a form of ultra-ultraviolet light? Finally, quartz appeared to me to be the most promising material, since it best transmitted all the radiations that were then known. Here (Fig. 2) is the tube I built at the time with the cathode plate, and at the top of it you will see the opening sealed by a quartz plate 2.4 mm thick. The test was unsuccessful, however; outside the quartz I found no phosphorescence nor even any electrical* effects that could be ascribed with certainty to the light issuing from the tube.

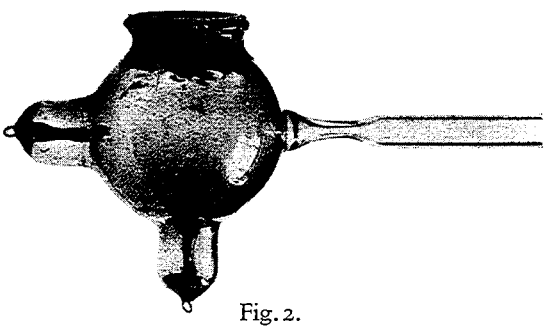


Fig. 2.

It was four years later, in 1892, that another opportunity arose. Hertz, whose assistant I then was, had found that thin metal leaf transmits cathode rays (15). He used quite thin, very soft and porous gold, silver and aluminium leaf used in bookbinding, but showed that the cathode rays not only pass through the holes but through the material itself, the metal of the leaf. One day he called me over – an event which to my great regret at the time did not occur often – and showed me what he had just found: uranium glass covered with aluminium leaf inside a discharge tube, glowed under the leaf when irradiated from above. He said to me: « We ought – and I might simply do this for he was prevented – to separate two chambers with aluminium leaf, and produce the rays as usual in one of the chambers. It should then be possible to observe the rays in the other chamber more purely than has been done so far and even though the difference in air pressure between the two chambers is low because of the softness of the leaf, it might be possible to completely evacuate the observation chamber and see whether this impedes the spread of the cathode rays – in other words find out whether the rays are

* Hertz' discovery of such effects of ultraviolet light had at that time just been made (8).

phenomena in matter or phenomena in ether.» He appeared to consider this last question to be the most important one. I did carry out the test later; but I was primarily interested in my earlier question, that of cathode rays in the open air. I was not put off by the softness of the leaf used by Hertz. I laid more and more of such leaves on top of each other in a suitable tube and found that 10 and 15 layers still transmitted the rays fairly well. I then procured some pieces of aluminium foil of comparable thickness, to see whether they would withstand the air pressure. Such was the case, provided that a sufficiently small area of foil was used. Then, taking the old tube again, I replaced the quartz by a metal plate containing a small hole sealed with aluminium foil, spread a few small grains of alkaline-earth phosphor on this small aluminium window, excited the tube and, lo and behold, the grains glowed brightly! I then fixed them slightly above the aluminium window and they glowed brightly there as well! Thus not only had the cathode rays passed out of the interior of the discharge tube to which they had been hitherto confined, in addition – and nobody could have predicted this – they could pass through air of normal density. It thus became clear that a vast new field of investigation had opened up in front of me, a field that not only embraced hitherto unseen phenomena but also gave promise of a breakthrough into the unknown. Cathode rays, which had hitherto stubbornly eluded explanation, had yielded their secret and, more important, now for the first time tests of maximum purity could be carried out. Let us compare the position with that of another type of radiation, light: hitherto it was as if it had been impossible to study light except in the interior of furnaces and flames where it is produced, like the cathode rays in the tube. Where then would the great and detailed science of optics have stopped?! A window had now been built in the furnace through which pure light only could emerge, freed from the complex and still unexplained processes of its formation. Such processes remain confined to the interior of the discharge tube and, as has since been found, could not be understood until a sufficient study had been made of the cathode rays themselves. As we shall see in our historical survey, this study has also provided a great deal of other information, some of which is now general knowledge, on X-rays and radioactivity, as well as a deeper understanding of electricity and matter.

It was now necessary above all to widen the inroad already made into the new field of knowledge. It was important to increase the intensity of the rays coming out of the window, and to improve the conditions of their production compared with the first tube. This led to the construction of the tube

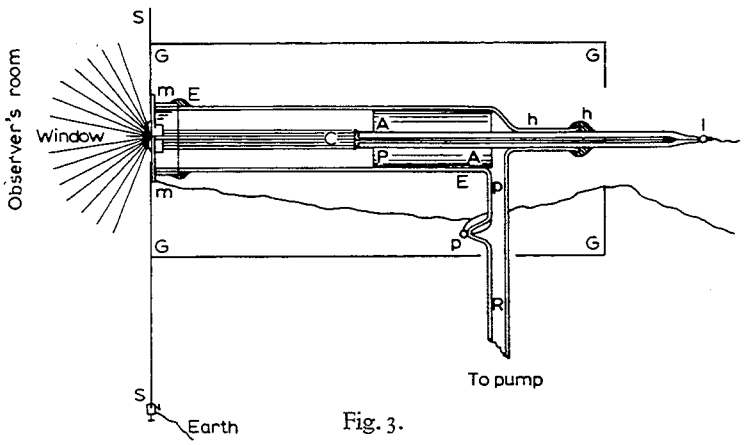


Fig. 3.

illustrated in Fig. 3, which was used in a large number of experiments (18). Here will be seen the production chamber with the anode (A) and the cathode (C), the seal (m m) with the window, and beyond it the observation chamber into which the rays emerge. The number of phenomena possible here is such that although the most obvious ones and also the slightly less obvious ones have probably now all been discovered, so far the consequences of all the phenomena have not yet been studied sufficiently.

It must be noted that the rays are not directly visible; it would be useless to put one's eye to the window, as this organ is not receptive to cathode rays. On the other hand, materials that are capable of becoming luminous without heat, phosphorescent materials as they are called, are suitable for making the rays visible. It is best to use sheets of paper coated with such materials, e.g. a certain ketone, platinum cyanide, or an alkaline-earth phosphor and to hold them as a screen against the rays. If the screen glows, it indicates that it has been hit by the rays. The rays can also be photographed directly. These are the same methods that are used to make visible ultraviolet light, at that time the only known example of such demonstrable invisible radiation.

When we use the phosphorescent screen, we find it glowing brightly close to the window; as the distance from the window increases, the intensity of the rays progressively diminishes until at a distance of about 8 cm the screen remains quite dark. Apparently air at full atmospheric pressure is not very permeable to cathode rays, certainly far less permeable than it is to light. But it was far more interesting to find that air is even a turbid medium for these rays, just as milk is for light. If we place an impermeable wall with a hole

in it a suitable distance from the window and put the edge of the screen against it, we then get this view (Fig. 4). Here the dotted lines indicate the narrow pencil of rays that we should expect in the case of rectilinear propagation; but it is the broad bent bunch of rays that we really see on the screen in the open air, just as if we had passed light through the same hole into a tank containing slightly diluted milk. What clouds the air? In milk it

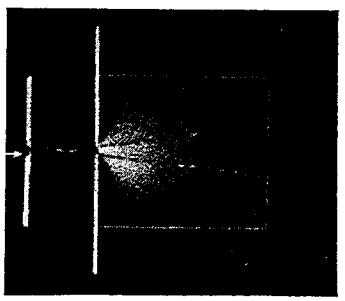


Fig. 4.

is numerous small suspended fat particles that make it turbid to light. Pure air on the other hand contains nothing except molecules of the gases contained in it, suspended in the ether. These molecules are extremely small, 10,000 times smaller than the fat particles, far too small to act individually on light. But, as we see, the cathode rays are hindered by each of these molecules. Thus these rays must be extremely fine, so fine that the molecular structure of matter, which is minute compared with the very fine light waves, becomes pronounced in comparison with them. It may then be possible to obtain data by means of these rays concerning the nature of molecules and atoms.

It is therefore particularly interesting to study the behaviour of a wide variety of materials relative to cathode rays. The first point to be studied was the *permeability*. Some idea of this can be obtained by holding a thin layer of the material being studied, between the window and the screen. It is abundantly clear that the permeability or impermeability of a material to light is is not even slightly related to its behaviour in relation to cathode rays. Here is an example (Fig. 5), a print of a direct photograph taken at the aluminium window. In the top half will be seen the deep shadow of a completely light-permeable $\frac{1}{2}$ mm thick rectangular quartz plate, and in the left half, as a very mat veil, the picture of an ordinary aluminium leaf, impermeable to light and with somewhat irregular boundaries, laid over the whole of this

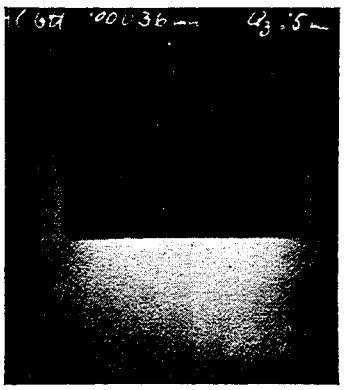


Fig. 5.

half. Great care must be taken in selecting the thickness of the layer throwing the shadow. Thus, for example, the quartz plate used in this experiment is impermeable simply because it is too thick, and the reason why metal leaf was found to be the only example of permeable layers in Hertz' tests lay in the thinness at which metal leaf is available. We shall soon see that most other materials, when of the same thickness, are even more permeable than gold and silver. It is soon evident that the absorption of cathode rays in any substance is a very gradual process, just as in the case of light, where as we know, gold is permeable when it is made sufficiently thin. Here (Fig. 6) we see the shadow of leaves of aluminium laid stepwise on top of each other; the numbers on the left give the number of leaves, those on the right their total thickness. Each increase in the number of leaves, and also any unevenness in

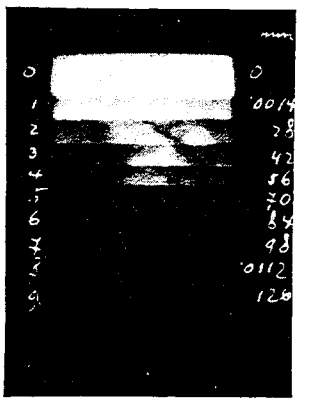


Fig. 6.

the thickness of the individual leaves, can be noted, and it will be seen how almost total permeability changes into almost total impermeability. Thus, with each material it was not a question of simply deciding between «permeable» and «impermeable», but of finding a numerical measure of the degree of absorption of cathode rays therein, of measuring its *absorptive power*, and I did this for a large number of solid and gaseous materials.

The result was astounding. All the great multiplicity of properties that we associate with the different materials around us, disappeared. The sole determining characteristic was the *weight* of the materials(21). Everything of equal weight absorbed equally, anything heavier absorbed more, anything lighter absorbed less, and always in proportion to the weights or the masses. At a first approximation, the chemical composition of the materials, their state of aggregation and other properties, did not count at all – a quite unprecedented finding that was not valid for any radiation known at the time.* At a second approximation, on closer inspection it is seen that the chemical composition has a slight effect: thus, e.g. hydrogen, and anything containing hydrogen, absorbs slightly more than one would expect in proportion to its weight. I must forbear to discuss these deviations and their significance in detail here.** As an illustration of the law of proportionality between mass and cathode ray absorption, that is valid at a first approximation, let us see the direct photographs of the shadows of layers of aluminium, silver and gold of equal thickness (Fig. 7). It will be seen that the heavy silver absorbs more than the lighter aluminium, and that gold, which is the heaviest, absorbs the most. If on the other hand we take layers of equal weights of the three metals (Fig. 8), we also get equal shadows and equal absorption, and the result would

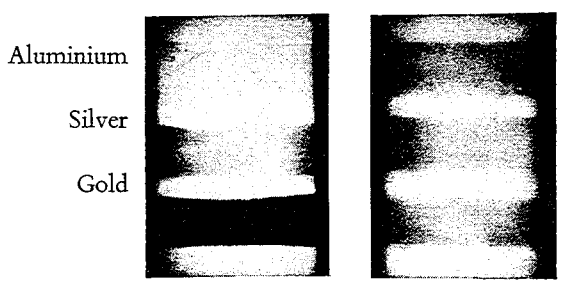


Fig. 7. Fig. 8.

* Later X-rays were found to be a second example of radiation that is absorbed more or less in proportion to the mass.

** Compare (21, 47, 52).

be the same if we had taken any other materials in layers of equal weight.

In fact, in relation to cathode rays, not only the absorption, but also the turbidity, which I also studied in relation to a number of different materials (18*b*, p.257; 21, p.265) was found to be related solely to the weight, the mass of the material in question, the *quantity* of matter – as Newton put it – and not the *quality* of the material.* If we now recall that cathode rays as they spread in matter are simply affected by the individual molecules of the material, we can conclude that the molecules of the most varied materials, and thus also the atoms of the different chemical elements vary, not qualitatively but only quantitatively, from each other, i.e. they all consist of the same basic material but contain different amounts of it. This old but because of the lack of valid data almost forgotten hypothesis of the alchemists was brought back vividly to our mind, this time however not to disappear again but to be proved; as evidence of this we can quote the recent results of Ramsay (54) and Rutherford (51) concerning the amazing transformation of radium** into other elements. But in order to use the law of proportionality between the mass and cathode ray absorption as a basis for drawing more detailed conclusions on the constitution of matter, it was first necessary to know something about the nature of cathode rays themselves. Let us now turn to this problem, which I have also borne in mind throughout my work.

Straightway we can decide whether cathode rays are phenomena that take place in matter or in the ether. When we completely evacuate a chamber by means of an air pump, it then does not contain any matter, only the ether, as present in the heavens. Now it has long been known for instance that sound cannot pass through such evacuated chambers, while light, and electrical and magnetic forces can. Thus there is no doubt that sound is a phenomenon in matter while light and electrical and magnetic forces are phenomena in the ether. We had been unable to carry out the corresponding test in relation to cathode rays in the ordinary discharge tubes, because once all the air is removed the production of the rays in such a tube ceases. But, without interfering in the least with production, we were able to completely evacuate our observation chamber on the other side of the window and see whether de-

* The diffuse reflection of cathode rays, which can be considered to be pronounced backwards-directed scatter, is also determined by the mass, as can be clearly seen from the measurements of A. Becker (52, p. 448).

** As evidence of the elementary nature of radium its spectrum and atomic weight are given (38, 39).

spite this the cathode rays spread in this chamber. We found that the propagation of the rays is particularly good in an extreme vacuum; all absorption and turbidity due to the gas molecules disappear, the rays attain lengths of several meters and are of such rectilinear sharpness as we are accustomed to find only in light rays(18). Thus cathode rays are *phenomena in the ether*. In particular, on the basis of the hypotheses which we have mentioned, it could be stated that cathode rays were not radiating *matter*, nor emitted gas molecules, as they had come to be regarded, especially in England.* We were still not clear as to what type of phenomena in the ether they were. Many of my readers believed, very wrongly, that I had concluded beforehand that cathode rays were « waves in the ether »; I had no desire to say this or in fact anything unless it was shown to be so in my experiments and appeared to provide an explanation. I had the means available to discover new things daily from Nature herself, in further experiments. I hoped so or thought so, at least. I greatly regretted, therefore, that at this stage my experiments were interrupted for considerable time, first by a far-from-simple task that devolved unexpectedly on me through the untimely death of Heinrich Hertz – the publication of his *Prinzipien der Mechanik* (Principles of Mechanics) and then when I was appointed to a theoretical professorship.

It is barely worth mentioning, but not unimportant for the further development of our subject, that even before this interruption I had designed a new and far more convenient type of discharge tube. I had tested it as far as possible, and had recommended its use and made it generally available (18*b*, p. 228). Here (Fig. 9) the window seal is fitted to a platinum tube, which in turn is fused into the glass; this means the large amount of puttying which often made the tube very difficult to use, is avoided. This type of tube had however a special advantage that could not be foreseen at the time. In it the

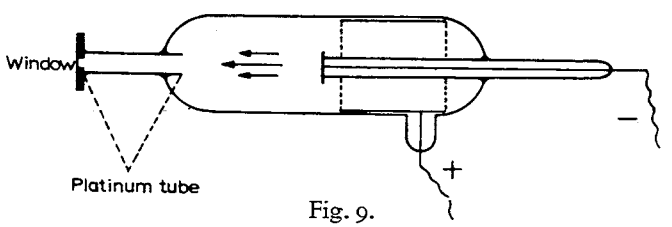


Fig. 9.

* Even after the experiments with the aluminium window had been reported, this theory continued to be held for some time, and it was suggested that molecular collisions actuated, via the window, the molecules of the outside air or molecules remaining in the vacuum.

intensive cathode rays impinge on the large area of platinum – the metal which, as we now know, is most effective in turning the rays into the – then undiscovered – X-rays. Thus X-rays are produced here in very large quantities, and they are also able to pass through the window, either mixed with or separate from the cathode rays, into the observation chamber. This was not possible in the earlier tube because of the large thick metal cover located in the path of the rays (27). The discovery soon after this of X-rays by Röntgen (22), the first investigator to use the type of tube described above, is generally considered to be a good example of a lucky discovery. But, given the tube, the fact that the attention of the observer was already turned from the interior to the outside of the tube, and the presence of phosphorescent screens outside the tube because of the purpose of the tube, it appeared to me that this discovery had of necessity to be made at this stage of development.

On resuming my experiments I soon occupied myself exclusively with an idea already put forward by Hertz (7*b*, p. 275) and Schuster (13), which in relation to the nature of the cathode rays had appeared to me to be very important right from the start, and which I had already begun to pursue in the first period of my experiments. It had been known since Hittorf's days that cathode rays are deflected by *magnets* (2); similarly the deflection of cathode rays noted by Goldstein (4) could be interpreted as being an influence of *electrical* forces on the rays. Now both the magnetic and the electrical deflection of the rays suggest that the cathode rays consist of emitted negatively charged masses. Moreover, from experiments made to measure the magnitude of the magnetic and electrical effects on a ray it is even possible to calculate the velocity of the supposed masses and also the electric charge per unit of mass (the charge/mass ratio). This is what Hertz and Schuster had done, but their results had contradicted each other. Hertz found that his observations refuted the theory of ejected gas molecules, while Schuster found that his fitted in with it, and thus he took them as support for this theory.

This contradiction did not surprise me. For both investigators had observed the interior of the discharge tube, and they might have been confused by the complications of the production process and the presence of the gas, as in fact they both admitted with reserve. It was now time to carry out these important tests under clearly-defined experimental conditions, i.e. outside the discharge tube and in a very high vacuum, and some excitement as to the result was permissible. For if we know already that the rays are ether, not material phenomena, it should be astonishing that their behaviour still resembled so deceptively that of ejected negatively charged gas molecules.

Nothing known so far had made solving this dilemma of the streaming molecules and the ether phenomena possible; these experiments would do this, and in any case therefore they would reveal something quite new.

While I was still doing the preparatory work for the experiments* I heard that others were already convinced of their importance. J. J. Thomson was the first to publish a detailed publication on the subject (25). His experiments, like those of Hertz and Schuster, were carried out in the discharge tube. He sought to avoid the danger of confusion as a result of interaction of the production discharge, by means of shielding devices, and to compensate the lack of reliability due to the presence of the gas by greatly varying the test conditions. It was clear to me that all that followed would have to rest on this pillar which I had learned to distrust from the discrepancy between Hertz and Schuster. It seemed to me that before it was made a part of the structure of science it should be tested as directly and rigorously as was possible with the means at our disposal. I therefore concluded my experiments, the results of which were as follows (28).

The velocity of the supposed masses was about one third the velocity of light, and the ratio of the charge to the mass was about 1,000 times that of a hydrogen atom in electrolysis, which atom is the lightest material carrier of electricity known to us. So, if the rays were streaming hydrogen atoms, their charge would have to be taken as being 1,000 times that in electrolysis. This possibility had however been excluded by my previous tests, which had shown that the rays are not material bodies. It seemed evident that I had discovered hitherto unknown parts of the ether, representing electric charges and moving like inert masses. The smallness of the inertia determined – 1/1,000 of the inertia of the hydrogen ion, at an equal charge – and the other behaviour (30) of these parts of the ether, made it easier to identify them with what had long been known as the « electrical fluidum ». The solution to the dilemma was therefore as follows: The rays are not emitted electrically-charged molecules but simply streaming *electricity*. Thus, in cathode rays we have found under our very noses what we never believed we should see: electricity without material, electrical charges without charged bodies. We have, in a sense, discovered *electricity itself*, a thing whose existence or non-existence and whose properties have puzzled investigators since Gilbert and Franklin. Earlier workers, even Coulomb, had referred naively to electric-

* These were resumed at Aix under the direction of and with a grant from Wüllner's Institute (1896), and they had again been interrupted when I was appointed to another professorship elsewhere.

ities as something that existed and could almost be grasped. But the number of electrical phenomena known grew steadily without anybody being able to state that they had seen anything of the supposed electricity itself. Thus it was that – about a generation after Coulomb – Faraday (1) and then Maxwell (3) turned their attention completely away from the electricities to concentrate on the *electrical forces* that could be observed. These forces – thought of as states in the ether – appeared in fact in the famous experiments of Hertz (9) to be so likely to exist independently that from then on one felt increasingly inclined to forget their centres, the electricities, that had formerly been regarded as indispensible. Now – again about a generation after Faraday and Maxwell – the picture has changed somewhat: it has become more complete. We have found in cathode rays just as good a way of studying electricity as we found earlier for the electrical forces alone; we can follow the motion of electricity to and fro in these rays over distances stretching several meters, at will and directly with our senses – without any intermediate theoretical conclusions; we can see how electricity behaves under different conditions, and what its properties are; we are now in a position to give to the old term «electricity» a new content based on experience.

This new content, of which we now know a great deal, appears quite different now in many ways from what could have been supposed earlier.

Here it should be noted that all our remarks concerning electricity apply only to *negative* electricity, not positive electricity, about which even today little can be said that is concrete. We cannot claim to be acquainted with it; we can only recognize positively-charged material, whether it be atoms, molecules or groups of molecules.* We thus use the unitary means of expression, and say that a piece of material is positively charged when it has lost negative electricity.

Let us now therefore consider negative electricity, as it appears in our tests. Here we are amazed by the freedom of its motion, which we hitherto believed was only present inside metallic conductors. Already in the discharge tube, in the centre of the gas, we set this electricity in accelerated motion through the voltage applied to the electrodes, and immediately its velocity becomes one third that of light, 100,000 km/sec, and it represents a cathode ray. Now it impinges on the aluminium window. «It will adhere to it and flow to earth» one would have said on the basis of previous knowledge. Far

* Thus, for instance, canal rays and, so far as they have been studied, also the α-rays of radium, have been found to be emitted positively charged molecules.

from it: it passes through the metal plate (28, p.28) and, as I was able to check, its velocity does not diminish appreciably (19; 46, p.479). Beyond the window it can enter a more or less total vacuum, in which it continues its course linearly, representing an electric current in the empty ether, a phenomenon which we had earlier also thought to be impossible. When it finally hits a piece of metal of sufficient thickness, it penetrates it and sticks there; finally, after following such unusual courses it appears as an ordinary charge on the surface of the metal (28).

The problem of whether electricity fills space continuously or not, of whether it has a structure, is of particular interest. I have seen two cathode rays pass through the same chamber in opposite directions, and found in a quantitative investigation of the phenomena that the two rays did not interfere with each other in the slightest (44, p.165). This indicates that the electricity of these rays consists of discrete and very small parts separated by a large volume of free space. We can represent the parts themselves as being more or less impenetrable to each other, because according to Coulomb's law, as soon as two of the parts come very close to each other they must exert enormous repulsive forces on each other. But the best indication of the structure of electricity comes from quite a different source, and is much older.

Here we come to the connexion between our findings and earlier knowledge. Such knowledge was very scanty, and was related to phenomena taking place in and on individual atoms, i.e. phenomena that could not be studied directly, but the connexion was a very good and fruitful one.

Years earlier, Helmholtz in his lecture in memory of Faraday had noted that electrolysis phenomena would suggest that electricity is split up into pieces of constant size, just as matter is split up in atoms (6). This was the indication already available concerning the structure of electricity, the existence of electrical atoms, *electrical elementary quanta*, as Helmholtz called them.*

In the field of optics moreover, the theory already firmly supported by Hertz' famous experiments (9), that each luminous atom be regarded as an electrical oscillator, had suddenly been given a tangible form by the discovery of Zeeman, who in conjunction with Lorentz concluded from his observations that it is *negative*** – not *positive* – electrical mass that oscillates in the lu-

* I recall hearing him use this expression many times in his demonstration lecture in the summer term of 1885.

** It is interesting that in Zeeman's first publications the word «positive» – not «negative» – was printed (24, p. 18), so that there was some delay in recognizing the relationship between his discovery and cathode rays.

minous atoms of a sodium or other metal flame, and that there is a definite ratio between the charge and mass of the oscillating material (24). The ratio was of the same magnitude as that found shortly afterwards, in the way described, for cathode rays.

It seemed likely that in all these cases, in the ions in electrolysis, in the luminous metal atoms and in the cathode rays, and perhaps everywhere where electricity plays a part, we might be concerned with the *same* electrical elementary quanta, the existence of which had first been indicated by Faraday's electrolysis law and which might be further elucidated by means of cathode rays. This theory has been proved, so much so that it has engendered a new branch of physics, so fruitful and already so vast that in this paper, which is devoted primarily to my own work, I cannot say any more in general on the subject. I would simply like to mention three points.

First, as an important initial quantitative check on our conclusions, the direct experimental measurement of the velocity of cathode rays carried out by Wiechert, in which the figure obtained was the same as that obtained from the electrical and magnetic deflections (see above) – about one third of the velocity of light (29).

Second, Kaufmann's experimental result, obtained on the basis of the work of J. J. Thomson and Heaviside (10), and concerning electrical elementary quanta, namely that their mass and inertia are of a purely electromagnetic nature (55), a result that we can interpret as follows: We have no evidence that (negative) electricity is a special *material* with inertia; it appears to be simply a *state*, the state of the ether which we were accustomed, after Faraday (1), Maxwell (3), and Hertz (9), to denote as the electric force field in the environment of electrified bodies, a state which according to Hertz (20) and Bjerknes (33) might consist of latent motion of the ether. Thus, even with the pure elementary quanta of electricity nothing else has been discovered except this ether state in their area. These elementary quanta themselves appear to us in Maxwell's sense to be the probably empty and only purely geometric centres of the electrical forces, except that we can now claim to be able successfully to observe these centres individually, follow their courses and study the geometric proportions of their size and shape. According to this finding, cathode rays, the streaming centres of state, appear to us, more than ever, to be what they seemed to us to be from the beginning, pure ether phenomena.

Thirdly, we must list the names given to these parts of electricity, or centres of state: I have called them *elementary quanta of electricity* or for short *quanta*, after Helmholtz; J. J. Thomson speaks of *corpuscles*, Lord

Kelvin of *electrions*; but the name preferred by Lorentz and Zeeman *electrons* has become the everyday term.

So far we have spoken of cathode rays as such; we shall now discuss their modes of formation, their generation.

The oldest, and for a long time the only known method of generation and the one which we have hitherto used to the exclusion of all others, is the discharge tube. Here, as their name suggests, the rays originate at the cathode. The gas molecules which are under the influence of the prevailing electrical forces have an effect – the proximity effect as I call it (53) – on the electrode metal, whereby quanta are withdrawn from the latter. Immediately they are free they are subjected to the accelerating forces of the field between the electrodes and thus move with increasing velocity away from the cathode; the ray is complete. The ultimate velocity at which we allow it to leave the tube through, say, a window is given by the size of the voltage used; and the very fact that in effect this whole voltage and not just a fraction thereof is determinative for the ultimate velocity proves that the origin of the ray must be sought at the cathode surface and not, say, in the centre of the gas.* By the magnitude of the voltage we can thus produce faster or slower cathode rays and when we previously spoke of $\frac{1}{3}$ the speed of light that applied only to one particular voltage about 30,000 V, which I used generally throughout my experiments.

How would faster or slower rays behave? Some predictions could be made on the basis of my first experiments in which the voltage, and hence the velocity, was slightly varied (18*b*, p.266; 19; 21, p.261). Very fast rays could be expected to have extremely slight absorbability (high penetrating power)**; the slow rays on the other hand appeared best suited to yield information on the forces of atoms, the constitution of matter. For a long time, however, it seemed impossible to carry out pure tests over a sufficiently wide

* To start with this was an arbitrary assumption made in many previous studies on cathode rays; the proof of its correctness was supplied with increasing accuracy as time went on, with most accuracy probably by A. Becker (52, p.404).

** When X-rays were discovered this expectation of them appeared to be fulfilled; their first properties to become known agreed with those to be anticipated from my tests for the fastest cathode rays (27). It was just at first Righi's convincing observation that the X-rays do not carry with them a negative charge (23) that had shown the untenability of the theory that they were extremely fast cathode rays. They are nowadays considered to be short, transverse impulses in the ether, a kind of ultra-ultraviolet light. The fact

range of velocities since the glass of the discharge tube could not withstand the heavy voltages needed for the very fast rays, and the slow rays, although easy to produce in the tube, failed to emerge through the window; they were too absorbable. Other arrangements failed, too.*

Both problems, that of the slowest and that of the fastest rays, were finally solved in quite novel ways.

A discovery by Hertz as early as 1887 (8), completed shortly afterwards by Hallwachs (11), had shown that by mere exposure to ultraviolet light metal plates give off negative electricity to the air. This remarkable fact – nowadays usually referred to as the photo-electric effect – immediately captured my interest at that time and has also continued to do so since. Experiments carried out in collaboration with the astronomer Wolf showed me first of all that ultraviolet light roughens substances or pulverizes them (12; 46, p. 490). Subsequent experiments, however, caused me to think it unlikely that metal particles carried the negative charge off the plate. At the time I conducted my first experiments on cathode rays, when I had discovered that the air in front of the aluminium window becomes conductive (18), I formed the idea that cathode rays could be driven from the plate into the air by ultraviolet light. Both then and later I made repeated vain attempts to detect possible rays in the vacuum on fluorescent screens. Only my decision – based on Righi's work (14) – to use the electrometer instead of the fluorescent screen revealed the existence of the rays. The apparatus used is illustrated in Fig. 10. U is the plate to be irradiated and is in a complete vacuum; the quartz seal at B admits the ultraviolet light. The cathode rays start from U and a narrow beam is separated out by the hole in the counterplate E. This beam impinges on the small plate α which collects the negative charge brought by the beam and thereby indicates the existence of the radiation on the electrometer. We bring a magnet or the coil indicated by a broken line close to the tube in a suitable manner and then find the charge on the plate β instead of on α, in-

that such impulses can actually occur side by side with heavy absorption of cathode rays, such as e.g. in heavy platinum, follows, as first put forward by Heaviside, from Maxwell's theory (10). The fact that extremely short-wave ultraviolet light will pass unrefracted through prisms was foreseen by Helmholtz in his dispersion theory (17c, p. 517). The low-absorbability cathode rays forecast first became a reality in the rays emitted by radium.

* Amongst other measures I had tried inserting, instead of the window, narrow channels between generating chamber and completely evacuated observation chamber, but they allowed too much gas through.

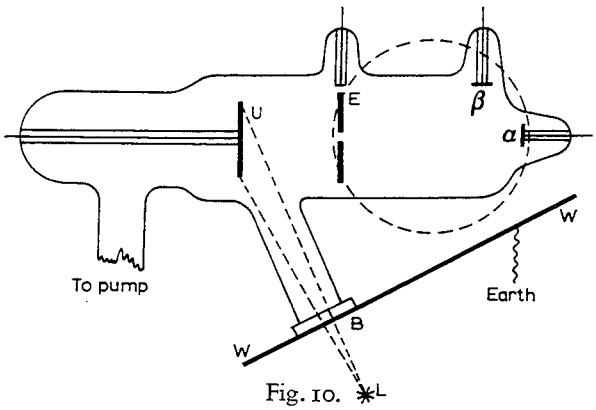


Fig. 10.

dicating that the invisible ray is actually deflected by the magnet and in the appropriate direction for cathode rays. When carried out quantitatively, the experiment showed that the deflection is also of the correct degree, and that the same ratio obtains between charge and mass of the quanta as in the case of the rays generated in discharge tubes (32; 44, p. 150; 46).

Immediately it had been established beyond doubt in this way that cathode rays are produced by ultraviolet light and that their behaviour had become sufficiently well known, I was soon able to detect them on fluorescent screens (44), then follow them further and use them. I shall refer to those aspects later. The following should be noted as regards the actual generation.

Firstly – an important point for pure experiments – it also occurs in a complete vacuum where the usual method fails. A gas need not be present but it does not interfere with the generation of the rays. What is involved is the direct action of the light on the metal of the plate. The initial velocities with which the quanta leave the plate are so slight that a negative charge of only a few volts on the counterplate is sufficient to compel the rays to reverse before reaching it. They then return to the irradiated plate in the same way as a stone thrown upwards falls black to the ground (32; 44).*

* My first detailed communication on the subject (32) appeared in the *Sitzungsberichte der Kaiserl. Akademie der Wiss. zu Wien* for 19th October 1899. In the December issue of *The Philosophical Magazine* of the same year J. J. Thomson published studies «On the mass of the ions in gases at low pressures» in which the photo-electric effect is involved although its centre is still sought in the gas adjacent to the irradiated plate, as the remarks on p. 552 indicate. In the same author's book *Conduction of Electricity through Gases*, 2nd. ed., 1903, p. 109, my publication is dated one year later than that just mentioned since a later reprint (*Ann. Physik*, 2 (1900) 359, where it is expressly marked as a reprint), and not the original is cited.

Here, therefore, we obtain extremely slow cathode rays; faster ones can be produced merely by charging the counterplate positively. The velocity of the rays can be controlled freely by the level of the voltage of the counterplate.

Secondly, considering the effect of the ultraviolet light on the plate, we must imagine that the light waves cause the interior of the metal atoms in the plate to vibrate. We have previously mentioned that Zeeman's discovery has proved atoms to contain negative electricity capable of vibration. If the co-vibration of a negative quantum in the atom with the light waves becomes too violent, the quantum escapes from the atom* and so from the plate; we have a cathode ray.

The velocity at escape we have already mentioned as very low. I have also found that the velocity is independent of the ultraviolet light intensity (44), and thus concluded that the energy at escape does not come from the light at all, but from the interior of the particular atom. The light only has an initiating action, rather like that of the fuse in firing a loaded gun. I find this conclusion important since from it we learn that not only the atoms of radium – the properties of which were just beginning to be discerned in more detail at that time – contain reserves of energy, but also the atoms of the other elements; these too are capable of emitting radiation and in doing so perhaps completely break down, corresponding to the disintegration and roughening of the substances in ultraviolet light. This view has quite recently been corroborated at the Kiel Institute by special experiments which also showed that the photo-electric effect occurs with unchanged initial velocities even at the temperature of liquid air.

We cannot regard the action of the light as restricted only to the solid state of aggregation. The molecules, or atoms of gases undergo a completely analogous effect under the action of ultraviolet light (35; 40); it is reasonable to assume that quanta escape from them (49, p. 486); the gas thus becomes electrically conductive in a manner which we shall discuss in detail later. If the gas contains oxygen like the air, ozone is formed as a by-product (35).**

* This is a process which was earlier anticipated in Helmholtz's comprehensive dispersion theory (17 *c*, p. 518).

** In the light of subsequent studies by Warburg it can be assumed that the most productive of current methods of producing ozone, i.e. those using what are termed «silent electrical discharges», are wholly or largely effective owing to the ultraviolet light of these discharges (48). The rich sources of ultraviolet light obtainable nowadays, e.g. electrical mercury quartz lamps, propagate such a noticeable odour of ozone in their environment that this effect of ultraviolet light has now became a commonplace.

This same action of light, namely the production of cathode rays, the vibration of atoms and the releasing of quanta therefrom, is also involved in phosphorescence (50, p. 671) and hence probably also in fluorescence, perhaps, too, in all photochemical effects. Bearing in mind that we have detected transformation of energies from the interior of the atoms associated with the photoelectric effect, we should not be surprised if in future perhaps we encounter phenomena of the same type acting as sources of energy not introduced from outside.

It should also be mentioned that the research carried out by Curie and Sagnac (37) as well as that by Dorn (42) indicates that in common with ultraviolet light, X-rays too have the effect of generating cathode rays. This is consistent with their ability to make gases electrically conductive and induce phosphorescent and photochemical effects.

Scarcely had ultraviolet light been shown suited for the generation of the slowest rays when the solution was found to the problem of how the fastest rays originate. The rays emitted by uranium and radium were already known; Becquerel, P. and M. Curie were engaged in pursuing further these discoveries of theirs. By applying to these new rays the methods developed as described for the cathode rays from discharge tubes, it was shown that the new rays are partly cathode rays (34; 36; 41)*, and amazingly cathode rays of almost or entirely the speed of light (43). What no discharge tube could withstand is thus achieved by the radium atom – and quite spontaneously – although admittedly not without being completely broken down in the process (51; 54).

Once the entire range of velocities from rest to the speed of light was thus available it was worthwhile re-examining in more detail the behaviour of matter to irradiation.

From the turbidity of all substances, including e.g. air, to cathode rays, we had concluded that each molecule or atom acts as a separate obstacle to the rays, an obstacle which deflects them from their path to a greater or lesser

Nevertheless, the meteorological significance of the action of ultraviolet sunlight in the upper layers of the atmosphere (35, p. 504) still does not appear to have been sufficiently appreciated. Whether the ionization by cathode rays first found at the aluminium window (18) is also an effect of the light which occurs there, or else the direct effect of the cathode rays, is still obscure.

* This is the part of the uranium or radium radiation normally designated as β.

extent. How should we visualize this deflection? Let us first examine whether perhaps the quanta of the rays are reflected from the molecules of the substance in the same way as the molecules of a gas are reflected on one another when they collide. Were that so a cathode ray in the gas would be restricted to that length which can readily and accurately be calculated as the *mean free path* of very small particles between the gas molecules from the data of the kinetic gas theory. These path lengths are, however, very small; in hydrogen at 40 mm pressure, for example, about two hundredths of a millimeter. Beyond this short length, a ray would not be able to develop at all in this gas, that is to say almost instantaneous diffusion would ensue. However, gases are by no means so turbid as my observations published in diagrammatic form had earlier shown (18*b*). Even the air at full atmospheric pressure proved much clearer, as we have already seen (Fig. 4), and the lighter hydrogen rarified to the specified pressure of 40 mm is much clearer still; Fig. 11 illustrates the path of the ray in hydrogen as observed on the fluorescent

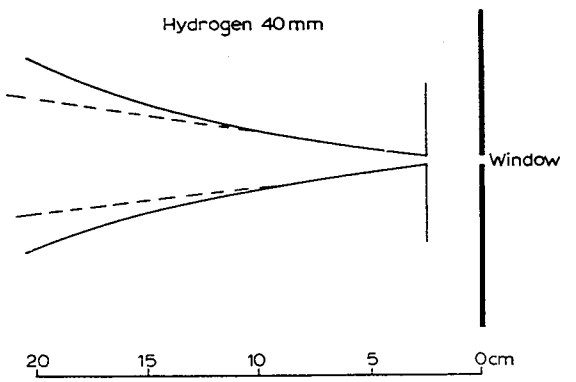


Fig. 11.

screen. The broken lines show the extent to which rectilinear light would propagate under the same conditions. It will be seen that over a length of 10 cm the cathode ray still scarcely deviates from this rectilinear propagation, and it becomes distinctly broader only beyond that length. The length of 10 cm is, however, 5,000-fold that of the free path length of 0.02 mm. It thus follows that here the radiant quanta must have traversed 5,000 hydrogen molecules before undergoing the first noticeable change of direction. We are amazed to see that we have transcended the old impermeability of matter. Each atom in the substance occupies a space which is impermeable to the

other atoms*; but vis-à-vis the fine quanta of electricity all types of atoms are highly permeable structures as if built up of fine constituents with a great many interstices.

What are these fine constituents of atoms? That in all atoms they are the same, only present in varying numbers, we have already concluded from the law of proportionality between mass and absorption. We can now learn further details. We can use the quanta of the cathode rays as small test particles which we allow to traverse the interior of the atoms and thus provide us with information thereon.

The first and most noticeable thing to happen to them during this traverse, i.e. deflection from the rectilinear path, we have just discussed as *diffusion of the rays*. As far as we know, cathode rays experience such a deflection owing only to electrical and magnetic forces. To assume magnetic forces within the atoms would imply the assumption of mobile electricity in the atoms, thus again electrical forces. We must therefore regard the diffusion of cathode rays in matter as proof for the *existence of electrical forces in the interior of the atoms*. The magnitude of these forces can be estimated by considering the extent of the deflection together with the transit time, which latter depends on the velocity of the quanta and, of course, on the size of the atoms. If we take progressively slower rays, the transit times will become longer and correspondingly the diffusions occurring will be stronger (19, p. 30; 46, p. 480). In this way we find for the interior of the atoms electrical field intensities of unusual magnitude such that we can never produce by any means known to us owing to lack of sufficient resistance in even the best insulators: field intensities compared with which those occurring during the most violent storms are insignificantly small (47). The force effects of the radium atom then cease to seem so surprising but we should be more amazed that most of the atoms around us behave so placidly, only revealing something of the force stored within them when subjected to the photoelectric action or through other similar causes.

The further quantitative study of cathode-ray diffusion in the various materials promises to yield valuable information on the precise nature of the electrical fields of atoms. For the present we must turn to a second phenom-

* In any case at the normal velocities of the molecules. For very high velocities such as occur with the α-particles of radium, mutual penetrability of even whole atoms, although probably accompanied by their destruction, would not be impossible in the light of the concepts which we have arrived at regarding the constitution of atoms. Recent studies by Bragg and Kleemann promise to throw light on this subject.

enon, somewhat more easy to determine numerically, which is apt to occur in the course of such atom traverses. It can easily happen that the quantum, after successfully passing thousands of atoms, finally stops in an atom and does not emerge at once. This is the *absorption of cathode rays*. I have determined this effect quantitatively for the entire scale of available ray velocities and found the following (47).

The absorption increases with decreasing ray velocity, in common with the diffusion. This is also to be expected if the absorption, like the diffusion, is an effect of the electrical fields of force within the atoms and if these fields of force concentrate about certain centres in the atoms in the vicinity of which their intensity is greater than at longer range, in the same way as the intensity of a magnetic field of force concentrates about the two poles. A radiation quantum which traverses such fields with mobile centres is only stopped when it enters sufficiently intense parts of these fields along its particular path; otherwise it will pass through and be deflected to a greater or lesser extent. The entire cross-section of the atom, the area which the atom presents to a ray, thus consists of two parts, an absorbing and a transmitting part, and the former – which I refer to briefly as the absorbing cross-section – is known in square centimetres from my measurements. It affords a measure for the size of those parts of the force fields of the atom, the intensity of which is greater than the relevant level which is just sufficient to arrest the particular velocity of the quantum. The slower the quantum, the larger the parts of the atom's force field which act as absorbing cross-sections. For the slowest rays I have found that the absorbing cross-section not only becomes equal to the entire cross-section of the atom or molecule – which entire cross-section was known from the kinetic theory of gases – but even slightly larger. This is tantamount to direct proof for the existence of electrical fields of forces both within the atoms and molecules and also for a certain distance around them. It is probably correct to identify these external electrical forces of the molecules with the forces of strength, elasticity, cohesion and adhesion, in short with the *molecular forces* in general which have long been known although not immediately regarded as of an electrical nature. Berzelius' view that the *chemical forces* of atoms are of an electrical nature will now be held with all the more reason and as the research in progress continues it is to be hoped that concepts of the electrical force fields of the atoms will emerge which give a better and more complete picture of their chemical behaviour than the simple concept of a number of fixed valency positions equipped with electrical charges.

Of equal interest to the transition to the lowest velocities was that to the highest. As the ray velocity increases, the absorbing cross-section contracts; ultimately only those quanta are stopped whose path lies through the highest intensity parts of the internal force fields close to their centres. For that very reason the fastest rays are also capable of supplying the answer to the question whether perhaps these centres have a special, impenetrable proper volume, or in more general terms: whether apart from the force fields there is something else in the atoms which holds back our test particles. What happens when the test is carried out with the fastest rays can best be illustrated by means of an example. Let us imagine a cubic metre block of the most solid and heavy substance known to us, say, platinum. In this block we find altogether not more impenetrable proper volume than at most one cubic millimetre. Apart from this pinhead-sized portion, we find the remainder of our block as empty as the sky. We ought to be astounded at the insignificant degree to which the space in matter is actually filled! What we have found in the space occupied by matter have only been fields of force such as can also form in the free ether. What are then the basic constituents of all atoms to which we have been led by the mass dependence of cathode-ray absorption? Clearly they too are in the main only fields of force in common with the whole atoms. I have therefore termed these basic constituents of all matter «dynamids».

As constituents of electrically neutral atoms the dynamids will also be regarded as electrically neutral, and hence possess the same amounts of negative and positive electricity as the centres of their fields. We may then state: matter – all the tangible, ponderable substances around us – consists ultimately of equal quantities of negative and positive electricity. The previously mentioned findings derived from the Zeeman phenomenon, the photo-electric effect and the secondary cathode radiation, which will presently be discussed, show that the negative electricity is contained in the atoms as precisely the same quanta which we found in the cathode rays, and which the research worker has since variously encountered in their own right, separate from matter. The positive electricity, on the other hand, appears to be something much more specifically proper to the atoms of matter; as has been previously stressed they have not been found with certainty other than in atoms. From our findings on the packing of space it follows that for negative quanta the proper volume, impenetrable for things of the same kind, must then be extraordinarily small. This is consistent with the previously mentioned experiments by Kaufmann. The probable proper volume of the

positive electricity, provided it too were not extremely small, should be regarded as completely penetrable for negative quanta.

With this constitution of matter, the third phenomenon occurring during atom traverses, and one which has still to be referred to, can readily be understood. Owing to the repelling force which it exerts on the other negative quanta proper to the atom the traversing ray quantum will be capable of setting up a tremendous disturbance within the atom and as a result of this disturbance a quantum belonging to the atom can be flung out.*
The process is termed *secondary cathode radiation.* We have allowed one cathode ray – the primary – to penetrate into the atom, against which *two* emerge, the primary and the secondary** (46, p.481).

The *velocity* of the secondary rays – in common with that of the photo-electrically generated cathode rays – is very low, even when that of the primary rays is high. The *amount* of the secondary radiation, i.e. the probability of quanta emission from atoms during traverse, is largest at a given optimum of the primary velocity; both faster and slower primary rays are less effective and at quite a low primary velocity – below 1/200th of the speed of light – the secondary radiation is absent altogether (44, p.188 *et seq.*; 46, p.474 *et seq.*; 49). This is quite understandable for if the primary quantum approaches too slowly, it has too little energy to cause adequate disturbance of the interior of the atom, and if it approaches too quickly it will generally remain for too short a time in the atom to have that effect.

At its low velocity the secondary radiation must succumb to strong absorption in the surrounding molecules of the material. In gases where the molecules are free, a molecule which has absorbed a secondary quantum will act as a *mobile carrier of negative electricity*, while the molecule from which the secondary quantum has escaped has an excess of positive electricity and is thus a *positive* electricity carrier. The migrations of such carriers, however – for which knowledge we are indebted to the unremitting efforts of Arrhenius and after him to J.J.Thomson in particular – constitute the electrical conductivity in gases*** and in the secondary cathode radiation we have thus

* Two and more quanta can also escape from the atom (46, p.485).

** It seems often to be assumed that the secondary radiation is an exclusive result of the absorption of the primary radiation; but this is not the conception which I have formed from observation (46, p.474 *et seq.*).

*** Three consecutive steps seem to me to substantiate the belief that molecules or groups thereof carry the electrical conductivity in gases, notably: (*i*) the thorough study of one of the first cases of gas electrification in which dust – which at first was regarded

found and, as I believe, adequately ascertained by thorough observations, the mechanism whereby cathode rays cause a gas to become electrically conductive (46, p. 474).* This case of conductivity induced and maintained by cathode rays must also be a factor in all gas discharges where sufficiently fast cathode rays occur; hence also in the normal discharge tube which we took at the outset as our first generator of cathode rays.

In other cases as well where gases become electrically conductive the mechanism appears to be the same: escape of quanta from molecules, and reabsorption by other molecules; this occurs under the influence of ultraviolet light on gases, as aforementioned, and also in flames (45) but the cause underlying the escape of quanta is apt to vary from case to case (53, p. 242).

In conclusion, I wish to thank you for your attention.

as the sole or at any rate the main carrier of electrical discharges in gases – was found definitely not to be involved. This was the case of waterfall electricity (16); (*ii*) the first measurements of the migration speeds of gas carriers in various cases of conducting gases, performed in rapid sequence by a number of different observers, first of all by Rutherford (26); (*iii*) J. J. Thomson's essentially irreproachable first measurements of the absolute electrical charge of the individual gas carriers in different cases (31). — In the light of simple considerations of gas kinetics the size of the gas carriers was calculated from (*ii*) and (*iii*) as equal to that of molecules or groups of molecules.

* This mechanism differs from that of ion formation in liquid electrolytes where it consists in the splitting of electrically neutral molecules into two oppositely charged atoms or groups of atoms. In other respects, too, the analogy between the conduction of electricity in gases and in liquids breaks down precisely in the most characteristic points. Magnitude and sign of the charge on a gas carrier are – quite unlike the case with the ions in liquid electrolytes – not determined by the chemical nature of the carrier, and electrolysis proper – that so typically chemical mode of decomposition – hence does not occur at all in gases (53, p. 236). I therefore considered it better to emphasize this analogy no longer, notwithstanding its heuristic usefulness at the outset, and for that reason I have always avoided applying the name ions to the gas carriers, and calling the conduction of electricity in gases electrolytic.

*Chronological list of publications**

(1860) 1. M. Faraday, *Experimental Researches in Electricity* (from 1820 onwards).
(1869) 2. W. Hittorf, «Über die Elektrizitätsleitung der Gase» (The electrical conductivity of gases), *Pogg. Ann. Physik*, Vol. 136.
(1873) 3. J. C. Maxwell, *A Treatise on Electricity and Magnetism.*
(1876) 4. E. Goldstein, «Über elektrische Entladungen in verdünnten Gasen» (Electrical discharges in rarified gases), *Monatsber. Berl. Akad.* Also continued in the volumes of the same journal for 1880 and 1881.
(1879) 5. W. Crookes, *Strahlende Materie oder der vierte Aggregatzustand* (Radiating matter or the fourth state of aggregation). (German translation published in Leipzig.) Also *Phil. Trans. Roy. Soc.*, Vol. 170.
(1881) 6. H. Helmholtz, «On the modern development of Faraday's conceptions of electricity», *J. Chem. Soc.*, Vol. 39; *Wiss. Abhandl.*, Vol. 3, p. 52.
(1883) 7. H. Hertz, «Versuche über die Glimmentladung» (Experiments on the glow discharge). (*a*) *Wied. Ann. Physik*, Vol. 19. Also (*b*) *Ges. Werke* (Collected works), Vol. 1, p. 242.
(1887) 8. H. Hertz, «Über einen Einfluss des ultravioletten Lichtes auf die elektrische Entladung» (An effect of ultraviolet light on electrical discharge), *Sitz.-ber. Berl. Akad.*, 9th June. In more detail in *Wied. Ann. Physik*, Vol. 31.
(1887–1888) 9. H. Hertz, *Untersuchungen über die Ausbreitung der elektrischen Kraft* (Researches on the propagation of electrical force).
(1888) 10. O. Heaviside, «On electromagnetic waves», *Phil. Mag.*, Ser. 5, Vols. 25 and 26. Further: «On the electromagnetic effects due to the motion of electrification through a dielectric», *Phil. Mag.*, Ser. 5, Vol. 27 (1889); «A charge suddenly jerked into motion», «Sudden stoppage of charge», *The Electrician*, May 1, 1891. Also *Electrical Papers*, Vol. 2, pp. 375, 504, and *Electromagnetic Theory*, Vol. 1, p. 54.
– 11. W. Hallwachs, «Über den Einfluss des Lichtes auf elektrostatisch geladene Körper» (The effect of light on electrostatically charged substances), *Wied. Ann. Physik*, Vol. 33.
(1889) 12. P. Lenard and M. Wolf, «Zerstäuben der Körper durch das ultraviolette Licht» (Pulverization of substances under the action of ultraviolet light), *Wied. Ann. Physik*, Vol. 37.
(1890) 13. A. Schuster, «The discharge of electricity through gases», *Proc. Roy. Soc.*, Vol. 47. (Received 20th March.)
– 14. A Righi, «Sulle traiettorie percorse nella convezione foto-electrica, ...» (The trajectories described in photoelectric convection), *Atti Accad. Naz. Lincei*, Vol. 6 [2]. (Received 3rd August.)
(1892) 15. H. Hertz, «Über den Durchgang von Kathodenstrahlen durch dünne Me-

* The position of a publication in this sequence has been determined by its date of submission to a learned society or by its date of receipt by a journal editor. Where no such dates were available, the date of publication, as far as can be determined, has been taken.

tallschichten» (The passage of cathode rays through thin metal layers), *Wied. Ann. Phys*, Vol. 45. Also *Ges. Werke* (Collected works), Vol. 1, p. 355.
– 16. P. Lenard, «Über die Elektricität der Wasserfälle» (Waterfall electricity), *Wied. Ann. Physik*, Vol. 46.
– 17. H. Helmholtz, «Elektromagnetische Theorie der Farbenzerstreuung» (Electromagnetic theory of colour dispersion). (*a*) *Sitz.-ber. Berl. Akad.*, 15th December – (*b*) *Wied. Ann. Physik*, Vol. 48. – (*c*) *Wiss. Abhandl.*, Vol. 3, p. 505.
(1894) 18. P. Lenard, «Über Kathodenstrahlen in Gasen von atmosphärischem Druck und im äussersten Vakuum» (Cathode rays in gases at atmospheric pressure and in the highest vacuum). (*a*) *Sitz.-ber. Berl. Akad.* for 12th January 1893.–(*b*) In more detail, *Wied. Ann. Physik*, Vol. 51.
– 19. P. Lenard, «Über die magnetische Ablenkung der Kathodenstrahlen» (The magnetic deflection of cathode rays), *Wied. Ann. Physik*, Vol. 52.
– 20. H. Hertz, *Die Principien der Mechanik in neuem Zusammenhange dargestellt* (The principles of mechanics displayed in a new context.)
(1895) 21. P. Lenard, «Über die Absorption der Kathodenstrahlen» (The absorption of cathode rays), *Wied. Ann. Physik*, Vol. 56. (Published in October issue.)
– 22. W. C. Röntgen, «Über eine neue Art von Strahlen» (A new type of ray), *Sitz.-ber. Würzburger Phys. Med. Ges.*, for December. Reprinted in *Wied. Ann. Physik*, Vol. 64.
(1896) 23. A. Righi, «Sulla propagazione dell'elettricità nei gas attraversati dai raggi di Röntgen» (The propagation of electricity in gases traversed by X-rays), *Mem. Accad. Sci. Bologna*, Ser. 5, Vol. 6 (Submitted 31st May.)
– 24. P. Zeeman, «On the influence of magnetism on the nature of the light emitted by a substance», *Comm. Phys. Lab. Leiden*, transl. from: *Verslag. Koninkl. Ned. Akad. Wetenschap.*, 31st October.
(1897) 25. J. J. Thomson, «Cathode rays», *Phil. Mag.*, Ser. 5, Vol. 44. (Published in October issue.)
– 26. E. Rutherford, «The velocity and rate of recombination of the ions of gases exposed to Röntgen radiation», *Phil. Mag.*, Ser. 5, Vol. 44. (Published in November issue.)
– 27. P. Lenard, «Über die elektrische Wirkung der Kathodenstrahlen auf atmosphärische Luft» (The electrical effect of cathode rays on the air of the atmosphere), *Wied. Ann. Physik*, Vol. 63. (Published 11th December.)
(1898) 28. P. Lenard, «Über die elektrostatischen Eigenschaften der Kathodenstrahlen» (The electrostatic properties of cathode rays), *Wied. Ann.*, Vol. 64. (Received 2nd January.)
– 29. E. Wiechert, «Experimentelle Untersuchungen über die Geschwindigkeit und die magnetische Ablenkbarkeit der Kathodenstrahlen» (Experimental studies on the velocity and magnetic deflectability of cathode rays), *Nachr. Kgl. Ges. Wiss. Göttingen* (Submitted on 19th March.)
– 30. P. Lenard, «Über das Verhalten von Kathodenstrahlen parallel zu elektrischer Kraft» (The behaviour of cathode rays parallel to electrical force), *Wied. Ann. Physik*, Vol. 65. (Received 1st May.) Also published in *Math. Naturw. Ber. Ungarn*, Vol. 16.
– 31. J. J. Thomson, «On the charge of electricity carried by the ions produced by

Röntgen rays», *Phil. Mag.*, Ser. 5, Vol. 46. (Published in December issue.) Continued and subsequently corrected in *Phil. Mag.*, December issue 1899, and March issue 1903.

(1899) 32. P. Lenard, «Erzeugung von Kathodenstrahlen durch ultraviolettes Licht» (Production of cathode rays by ultraviolet light), *Sitz.-ber. Kaiserl. Akad. Wiss. Wien* for 19th October. Reprinted in *Ann. Physik*, Vol. 2.

(1900) 33. V. Bjerknes, *Vorlesungen über hydrodynamische Fernkräfte nach C. A. Bjerknes' Theorie* (Lectures on the hydrodynamic distant forces in accordance with C. A. Bjerknes' theory).

– 34. H. Becquerel, «Contribution à l'étude du rayonnement du radium» (Contribution to the study of the radiation from radium), and «Déviation du rayonnement du radium dans un champ électrique» (Deflection of radium radiation in an electrical field), *Compt. Rend. (Paris)* for 29th January and 26th March, respectively.

– 35. P. Lenard, «Über Wirkungen des ultravioletten Lichtes auf gasförmige Körper» (Effects of ultraviolet light on gaseous substances), *Ann. Physik*, Vol. 1. (Received 6th February.)

– 36. P. and M. Curie, «Sur la charge électrique des rayons déviables du radium» (The electrical charge of deflectable radium rays), *Compt. Rend. (Paris)* for 5th March.

– 37. P. Curie and G. Sagnac, «Électrisation négative des rayons secondaires produits au moyen des rayons Röntgen» (Negative electrification of secondary rays produced by means of X-rays), *Compt. Rend. (Paris)* 130, for 9th April.

– 38. E. Demarcay, «Sur le spectre du radium» (The radium spectrum), *Compt. Rend. (Paris)* 131, for 25th July.

– 39. M. Curie, «Sur le poids atomique du baryum radifère» (The atomic weight of radiferous barium), *Compt. Rend. (Paris)* 131, for 6th August. Also «Sur le poids atomique du radium» (The atomic weight of radium), same journal 21st July, 1902.

– 40. P. Lenard, «Über die Elektrizitätszerstreuung in ultraviolet durchstrahler Luft» (Scattering of electricity in air irradiated with ultraviolet light), *Ann. Physik*, Vol. 3. (Received 17th August.)

– 41. E. Dorn, «Elektrostatische Ablenkung der Radiumstrahlen» (Electrostatic deflection of radium rays), *Abhandl. Naturforsch. Ges. Halle*, Vol. 22.

– 42. E. Dorn, «Versuche über Sekundärstrahlen» (Experiments on secondary rays), *Arch. Néerl. Sci. (Haarlem)*, Ser. 2, Vol. 5.

(1901) 43. W. Kaufmann, «Die magnetische und elektrische Ablenkbarkeit der Becquerelstrahlen und die scheinbare Masse der Elektronen» (The magnetic and electrical deflectability of Becquerel rays and the apparent mass of electrons), *Nachr. Kgl. Ges. Wiss. Göttingen*, Vol. 8, for 8th November.

(1902) 44. P. Lenard, «Über die lichtelektrische Wirkung» (The photoelectric effect), *Ann. Physik*, Vol. 8. (Received 17th March.)

– 45. P. Lenard, «Über die Elektrizitätsleitung in Flammen» (Electrical conductivity in flames), *Ann. Physik*, Vol. 9. (Received 18th August.)

(1903) 46. P. Lenard, «Über die Beobachtung langsamer Kathodenstrahlen mit Hilfe der Phosphoreszenz und über Sekundärentstehung von Kathodenstrahlen» (The observation of slow cathode rays by means of phosphorescence, and the secondary occurrence of cathode rays), *Ann. Physik*, Vol. 12. (Received 28th June.)

– 47. P. Lenard, «Über die Absorption von Kathodenstrahlen verschiedener Geschwindigkeit» (The absorption of cathode rays of various velocities), *Ann. Physik*, Vol. 12. (Received 30th July.)

– 48. E. Warburg, «Über die Ozonisierung des Sauerstoffs durch stille elektrische Entladungen» (The ozonization of oxygen by silent electrical discharges), *Sitz.-ber. Berl. Akad.* for 11th November.

(1904) 49. P. Lenard, «Über sekundäre Kathodenstrahlung in gasförmigen und festen Körpern» (Secondary cathode radiation in gaseous and solid substances), *Ann. Physik*, Vol. 15. (Received 22nd August.)

– 50. P. Lenard and V. Klatt, «Über die Erdalkaliphosphore» (The alkaline-earth phosphors), *Ann. Physik*, Vol. 15. (Received 22nd August.) Extracts also published in the *Mat. Naturw. Ber. Ungarn*, Vol. 22.

– 51. E. Rutherford, «Slow transformation products of radium», *Phil. Mag.*, Ser. 6, Vol. 8. (Published in November issue.)

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– 53. P. Lenard, «Über die Lichtemissionen der Alkalimetalldämpfe und Salze und über die Zentren dieser Emissionen» (The photo-emissions of alkaline metal vapours and salts, and the centres of these emissions), *Ann. Physik*, Vol. 17. (Received 13th April.)

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It is not without interest to see from this list how, during the years 1887–1894, the subject under discussion has suddenly, as it were, become the field of more abundant and more successful activity. The years prior to that period, with the exception of the fundamental work by Faraday and Maxwell, are marked by only sporadic and isolated symptoms of that activity, the years thereafter and down to the present by its increasingly fruitful pursuit.

Biography

Philipp von Lenard was born at Pozsony (Pressburg) in Hungary on June 7, 1862. His family had originally come from the Tyrol. He studied physics successively at Budapest, Vienna, Berlin and Heidelberg under Bunsen, Helmholtz, Königsberger and Quincke and in 1886 took his Ph.D. at Heidelberg.

From 1892 he worked as a Privatdozent and assistant to Professor Hertz at the University of Bonn and in 1894 was appointed Professor Extraordinary at the University of Breslau. In 1895 he became Professor of Physics at Aix-la-Chapelle and in 1896 Professor of Theoretical Physics at the University of Heidelberg. In 1898 he was appointed Professor Ordinarius at the University of Kiel.

Lenard's first work was done in the field of mechanics, when he published a paper on the oscillation of precipitated water drops and allied problems and in 1894 he published the *Principles of Mechanics* left behind by Hertz.

Soon he became interested in the phenomena of phosphorescence and luminescence. This was a development of the mysterious attraction which weak light appearing in darkness had had for him since his boyhood, when he had, with his schoolfellows, warmed fluorine crystals to make them luminescent; and now he took up, with the astronomer W. Wolf, the study of the luminosity of pyrogallic acid when it is mixed with alkali and bisulphite for developing photographs. He found that its luminosity depended on the oxidation of the pyrogallic acid. At this time he also carried out studies of magnetism with bismuth and, in collaboration with V. Klatt, who had been his first teacher of physics in his native town, he studied, at the Modern College at Pressburg, the so-called self-luminous substances such as calcium sulphide on which Klatt had been working for some years. Together they found that calcium sulphide, after previous illumination, exerts light in the dark, but only if it contains at least some traces of heavy metals, such as copper and bismuth, which form crystals on which the colour and the intensity and durations of the luminosity depend; if it is quite pure, it is not luminous. This work with Klatt was the beginning of work in a field which occupied Lenard for the next 18 years.

In 1888, when he was working at Heidelberg under Quincke, Lenard had done his first work with cathode rays. He investigated the view then held by Hertz that these rays were analogous to ultraviolet light and he did an experiment to find out whether cathode rays would, like ultraviolet light, pass through a quartz window in the wall of a discharge tube. He found that they would not do this; but later, in 1892, when he was working as an assistant to Hertz at the University of Bonn, Hertz called him to see the discovery he had made that a piece of uranium glass covered with aluminium foil and put inside the discharge tube became luminous beneath the aluminium foil when the cathode rays struck it. Hertz then suggested that it would be possible to separate, by means of a thin plate of aluminium, two spaces, one in which the cathode rays were produced in the ordinary way and the other in which one could observe them in a pure state, which had never been done. Hertz was too busy to do this and gave Lenard permission to do it and it was then that he made the great discovery of the «Lenard window».

After many experiments with aluminium foil of various thicknesses he was able to publish, in 1894, his great discovery that the plate of quartz that had, until then, been used to close the discharge tube, could be replaced by a thin plate of aluminium foil just thick enough to maintain the vacuum inside the tube, but yet thin enough to allow the cathode rays to pass out. It thus became possible to study the cathode rays, and also the fluorescence they caused, outside the discharge tube and Lenard concluded from the experiments that he then did that the cathode rays were propagated through the air for distances of the order of a decimetre and that they travel in a vacuum for several metres without being weakened. Although Lenard at first followed Hertz in believing that the cathode rays were propagated in the ether, he later abandoned this view as a result of the work of Jean Perrin in 1895, Sir J. J. Thomson in 1897 and W. Wien in 1897, which proved the corpuscular nature of the cathode rays.

Later Lenard extended the work of Hertz on the photoelectric effect. Working in a high vacuum, he analysed the nature of this effect, showing that when ultraviolet light falls on a metal it takes from the metal electrons which are then propagated in the vacuum, in which they can be accelerated or retarded by an electric field, or their paths can be curved by a magnetic field. By exact measurements he showed that the number of electrons projected is proportional to the energy carried by the incident light, whilst their speed, that is to say, their kinetic energy, is quite independent of this number and varies only with the wavelength and increases when this diminishes.

These facts conflicted with current theory and were not explained until 1905, when Einstein produced his quantitative law and developed the theory of quanta of light or photons, which was verified much later by Millikan. But Lenard never forgave Einstein for discovering and attaching his own name to this law.

In the course of his work Lenard had, for the purpose of accelerating the speed of the electrons and measuring their energy, invented a photoelectric cell which was the first model of the «3-electrode lamp» which is so important today in radioelectric technique. The only difference between these two cells was that in Lenard's cell the electrons were taken from the cathode by light, whereas on the «3-electrode lamp» the cathode is a white-hot filament capable of sending into the vacuum currents of much higher intensity.

In 1902 Lenard showed that an electron must have a certain minimum energy before it could produce ionisation when it passed through a gas.

In 1903 he published his conception of the atom as an assemblage of what he called «dynamides», which were very small and were separated by wide spaces; they had mass and were imagined as electric dipoles connected by two equal charges of contrary sign and their number was equal to the atomic mass. The solid matter in the atom was, he thought, about one thousand millionth of the whole atom. This work contributed much to Lorentz' theory of electrons.

In his later years Lenard studied the nature and origin of the lines of the spectrum. Developing the work of Rydberg, Kayser and Runge, who had shown that the lines of the spectrum of a metal can be arranged in two or more different series and that there is a marked mathematical relationship between the wavelengths of these series, Lenard showed that in each series a definite modification of the atom has occurred and that these modifications determine the series and are differentiated by the number of electrons lost.

Lenard was an experimentalist of genius, but more doubtful as a theorist. Some of his discoveries were great ones and others were very important, but he claimed for them more than their true value. Although he was given many honours (for instance, he received Honorary Doctorates of the Universities of Christiania, now Oslo, in 1911, Dresden in 1922 and Pressburg in 1942, the Franklin Medal in 1905, the Eagle Shield of the German Reich in 1933, and was elected Freeman of Heidelberg in the same year), he believed that he was disregarded and this probably explains why he attacked other physicists in many countries. He became a convinced member of Hitler's National Socialist Party and maintained unreserved adherence to it. The party responded

by making him the Chief of Aryan or German Physics. Among his publications are several books: *Ueber Aether und Materie* (second edition 1911), *Quantitatives über Kathodenstrahlen* (1918), *Ueber das Relativitätsprinzip* (1918) and *Grosse Naturforscher* (second edition 1930).

Von Lenard, who was married to Katharina Schlehner, died on May 20, 1947 at Messehausen.

Physics 1906

JOSEPH JOHN THOMSON

« in recognition of the great merits of his theoretical and experimental investigations on the conduction of electricity by gases »

Physics 1906

Presentation Speech by Professor J. P. Klason, President of the Royal Swedish Academy of Sciences

Your Majesty, Your Royal Highnesses, Ladies and Gentlemen.

Every day that passes witnesses electricity obtaining an ever-increasing importance in practical life. The conceptions, which a few decades ago were the subject of investigation in the quiet studies or laboratories of sundry learned men, have by this time become the property of the public at large, who will soon be as familiar with them as with their ordinary weights and measures. Still greater however are the revolutions brought about by electricians' labours in the sphere of science. Immediately after Örsted's epoch-making discovery of the influence of the electric current on a magnetic needle (1820), Ampère, the ingenious French investigator, promulgated a theory explaining magnetic phenomena as results of electrical agencies. The investigations of Maxwell, the brilliantly gifted Scotch physicist (1873), were still more far-reaching in their effect, for by them the phenomenon of light was proved to be dependent upon electromagnetic undulatory movements in the ether. There is reason to believe that the grand discoveries of the last few years respecting the discharge of electricity through gases will prove to be of equally great, or perhaps still greater, importance, throwing as they do a great deal of light upon our conception of matter. In this domain Professor J. J. Thomson of Cambridge, this year's Prize-winner in Physics, has made most valuable contributions through his investigations and researches, which he has assiduously pursued for many years past.

By Faraday's great discovery in the year 1834 it had been shown that every atom carries an electric charge as large as that of the atom of hydrogen gas, or else a simple multiple of it corresponding to the chemical valency of the atom. It was, then, natural to speak, with the immortal Helmholtz, of an elementary charge or, as it is also called, an atom of electricity, as the quantity of electricity inherent in an atom of hydrogen gas in its chemical combinations.

Faraday's law may be expressed thus, that a gram of hydrogen, or a quantity equivalent thereto of some other chemical element, carries an electric charge of $28{,}950 \times 10^{10}$ electrostatic units. Now if we only knew how

many hydrogen atoms there are in a gram, we could calculate how large a charge there is in every hydrogen atom. The kinetic gas-theory, a field of investigation as popular as any among the scientists of the century recently ended, is based upon the assumption that the gases consist of freely moving molecules, the impact of which on the walls of the encompassing vessel is recognizable as the pressure of the gas. From this the velocity of the gas molecules could be calculated with great accuracy. From the velocity with which one gas diffuses in another, and from other closely allied phenomena, it was further possible to calculate the volume of space occupied by the molecules, and by that means the investigator was enabled to form an idea of the mass of the molecules and consequently of the number of molecules to be found in one gram of a chemical substance, such as, e.g. hydrogen. The figures thus obtained could not however lay claim to any great amount of accuracy and were regarded by many scientists as purely conjectural. If it had been possible to calculate the number of molecules in a drop of water by the aid of an exceedingly powerful microscope, the case would of course have been quite otherwise. But there was not the remotest hope of the investigator ever being successful in doing that, and thus the existence of the molecules was regarded as very problematical. If from the figures quoted by the champions of the kinetic gas-theory as the most probable ones for the sizes of molecules and atoms we calculate how large a quantity of electricity is carried by one hydrogen atom, we arrive at the conclusion that the atom charge lies between 1.3×10^{-10} and 6.1×10^{-10} electrostatic units.

What no one regarded as probable has however been achieved by J.J. Thomson by devious methods. Richard von Helmholtz found out, as long ago as 1887, that electrically charged small particles possess the remarkable property of condensing steam around them. J.J. Thomson and his pupil C. T.R. Wilson took up the study of this phenomenon. By the aid of Röntgen rays they procured some electrically charged small particles in air. Thomson assumes that each of those particles carries an electrical unit charge. By electrical measurements he was able to determine how great the electric charge was in a given quantity of air. Then, by means of a sudden expansion of the air, which was saturated with steam, he effected a condensation of the steam on the electrically charged small particles, the size of which he could calculate from the velocity with which they sank. Now as he knew the amount of water condensed and the size of each drop, it was not difficult to calculate the number of drops. That number was the same as that of the electrically charged small particles. Having before determined the total quan-

tity of electricity in the vessel, he could easily reckon out what quantity there was in each drop or, previously, in every small particle, that is to say the atomic charge. That was thus found to be 3.4×10^{-10} electrostatic units. This value is very close to the mean of the values previously obtained by the kinetic gas-theory, rendering the correctness of these different measurements and the accuracy of the reasoning employed in their determination in a very high degree probable.

Now, even if Thomson has not actually beheld the atoms, he has nevertheless achieved work commensurable therewith, by having directly observed the quantity of electricity carried by each atom. By the aid of this observation the number has been determined of the molecules in a cubic centimetre of gas at a temperature of zero and under the pressure of one atmosphere; that is to say, there has been thereby calculated what is perhaps the most fundamental natural constant in the material world. That number amounts to not less than forty trillions (40×10^{18}). By means of a series of exceedingly ingenious experiments, Professor Thomson, aided by his numerous pupils, has determined the most important properties (such as mass and velocity under the influence of a given force), of these electrically charged small particles, which are produced in gases by various methods, e.g. by Röntgen rays, Becquerel rays, ultraviolet light, needle-point discharge and incandescent metals. The most remarkable of these electrically charged small particles are those constituting the cathode rays in highly rarefied gases. These small particles are called electrons and have been made the object of very thorough-going researches on the part of a large number of investigators, foremost of whom are Lenard, last year's Nobel Prize winner in Physics, and J. J. Thomson. These small particles are to be met with also in the so-called β-rays, emitted by certain radioactive substances. Assuming, on the basis of Thomson's above-mentioned work, that they carry the negative unit charge, we are led to the result that they possess about a thousand times less mass than the least atoms hitherto known, viz. the atoms of hydrogen gas.

On the other hand, the least *positively* charged small particles we know are, according to Thomson's, Wien's and other investigators' calculations, of the same order in mass as ordinary atoms. Now, seeing that all substances yet examined are capable of giving off negatively charged electrons, Thomson was led by these circumstances to assume that the negative charge in the electrons has a real existence, whereas the charge of the positive small particles arises from a neutral atom losing one or more negative electrons with

their charges. Thomson has herewith given an actual physical import to the view put forward in 1747 by Benjamin Franklin that there is only one kind of electricity, a view eagerly championed too by Edlund. The actually existing electricity is negative electricity, according to Thomson.

As early as 1892 Thomson had shown that a charged body moving forward is thereby in possession of an electromagnetic energy, which produces the effect of the mass of the body being increased. From experiments carried out by Kaufmann regarding the velocity of β-rays from radium, Thomson concluded that the negative electrons do not possess any real, but only an apparent, mass due to their electric charge.

It might now be considered reasonable to assume that all matter is built up of negative electrons, and that consequently mass in matter was apparent and really depended on the effect of electric forces. An experiment of very great interest has moreover been made in this direction by Thomson, but his investigations of most recent date in the present year (1906) seem to intimate that only about a thousandth part of the material is apparent and due to electric forces.

Professor Thomson. As you are aware, the Royal Swedish Academy of Sciences has decided to award you the Nobel Prize in Physics for this year.

I am at a loss to explain how it is, but somehow or another the contemplation of the work you have achieved has revived in my mind a passage in the famous essay on Socrates by Xenophon, a work which you too no doubt perused in your youth. The author tells us that every time conversation turned upon the elements of the Earth, Socrates would say « of these matters we know nothing ». Will the sagacity which Socrates displayed in this answer and which has been approved by all ages up to and including our own, continue to be acknowledged as the conclusion of the whole matter? Who shall say? One thing we all know, and that is, that every great period of Natural Philosophy has evolved elements of its own, and furthermore we seem to feel as though we might be at the threshold of a new such period with new elements.

In the name and on behalf of our Academy I congratulate you upon having bestowed upon the world some of the main works which are enabling the natural philosopher of our time to take up new enquiries in new directions. You have thus been worthily treading in the footsteps of your great and renowned compatriots, Faraday and Maxwell, men who set to the world of science the highest and noblest examples.

JOSEPH J. THOMSON

Carriers of negative electricity

Nobel Lecture, December 11, 1906

Introductory

In this lecture I wish to give an account of some investigations which have led to the conclusion that the carriers of negative electricity are bodies, which I have called corpuscles, having a mass very much smaller than that of the atom of any known element, and are of the same character from whatever source the negative electricity may be derived.

The first place in which corpuscles were detected was a highly exhausted tube through which an electric discharge was passing. When an electric discharge is sent through a highly exhausted tube, the sides of the tube glow with a vivid green phosphorescence. That this is due to something proceeding in straight lines from the cathode – the electrode where the negative electricity enters the tube – can be shown in the following way (the experiment is one made many years ago by Sir William Crookes): A Maltese cross made of thin mica is placed between the cathode and the walls of the tube. When the discharge is past, the green phosphorescence no longer extends all over the end of the tube, as it did when the cross was absent. There is now a well-defined cross in the phosphorescence at the end of the tube; the mica cross has thrown a shadow and the shape of the shadow proves that the phosphorescence is due to something travelling from the cathode in straight lines, which is stopped by a thin plate of mica. The green phosphorescence is caused by cathode rays and at one time there was a keen controversy as to the nature of these rays. Two views were prevalent: one, which was chiefly supported by English physicists, was that the rays are negatively electrified bodies shot off from the cathode with great velocity; the other view, which was held by the great majority of German physicists, was that the rays are some kind of ethereal vibration or waves.

The arguments in favour of the rays being negatively charged particles are primarily that they are deflected by a magnet in just the same way as moving, negatively electrified particles. We know that such particles, when a magnet is placed near them, are acted upon by a force whose direction is

at right angles to the magnetic force, and also at right angles to the direction in which the particles are moving.

Thus, if the particles are moving horizontally from east to west, and the magnetic force is horizontal from north to south, the force acting on the negatively electrified particles will be vertical and downwards.

When the magnet is placed so that the magnetic force is along the direction in which the particle is moving, the latter will not be affected by the magnet.

The next step in the proof that cathode rays are negatively charged particles was to show that when they are caught in a metal vessel they give up to it a charge of negative electricity. This was first done by Perrin. This experiment was made conclusive by placing the catching vessel out of the path of the rays, and bending them into it by means of a magnet, when the vessel became negatively charged.

Electric deflection of the rays

If the rays are charged with negative electricity they ought to be deflected by an electrified body as well as by a magnet. In the earlier experiments made on this point no such deflection was observed. The reason of this has been shown to be that when cathode rays pass through a gas they make it a conductor of electricity, so that if there is any appreciable quantity of gas in the vessel through which the rays are passing, this gas will become a conductor of electricity and the rays will be surrounded by a conductor which will screen them from the effect of electric force, just as the metal covering of an electroscope screens off all external electric effects.

By exhausting the vacuum tube until there was only an exceedingly small quantity of air left in to be made a conductor, I was able to get rid of this effect and to obtain the electric deflection of the cathode rays. This deflection had a direction which indicated a negative charge on the rays.

Thus, cathode rays are deflected by both magnetic and electric forces, just as negatively electrified particles would be.

Hertz showed, however, that cathode particles possess another property which seemed inconsistent with the idea that they are particles of matter, for he found that they were able to penetrate very thin sheets of metal, e.g. pieces of gold leaf, and produce appreciable luminosity on glass behind them. The idea of particles as large as the molecules of a gas passing through

a solid plate was a somewhat startling one, and this led me to investigate more closely the nature of the particles which form the cathode rays.

The principle of the method used is as follows: When a particle carrying a charge e is moving with velocity v across the lines of force in a magnetic field, placed so that the lines of magnetic force are at right angles to the motion of the particle, then, if H is the magnetic force, the moving particle will be acted on by a force equal to Hev. This force acts in the direction which is at right angles to the magnetic force and to the direction of motion of the particle. If also we have an electric field of force X, the cathode ray will be acted upon by a force Xe. If the electric and magnetic fields are arranged so that they oppose each other, then, when the force Hev due to the magnetic field is adjusted to balance the force due to the electric field Xe, the green patch of phosphorescence due to the cathode rays striking the end of the tube will be undisturbed, and we have

$$Hev = Xe$$

or

$$v = \frac{X}{H}$$

Thus if we measure, as we can do without difficulty, the values of X and H when the rays are not deflected, we can determine the value of v, the velocity of the particles. In a very highly exhausted tube this may be $\frac{1}{3}$ the velocity of light, or about 60,000 miles per second; in tubes not so highly exhausted it may not be more than 5,000 miles per second, but in all cases when the cathode rays are produced in tubes their velocity is much greater than the velocity of any other moving body with which we are acquainted. It is, for example, many thousand times the average velocity with which the molecules of hydrogen are moving at ordinary temperatures, or indeed at any temperature yet realized.

Determination of e/m

Having found the velocity of the rays, let us now subject them to the action of the electric field alone. Then the particles forming the rays are acted upon by a constant force and the problem is like that of a bullet projected horizontally with a velocity v and falling under gravity. We know that in time

t, the bullet will fall a depth equal to $\frac{1}{2}gt^2$, where g is the acceleration due to gravity. In our case the acceleration due to the electric field is equal to Xe/m, where m is the mass of the particle. The time $t = l/v$, where l is the length of path, and v the velocity of projection.

Thus the displacement of the patch of phosphorescence where the rays strike the glass is equal to

$$\tfrac{1}{2}\frac{Xe}{m}\cdot\frac{l^2}{v^2}$$

We can easily measure this displacement d, and we can thus find e/m from the equation

$$\frac{e}{m}=\frac{2d}{X}\cdot\frac{v^2}{l^2}$$

The results of the determinations of the values of e/m made by this method are very interesting, for it is found that, however the cathode rays are produced, we always get the same value of e/m for all the particles in the rays. We may, for example, by altering the shape of the discharge tube and the pressure of the gas in the tube, produce great changes in the velocity of the particles, but unless the velocity of the particles becomes so great that they are moving nearly as fast as light, when other considerations have to be taken into account, the value of e/m is constant. The value of e/m is not merely independent of the velocity. What is even more remarkable is that it is independent of the kind of electrodes we use and also of the kind of gas in the tube. The particles which form the cathode rays must come either from the gas in the tube or from the electrodes; we may, however, use any kind of substance we please for the electrodes and fill the tube with gas of any kind and yet the value of e/m will remain unaltered.

This constant value, when we measure e/m in the c.g.s. system of magnetic units, is equal to about 1.7×10^7. If we compare this with the value of the ratio of the mass to the charge of electricity carried by any system previously known, we find that it is of quite a different order of magnitude. Before the cathode rays were investigated, the charged atom of hydrogen met with in the electrolysis of liquids was the system which had the greatest known value of e/m, and in this case the value is only 10^4, hence for the corpuscle in the cathode rays the value of e/m is 1,700 times the value of the correspond-

ing quantity for the charged hydrogen atom. This discrepancy must arise in one or other of two ways: either the mass of the corpuscle must be very small compared with that of the atom of hydrogen, which until quite recently was the smallest mass recognized in physics, or else the charge on the corpuscle must be very much greater than that on the hydrogen atom. Now it has been shown by a method which I shall shortly describe, that the electric charge is practically the same in the two cases; hence we are driven to the conclusion that the mass of the corpuscle is only about 1/1,700 of that of the hydrogen atom. Thus the atom is not the ultimate limit to the subdivision of matter; we may go further and get to the corpuscle, and at this stage the corpuscle is the same from whatever source it may be derived.

Corpuscles very widely distributed

It is not only from what may be regarded as a somewhat artificial and sophisticated source, viz. cathode rays, that we can obtain corpuscles. When once they had been discovered, it was found that they are of very general occurrence. They are given out by metals when raised to a red heat; indeed any substance when heated gives out corpuscles to some extent. We can detect the emission of them from some substances, such as rubidium and the alloy of sodium and potassium, even when they are cold; and it is perhaps allowable to suppose that there is some emission by all substances, though our instruments are not at present sufficiently delicate to detect it unless it is unusually large.

Corpuscles are also given out by metals and other bodies, but especially by the alkali metals, when these are exposed to light.

They are being continually given out in large quantities and with very great velocities by radioactive substances such as uranium and radium; they are produced in large quantities when salts are put into flames, and there is good reason to suppose that corpuscles reach us from the sun.

The corpuscle is thus very widely distributed, but wherever it is found, it preserves its individuality, e/m being always equal to a certain constant value.

The corpuscle appear to form a part of all kinds of matter under the most diverse conditions; it seems natural therefore to regard it as one of the bricks of which atoms are built up.

Magnitude of the electric charge carried by the corpuscle

I shall now return to the proof that the very large value of e/m for the corpuscle, as compared with that for the atom of hydrogen, is due to the smallness of m the mass, and not to the greatness of e the charge. We can do this by actually measuring the value of e, availing ourselves for this purpose of a discovery by C. T. R. Wilson, that a charged particle acts as a nucleus round which water vapour condenses and forms drops of water. If we have air saturated with water vapour and cool it, so that it would be supersaturated if there were no deposition of moisture, we know that if any dust is present, the particles of dust act as nuclei round which the water condenses and we get the familiar phenomena of fog and rain. If the air is quite dust-free, we can, however, cool it very considerably without any deposition of moisture taking place. If there is no dust, C. T. R. Wilson has shown that the cloud does not form until the temperature has been lowered to such a point that the supersaturation in about eightfold. When however this temperature is reached, a thick fog forms even in dust-free air.

When charged particles are present in the gas, Wilson showed that a much smaller amount of cooling is sufficient to produce the fog, a fourfold supersaturation being all that is required when the charged particles are those which occur in a gas when it is in a state in which it conducts electricity. Each of the charged particles becomes the centre round which a drop of water forms; the drops form a cloud, and thus the charged particles, however small to begin with, now become visible and can be observed.

The effect of the charged particles on the formation of a cloud can be shown very distinctly by the following experiment:

A vessel which is in contact with water is saturated with moisture at the temperature of the room. This vessel is in communication with a cylinder in which a large piston slides up and down. The piston to begin with is at the top of its travel; by suddenly exhausting the air from below the piston, the pressure of the air above it will force it down with great rapidity, and the air in the vessel will expand very quickly. When, however, air expands, it gets cool; thus the air in the vessel previously saturated is now supersaturated. If there is no dust present, no deposition of moisture will take place, unless the air is cooled to such a low temperature that the amount of moisture required to saturate it is only about $\frac{1}{8}$ of that actually present.

Now the amount of cooling, and therefore of supersaturation, depends upon the travel of the piston; the greater the travel the greater the cooling.

Suppose the travel is regulated so that the supersaturation is less than eightfold and greater than fourfold. We now free the air from dust by forming cloud after cloud in the dusty air; as the clouds fall they carry the dust down with them, just as in nature the air is cleared by showers. We find at last that when we make the expansion no cloud is visible.

The gas is now made in a conducting state by bringing a little radium near the vessel; this fills the gas with large quantities of both positively and negatively electrified particles. On making the expansion now an exceedingly dense cloud is formed. That this is due to the electrification in the gas can be shown by the following experiment:

Along the inside walls of the vessel we have two vertical insulated plates which can be electrified. If these plates are charged, they will drag the electrified particles out of the gas as fast as they are formed, so that in this way we can get rid of, or at any rate largely reduce, the number of electrified particles in the gas. If the expansion is now made with the plates charged before bringing up the radium, there is only a very small cloud formed.

We can use the drops to find the charge on the particles, for when we know the travel of the piston, we can deduce the amount of supersaturation, and hence the amount of water deposited when the cloud forms. The water is deposited in the form of a number of small drops all of the same size; thus the number of drops will be the volume of the water deposited divided by the volume of one of the drops. Hence, if we find the volume of one of the drops, we can find the number of drops which are formed round the charged particles. If the particles are not too numerous, each will have a drop round it, and we can thus find the number of electrified particles.

From the rate at which the drops slowly fall we can determine their size. In consequence of the viscosity or friction of the air small bodies do not fall with a constantly accelerated velocity, but soon reach a speed which remains uniform for the rest of the fall; the smaller the body the slower this speed. Sir George Stokes has shown that v, the speed at which a drop of rain falls, is given by the formula

$$v = \frac{2}{9} \times \frac{g\, a^2}{\mu}$$

where a is the radius of the drop, g the acceleration due to gravity, and μ the coefficient of viscosity of the air.

If we substitute the values of g and μ, we get

$$v = 1.28 \times 10^6 \cdot a^2$$

Hence if we measure v we can determine a, the radius of the drop.

We can in this way find the volume of a drop, and may therefore, as explained above, calculate the number of drops and therefore the number of electrified particles.

It is a simple matter to find by electrical methods the total quantity of electricity on these particles; and hence, as we know the number of particles, we can deduce at once the charge on each particle.

This was the method by which I first determined the charge on the particle; H. A. Wilson has since used a simpler method founded on the following principles: C. T. R. Wilson has shown that the drops of water condense more easily on negatively electrified particles than on positively electrified ones. Thus, by adjusting the expansion, it is possible to get drops of water round the negative particles and not round the positive; with this expansion, therefore, all the drops are negatively electrified. The size of these drops and therefore their weight can, as before, be determined by measuring the speed at which they fall under gravity. Suppose now, that we hold above the drops a positively electrified body: then, since the drops are negatively electrified, they will be attracted towards the positive electricity, and thus the downward force on the drops will be diminished and they will not fall so rapidly as they did when free from electrical attraction. If we adjust the electrical attraction so that the upward force on each drop is equal to the weight of the drop, the drops will not fall at all, but will, like Mahomet's coffin, remain suspended between heaven and earth. If then we adjust the electrical force until the drops are in equilibrium and neither fall nor rise, we know that the upward force on each drop is equal to the weight of the drop, which we have already determined by measuring the rate of fall when the drop was not exposed to any electrical force. If X is the electrical force, e the charge on the drop, and w its weight, we have, when there is equilibrium,

$$Xe = w$$

Since X can easily be measured and w is known, we can use this relation to determine e, the charge on the drop. The value of e, found by these methods, is 3.1×10^{-10} electrostatic units, or 10^{-20} electromagnetic units. This value is the same as that of the charge carried by a hydrogen atom in the electrolysis of dilute solutions, an approximate value of which has been long known.

It might be objected that the charge measured in the preceding experi-

ments is the charge on a molecule or collection of molecules of the gas, and not the charge on a corpuscle.

This objection does not, however, apply to another form in which I tried the experiment, where the charges on the corpuscles were got, not by exposing the gas to the effects of radium, but by allowing ultraviolet light to fall on a metal plate in contact with the gas. In this case, as experiments made in a very high vacuum show, the electrification, which is entirely negative, escapes from the metal in the form of corpuscles. When a gas is present, the corpuscles strike against the molecules of the gas and stick to them.

Thus, though it is the molecules which are charged, the charge on a molecule is equal to the charge on a corpuscle, and when we determine the charge on the molecules by the methods I have just described, we determine the charge carried by the corpuscle.

The value of the charge when the electrification is produced by ultraviolet light is the same as when the electrification is produced by radium.

We have just seen that e, the charge on the corpuscle, is in electromagnetic units equal to 10^{-20}, and we have previously found that e/m, m being the mass of a corpuscle, is equal to 1.7×10^{7}, hence $m = 6 \times 10^{-28}$ grammes.

We can realize more easily what this means if we express the mass of the corpuscle in terms of the mass of the atom of hydrogen.

We have seen that for the corpuscle $e/m = 1.7 \times 10^{7}$. If E is the charge carried by an atom of hydrogen in the electrolysis of dilute solutions, and M is the mass of the hydrogen atom, $E/M = 10^{4}$; hence $e/m = 1{,}700\ E/M$.

We have already stated that the value of e found by the preceding methods agrees well with the value of E which has long been approximately known. Townsend has used a method in which the value of e/E is directly measured, and has shown in this way also that e equal to E. Hence, since $e/m = 1{,}700\ E/M$, we have $M = 1{,}700\ m$, i.e. the mass of a corpuscle is only about $1/1{,}700$ part of the mass of the hydrogen atom.

In all known cases in which negative electricity occurs in gases at very low pressures, it occurs in the form of corpuscles, small bodies with an invariable charge and mass. The case is entirely different with positive electricity.

Biography

John Joseph Thomson was born in Cheetham Hill, a suburb of Manchester on December 18, 1856. He enrolled at Owens College, Manchester in 1870 and in 1876 entered Trinity College, Cambridge as a minor scholar. He became a Fellow of Trinity College in 1880, when he was Second Wrangler and Second Smith's Prizeman, and he remained a member of the College for the rest of his life, becoming Lecturer in 1883 and Master in 1918. He was Cavendish Professor of Experimental Physics at Cambridge, where he succeeded Lord Rayleigh, from 1884 to 1918 and Honorary Professor of Physics, Cambridge and Royal Institution, London.

Thomson's early interest in atomic structure was reflected in his *Treatise on the Motion of Vortex Rings* which won him the Adams Prize in 1884. His *Application of Dynamics to Physics and Chemistry* appeared in 1886 and in 1892 he had his *Notes on Recent Researches in Electricity and Magnetism* published. This latter work covered results obtained subsequent to the appearance of James Clerk Maxwell's famous «Treatise» and it is often referred to as «the third volume of Maxwell». Thomson co-operated with Professor J. H. Poynting in a four-volume textbook of physics, *Properties of Matter* and in 1895 he produced *Elements of the Mathematical Theory of Electricity and Magnetism*, the 5th edition of which appeared in 1921.

In 1896, Thomson visited America to give a course of four lectures, which summarised his current researches, at Princeton. These lectures were subsequently published as *Discharge of Electricity through Gases* (1897). On his return from America, he achieved the most brilliant work of his life – an original study of cathode rays culminating in the discovery of the electron, which was announced during the course of his evening lecture to the Royal Institution on Friday, April 30, 1897. His book, *Conduction of Electricity through Gases*, published in 1903 was described by Lord Rayleigh as a review of «Thomson's great days at the Cavendish Laboratory». A later edition, written in collaboration with his son, George, appeared in two volumes (1928 and 1933).

Thomson returned to America in 1904 to deliver six lectures on electricity

and matter at Yale University. They contained some important suggestions as to the structure of the atom. He discovered a method for separating different kinds of atoms and molecules by the use of positive rays, an idea developed by Aston, Dempster and others towards the discovery of many isotopes. In addition to those just mentioned, he wrote the books, *The Structure of Light* (1907), *The Corpuscular Theory of Matter* (1907), *Rays of Positive Electricity* (1913), *The Electron in Chemistry* (1923) and his autobiography *Recollections and Reflections* (1936), among many other publications.

Thomson, a recipient of the Order of Merit, was knighted in 1908. He was elected Fellow of the Royal Society in 1884 and was President during 1916–1920: he received the Royal and Hughes Medals in 1894 and 1902 and the Copley Medal in 1914. He was awarded the Hodgkins Medal (Smithsonian Institute, Washington) in 1902; the Franklin Medal and Scott Medal (Philadelphia) 1923; the Mascart Medal (Paris) 1927; the Dalton Medal (Manchester) 1931; and the Faraday Medal (Institute of Civil Engineers) in 1938. He was President of the British Association in 1909 (and of Section A in 1896 and 1931) and he held honorary doctorate degrees from the Universities of Oxford, Dublin, London, Victoria, Columbia, Cambridge, Durham, Birmingham, Göttingen, Leeds, Oslo, Sorbonne, Edinburgh, Reading, Princeton, Glasgow, Johns Hopkins, Aberdeen, Athens, Cracow and Philadelphia.

In 1890, he married Rose Elisabeth, daughter of Sir George E. Paget, K.C.B. They had one son, now Sir George Paget Thomson, Emeritus Professor of Physics at London University, who was awarded the Nobel Prize for Physics in 1937, and one daughter.

Sir Joseph Thomson died on August 30, 1940.

Physics 1907

ALBERT ABRAHAM MICHELSON

«for his optical precision instruments and the spectroscopic and metrological investigations carried out with their aid»

Physics 1907

Presentation Speech by Professor K. B. Hasselberg, member of the Royal Swedish Academy of Sciences*

The Royal Academy of Sciences has decided to award this year's Nobel Prize for Physics to Professor Albert A. Michelson of Chicago, for his optical precision instruments and the research which he has carried out with their help in the fields of precision metrology and spectroscopy.

With untiring eagerness and, it can truly be said, with brilliant results, work is forging ahead today in every field of research in the natural sciences, and new information of ever greater significance is accumulating every day in unprecedented profusion. This is especially true in the case of the exact sciences – astronomy and physics – in which fields we are now obtaining solutions to problems, the mere mention of which up till a short while ago had to be regarded as unreal as Utopia itself. The reason for this gratifying development may be found in improvements in the methods and means of making observations and experiments, and also in the increase in accuracy brought about by these improvements in the quantitative examination of observed phenomena.

Astronomy, the precision science *par excellence*, has not only thus acquired whole new branches, but has also undergone in its older parts a transformation of more far-reaching significance than anything since the time of Galileo; and as for physics, it has developed remarkably as a precision science, in such a way that we can justifiably claim that the majority of all the greatest discoveries in physics are very largely based on the high degree of accuracy which can now be obtained in measurements made during the study of physical phenomena. We can judge how high our standards in this respect have risen from the fact that, for example, as recently as the beginning of the last century an accuracy of two to three hundredths of a millimetre in a measurement of length would have been regarded as quite fantastic. Today, however, scientific research not only demands but achieves an

* Owing to the decease of King Oscar II two days earlier, the presentation ceremony had to be cancelled. The speech, of which the text is rendered here, was therefore not delivered orally.

accuracy from ten to a hundred times as great. From this it is obvious how fundamental is the importance which must be attached to every step in this direction, for it is the very root, the essential condition, of our penetration deeper into the laws of physics – our only way to new discoveries.

It is an advance of this kind which the Academy wishes to recognize with the Nobel Prize for Physics this year. Everyone is familiar with the significance and scope of the uses to which the telescope and the microscope can be put as measuring instruments in precision physics; but a limit to the efficiency of these instruments has been reached, a limit which cannot be exceeded appreciably, for both theoretical and practical reasons. Professor Michelson's brilliant adaptation of the laws of light interference has, however, perfected a group of measuring instruments, the so-called interferometers, based on those laws, which previously only had occasional uses, to such a degree that an increase in accuracy in measurement of from twenty to a hundred times what can be achieved with the best microscopes has been brought well within our grasp. This is due to the fact that, owing to the peculiar nature of the interference phenomena, the desired value – usually a length is measured – can be obtained in numbers of wavelengths of the type of light in use in the experiment directly from observation in the interferometer of the changes in the image, caused by interference. An accuracy of up to 1/50 of a wavelength – about 1/100,000 of a millimetre – can be achieved by this method. If we now remind ourselves that the quantities the measurement of which has been made possible by this increase in accuracy – that is, small distances and angles – are precisely those which it is most often necessary to determine in research in precision physics, then without further ado it becomes obvious how powerful an aid has been presented to the physicist in Michelson's interferometer – an invaluable aid, not only because of its efficiency, but also because of the multiplicity of its uses. To illustrate this latter point, it is enough to mention such achievements as, for example, measuring the heat expansion of solid bodies, investigating their elastic behaviour under stress and rotation, determining the margin of error of a micrometric screw, measuring the thickness of thin laminae of transparent solids or liquids, and obtaining the gravitational constant, mass, and average density of the Earth, using both ordinary and torsion balances. Among the more recent uses of the interferometer, by means of which small angle deviations can be recorded with an accuracy of minute fractions of a second, may be mentioned Wadsworth's galvanometric construction, with which can be measured electric currents of vanishingly small intensities with

a hitherto unknown degree of accuracy. However, although these uses of the interferometer are important and interesting, nevertheless they are of relatively minor significance in comparison with the fundamental research done by Professor Michelson in the fields of metrology and spectroscopy with the help of these instruments and which, in view of its far-reaching significance for the whole of precision physics, surely deserves in itself to have been recognized with a Nobel Prize. In fact, metrology is concerned with nothing less than finding a method of being able to control the constancy of the international prototype metre, the basis of the whole metric system, so accurately that not only will every change, however small, which could possibly occur in it be accurately measured, but also if the prototype were entirely lost, it could nevertheless be reproduced so exactly that no microscope could ever reveal any divergence from the original prototype. The significance of this does not need any particular emphasis, but an outline, however brief, of the course of this research and its results would not be out of place here.

I have already laid emphasis above on the facts that with the help of the interferometer measurements of small length can be made with an extraordinarily high degree of accuracy, and that they may be expressed using the wavelengths of any one type of light as a unit. Moreover, it is possible to measure in this way lengths up to 0.1 metre or more, in suitable conditions, without impairing the accuracy. Thus Michelson's research has first of all prepared the way for the measurement of the value of a standard length of 10 cm in wavelengths of a particular radiation in the cadmium spectrum. Proceeding from the value obtained in this way for the standard 10 cm, with a probable error of at the most ± 0.00004 mm, Michelson was able, likewise using the interferometer, to ascertain on that basis the length of the normal metre, ten times greater, and he obtained for this length a value of

1,553,164.03

wavelengths of this kind to the metre. The probable error in this measurement can in the least favourable conditions amount to only ± 0.0004 mm – that is, less than one wavelength – a value which is far too small to be detected directly by the microscope. Subsequently measurements were carried out in the International Bureau of Weights and Measures in Paris by different observers following an entirely different method, which showed that the error was in fact considerably smaller. These measurements actually give as a value for the length of the metre

1,553,164.13

wavelengths of this kind – a result which differs from Michelson's figure by only 0.1 wavelength, or 0.00006 mm. It is clear from this that Michelson's measurement of the length of the prototype metre must be accurate to within at least 0.0001 mm, and further, that this length can, by the use of his methods, be verified, or in the case of the loss of the prototype be reproduced, with the same degree of accuracy on every occasion. Finally, it also emerges from this that during the interval of 15 years which elapsed between the two series of measurements under discussion, no variation whatsoever from this figure had taken place in the prototype. The great care which had been taken in the execution and preservation of the prototype gave it at least the appearance of a high degree of constancy, but no more; it was only possible to obtain a real proof of its constancy when the metre could be compared with an absolute measure of length, independent of any physical element giving rise to it, the constancy of which under certain given conditions appeared to be guaranteed beyond a shadow of doubt. As far as our present knowledge goes, this is the case with wavelengths of light. It is to Michelson's eternal honour that by his classical research he has been the first to provide such proof.

From the value obtained in this way for the metre in wavelengths of one particular light radiation, it is now also possible to obtain, *vice versa*, figures for these wavelengths on an absolute scale of measurement, with a corresponding degree of accuracy. This accuracy is exceptionally high, and is in fact about fifty times greater than anything obtained by absolute methods in use up till now to determine wavelength. The conviction which had steadily been gaining ground for a long time past, that Rowland's wavelength system, otherwise quite accurate, which has been in use for the last twenty years as the exclusive basis of all spectroscopic research, is with respect to their absolute values subject to quite considerable errors, has thus received full confirmation; it has thus become apparent that a thoroughgoing reassessment of these values is necessary, using either Michelson's or some other similar interference method. And so we have reached the field of spectroscopy, in which it is clear that Michelson's interferometer is capable of an application no less significant than those which we have already considered. This is, however, not its only use. Considering the almost perfect clarity with which the majority of spectral lines appear in the emission spectra produced with the powerful diffraction-grating spectroscopes of our

day, there were good grounds for regarding these radiated lines as simple and indivisible things; this is, however, not the case. Making use of his interferometer Michelson has in fact proved that they are, on the contrary, for the most part more or less complex groups of extremely closely packed lines, for the resolution of which the resolving power of even the strongest spectroscope proved utterly inadequate. The discovery of this internal structure of spectral lines to the more thorough investigation of which Michelson later contributed, in the form of the echelon grating invented by him, an even finer means of research than the interferometer, definitely belongs among the most important advances which the history of spectroscopy has ever been able to record, the more so as the nature and condition of the molecular structure of luminous bodies is extremely closely bound up with this structure of spectral lines. Here we are on the threshold of entirely new fields of research, over the unexplored expanses of which Michelson's experiments enable us to cast our first gaze, and his experiments can at the same time serve as a lead to those who are capable of carrying his work a stage further.

In addition to the more or less complicated structure which owing to the peculiar internal nature of luminous bodies is found in spectral lines, it is also possible to split them under the influence of a magnetic force into several more or less closely packed components. A few years ago this Academy was in a position to reward with the Nobel Prize the first exhaustive research, carried out by Professor Zeeman, into this phenomenon, which is extremely important to the science of physics. By using a powerful spectroscope, it is of course possible to examine this phenomenon in its general aspects; as a rule, however, the details are so subtle and so difficult to make out that the resolving power of that instrument is just not adequate for a full investigation. In this case the interferometer – or still better the echelon grating – may be used to advantage, as Michelson has shown. There can remain no shadow of doubt that through this instrument it will be possible to facilitate substantially research into the Zeeman effect.

I have only been able to give here a brief account of the numerous important problems whose solution has been brought so much nearer by the powerful aid to research, with its unprecedented degree of accuracy, which we have received in Michelson's optical precision instruments. This account would certainly seem incomplete if no mention were made of those applications which these instruments have already found, and will surely go on finding, in the field of astronomy, which are almost as important as

those in the field of physics. Among these belong the series of measurements of the diameters of the satellites of Jupiter, which have been carried out partly by Michelson himself in the Lick observatory, and partly with the interferometer by Hamy in Paris – a series within which there is substantially closer agreement than it has been possible to achieve with normal micrometric observations through the biggest refracting telescopes of the present day. Similarly, there can be no doubt whatsoever that it will be possible to obtain considerably more reliable figures in measuring the small planets between Mars and Jupiter than those which have been obtained by the photometric method of observation, which up till now has been the only one available, but which is extremely unreliable. The interferometer method can likewise be of some importance in the investigation of close double and multiple stars, and in this way we may cease to regard as utterly hopeless the problem, which has long been abandoned as completely insoluble, of finding by measurement true values for the diameters of at least the brighter stars. Thus astronomy has once again received from physics in the interferometer – as earlier in the spectroscope – a new aid to research which seems particularly suited to tackling problems whose solution was formerly impossible, as there were no, or at the most inadequate, instruments available.

The aforegoing will suffice, not only to explain to those who are not themselves closely involved in these problems the comprehensive and fundamental nature of Michelson's research in one of the most difficult fields of precision physics, but also to demonstrate how fully justified is the decision of this Academy to reward it with the Nobel Prize in Physics.

The following words were spoken to Professor A. A. Michelson, by Professor the Count K. A. H. Mörner, President of the Royal Swedish Academy of Sciences, during a private ceremony in the premises of the Academy.

Professor Michelson. The Swedish Academy of Sciences has awarded you this year the Nobel Prize in Physics in recognition of the methods which you have discovered for insuring exactness in measurements, and also of the investigations in spectrology which you have carried out in connection therewith.

Your interferometer has rendered it possible to obtain a non-material standard of length, possessed of a degree of accuracy never hitherto attained. By its means we are enabled to ensure that the prototype of the metre has

remained unaltered in length, and to restore it with absolute infallibility, supposing it were to get lost.

Your contributions to spectrology embrace methods for the determination of the length of waves in a more exact manner than those hitherto known.

Furthermore, you have discovered the important fact that the lines in the spectra, which had been regarded as perfectly distinct, are really in most cases groups of lines. You have also afforded us the means of closely investigating this phenomenon, both in its spontaneous occurrence and when it is produced by magnetic influence, as in Zeeman's interesting experiments.

Astronomy has also derived great advantage, and will do so yet more in the future, from your method of measurements.

In bestowing the Nobel Prize in Physics upon you the Academy of Sciences desires to signalize as worthy of especial honour the eminently successful researches you have carried out. The results you have attained are excellent in themselves and are calculated to pave the way for the future advancement of science.

ALBERT A. MICHELSON

Recent advances in spectroscopy

Nobel Lecture, December 12, 1907

The fame of Newton rests chiefly on his epoch-making discovery of the laws of gravitational astronomy – by means of which the position of the moons, the planets, and the comets, and other members of our solar system can be calculated and verified with the utmost precision – and in many cases such calculation and verification may be extended to systems of suns and planets outside our own.

But in no less degree are we indebted to this monumental genius for that equally important branch of Astrophysics – in which the spectroscope plays so fundamental a role – by means of which we are enabled to discover the physical and chemical constitution of the heavenly bodies, as well as their positions and motions. As the number and intricacy of the wonderful systems of stellar worlds which the telescope can reveal increase with its power, so also do the evidences of the innermost molecular structure of matter increase with the power of the spectroscope. If Newton's fundamental experiment of separating the colors of sunlight had been made under conditions so slightly different from those in his actual experiment that in the present stage of experimental science, they would at once suggest themselves to the veriest tyro – the science of spectroscopy would have been founded.

So simple a matter as the narrowing of the aperture through which the sunlight streamed before it fell upon the prism which separates it into its constituent colors – would have sufficed to show that the spectrum was crossed by dark lines, named after their discoverer, the Fraunhofer lines of the Solar Spectrum. These may be readily enough observed, with no other appliances than a slit in a shutter which is observed through an ordinary prism of glass. Fraunhofer increased the power of the combination enormously by observing with a telescope – and this simple combination, omitting minor details, constitutes that wonder of modern science, the Spectroscope. As the power of a telescope is measured by the closeness of the double stars which it can «resolve», so that of the Spectroscope may be estimated by the closeness of the spectral lines which it can separate. In order to form an idea of the advance in the power of spectroscopes

let us for a moment consider the map of the Solar Spectrum (Figure 1).

The portion which is visible to the unaided eye extends from the Fraunhofer line *A* to *H*; but by photography it may be traced far into the ultraviolet region and by bolometric measurements it is found to extend enormously farther in the region beyond the red. In the yellow we observe a dark line marked *D*, which coincides in position with the bright light emitted by sodium – as when salt is placed in an alcohol flame. It may be readily shown by a prism of very moderate power that this line is double, and as the power of the instrument increases the distance apart or separation of this doublet furnishes a very convenient measure of its separating or resolving power. Of course this separation may be effected by simple magnification, but this would in itself be of no service, as the «lines» themselves would be broadened by the magnification in the same proportion. It can be shown that the effective resolving power depends on the material of the prism which must be as highly dispersive as possible and on the size, or number, of the prisms employed – and by increasing these it has been found possible to «resolve» double lines thirty or forty times as near together as are the sodium lines. It will be convenient to take the measure of the resolving power when just sufficient to separate the sodium lines as 1,000. Then the limit of resolving power of prism spectroscopes may be said not much to exceed 40,000.*

This value of resolving power is found in practice to obtain under average conditions. Theoretically there is no limit save that imposed by the optical conditions to be fulfilled – and especially by the difficulty in obtaining large masses of the refracting material of sufficient homogeneity and high dispersive power. It is very likely that this limit has not yet been reached.

Meanwhile another device for analysing light into its component parts has been found by Fraunhofer (1821) which at present has practically superseded the prism – namely, the diffraction grating. Fraunhofer's original grating consisted of a number of fine equidistant wires, but he afterwards made them by ruling fine lines on a glass plate covered with gold leaf and removing the alternate strips. They are now made by ruling upon a glass or a metal surface fine equidistant lines with a diamond point.

The separation of light into its elements by a grating depends on its action on the constituent light-waves.

Let Fig. 2 represent a highly magnified cross section of a diffraction grating with plane waves of light falling upon it normally, as indicated by the ar-

* Lord Rayleigh has obtained results with prism of carbon disulphide which promise a much higher resolving power.

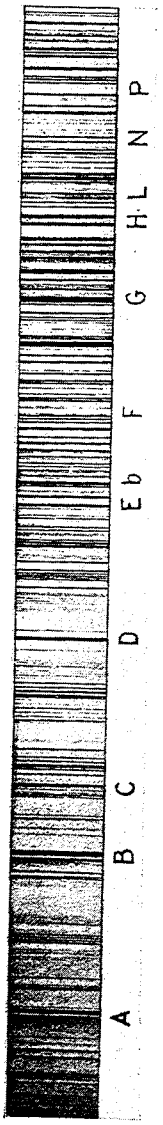


Fig. 1.

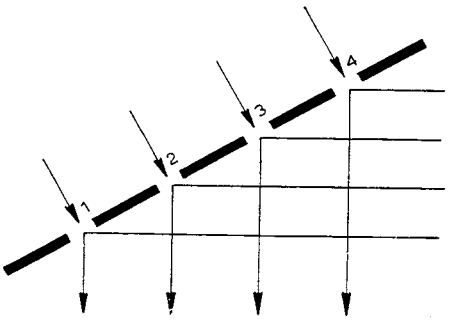


Fig. 2.

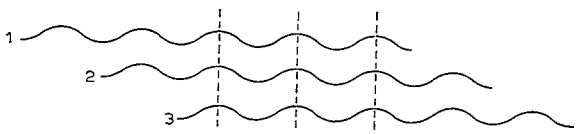


Fig. 3.

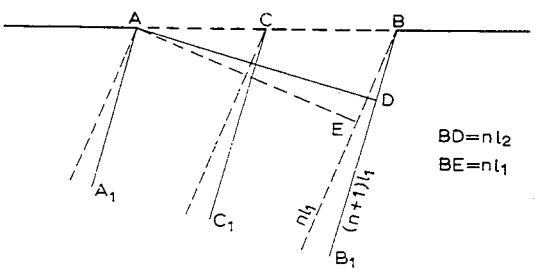


Fig. 4.

rows. The wave motion will pass through the apertures, and will continue as a series of plane waves; and if brought to a focus by a telescope will produce an image of the slit source just as if no grating were present (save that it is fainter, as some of the light is cut off by the opaque portions). This image may be considered as produced by the concurrence of all the elementary waves from the separate apertures meeting in the same phase of vibration, thus re-inforcing each other. But this may also be true in an oblique direction, as shown in the figure, if the retardation of the successive waves is just one whole wavelength (or any whole number) as is illustrated in Fig. 3, where the successive waves from apertures 1, 2, 3... are shown to re-inforce each other just as if they all belong to a single wave-train. In this direction therefore there will also be an image of the slit source; and this direction is determined by the relation:

$$\sin \theta = \frac{m\, l}{s}$$

where l is the length of the light-wave of this particular color, s the distance between the apertures (the grating space), and m the number of waves in the common retardation (1, 2, 3, etc.). But even if the light thus diffracted be absolutely homogeneous (that is, consist of an infinite wave-train of constant wavelength) it does not follow that the light is all diffracted in the given direction; there will be some light in directions differing slightly from this – growing less until the extreme difference of path is (say) $n + 1$ waves, (instead of n when it is nil).

In fact, if we divide the pencil having this new direction into two equal parts AC and CB, the ray AA_1 will be $n + \frac{1}{2}$ waves in advance of CC_1, and the two will be in opposite phases of vibration, and will therefore neutralize each other. The same will be true of each pair of rays taken in the same manner over the whole grating space, and the result is total darkness for this direction. Let us suppose we are examining the double sodium line. The difference between the components is about one thousandth of the wavelength. With a grating of n lines there will be total darkness in a direction corresponding to a retardation of $(n + 1)l$. Let this direction correspond to the brightest part of the image for the second sodium line l_2, so that $(n + 1)\, l = nl_2$ or $(l_2 - l_1)/s = l/n$. Under these conditions the two images are just « resolved ». But $(l_2 - l_1)/l = l/1{,}000$ for sodium lines, whence $n = 1{,}000$. That is, a grating of 1,000 lines will « resolve » the sodium lines in the first spectrum, or $R = 1{,}000$. In the second (where the common retardation

is two wavelengths) the resolving power is twice as great or $2n$, and in the mth spectrum, m times as great. The resolving power is therefore the product of the number of lines in the grating by the order of the spectrum, that is, $R = mn$.

In order, therefore, to obtain high resolving power the grating must have a great number of rulings and if possible a high order of spectrum should be used. The rulings need not be exceedingly close together, but it is found practically sufficient if there are from 500 to 1,000 lines per millimeter. The earlier gratings were relatively small and contained only a few thousand lines. The best of these were ruled by Nobert (1851). A very great advance was made by Rutherford of New York, who (1868) ruled gratings two inches long on speculum metal and containing about 20,000 lines. These gratings exceeded in resolving power the best prism trains in use at the time. The next advance was made by Rowland of the Johns Hopkins University, who succeeded in ruling gratings six inches long (by two to three inches stroke) having about one hundred thousand lines, and capable (theoretically, at least) of resolving in the spectrum, double lines whose distance apart was only one one-hundredth as great as that of the sodium lines. Practically this is about the limit of the power of the best Rowland grating which I have examined.

The difference between the theoretical and the actual performance is due to want of absolute uniformity in the grating space. This is due to the enormous difficulty in constructing a screw which shall be practically perfect throughout its whole length, a difficulty which increases very rapidly as the length of the screw increases; and it has been supposed that the limit of accuracy was reached in these gratings.

The great and rapidly increasing importance of spectrum analysis – especially in determining the distribution of light in so-called spectral lines under normal conditions, the resolution of complicated systems of lines, and in the investigation of the effects of temperature, of pressure, and especially of a magnetic field – justified the untertaking of much larger gratings than these. As an example of progress made in this direction, I have the honor of exhibiting a grating having a ruled surface nine inches long by four and one half inches stroke (220 × 110 mm). This has one hundred and ten thousand lines and is nearly perfect in the second order; so that its resolving power is theoretically 220,000 and this is very nearly realized in actual experiments.

It will be observed that the effect produced at the focus of the telescope depends on the concurrence or opposition – in general on the *interference* of

the elementary trains of light-waves. We are again indebted to the genius of Newton for the first observation of such interference; and a comparatively slight modification of the celebrated experiment of «Newton's rings» leads to a third method of spectrum analysis which, if more indirect and less convenient than the methods just described, is far more powerful. If two plane surfaces (say the inner surfaces of two glass plates) are adjusted very accurately to parallelism, and sodium light fall on the combination at nearly normal incidence, the light reflected from the two surfaces will interfere, showing a series of concentric rings alternately bright and dark, according to the relative retardation of the two reflected light beams.

If this retardation changes (by slowly increasing the distance between the surfaces) the center of the ring system goes through alternations of light and darkness, the number of these alternations corresponding exactly to the number of light-waves in twice the increase in distance. Hence the measurement of the length of the waves of any monochromatic light may be obtained by counting the number of such alternations in a given distance. Such measurement of wavelengths constitutes one of the most important objects of spectroscopic research.

Another object accomplished by such measurement is the establishment of a natural standard of length in place of the arbitrary standard at present in use – the meter. Originally it was intended this should be the ten-millionth part of an earth-quadrant, but it was found that the results of measurements differed so much that this definition was abandoned. The proposition to make the ultimate standard the length of a pendulum which vibrates seconds at Paris met with a similar fate.

Shortly after the excellent gratings made by Rutherford appeared, it was proposed (by Dr. B. A. Gould) to make the length of a wave of sodium light the ultimate standard; but this idea was never carried out. It can be shown that it also is not susceptible of the requisite degree of accuracy, and in fact a number of measurements made with a Rowland grating have been shown to be in error by about one part in thirty thousand. But modern conditions require a much higher degree of accuracy. In fact, it is doubtful if any natural standard could replace the arbitrary standard meter, unless it can be shown that it admits of realization in the shape of a material standard which can not be distinguished from the original.

One of the most serious difficulties encountered in the attempt to carry into practice the method of counting the alternations of light and darkness in the interference method, is the defect in homogeneity of the light em-

ployed. This causes indistinctness of the interference rings when the distance is greater than a few centimeters. The light emitted by various kinds of gases and metallic vapors, when made luminous by the electric discharge, differ enormously in this respect. A systematic search showed that among some forty or more radiations nearly all were defective, some being represented by a spectrum of broad hazy «lines», others being double, triple, or even more highly complex. But the red light emitted by luminous vapor of metallic cadmium was found to be almost ideally adapted for the purpose. Accordingly this was employed: and the results of three independent measurements, made by different observers and at different times, of the number of light-waves of red cadmium light in the standard meter are as follows:

I	1,553,392.4
II	1,553,393.2
III	1,553,393.4.

It will be seen that the differences are less than half a millionth part, and this is about the limit of accuracy of the comparative measurements of the material standards. Within the last year a similar determination has been carried out by Perot and Fabry, with a result not to be distinguished from the above. It follows that we now have a natural standard of length, the length of a light-wave of incandescent cadmium vapor; by means of which a material standard can be realized, whose length can not be distinguished from the actual standard meter – so that if, through accident or in time, the actual standard meter should alter, or if it were lost or destroyed, it could be replaced so accurately that the difference could not be observed.

In the search for a radiation sufficiently homogeneous for the purpose of a standard it became evident that the interference method might be made to yield information concerning the distribution of light in an approximately homogeneous source when such observations would be entirely beyond the power of the best spectroscopes. To illustrate, suppose this source to be again the double radiation from sodium vapor. As the wavelengths of these two radiations differ by about one part in a thousand, then at a difference of path of five hundred waves (about 0.36 mm) the bright fringes of one wave-train would cover the dark fringes of the other, so that if the two radiations were of equal intensity, all traces of interference would vanish. At twice this distance they would reappear and so on indefinitely, if the separate radiations were absolutely homogeneous. As this is not the case, however, there would

be a gradual falling off in the clearness or visibility of the bands. Inversely, if such changes are observed in actual experiment, we infer that we are dealing with a double source. Further, from the distance between the maxima of distinctness, we may determine (and with extraordinary accuracy) the ratio of wavelenghts of the components; from the ratio of maxima to minima we may infer the ratio of their intensities; and finally the gradual falling off when the distance becomes large gives accurate information of the «width» of the corresponding spectral lines.

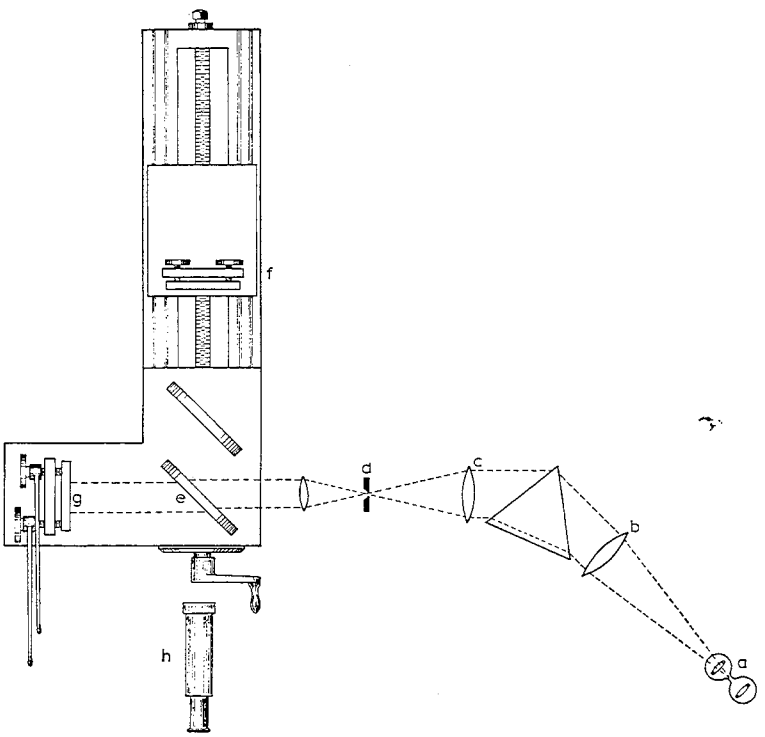


Fig. 5.

In this way it was found that the red line of hydrogen is a double with components about one fortieth of the distance apart of the sodium lines. Thallium has a brilliant green radiation which is also double, the distance being one sixtieth that of the sodium lines. Mercury shows a brilliant green line, which is highly complex, but whose chief component is a doublet, whose separation is only one seven-hundredth of that of sodium. The inter-

ference fringes are still visible when the difference of path is of the order of five hundred millimeters, corresponding to over a million light-waves; and the corresponding width of spectral line would be less than a thousandth part of that which separates the sodium lines.

Fig. 5 illustrates the arrangement of the apparatus as it is actually used. An ordinary prism spectroscope gives a preliminary analysis of the light from the source. This is necessary because the spectra of most substances consist of numerous lines. For example, the spectrum of mercury contains two yellow lines, a very brilliant green line, and a less brilliant violet line, so that if we pass all the light together into the interferometer, we have a combination of all four. It is usually better to separate the various radiations before they enter the interferometer. Accordingly, the light from the vacuum tube at *a* passes through an ordinary spectroscope *b c*, and the light from only one of the lines in the spectrum thus formed is allowed to pass through the slit *d* into the interferometer.

As explained above, the light divides at the plate *e*, part going to the mirror *f*, which is movable, and part passing through, to the mirror *g*. The first ray returns on the path *f e h*. The second returns to *e*, is reflected, and passes into the telescope *h*.

The resolving power of the interferometer is measured by the number of light-waves in the difference of path of the two interfering pencils, and as this is unlimited, the interferometer furnishes the most powerful means for investigating the structure of spectral lines or groups. Its use is, however, somewhat handicapped by the fact that the examination of a single group of lines may require a considerable number of observations which take some time and during which it may be difficult to prevent changes in the light source. Nevertheless it was found possible by its means to investigate the wonderful discovery of Zeeman – of the effect of a magnetic field on the character of the radiation from a source subjected to its influence; and the results thus obtained have been since confirmed by methods which have been subsequently devised.

One of these is the application of the echelon. This is in effect a diffraction grating in which high resolving power is obtained by using a very high order of spectrum into which practically all the light is concentrated. The number of elements may be quite moderate – since the resolving power is the product of the two. The order of the spectrum is the number of wave-lengths in the retardation at each step. This retardation (which must be very accurately constant) is secured by allowing the incident light to fall upon a

pile of glass plates optically plane parallel and of the same thickness – each one a little wider than the preceding as in Fig. 6.

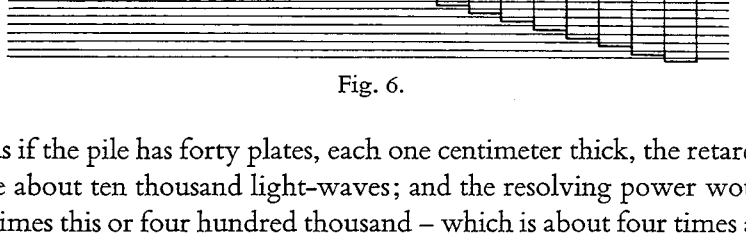


Fig. 6.

Thus if the pile has forty plates, each one centimeter thick, the retardation will be about ten thousand light-waves; and the resolving power would be forty times this or four hundred thousand – which is about four times as that of a six-inch diffraction grating of the usual form. The number of elements might be increased till the absorption of the glass brought a limit. A difficulty, which appears long before this limit is reached, is due to the loss of light by repeated reflections between the many surfaces. This has been very ingeniously overcome by Mr. Twyman of the firm of Hilger & Company by pressing the plates together to actual contact – when the reflection vanishes. It is likely that the echelon under these conditions may be used by reflection instead of transmission (the plates being silvered for the purpose) with the advantage of quadrupling the resolving power for the same number of plates and eliminating the absorption.

An illustration of the efficiency of the echelon spectroscope is furnished by the following photographs of the spectrum of green radiations from mercury vapor. The first of the figures shows the spectrum of the second order of a diffraction grating whose ruled surface is nine inches by four and a half – the largest in existence. The second is by an echelon of thirty plates, seven millimeters thick, and in the third the echelon consisted of forty plates, each an inch and a fourth thick (30 mm). The corresponding lines are similarly lettered in the three figures. The scale is in Å. U. (Ångström units). It will be noted in the last of the three figures that the midth of the fainter companion is about one one-hundredth of an Å.U. The limit of resolution of the instrument is about half as much, or its resolving power is over a million (Figs. 7, 8 and 9).

It will be observed that the echelon spectra are repeated – thus a_1 and a_2 are two successive spectra of the same line. This is true of any grating spectrum, and the difficulties which arise from the overlapping of the suc-

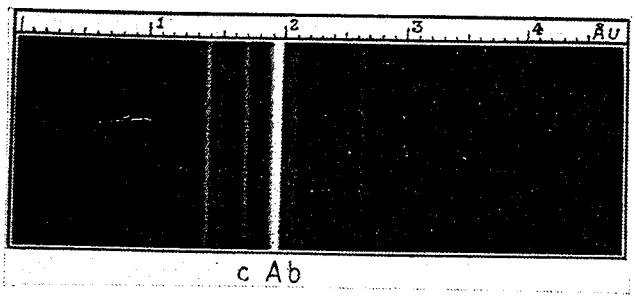


Fig. 7.

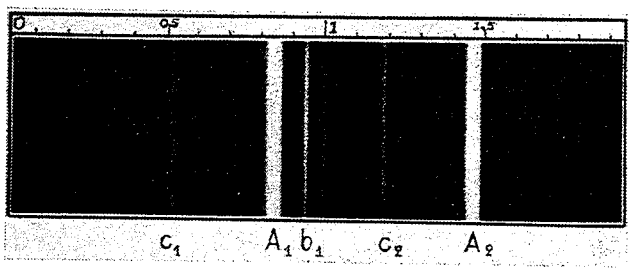


Fig. 8.

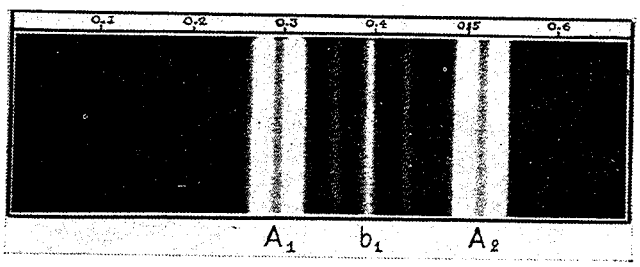


Fig. 9.

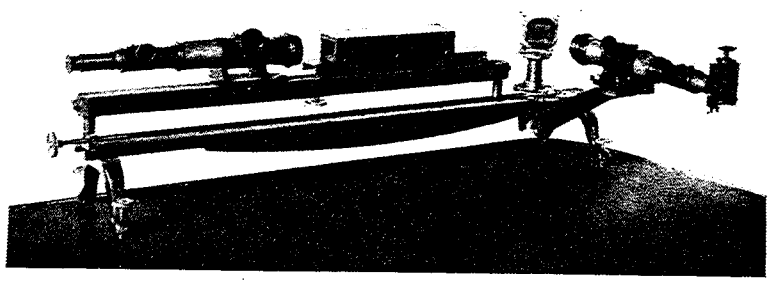


Fig. 10.

Fig. 11.

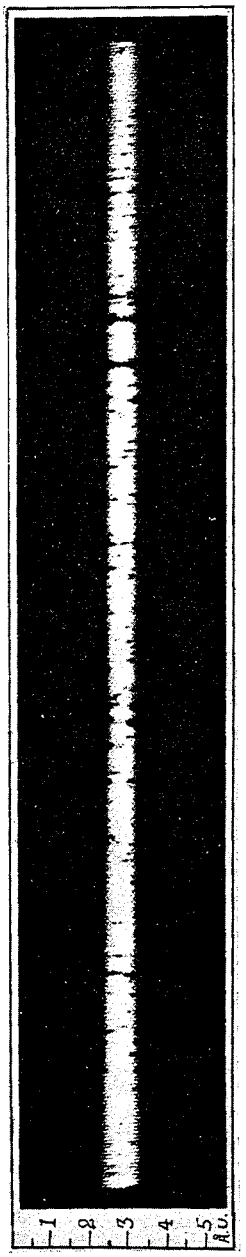


Fig. 12.

cessive orders of spectrum may be overcome by separating these by a prism whose refracting edge is perpendicular to the lines of the grating. The same is true of the echelon spectrum – save that the order of the overlapping spectra is so high that a prism is hardly adequate and recourse must be had to a grating – with its plane of diffraction perpendicular to that of the echelon, as shown in Fig. 10.

With this arrangement it is possible to photograph a large part of the spectrum at once.*

Fig. 11 shows such a photograph of the iron spectrum, and it may be noted that this combination of grating and echelon makes it possible to observe absorption spectra as well as bright line spectra.

Fig. 12 shows a photograph of the solar spectrum taken in this way. It will be noted that the spectral «lines» are generally too broad to justify the use of so great a resolving power.

Finally it may be pointed out that this combination gives us the means of comparing the wavelengths of spectral lines with a degree of accuracy far superior to that of the grating.

* If the preliminary analysis has been made before the light entered the slit of echelon spectroscope, it would be possible to examine but one – at most a few – lines at a time.

Biography

Albert Abraham Michelson was born in Strelno, Prussia, on December 19, 1852. Two years later his family emigrated to the United States to settle at Virginia City, Nevada, but they eventually moved to San Francisco where Michelson received his early education in public schools, matriculating from the High School in 1869. He was appointed by President Grant to the U.S. Naval Academy and, after graduation as Ensign in 1873 and a two-years' cruise in the West Indies, he became an instructor in physics and chemistry at the Academy under Admiral Sampson. In 1879, he was posted to the Nautical Almanac Office, Washington, to work with Simon Newcomb, but in the following year, he obtained leave of absence to continue his studies in Europe. He visited the Universities of Berlin and Heidelberg, and the Collège de France and École Polytechnique in Paris. He resigned from the Navy and in 1883 returned to America to take an appointment as Professor of Physics in the Case School of Applied Science, Cleveland, Ohio. In 1890 he accepted a similar position at Clark University, Worcester, Massachusetts, and in 1892 he became Professor of Physics and the first Head of Department at the new University of Chicago. He rejoined the Navy during World War I, and in 1918 returned to Chicago where in 1925 he was appointed to the first of the Distinguished Service Professorships. Michelson resigned in 1929 to work at the Mount Wilson Observatory, Pasadema.

During his career, Michelson touched on many departments of physics but, perhaps due to a special instinct which he appeared to possess, he excelled in optics. He performed early measurements of the velocity of light with amazing delicacy and in 1881 he invented his interferometer for the purpose of discovering the effect of the Earth's motion on the observed velocity. In cooperation with Professor E. W. Morley, and using the interferometer, it was shown that light travels at a constant speed in all internal systems of reference. The instrument also enabled distances to be measured with greater accuracy by means of the length of light-waves. At the request of the International Committee of Weights and Measures, Michelson measured the standard metre in terms of wavelength of cadmium light. He in-

vented the echelon spectroscope and during his wartime service in the Navy he performed research work on devices for naval use – he developed a range-finder which was adapted as part of U.S. Navy equipment. On his return to civilian life, Michelson became more interested in astronomy and in 1920, using light interference and a highly developed version of his earlier instrument, he measured the diameter of the star Betelgeuse: this was the first determination of the size of a star that could be regarded as accurate.

Michelson has contributed numerous papers to many scientific periodicals and among his more substantial works are the classics, *Velocity of Light* (1902) *Light Waves and their Uses* (1899–1903); and *Studies in Optics* (1927).

Michelson was honoured by memberships of many learned societies throughout America and ten European countries, and he received honorary science and law degrees from ten American and foreign universities. He was President of the American Physical Society (1900), the American Association for the Advancement of Science (1910–1911), and the National Academy of Sciences (1923–1927). He was also a Fellow of the Royal Astronomical Society, the Royal Society of London and the Optical Society, an Associate of l'Académie Française and among the many awards he has received are the Matteucci Medal (Societá Italiana), 1904; Copley Medal (Royal Society), 1907; Elliot Cresson Medal (Franklin Institute), 1912; Draper Medal (National Academy of Sciences), 1916; Franklin Medal (Franklin Institute) and the Medal of the Royal Astronomical Society, 1923; and the Duddell Medal (Physical Society), 1929.

Michelson married Edna Stanton of Lake Forest, Illinois in 1899. They had one son and three daughters. He died in 1941.

Physics 1908

GABRIEL LIPPMANN

«for his method of reproducing colours photographically based on the phenomenon of interference»

Physics 1908

Presentation Speech by Professor K. B. Hasselberg, President of the Royal Swedish Academy of Sciences

Your Majesty, Your Royal Highnesses, Ladies and Gentlemen.

The Royal Academy of Sciences has awarded the Nobel Prize for Physics for 1908 to Professor Gabriel Lippmann of the Sorbonne for his method, based on the phenomenon of interference, which permits the reproduction of colours by photography.

Even before 1849, when the art of photographic reproduction was discovered by the pioneers of Science, Niepce, Daguerre, Talbot and others, the question of means of rendering and of fixing colours on the photographic plate has loomed large. It looked as though the answer was at hand when Edmond Becquerel showed that a silver plate coated with a thin layer of silver chloride coloured up under the action of light with a colour corresponding to that of the light used. This observation led no further. Becquerel had no explanation for the origin of the colours nor did he find a means of fixing them on the plate. They passed off rapidly and so his method, being thus of no practical use, failed to win the attention it undoubtedly deserved.

One explanation for the origin of Becquerel's coloured images was given in 1868 by the German Wilhelm Zenker and then taken further by the Nobel Prize winner Lord Rayleigh. According to this explanation, the colour phenomenon is due to standing light waves that by chemical action form grains of silver metal from the silver chloride. Colour is an interferential phenomenon produced by the reflection of light on this silver layer.

The phenomenon thus became of theoretical interest. If the truth of this theory could be shown, the work of Becquerel would afford further proof of the correctness of our concept of light considered as the result of vibratory movement, since one of the fundamental phenomena of vibratory movement – the standing wave – would thus be verified for light. It was, however, not until 1890 that Otto Wiener by a particularly fine experiment furnished conclusive evidence of the correctness of Zenker's theory.

It was now possible to reproduce pictures in more or less exact colours but still not stable. Explanation had also been found for the origin of these pic-

tures. It was still not time to talk of photographic reproduction of the colour of objects and their *fixation*. This was the point reached when Professor Lippmann in 1891 communicated to the Paris Academy of Sciences his sensational work *Colour Photography*.

The main features of the Lippmann method are doubtless fairly well known. On plane glass a layer sensitive to light is spread, consisting of gelatine emulsion, silver nitrate, and potassium bromide. To this sensitive layer a layer of mercury is applied, forming a mirror. This is exposed in the dark room in such a way that the glass side of the plate is turned towards the objective. During exposure light has to pass through the glass first, then penetrate the imprint layer and encounter the reflecting surface of the mercury, which throws it back. These incident and reflected light waves form what are called standing waves, characterized by a series of maxima and minima of illumination, distant from each other by half a wavelength of the incident light. Once the plate is developed, fixed, and dried by normal processes, there will be found in the layer of gelatine planes of reduced silver whose reciprocal distances depend on the wavelength – that is to say, on the colour of the light which produced the image. Let us suppose that white light falls in the normal way on a photographic plate disposed as we have described. The ray will be reflected by the different planes of silver and, following known laws of interference of light in thin laminae, the foil will appear coloured – and coloured with the same colour as the light that gave rise to the corresponding photographic print. The reproduction of colours is thus being carried out here by the same way as in soap bubbles and thin laminae in general, with additional strengthening by the existence of successive planes. The effect of colour in Lippmann's experiments does not therefore arise from pigment colours. We have to do with what are called virtual colours, unalterable in composition and bright for as long as the photographic plate is intact. Thus Lippmann's photographs show up favourably in comparison with later attempts at solving this problem of colour reproduction – Lumière's photographs – so-called three-colour photographs, obtained by using pigment colours, a delightful discovery, which owing to the simplicity of the operational method has rightly won a large measure of popularity.

One glance at the illustrated works of our day, both in the domain of science and of art and industry, is enough to show the key position of photographic reproduction in present-day life. Lippmann's colour photography marks a further step forward, which is of great importance, in the art of photography, since his method has been the first to give us the means of

presenting to posterity in unalterable picture form not only the shape of an object with its play of light and shade but its colours as well.

Through sustained effort directed towards his end and through his complete grasp of all the resources that physics can offer, Professor Lippmann has created this elegant method of obtaining images which combine stability with colorific splendour. This achievement the Royal Academy of Sciences has considered worthy of the award of the Nobel Prize for Physics for 1908.

GABRIEL LIPPMANN

Colour photography

Nobel Lecture, December 14, 1908

The problem of direct colour photography has been facing us since the turn of the last century. Edmond Becquerel, as is known, gave a first solution though only an imperfect one. Becquerel showed that the colours of the image of the dark room print on a layer of violet silver chloride. Zenker explained Becquerel's finding by a phenomenon of interference. Experiment shows that this explanation is not true and that Zenker's theory does not hold good for silver chloride. Becquerel's prints remained, however, what they were: not fixed, and fading in light. Then Otto Wiener fixed by photography a shot of interference fringes that are found in the neighbourhood of a silver mirror. That physicist did not, however, envisage obtaining colours by an interference method. I will not lay any further stress on the background of experiments and ideas which preceded the method on which I am to have the honour of addressing you, and which furnishes the coloured image of objects.

The method is very simple. A plate is covered with a sensitive transparent layer that is even and grainless. This is placed in a holder containing mercury. During the take, the mercury touches the sensitive layer and forms a mirror. After exposure, the plate is developed by ordinary processes. After drying the colours appear, visible by reflection and now fixed.

This result is due to a phenomenon of interference which occurs within the sensitive layer. During exposure, interference takes place between the incident rays and those reflected by the mirror, with the formation of interference fringes half a wavelength distant from each other. The fringes imprint photographically through the whole thickness of the film and form a casting for the light rays. When the shot is afterwards subjected to white light, colour appears because of selective reflection. The plate at each point only sends back to the eye the simple colour imprinted. The other colours are destroyed by interference. The eye thus perceives at each point the constituent colour of the image. This is no more than a phenomenon of selective reflection as in the case of the soap bubble or mother-of-pearl. The print in itself is formed of colourless matter like that of mother-of-pearl or soap film.

This explanation can be checked by an experiment we are going to carry out in front of you. Here first is a print of the spectrum projected on to the screen. As you see, the colours are bright. We wet the plate and project it on to the screen again. There is no colour there. The gelatine has swollen and the intervals between the images of the interference fringes (Zenker's laminae) have become two or three times too large. Wait one minute while the water dries off. We see the colours re-appear in accordance with and at the speed of the drying process. They re-appear according to an order which can be predicted. Red, which corresponds to the greatest wavelengths, re-appears first, followed by orange, green, blue, and violet.

The reproduction of the simple colours of the spectrum was the easiest to carry out. The photography of composite colours that exterior objects present posed a harder problem. At first sight it might have been held impossible. In effect, in the case of simple light, the interference maxima are equidistant planes separated by intervals equal to half a wavelength. In the case of composite colour, an infinity of systems must be obtained for maxima infinitely slight and with an infinity of interval values separating them – that is to say, the whole thickness of the sensitive layer is occupied in continuous manner by these maxima. The spaces that exist in the instance of simple light and which allow to assimilate the photographic plate with a series of fine laminae have disappeared. It was thus necessary to reshape the theory of the phenomenon in wider terms. First it must be noted that the amplitude resulting from the interference varies according to a function that is continuous even in the case of simple light. The general case is derived by an analysis based on one of Fourier's chapters. It can thus be demonstrated that photography of composite colours is possible.

Once all theoretic reserve was gone, the technical difficulties appertaining to the isochromatism of the films remained to be overcome. I got quite good results from protein plates. Later, Valenta in Vienna and the Lumières at Lyons found means of coating the plates in grainless gelatine, sufficiently isochromatic and very much better than the protein plate. Dr. Neuhauss in Berlin carried isochromatism to perfection. Thanks to the work of Messrs. Miethe, Krone, H. Lehmann, and others whom I will not detain you by mentioning, the technique of colour photography has been perfected. Allow me to show you projections of results obtained.

(Series of slides – still-life paintings, vases with flowers, views of Fontainebleau, Lake Annecy, Biarritz, Zermatt, Venice, and child portrait from life.)

The photographs that you are seeing needed approximately one minute

of exposure to sunlight. The series of photographic operations, developing, washing, final drying, takes about quarter of an hour. Most of these pictures, taken while travelling, were developed on the mantelpiece of a hotel room, which proves that the method is easy enough to carry out.

It nevertheless still remains to be perfected in some points. The length of exposure (one minute in sunlight) is still too long for the portrait. It was fifteen minutes when I first began my work. Progress may continue. Life is short and progress is slow.

Biography

Gabriel Lippmann was born of French parents at Hollerich, Luxembourg on August 16, 1845. The family moved to Paris and he received his early education at home. In 1858 he entered the Lycée Napoléon and ten years later he was admitted to the École Normale. His school career was not markedly successful, for he concentrated only on the work which interested him and neglected that which did not appeal to his taste, and he failed the examination which would have qualified him as a teacher. In 1873, he was appointed to a Government scientific mission visiting Germany to study methods for teaching science: he worked with Kühne and Kirchhoff in Heidelberg and with Helmholtz in Berlin.

Lippmann joined the Faculty of Science in Paris in 1878 and in 1883 he was appointed Professor of Mathematical Physics. Three years later he became Professor of Experimental Physics, succeeding Jamin, and he was appointed Director of the Research Laboratory which was subsequently transferred to the Sorbonne. He retained this position until his death.

Lippmann, of original and independent mind, made many valuable fundamental contributions to many different branches of physics, especially electricity, thermodynamics, optics and photochemistry. In Heidelberg he studied the relationship between electrical and capillary phenomena: this led to the development, amongst other instruments, of his extraordinarily sensitive capillary electrometer.

Professor Lippmann had evolved the general theory of his process for the photographic reproduction of colour in 1886 but the practical execution presented great difficulties. However, after years of patient and skilful experiment, he was able to communicate the process to the Academy of Sciences in 1891, although the photographs were somewhat defective due to the varying sensitivity of the photographic film. In 1893, he was able to present to the Academy photographs taken by A. and L. Lumière in which the colours were produced with perfect ortho-chromatism. He published the complete theory in 1894.

In 1895, Lippmann evolved a method of eliminating the personal equation

in measurements of time, using photographic registration, and he studied the eradication of irregularities of pendulum clocks, devising a method of comparing the times of oscillation of two pendulums of nearly equal period. He contributed to astronomy with his invention of the coelostat, a device which immobilizes the image of a star and its surrounding stars so that a photograph may be taken. He was also responsible for many more ingenious devices and improvements to standard instruments to the benefit of many branches of physics.

His work is mainly recorded in communications to the Paris Academy of Sciences where his papers are noted for their conciseness and originality. His method of reproducing colours in photography, based on the interference phenomenon, gained him the Nobel Prize for Physics for 1908.

Professor Lippmann became a member of the Academy of Sciences in 1883 and served as its President in 1912. He was a member of the Board of the Bureau des Longitudes and a Foreign Member of the Royal Society of London.

In 1888 Lippmann married the daughter of the writer V. Cherbuliez, member of the French Academy.

He died at sea on July 13, 1921, during his return from a journey to North America as a member of a mission headed by Marshal Fayolle.

Physics 1909

GUGLIELMO MARCONI

CARL FERDINAND BRAUN

«in recognition of their contributions to the development of wireless telegraphy»

Physics 1909

Presentation Speech by the former Rector General of National Antiquities H. Hildebrand, President of the Royal Swedish Academy of Sciences

Your Majesty, Your Royal Highnesses, Ladies and Gentlemen.

Research in physics has provided us with many surprises. Discoveries which at first seemed to have but theoretical interest have often led to inventions of the greatest importance to the advancement of mankind. And if this holds good for physics in general, it is even more true in the case of research in the field of electricity.

The discoveries and inventions for which the Royal Academy of Sciences has decided to award this year's Nobel Prize for Physics, also have their origin in purely theoretical work and study. Important and epoch-making, however, as these were in their particular fields, no one could have guessed at the start that they would lead to the practical applications witnessed later.

While we are, this evening, conferring Nobel's Prize upon two of the men who have contributed most to the development of wireless telegraphy, we must first register our admiration for those great research workers, now dead, who through their brilliant and gifted work in the fields of mathematical and experimental physics, opened up the path to great practical applications. It was Faraday with his unique penetrating power of mind, who first suspected a close connection between the phenomena of light and electricity, and it was Maxwell who transformed his bold concepts and thoughts into mathematical language, and finally, it was Hertz who through his classical experiments showed that the new ideas as to the nature of electricity and light had a real basis in fact. To be sure, it was already well known before Hertz's time, that a capacitor charged with electricity can under certain circumstances discharge itself oscillatorily, that is to say, by electric currents passing to and fro. Hertz, however, was the first to demonstrate that the effects of these currents propagate themselves in space with the velocity of light, thereby producing a wave motion having all the distinguishing characteristics of light. This discovery – perhaps the greatest in the field of physics throughout the last half-century – was made in 1888. It forms the foundation, not only for modern science of electricity, but also for wireless telegraphy.

But it was still a great step from laboratory trials in miniature where the electrical waves could be traced over but a small number of metres, to the transmission of signals over great distances. A man was needed who was able to grasp the potentialities of the enterprise and who could overcome all the various difficulties which stood in the way of the practical realization of the idea. The carrying out of this great task was reserved for Guglielmo Marconi. Even when taking into account previous attempts at this work and the fact that the conditions and prerequisites for the feasibility of this enterprise were already given, the honour of the first trials is nevertheless due, by and large, to Marconi, and we must freely acknowledge that the first success was gained as a result of his ability to shape the whole thing into a practical, usable system, added to his inflexible energy with which he pursued his self-appointed aim.

Marconi's first experiment to transmit a signal by means of Hertzian waves was carried out in 1895. During the 14 years which have elapsed since then, wireless telegraphy has progressed without pause until it has attained the great importance it possesses today. In 1897 it was still only possible to effect a wireless communication over a distance of 14–20 km. Today, electrical waves are despatched between the Old and the New World, all the larger ocean-going steamers have their own wireless telegraphy equipment on board, and every Navy of significance uses a system of wireless telegraphy. The development of a great invention seldom occurs through one individual man, and many forces have contributed to the remarkable results now achieved. Marconi's original system had its weak points. The electrical oscillations sent out from the transmitting station were relatively weak and consisted of wave-series following each other, of which the amplitude rapidly fell – so-called «damped oscillations». A result of this was that the waves had a very weak effect at the receiving station, with the further result that waves from various other transmitting stations readily interfered, thus acting disturbing at the receiving station. It is due above all to the inspired work of Professor Ferdinand Braun that this unsatisfactory state of affairs was overcome. Braun made a modification in the layout of the circuit for the despatch of electrical waves so that it was possible to produce intense waves with very little damping. It was only through this that the so-called «long-distance telegraphy» became possible, where the oscillations from the transmitting station, as a result of resonance, could exert the maximum possible effect upon the receiving station. The further advantage was obtained that in the main only waves of the frequency used by the transmitting station

were effective at the receiving station. It is only through the introduction of these improvements that the magnificent results in the use of wireless telegraphy have been attained in recent times.

Research workers and engineers toil unceasingly on the development of wireless telegraphy. Where this development can lead, we know not. However, with the results already achieved, telegraphy over wires has been extended by this invention in the most fortunate way. Independent of fixed conductor routes and independent of space, we can produce connections between far-distant places, over far-reaching waters and deserts. This is the magnificent practical invention which has flowered upon one of the most brilliant scientific discovery of our time!

GUGLIELMO MARCONI

Wireless telegraphic communication

Nobel Lecture, December 11, 1909

The discoveries connected with the propagation of electric waves over long distances and the practical applications of telegraphy through space, which have gained for me the high honour of sharing the Nobel Prize for Physics, have been to a great extent the results of one another.

The application of electric waves to the purposes of wireless telegraphic communication between distant parts of the earth, and the experiments which I have been fortunate enough to be able to carry out on a larger scale than is attainable in ordinary laboratories, have made it possible to investigate phenomena and note results often novel and unexpected.

In my opinion many facts connected with the transmission of electric waves over great distances still await a satisfactory explanation, and I hope to be able in this lecture to refer to some observations, which appear to require the attention of physicists.

In sketching the history of my association with radiotelegraphy, I might mention that I never studied physics or electrotechnics in the regular manner, although as a boy I was deeply interested in those subjects.

I did, however, attend one course of lectures on physics under the late Professor Rosa at Livorno, and I was, I think I might say, fairly well acquainted with the publications of that time dealing with scientific subjects including the works of Hertz, Branly, and Righi.

At my home near Bologna, in Italy, I commenced early in 1895 to carry out tests and experiments with the object of determining whether it would be possible by means of Hertzian waves to transmit to a distance telegraphic signs and symbols without the aid of connecting wires.

After a few preliminary experiments with Hertzian waves I became very soon convinced, that if these waves or similar waves could be reliably transmitted and received over considerable distances a new system of communication would become available possessing enormous advantages over flashlights and optical methods, which are so much dependent for their success on the clearness of the atmosphere.

My first tests were carried out with an ordinary Hertz oscillator and a

Branly coherer as detector, but I soon found out that the Branly coherer was far too erratic and unreliable for practical work.

After some experiments I found that a coherer constructed as shown in Fig. 1, and consisting of nickel and silver filings placed in a small gap between two silver plugs in a tube, was remarkably sensitive and reliable. This improvement together with the inclusion of the coherer in a circuit tuned to the wavelength of the transmitted radiation, allowed me to gradually extend up to about a mile the distance at which I could affect the receiver.

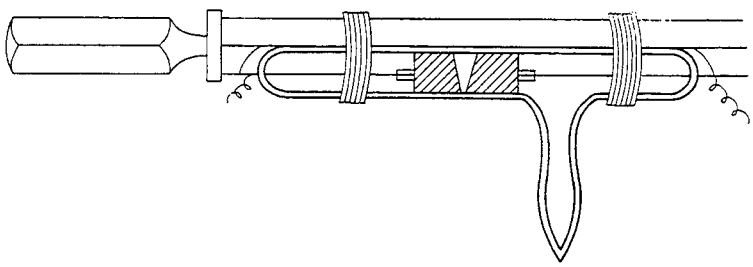


Fig. 1.

Another, now well-known, arrangement which I adopted was to place the coherer in a circuit containing a voltaic cell and a sensitive telegraph relay actuating another circuit, which worked a tapper or trembler and a recording instrument. By means of a Morse telegraphic key placed in one of the circuits of the oscillator or transmitter it was possible to emit long or short successions of electric waves, which would affect the receiver at a distance and accurately reproduce the telegraphic signs transmitted through space by the oscillator.

With such apparatus I was able to telegraph up to a distance of about half a mile.

Some further improvements were obtained by using reflectors with both the transmitters and receivers, the transmitter being in this case a Righi oscillator.

This arrangement made it possible to send signals in one definite direction, but was inoperative if hills or any large obstacle happened to intervene between the transmitter and receiver.

In August 1895 I discovered a new arrangement which not only greatly increased the distance over which I could communicate, but also seemed to make the transmission independent from the effects of intervening obstacles.

This arrangement (Figs. 2 and 3) consisted in connecting one terminal of the Hertzian oscillator, or spark producer, to earth and the other terminal to a wire or capacity area placed at a height above the ground, and in also connecting at the receiving end one terminal of the coherer to earth and the other to an elevated conductor.

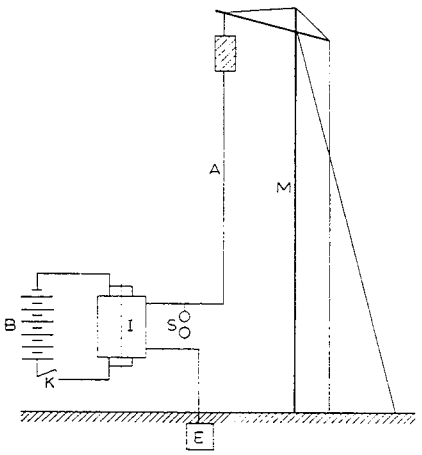


Fig. 2.

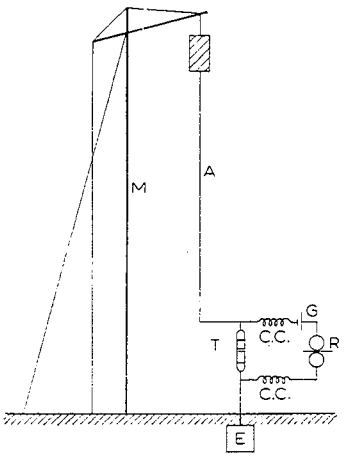


Fig. 3.

I then began to examine the relation between the distance at which the transmitter could affect the receiver and the elevation of the capacity areas above the earth, and I very soon definitely ascertained that the higher the wires or capacity areas, the greater the distance over which it was possible to telegraph.

Thus I found that when using cubes of tin of about 30 cm side as elevated conductors or capacities, placed at the top of poles 2 meters high, I could receive signals at 30 meters distance, and when placed on poles 4 meters high, at 100 meters, and at 8 meters high at 400 meters. With larger cubes 100 cm side, fixed at a height of 8 meters, signals could be transmitted 2,400 meters all round.[1]

These experiments were continued in England, where in September 1896 a distance of $1\frac{3}{4}$ miles was obtained in tests carried out for the British Government at Salisbury. The distance of communication was extended to 4 miles in March 1897, and in May of the same year to 9 miles. Tape messages obtained during these tests, signed by the British Government Officers who were present, are exhibited.[2]

In all these experiments a very small amount of electrical power was used,

the high tension current being produced by an ordinary Rhumkorff coil.

The results obtained attracted a good deal of public attention at the time, such distances of communication being considered remarkable.

As I have explained, the main feature in my system consisted in the use of elevated capacity areas or antennae attached to one pole of the high frequency oscillators and receivers, the other pole of which was earthed.

The practical value of this innovation was not understood by many physicists[3] for quite a considerable period, and the results which I obtained were by many erroneously considered simply due to efficiency in details of construction of the receiver, and to the employment of a large amount of energy.

Others did not overlook the fact that a radical change had been introduced by making these elevated capacities and the earth form part of the high frequency oscillators and receivers.

Prof. Ascoli of Rome gave a very interesting theory of the mode of operation of my transmitters and receivers in the *Elettricista (Rome)* issue of August 1897, in which he correctly attributed the results obtained to the use of elevated wires or antennae.

Prof. A. Slaby of Charlottenburg, after witnessing my tests in England in 1897, came to somewhat similar conclusions.[4]

Many technical writers have stated that an elevated capacity at the top of the vertical wire is unnecessary.

This is true if the length or height of the wire is made sufficiently great, but as this height may be much smaller for a given distance if a capacity area is used, it is more economical to use such capacities, which now usually consist of a number of wires spreading out from the top of the vertical conductor.

The necessity or utility of the earth connection has been sometimes questioned, but in my opinion no practical system of wireless telegraphy exists where the instruments are not connected to earth.

By « connected to earth » I do not necessarily mean an ordinary metallic connection as used for ordinary wire telegraphs.

The earth wire may have a condenser in series with it, or it may be connected to what is really equivalent, a capacity area placed close to the surface of the ground (Fig. 4).

It is now perfectly well known that a condenser, if large enough, does not prevent the passage of high frequency oscillations, and therefore in these cases the earth is for all practical purposes connected to the antennae.

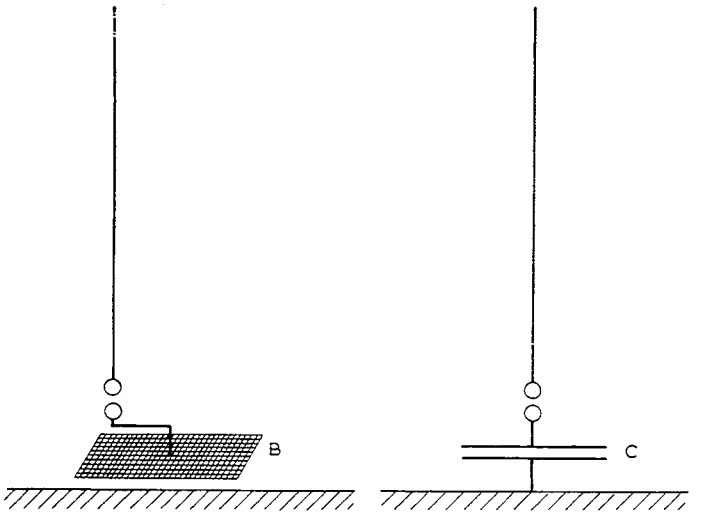


Fig. 4.

After numerous tests and demonstrations in Italy and England over distances varying up to 40 miles, communication was established for the first time across the English Channel between England and France[5] in March 1899 (Fig. 5).

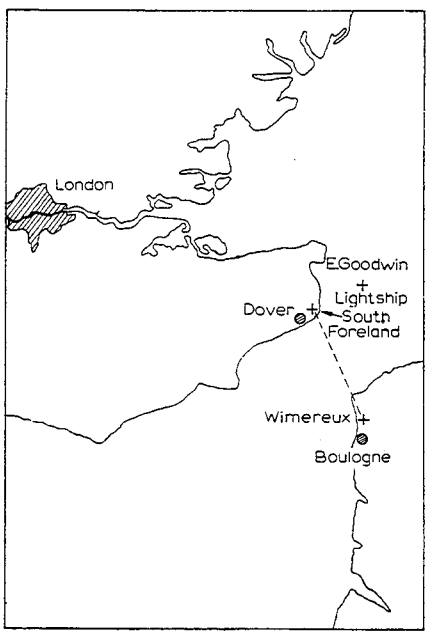


Fig. 5.

From the beginning of 1898 I had practically abandoned the system of connection shown in Fig. 2, and instead of joining the coherer or detector directly to the aerial and earth, I connected it between the ends of the secondary of a suitable oscillation transformer containing a condenser and tuned to the period of the electrical waves received. The primary of this oscillation transformer was connected to the elevated wire and to earth (See Fig. 6.)

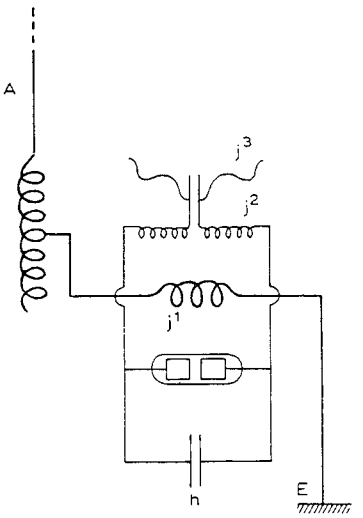


Fig. 6.

This arrangement allowed of a certain degree of syntony, as by varying the period of oscillation of the transmitting antennae, it was possible to send messages to a tuned receiver without interfering with others differently syntonized.[6]

As is now well known, a transmitter consisting of a vertical wire discharging through a spark gap is not a very persistent oscillator, the radiation it produces being considerably damped. Its electrical capacity is comparatively so small and its capability of radiating energy so large, that the oscillations decrease or die off with rapidity. In this case receivers or resonators of a considerably different period or pitch are likely to be affected by it.

Early in 1899 I was able to improve the resonance effects obtainable by increasing the capacity of the elevated wires by placing adjacently to them earthed conductors, and inserting in series with the aerials suitable inductance coils.[7]

By these means the energy-storing capacity of the aerial was increased,

whilst its capability to radiate was decreased, with the result that the energy set in motion by the discharge formed a train or succession of feebly damped oscillations.

A modification of this arrangement, by which excellent results were obtained, is shown in Fig. 7.

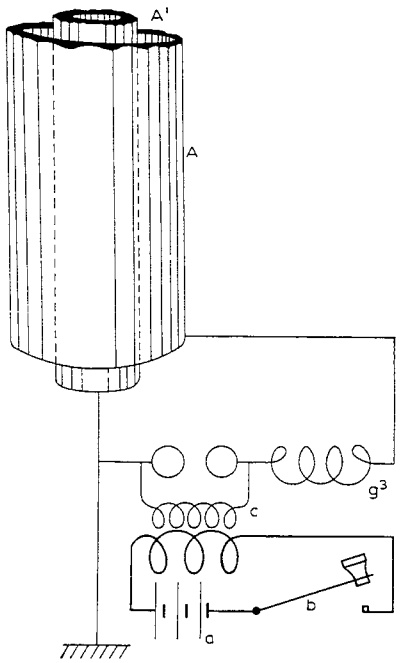


Fig. 7.

In 1900 I constructed and patented a complete system of transmitters and receivers[8] which consisted of the usual kind of elevated capacity area and earth connection, but these were inductively coupled to an oscillation circuit containing a condenser, an inductance, and a spark gap or detector, the conditions which I found essential for efficiency being that the periods of electrical oscillation of the elevated wire or conductor should be in tune or resonance with that of the condenser circuit, and that the two circuits of the receiver should be in electrical resonance with those of the transmitter (Fig. 8).

The circuits consisting of the oscillating circuit and the radiating circuit were more or less closely « coupled » by varying the distance between them.

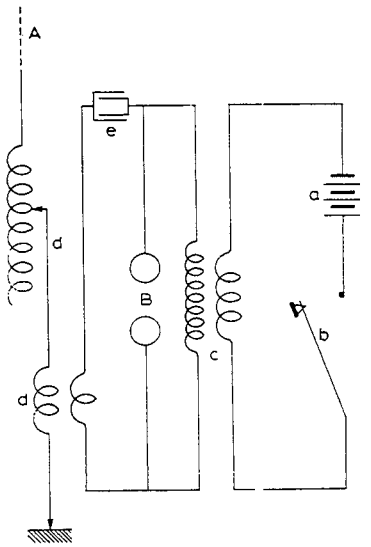


Fig. 8.

By the adjustment of the inductance inserted in the elevated conductor and by the variation of capacity of the condenser circuit, the two circuits were brought into resonance, a condition which, as I have said, I found essential in order to obtain efficient radiation.

Part of my work regarding the utilization of condenser circuits in association with the radiating antennae was carried out simultaneously to that of Prof. Braun, without, however, either of us knowing at the time anything of the contemporary work of the other.

A syntonic receiver has already been shown in Fig. 6, and consists of a vertical conductor or aerial connected to earth through the primary of an oscillation transformer, the secondary circuit of which included a condenser and a detector, it being necessary that the circuit containing the aerial and the circuit containing the detector should be in electrical resonance with each other, and also in tune with the periodicity of the electric waves transmitted from the sending station.

In this manner it was possible to utilize electric waves of low decrement and cause the receiver to integrate the effect of comparatively feeble but properly timed electrical oscillations in the same way as in acoustics two tuning forks can be made to affect each other at short distances if tuned to the same period of vibration.

It is also possible to couple to one sending conductor several differently tuned transmitters and to a receiving wire a number of corresponding receivers, as is shown in Figs. 9 and 10, each individual receiver responding

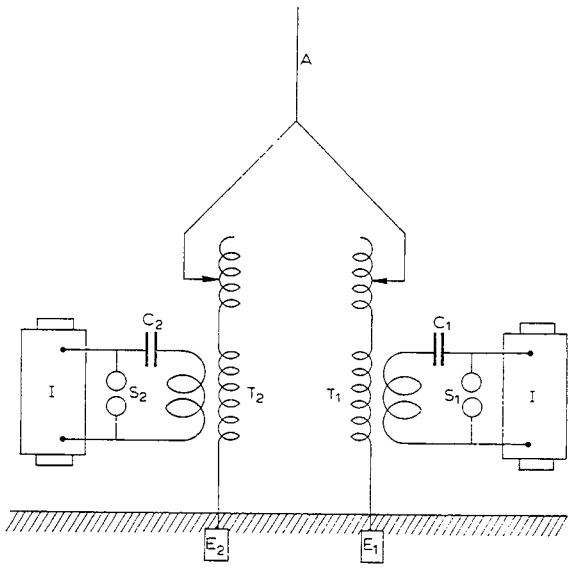


Fig. 9.

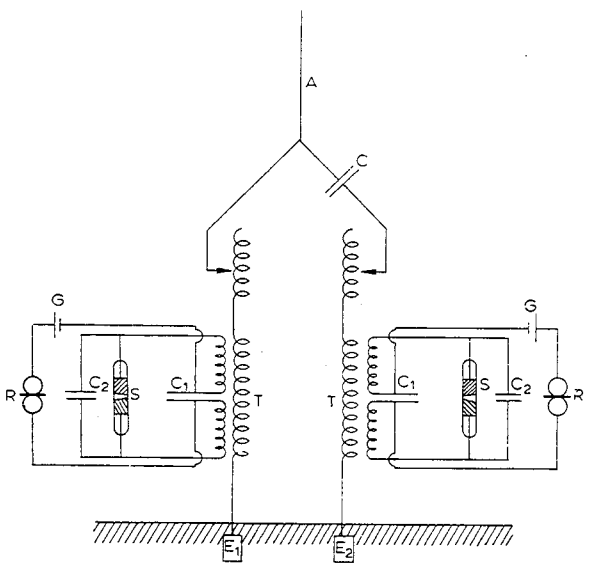


Fig. 10.

only to the radiations of the transmitter with which it is in resonance.[9]

At the time (twelve years ago) when communication was first established by means of radiotelegraphy between England and France, much discussion and speculation took place as to whether or not wireless telegraphy would be practicable for much longer distances than those then covered, and a somewhat general opinion prevailed that the curvature of the Earth would be an insurmountable obstacle to long distance transmission, in the same way as it was, and is, an obstacle to signalling over considerable distances by means of light flashes.

Difficulties were also anticipated as to the possibility of being able to control the large amount of energy which it appeared would be necessary to cover long distances.

What often happens in pioneer work repeated itself in the case of radiotelegraphy, the anticipated obstacles or difficulties were either purely imaginary or else easily surmountable, but in their place unexpected barriers manifested themselves, and recent work has been mainly directed to the solution of problems presented by difficulties which were certainly neither expected nor anticipated when long distances were first attempted.

With regard to the presumed obstacle of the curvature of the Earth, I am of opinion that those who anticipated difficulties in consequence of the shape of our planet had not taken sufficient account of the particular effect of the earth connection to both transmitter and receiver, which earth connection introduced effects of conduction which were generally at that time overlooked.

Physicists seemed to consider for a long time that wireless telegraphy was solely dependent on the effects of free Hertzian radiation through space, and it was years before the probable effect of the conductivity of the Earth between the stations was satisfactorily considered or discussed.

Lord Rayleigh, in referring to transatlantic telegraphy, stated in May 1903: « The remarkable success of Marconi in signalling across the Atlantic

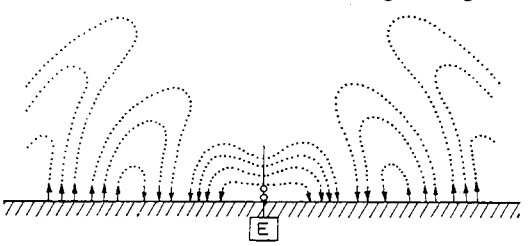


Fig. 11.

suggests a more decided bending or diffraction of the waves round the perturberant Earth than had been expected, and it imparts a great interest to the theoretical problem[10].

Prof. J. A. Fleming, in his book on *The Principles of Electric Wave Telegraphy*[11], gives diagrams showing what is now believed to be the diagrammatic representation of the detachment of semi-loops of electric strain from a simple vertical wire (Fig. 11). As will be seen, these waves do not propagate in the same manner as free radiation from a classical Hertzian oscillator, but glide along the surface of the Earth.

Prof. Fleming further states in the above quoted work:

« The view we here take is that the ends of the semi-loops of electric force, which terminate perpendicularly on the Earth, cannot move along unless there are movements of electrons in the Earth corresponding to the wave-motions above it. From the point of view of the electronic theory of electricity, every line of electric force in the ether must be either a closed line or its ends must terminate on electrons of opposite sign. If the end of a line of strain abuts on the Earth and moves, there must be atom-to-atom exchange of electrons, or movements of electrons in it. We have many reasons for concluding that the substances we call conductors are those in which free movements of electrons can take place. Hence the movements of the semi-loops of electric force outwards from an earthed oscillator or Marconi aerial is hindered by bad conductivity on the surface of the Earth and facilitated over the surface of a fairly good electrolyte, such as sea-water. »

Prof. Zenneck[12] has carefully examined the effect of earthed transmitting and receiving aerials, and has endeavoured to show mathematically that when the lines of electrical force, constituting a wave front, pass along a surface of low specific inductive capacity, such as the Earth, they become inclined forward, their lower ends being retarded by the resistance of the conductor to which they are attached.

It therefore seems well established that wireless telegraphy, as practised at the present day, is dependent for its operation over long distances on the conductivity of the Earth, and that the difference in conductivity between the surface of the sea and land is sufficient to explain the increased distance obtainable with the same amount of energy in communicating over sea as compared to over land.

I carried out some tests between a shore station and a ship at Poole, in England, in 1902, for the purpose of obtaining some data on this point, and I noticed that at equal distances a perceptible diminution in the energy of the

received waves always occurred when the ship was in such a position as to allow a low spit of sand about 1 kilometer broad to intervene between it and the land station.

I therefore believe that there was some foundation for the statement so often criticized which I made in my first English Patent of June 2, 1896 to the effect that when transmitting through the earth or water I connected one end of the transmitter and one end of the receiver to earth.

In January 1901 some successful experiments[13] were carried out between two points on the South Coast of England 186 miles apart, i.e. St. Catherines' Point (Isle of Wight) and The Lizard in Cornwall (Fig. 12).

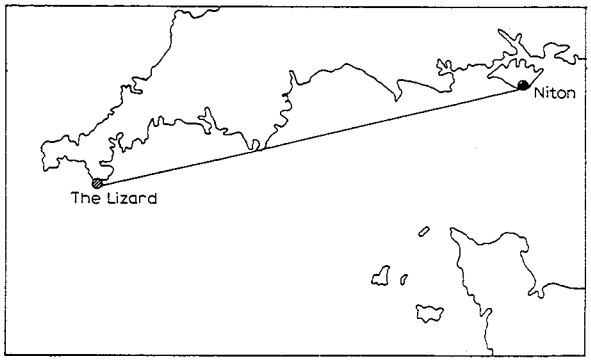


Fig. 12.

The total height of these stations above sea level did not exceed 100 meters, whereas to clear the curvature of the Earth a height of more than 1,600 meters at each end would have been necessary.

The results obtained from these tests, which at the time constituted a record distance, seemed to indicate that electric waves produced in the manner I had adopted would most probably be able to make their way round the curvature of the Earth, and that therefore even at great distances, such as those dividing America from Europe, the factor of the Earth's curvature would not constitute an insurmountable barrier to the extension of telegraphy through space.

The belief that the curvature of the Earth would not stop the propagation of the waves, and the success obtained by syntonic methods in preventing mutual interference, led me in 1900 to decide to attempt the experiment of testing whether or not it would be possible to detect electric waves over a

distance of 4,000 kilometers, which, if successful, would immediately prove the possibility of telegraphing without wires between Europe and America.

The experiment was in my opinion of great importance from a scientific point of view, and I was convinced that the discovery of the possibility to transmit electric waves across the Atlantic Ocean, and the exact knowledge of the real conditions under which telegraphy over such distances could be carried out, would do much to improve our understanding of the phenomena connected with wireless transmission.

The transmitter erected at Poldhu, on the coast of Cornwall, was similar in principle to the one I have already referred to, but on a very much larger scale than anything previously attempted.[14]

The power of the generating plant was about 25 kilowatts.

Numerous difficulties were encountered in producing and controlling for the first time electrical oscillations of such power. In much of the work I obtained valuable assistance from Prof. J. A. Fleming, Mr. R. N. Vyvyan, and Mr. W. S. Entwistle.

My previous tests had convinced me that when endeavouring to extend the distance of communication, it was not merely sufficient to augment the power of the electrical energy of the sender, but that it was also necessary to increase the area or height of the transmitting and receiving elevated conductors.

As it would have been too expensive to employ vertical wires of great height, I decided to increase their number and capacity, which seemed likely to make possible the efficient utilization of large amounts of energy.

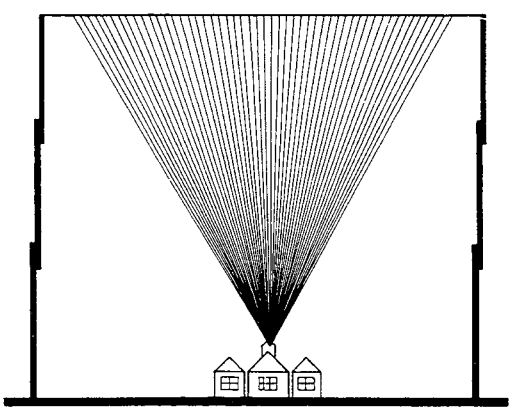


Fig. 13.

The arrangement of transmitting antennae which was used at Poldhu is shown in Fig. 13, and consisted of a fan-like arrangement of wires supported by an insulated stay between masts only 48 meters high and 60 meters apart. These wires converged together at the lower end and were connected to the transmitting apparatus contained in a building.

For the purpose of the test a powerful station had been erected at Cape Cod, near New York, but the completion of the arrangements at that station were delayed in consequence of a storm which destroyed the masts and antennae.

I therefore decided to try the experiments by means of a temporary receiving station erected in Newfoundland, to which country I proceeded with two assistants about the end of November 1901.

The tests were commenced early in December 1901 and on the 12th of that month the signals transmitted from England were clearly and distinctly received at the temporary station at St. John's in Newfoundland.

Confirmatory tests were carried out in February 1902 between Poldhu and a receiving station on the S.S. « Philadelphia » of the American Line. On board this ship readable messages were received by means of a recording instrument up to a distance of 1,551 miles and test letters as far as 2,099 miles from Poldhu (Fig. 14).

The tape records obtained on the « Philadelphia » at the various distances were exceedingly clear and distinct, as can be seen by the specimens exhibited.

These results, although achieved with imperfect apparatus, were sufficient to convince me and my co-workers that by means of permanent stations and the employment of sufficient power it would be possible to transmit messages across the Atlantic Ocean in the same way as they were sent over much shorter distances.

The tests could not be continued in Newfoundland owing to the hostility of a cable company, which claimed all rights for telegraphy, whether wireless or otherwise, in that colony.

A result of scientific interest which I first noticed during the tests on S.S. « Philadelphia » and which is a most important factor in long distance radiotelegraphy, was the very marked and detrimental effect of daylight on the propagation of electric waves at great distances, the range by night being usually more than double that attainable during daytime.[15]

I do not think that this effect has yet been satisfactorily investigated or explained. At the time I carried out the tests I was of opinion that it might be

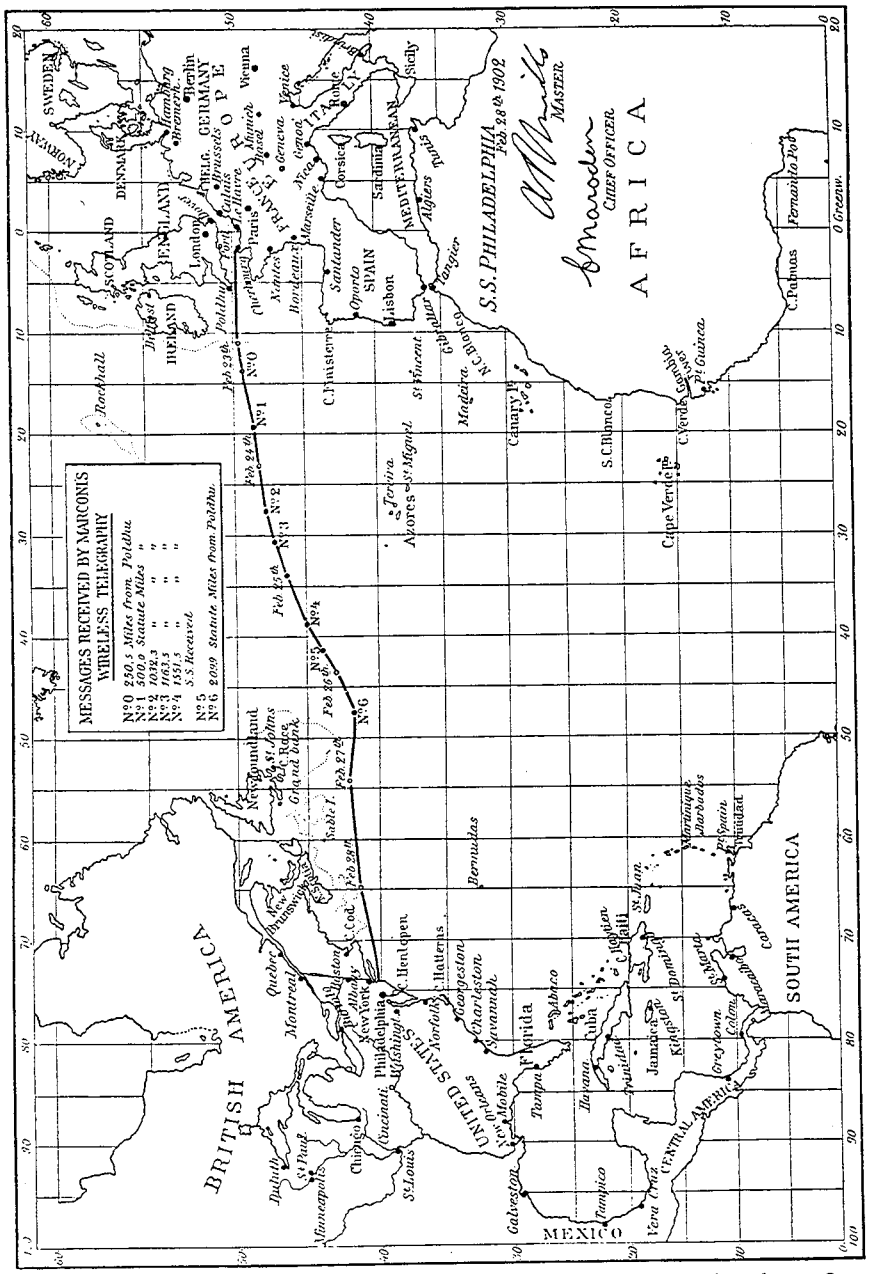


Fig. 14.

due to the loss of energy at the transmitter, caused by the dis-electrification of the highly charged transmitting elevated conductor under the influence of sunlight.

I am now inclined to believe that the absorption of electric waves during the daytime is due to the electrons propagated into space by the sun, and that if these are continually falling like a shower upon the earth, in accordance with the hypothesis of Prof. Arrhenius, then that portion of the Earth's atmosphere which is facing the sun will have in it more electrons than the part which is not facing the sun, and therefore it may be less transparent to electric waves.

Sir J. J. Thomson has shown in an interesting paper in the *Philosophical Magazine* that if electrons are distributed in a space traversed by electric waves, these will tend to move the electrons in the direction of the wave, and will therefore absorb some of the energy of the wave. Hence, as Prof. Fleming has pointed out in his Cantor Lectures delivered at the Society of Arts, a medium through which electrons or ions are distributed acts as a slightly turbid medium to long electric waves.[16]

Apparently the length of wave and amplitude of the electrical oscillations have much to do with this interesting phenomenon, long waves and small amplitudes being subject to the effect of daylight to a much lesser degree than short waves and large amplitudes.

According to Prof. Fleming[17] the daylight effect should be more marked on long waves, but this has not been my experience. Indeed, in some very recent experiments in which waves of about 8,000 meters long were used, the energy received by day was usually greater than at night.

The fact remains, however, that for comparatively short waves, such as are used for ship communication, clear sunlight and blue skies, though transparent to light, act as a kind of fog to these waves. Hence the weather conditions prevailing in England, and perhaps in this country, are usually suitable for wireless telegraphy.

During the year 1902 I carried out some further tests between the station at Poldhu and a receiving installation erected on the Italian Cruiser « Carlo Alberto », kindly placed at my disposal by H.M. The King of Italy. (See Fig. 15.[18])

During these experiments the interesting fact was observed that, even when using waves as short as 1,000 feet, intervening ranges of mountains, such as the Alps or Pyrenees, did not, during the night time, bring about any considerable reduction in the distance over which it was possible to communicate. During daytime, unless much longer waves and more power were used, intervening mountains greatly reduced the apparent range of the transmitter.

Messages and press despatches of considerable length were received from

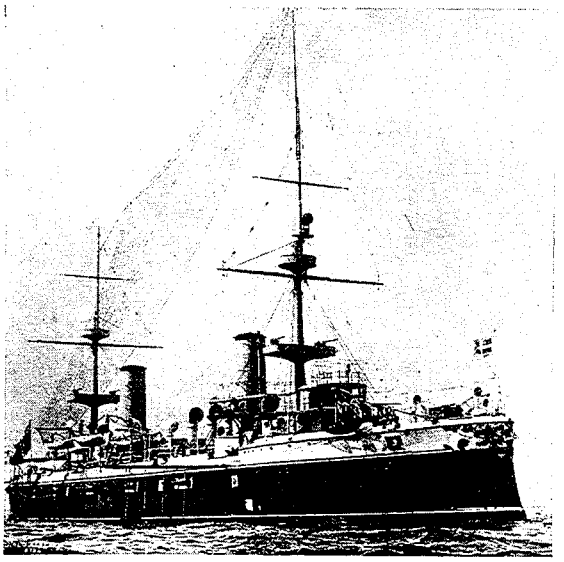


Fig. 15.

Poldhu at the positions marked on the map, which map is a copy, on a reduced scale, of the one accompanying the official report of the experiments (Fig. 16).

With the active encouragement and financial assistance of the Canadian Government, a high power station was constructed at Glace Bay, Nova Scotia, in order that I should be able to continue my long-distance tests with a view to establishing radiotelegraphic communication on a commercial basis between England and America.[19]

On December 16, 1902 the first official messages were exchanged at night across the Atlantic, between the stations at Poldhu and Glace Bay (Figs. 17 and 18).

Further tests were shortly afterwards carried out with another long-distance station at Cape Cod in the United States of America, and under favourable circumstances it was found possible to transmit messages to Poldhu 3,000 miles away with an expenditure of electrical energy of only about 10 kilowatts.

In the spring of 1903 the transmission of press messages by radiotelegraphy from America to Europe was attempted, and for a time the London *Times* published, during the latter part of March and the early part of April of that year, news messages from its New York correspondent sent across the Atlantic without the aid of cables.

A breakdown in the insulation of the apparatus at Glace Bay made it necessary, however, to suspend the service and unfortunately further accidents made the transmission of messages uncertain and unreliable.

As a result of the data and experience gained by these and other tests which I carried out for the British Government, between England and Gibraltar, I was able to erect a new station at Clifden in Ireland, and enlarge the one at Glace Bay in Canada, so as to enable me to initiate, in October 1907, communication for commercial purposes across the Atlantic between England and Canada.

Although the stations at Clifden and Glace Bay had to be put into operation before they were altogether complete, nevertheless communication across the Atlantic by radiotelegraphy never suffered any serious interruption during nearly two years, until, in consequence of a fire at Glace Bay this autumn, it has had to be suspended for three or four months.

This suspension has not, however, been altogether an unmitigated evil, as

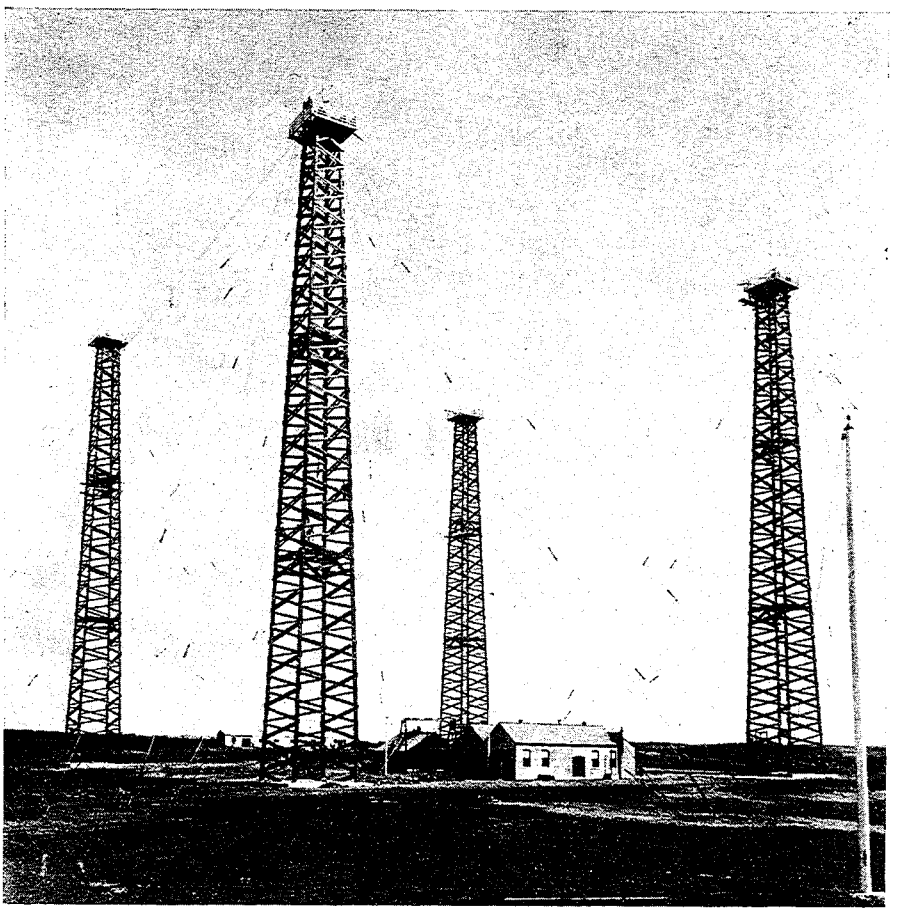


Fig. 17.

it has given me the opportunity of installing more efficient and up-to-date machinery.

The arrangements of elevated conductors or aerials which I have tried[20] during my long-distance tests, are shown in Figs. 19, 20 and 21.

The aerial shown in Fig. 21 consisted of a nearly vertical portion in the middle, 220 feet high, supported by four towers, and attached at the top to nearly horizontal wires, 200 in number and each 1,000 feet long, extending radially all round and supported at a height of 180 feet from the ground by an inner circle of 8, and an outer circle of 16 masts.

The natural period of oscillation of this aerial system gave a wavelength of 12,000 feet. Experiments were made with this arrangement in 1905 and with

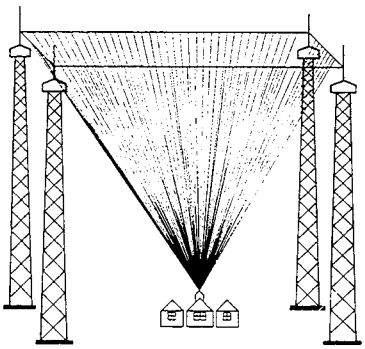


Fig. 19.

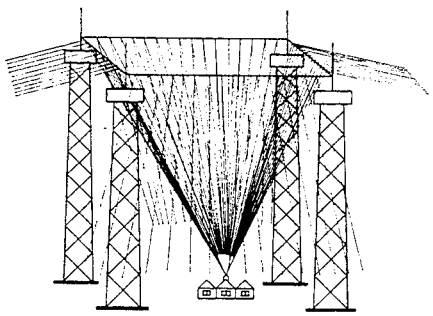


Fig. 20.

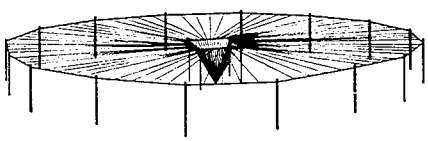


Fig. 21.

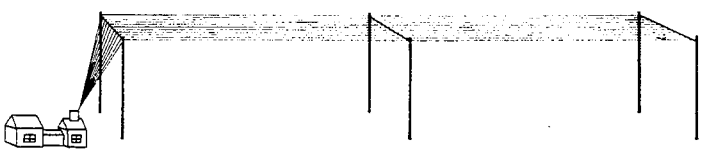


Fig. 22.

cordance with a comparatively recent system for producing continuous or slightly damped oscillations, which I referred to in a lecture before the Royal Institution of Great Britain on March 13, 1908.

An insulated metal disc A (see Fig. 23) is caused to rotate at a high rate of speed by means of an electric motor or steam turbine. Adjacent to this disc, which I will call the middle disc, are placed two other discs C′ and C″ which may be called polar discs, and which are also revolved. These polar discs have their peripheries very close to the surface or edges of the middle disc. The two polar discs are connected by rubbing contacts to the outer ends of two condensers K, joined in series, and these condensers are also connected through suitable brushes to the terminals of a generator which should be a high-tension continuous-current generator.

On the middle disc a suitable brush or rubbing contact is provided and between this contact and the middle point of the two condensers an oscillating circuit is inserted, consisting of a condenser E in series with an in-

ductance, which last is inductively connected with the radiating antennae.

The apparatus works probably in the following manner:

The generator charges the double condenser, making the potential of the discs, say C′ positive and C″ negative. The potential, if high enough will cause a discharge to pass across one of the gaps, say between C′ and A. This charges the condenser E through the inductance F, and starts oscillations in the circuit. The charge of F in swinging back will jump from A to C″, the potential of which is of opposite sign to A, the dielectric strength between C′ and A having meanwhile been restored by the rapid motion of the disc, driving away the ionized air.

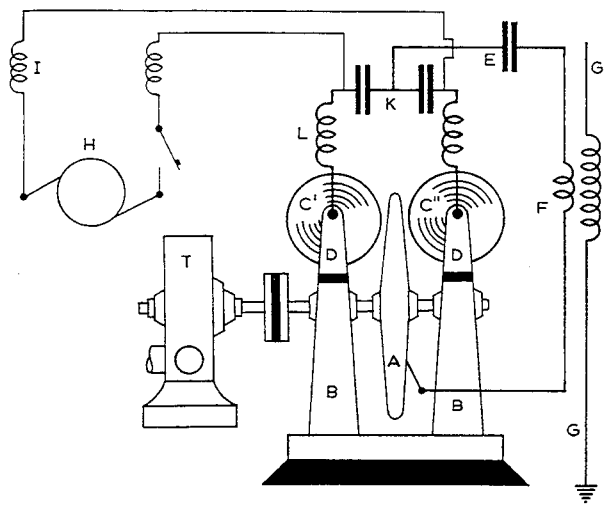


Fig. 23.

The condenser E therefore discharges and recharges alternatively in reverse directions, the same process going on so long as energy is supplied to the condensers K by the generator H.

It is clear that the discharges between C′ and C″ and A are never simultaneous as otherwise the centre electrode would not be alternatively positive and negative.

The best results have, however, been obtained by an arrangement as shown in Fig. 24, in which the active surface of the middle disc is not smooth, but consists of a number of regularly spaced copper knobs or pegs, at the ends of which the discharges take place at regular intervals. I have found that with this arrangement each train of oscillations may have a decrement as low as 0.02.

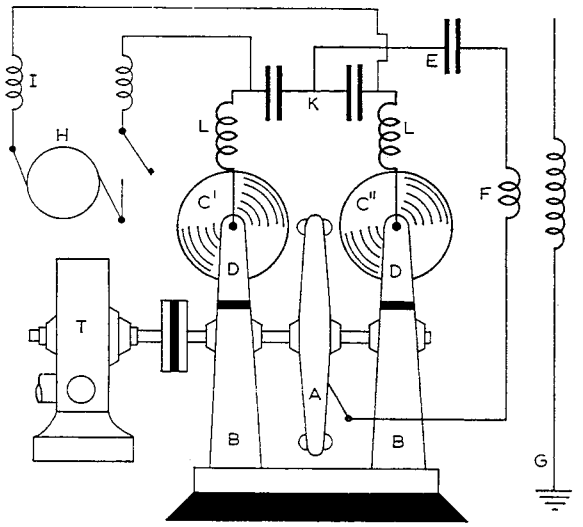


Fig. 24.

In this way it is also possible to cause the groups of oscillations radiated to reproduce a high and clear musical note in a receiver, thereby making it easy to differentiate between the signals emanating from the sending station and noises caused by atmospheric electrical discharges. By this method very efficient resonance can be also obtained in appropriately designed receivers.

With regard to the receivers employed, important changes have taken place. By far the larger portion of electric wave telegraphy was, until a few years ago, conducted by means of some form or other of coherer, or variable contact either requiring tapping or else self-restoring

in iron when under certain conditions this metal is subjected to the effects of electrical waves of high frequency.

It has recently been found possible to increase the sensitiveness of these receivers, and to employ them in connection with a high-speed relay, so as to record messages at great speed.

A remarkable fact, not generally known, in regard to transmitters is, that none of the arrangements employing condensers exceed in efficiency the plain elevated aerial or vertical wire discharging to Earth through a spark gap, as used in my first experiments (see Figs. 2 and 3).

I have also recently been able to confirm the statement made by Prof. Fleming in his book *The Principles of Electric Wave Telegraphy*, 1906, page 555, that with a power of 8 watts in the aerial it is possible to communicate to distances of over 100 miles.

I have also found that by this method, when using large aerials, it is possible to send signals 2,000 miles across the Atlantic, with a smaller expenditure of energy than by any other method known to myself.

The only drawback to this arrangement is, that unless very large aerials are used, the amount of energy which can be efficiently employed is limited by the potential beyond which brush discharges and the resistance of the spark gap begin to dissipate a large proportion of the energy.

By means of spark gaps in compressed air and the addition of inductance coils placed between the aerial and earth, the system can be made to radiate very pure and slightly damped waves, eminently suitable for sharp tuning.

In regard to the general working of wireless telegraphy, the widespread application of the system and the multiplicity of the stations have greatly facilitated the observation of facts not easily explainable.

Thus it has been observed that an ordinary ship station, utilizing about $\frac{1}{2}$ kilowatt of electrical energy, the normal range of which is not greater than

Although high power stations are now used for communicating across the Atlantic, and messages can be sent by day as well as by night, there still exist short periods of daily occurrence, during which transmission from England to America, or vice versa, is difficult. Thus in the morning and evening, when in consequence of the difference in longitude, daylight or darkness extends only part of the way across the ocean, the received signals are weak and sometimes cease altogether. It would almost appear as if electric waves in passing from dark space to illuminated space, and vice versa, were reflected in such a manner as to be deviated from their normal path.

It is probable that these difficulties would not be experienced in telegraphing over equal distances north and south, on about the same meridian, as in this case the passage from daylight to darkness would occur almost simultaneously over the whole distance between the two points.

Another curious result, on which hundreds of observations continued for years leave no further doubt, is that regularly, for short periods, at sunrise and sunset, and occasionally at other times, a shorter wave can be detected across the Atlantic in preference to the longer wave normally employed.

Thus at Clifden and Glace Bay when sending on an ordinary coupled circuit arranged so as to simultaneously radiate two waves, one 12,500 feet and the other 14,700 feet, although the longer wave is the one usually received at the other side of the ocean, regularly, about three hours after sunset at Clifden, and three hours before sunrise at Glace Bay, the shorter wave alone was received with remarkable strength, for a period of about one hour.

This effect occurred so regularly that the operators tuned their receivers to the shorter wave at the times mentioned, as a matter of ordinary routine.

With regard to the utility of wireless telegraphy there is no doubt that its use has become a necessity for the safety of shipping, all the principal liners and warships being already equipped, its extension to less important ships being only a matter of time, in view of the assistance it has provided in cases of danger.

Its application is also increasing as a means of communicating between outlying islands, and also for the ordinary purposes of telegraphic communication between villages and towns, especially in the colonies and in newly developed countries.

However great may be the importance of wireless telegraphy to ships and shipping, I believe it is destined to an equal position of importance in furnishing efficient and economical communication between distant parts of the world and in connecting European countries with their colonies and with

America. As a matter of fact, I am at the present time erecting a very large power station for the Italian Government at Coltano, for the purpose of communicating with the Italian colonies in East Africa, and with South America.

Whatever may be its present shortcomings and defects, there can be no doubt that wireless telegraphy – even over great distances – has come to stay, and will not only stay, but continue to advance.

If it should become possible to transmit waves right round the world, it may be found that the electrical energy travelling round all parts of the globe may be made to concentrate at the antipodes of the sending station. In this way it may some day be possible for messages to be sent to such distant lands by means of a very small amount of electrical energy, and therefore at a correspondingly small expense.

But I am leaving the regions of fact, and entering the regions of speculation, which, however, with the knowledge we have gradually gained on the subject, promise results both useful and instructive.

Not having the fortune of being conversant with the Swedish language, I have thought it best, although an Italian, to use the medium of the English language in delivering this address, as I know that English is more generally understood here than Italian.

1. G. Marconi, *Brit. Patent* No. 12,039, June 2, 1896.
2. *J. Inst. Elec. Engrs. (London)*, 28 (1899) 278.
3. See letter of Dr. Lodge in *The Times (London)*, June 22, 1897.
4. A. Slaby, *Die Funkentelegraphie*, Verlag von Leonhardt Simion, Berlin, 1897; see also A. Slaby, «The New Telegraphy», *The Century Magazine*, 55 (1898) 867.
5. *J. Inst. Elec. Engrs. (London)*, 28 (1899) 291.
6. *Brit. Patent* No. 12,326, June 1, 1898; *Brit. Patent* No. 6,982, April 1, 1899.
7. A. Blondel and G. Ferrie, «État actuel et Progrès de la Télégraphie sans Fil», read

15. G. Marconi, «A Note on the Effect of Daylight upon the Propagation of Electromagnetic Impulses», *Proc. Roy. Soc. (London)*, 70 (June 12, 1902).
16. See also J. J. Thomson, «On Some Consequences, etc.», *Phil. Mag.*, [6] 4 (1902) 253.
17. See Ref. 11, p. 618.
18. *Riv. Marittima (Rome)*, October 1902.
19. G. Marconi, Paper read before the Royal Institution of Great Britain, March 3, 1905.
20. See also G. Marconi, Lecture before the Royal Institution of Great Britain, March 13, 1908.
21. G. Marconi, «On Methods whereby the Radiation of Electric Waves may be mainly confined, etc.», *Proc. Roy. Soc. (London)*, A 77 (1906).
22. G. Marconi, «Note on a Magnetic Detector of Electric Waves», *Proc. Roy. Soc. (London)*, 70 (1902) 341.

Biography

Guglielmo Marconi was born at Bologna, Italy, on April 25, 1874, the second son of Giuseppe Marconi, an Italian country gentleman, and Annie Jameson, daughter of Andrew Jameson of Daphne Castle in the County Wexford, Ireland. He was educated privately at Bologna, Florence and Leghorn. Even as a boy he took a keen interest in physical and electrical science and studied the works of Maxwell, Hertz, Righi, Lodge and others. In 1895 he began laboratory experiments at his father's country estate at Pontecchio where he succeeded in sending wireless signals over a distance of one and a half miles, thus becoming the inventor of the first practical system of wireless telegraphy.

In 1896 Marconi took his apparatus to England where he was introduced to Mr. (later Sir) William Preece, Engineer-in-Chief of the Post Office, and later that year was granted the world's first patent for a system of wireless telegraphy. He demonstrated his system successfully in London, on Salisbury Plain and across the Bristol Channel, and in July 1897 formed The Wireless Telegraph& Signal Company Limited (in 1900 re-named Marconi's Wireless Telegraph Company Limited). In the same year he gave a demonstration to the Italian Government at Spezia where wireless signals were sent over a distance of twelve miles. In 1899 he established wireless communication between France and England across the English Channel. He erected permanent wireless stations at The Needles, Isle of Wight, at Bournemouth and later at the Haven Hotel, Poole, Dorset.

In 1900 he took out his famous patent No. 7777 for «tuned or syntonic telegraphy» and, on an historic day in December 1901, determined to prove that wireless waves were not affected by the curvature of the Earth, he used his system for transmitting the first wireless signals across the Atlantic between Poldhu, Cornwall, and St. John's, Newfoundland, a distance of 2100 miles.

Between 1902 and 1912 he patented several new inventions. In 1902, during a voyage in the American liner «Philadelphia», he first demonstrated «daylight effect» relative to wireless communication and in the same year patented his magnetic detector which then became the standard wireless re-

ceiver for many years. In December 1902 he transmitted the first complete messages to Poldhu from stations at Glace Bay, Nova Scotia, and later Cape Cod, Massachusetts, these early tests culminating in 1907 in the opening of the first transatlantic commercial service between Glace Bay and Clifden, Ireland, after the first shorter-distance public service of wireless telegraphy had been established between Bari in Italy and Avidari in Montenegro. In 1905 he patented his horizontal directional aerial and in 1912 a «timed spark» system for generating continuous waves.

In 1914 he was commissioned in the Italian Army as a Lieutenant being later promoted to Captain, and in 1916 transferred to the Navy in the rank of Commander. He was a member of the Italian Government mission to the United States in 1917 and in 1919 was appointed Italian plenipotentiary delegate to the Paris Peace Conference. He was awarded the Italian Military Medal in 1919 in recognition of his war service.

During his war service in Italy he returned to his investigation of short waves, which he had used in his first experiments. After further tests by his collaborators in England, an intensive series of trials was conducted in 1923 between experimental installations at the Poldhu Station and in Marconi's yacht «Elettra» cruising in the Atlantic and Mediterranean, and this led to the establishment of the beam system for long distance communication. Proposals to use this system as a means of Imperial communications were accepted by the British Government and the first beam station, linking England and

Italy in 1902. Marconi also received the freedom of the City of Rome (1903), and was created Chevalier of the Civil Order of Savoy in 1905. Many other distinctions of this kind followed. In 1914 he was both created a Senatore in the Italian Senate and appointed Honorary Knight Grand Cross of the Royal Victorian Order in England. He received the hereditary title of Marchese in 1929.

In 1905 he married the Hon. Beatrice O'Brien, daughter of the 14th Baron Inchiquin, the marriage being annulled in 1927, in which year he married the Countess Bezzi-Scali of Rome. He had one son and two daughters by his first and one daughter by his second wife. His recreations were hunting, cycling and motoring.

Marconi died in Rome on July 20, 1937.

CARL FERDINAND BRAUN

Electrical oscillations and wireless telegraphy

Nobel Lecture, December 11, 1909

In accepting, today, the great honour and privilege of addressing the members of an Academy which though of venerable age is constantly renewed and invigorated by the contribution of fresh strength and energy, I hope for your indulgence and understanding when I conceive my task not to be that of talking about wireless telegraphy in general. I have felt it more fitting to limit myself to the narrower field of the activities in which I have been successful in taking some part in the development of the whole.

I shall ignore my experiments on the propagation of electrical waves through water which I carried out in the summer of 1898, and shall turn at once to the experiments which were described and conceived at that time as being transmission through the air.

The following should first be mentioned: Marconi, as far as I know, had begun his experiments on his father's estate in 1895, and continued them in England in 1896. His experiments in Spezia harbour were, with other ones, carried out in 1897, and a distance of 15 km was attained. In the autumn of the same year, Slaby, using much the same arrangement, reached 21 km over land but only by means of balloons to which were attached wires of 300 m in length. Why, must one ask, was it so difficult to increase the range? If the

active». Hertz had already, in his first work, called attention to the strong damping of the oscillators and compared their electrical oscillations with the ill-defined acoustic oscillations of wooden rods. Bjerknes, in 1891, had successfully measured the damping and found the logarithmic decrement (as well known the measure for damping) for a linear oscillator to be 0.26, when he used only a minute spark gap. When, however, the spark gap was increased to 5 mm, the decrement rose to 0.40. This, and a series of other facts, indicated the existence of strong spark damping. All known facts became understandable if one assumed that at low capacities the spark consumed a great part of the energy, and the longer the spark was, the larger was the part of energy it consumed. On the other hand, it had long been known that the discharge of bigger capacities in the customary arcs was always oscillatory, and (in radiation-free paths) was obviously much less attenuated. In fact Feddersen had already directly photographed up to 20 half-cycles of oscillations in 1862. I took hold of this fact.

Considering the greater amounts of energy which can be collected and stored in suitable experimental form in capacitors, one could expect to deliver radiated energy for some time from them. Taken all-in-all, I concluded that if a *sparkless* antenna could be excited, from a closed Leyden-jar circuit of large capacity, into potential oscillations whose average value was that

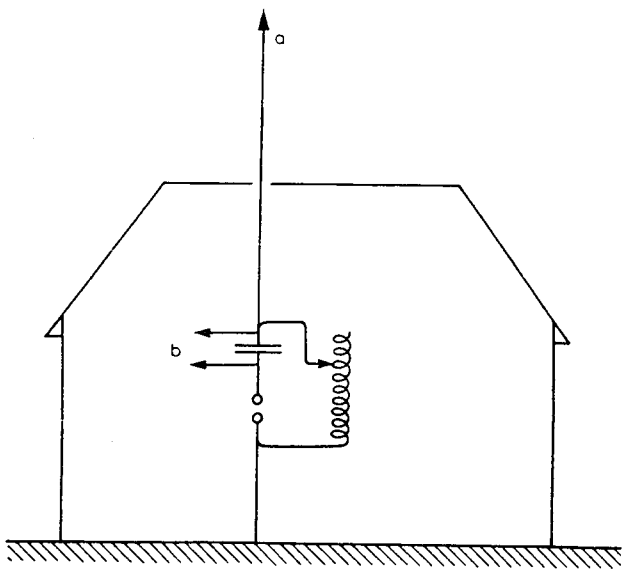


Fig. 1.

of the initial charge in a Marconi transmitter, then one would possess a more effective transmitter. There was some doubt as to whether this could be attained. And further, it was necessary to decide, by experiments on effects at a distance, whether any disturbing factor had been overlooked in these considerations. By suitable dimensioning of the exciter circuit it was found possible to fulfil the first requirement, and comparative experiments on long-distance effects were in favour of the new arrangement.

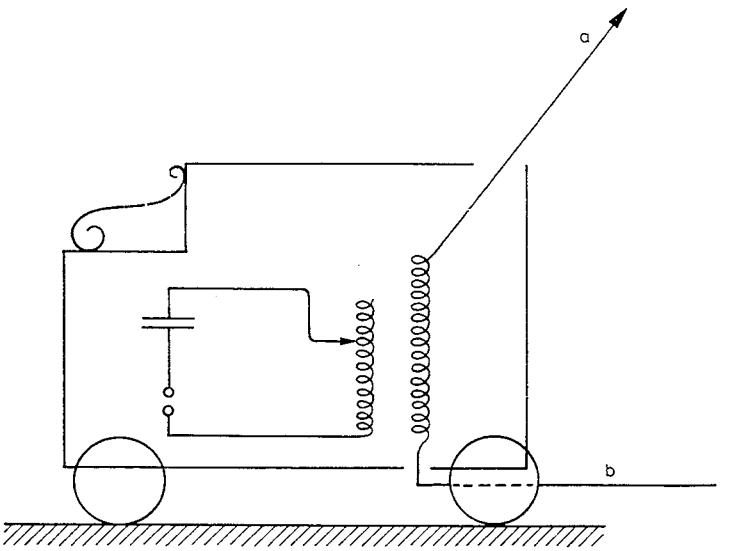


Fig. 2.

By a suitable design of the Leyden-jar circuit, significantly higher voltages are attained in the transmitter than the charging voltage of the Leyden-jar circuit. There was some suspicion in my mind that large capacities with bigger spark lengths would behave in the same way as small capacities. At that time, little was known about this. The results of later experiments have, in part, contradicted each other, since other losses appearing with high voltages were overlooked. But as far as the spark resistance was concerned my fears, as M. Wien has recently shown, were without foundation. Since I wanted, however, to be prepared for every eventuality, I asked myself whether it might not still be possible to increase the power, for instance by connecting *several* circuits of the *same* frequency of oscillation into the excitation circuit of the transmitter. The difficulty was to so couple circuits of this kind together that they would all start to discharge at the same moment, for example within exactly $\frac{1}{10}$ of a millionth of a second. This task occupied me on repeated occasions. One solution, attained in a somewhat different way and to which I was led in the course of my experiments, is given here (Fig. 3). It has been described as an « energy coupling ». I will touch later upon the advantages possessed by this arrangement, which remain despite the results obtained by Wien.

The experiments were to be carried further under practical conditions after Easter of 1899. The choice of location for the tests fell upon Cuxhaven. In addition to the main task there was an almost overwhelming pressure of

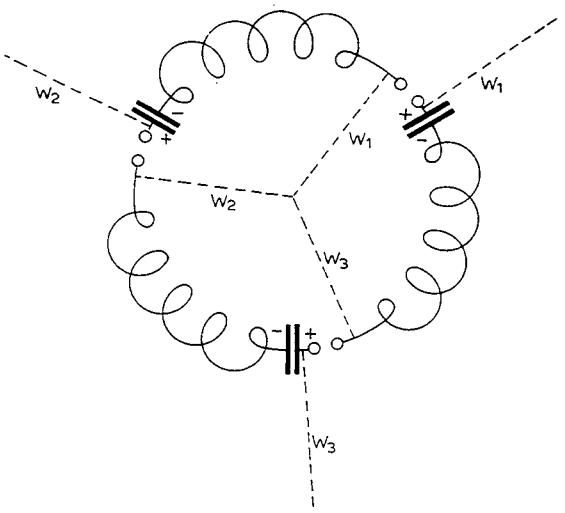
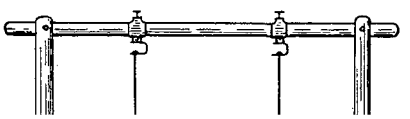


Fig. 3.

other allied tests and problems, e.g., how does the coherer work, particularly under the practical conditions occurring? Is it a resistance or does it behave like a capacitance or both? Can it be replaced by something better-defined, and if possible, more quantitatively informative? How do nearby buildings or metal masses such as masts and stays, which play such an important part in practice, affect the antenna? And there was a further multitude of problems with respect to the particular receiving apparatus. And all these problems affected the overall solution so that they all needed to be solved nearly simultaneously. Owing to my professional duties I could devote but little time to the tests, and they were carried on by two of my assistants until the autumn of 1900. The way in which the most favourable conditions were discovered in practice by systematic methods has been described by me elsewhere.

On November 16, 1900, I gave my first public lecture on this subject to the Natural Sciences Society in Strasbourg. There I described, among other matters, the advantages offered by my circuits for tuned telegraphy, advantages which Marconi had by then also recognized. On the following February 1, I demonstrated before the same Society the methods on which I had based the tuning of a receiver. I carried out more or less the same experiments before the Assembly of Research Workers in Natural Sciences in Hamburg during the autumn of the same year, as well as demonstrating the practical results on the station at Heligoland.

In the receiver, too, the most important feature was the capacitor circuit



which was directly coupled to the antenna, and which, as I expressed it, collects the energy radiated towards the receiver into the best possible loss-free paths, localizes it and thus passes it onwards in the most suitable form for the detector.

By my arrangements, so-called *coupled* systems were introduced throughout in the wireless telegraphy system, and at this point we might briefly examine their properties. For preference I have used Oberbeck's pendulum model for illustration although it does not correspond completely to the electrical conditions. I produce it here (Fig. 4). Two pendulums of identical frequency are «coupled» through a loaded thread. I draw the first pendulum away from the position of rest and release it. It transmits its energy to the second pendulum and the latter increases its energy at the cost of the first, exciting pendulum. After some time the whole of the energy appears in the second pendulum. At this point, however, the process repeats itself in the opposite sequence. If I make the first pendulum heavy and the second one light, I can make the oscillation amplitude of the second greater than that of the first. The first pendulum represents the Leyden-jar circuit, the second the transmitter to which – in this case – the whole of the energy of the Leyden-jar circuit is passed. According to the ratio of the capacities the voltage can be amplified (or, if desired, reduced).

Now Oberbeck in 1895 demonstrated the following by calculation. If a capacitor circuit is allowed to operate inductively upon a second circuit of the *same* natural frequency there appear – most strikingly – *two* oscillations in both circuits, one higher and one lower than the natural frequency of oscillation. The closer the coupling, i.e., the quicker the energy transference from the first to the second circuit becomes the further apart lie the frequencies. Only for the case of infinitely loose coupling do the two oscillations approximate to the natural frequency, that is to say, become equal to each other.

This result holds also for mechanical systems, which includes our pendulums. If our two equally-tuned pendulums are coupled, then each should exhibit *two* different frequencies of oscillation. The result loses its surprise, when the phenomenon, I would like to say, is not treated mathematically but actually takes place before our eyes. The characteristic is this: the oscillations of the second pendulum increase steadily from zero upwards, then again decrease, and vice versa. We note from each pendulum what is known in acoustics as «beating». I shall recall now a method of representing graphically the acoustic beating (Fig. 5). An oscillating tuning fork carries a glass

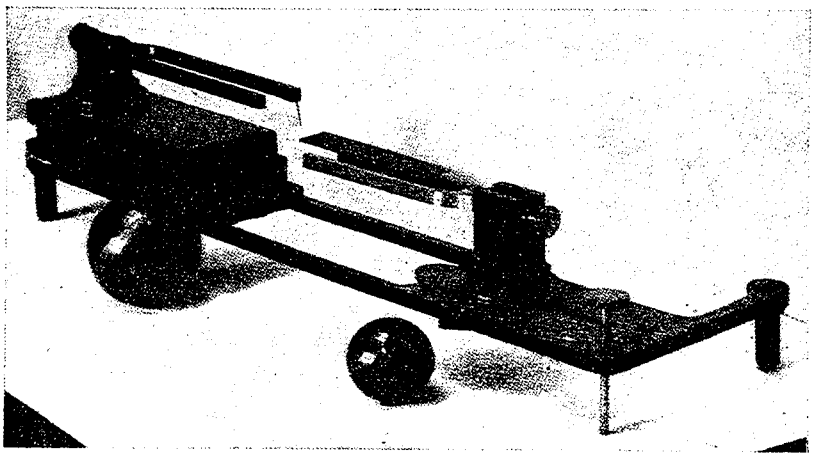


plate covered in soot (carbon black). A second tuning fork writes upon this oscillating glass plate by means of a small pin whilst it itself is drawn across the plate. *One* tuning fork would describe a curve of constant amplitude (Fig. 6). The oscillations of both forks are added algebraically. And when both forks have different frequencies, as is here the case, a curve as in Fig. 7 results.

I shall call them briefly «beating (or pulsating) oscillations». Just such curves would arise if we allowed our *pendulum* to write upon a moving plate. If the exciting pendulum gave the upper curve, *a*, then the excited pendulum would give the lower curve, *b*.

Each *such* beating oscillation can be considered – from the elementary laws of trigonometry – as arising from the superposition of two harmonic oscillations of different frequency, say n_1 and n_2.

Although this is mathematically possible, however, experience teaches us that if this pulsating oscillation is applied to a structure capable of oscillation whose natural frequency fits in with one or other of the frequencies n_1 or n_2, then it will be excited in its own natural frequency of oscillation. It selects one of the fictitious harmonic components and endows it thereby with an independent existence. A body so excited is called a resonator, and the phenomenon itself is known as resonance.

The thing is clear to the mind in the case of the tuning fork example. In space we would observe the beats, but resonators would separate the two tuning-fork tones. The application to our pendulum model is also obvious. Each part of the system performs pulsating oscillations, resonators react to two different harmonic oscillations.* If we wish (and here I return to the electrical example), to record, by means of resonators, oscillations from the radiation sent into space by the antenna, then we have to adjust the resonator to one of the two oscillations.

These electrical oscillations can be separated by means of a variable capacitor circuit, the so-called resonance Leyden-jar circuit (to which I will return later), *so long as care is taken* that as far as possible the circuit has no feed-back into the system being investigated. Oberbeck's result was concerned with the case where both system components had *closed* current circuits (with quasi-stationary flow) and were inductively coupled. It can now be easily

* If time allowed, I would be able to demonstrate both these oscillations «analytically» by means of the pendulum model. This second model, introduced by Dr. Mandelstam, and representing direct coupling through its correct mechanical analogue, allows every detail to be recognized.

shown that both oscillations also exist in the open current path of an antenna, whether it is excited directly or inductively (I am ignoring higher harmonics).

In the summer of 1902 I was able to erect two experimental stations on two forts at Strasbourg for the purpose of closer study. The task which I had set for us was to determine the most favourable conditions in the receiver. We adopted the resonant circuit, in which known capacitances were combined with calculated self-inductances, so as to bring both parts of the transmitter system into the same natural frequency of oscillation. We fixed likewise the two oscillations arising from the coupling and searched for these with the receiver. The result of the test was, for that time, surprising, as an example will show. If, by means of a coil in the receiver circuit, the oscillations were transferred inductively into a second coil located in a tuned circuit containing the indicator (parallel to a small capacitor), not only was the sharpness of the resonance but also – and here was the surprise – the *intensity* of the excitation was raised as soon as the two coils were *moved away* from one another. The intensity increased with increasing distance between the coils, though naturally beyond a certain limit there was again a decrease. Described in the customary expression, the effectiveness increased with *looser* coupling. This result in the receiver was *not* subject to a similar loose coupling in the transmitter.

There were two important results from these experiments: (*a*) greater freedom from disturbance in the receiver; and (*b*) a valuable measuring instrument for wireless engineering. When Dr. Franke of Siemens & Halske (who were working with us) saw the tests he proposed to base technically-useable equipment on them. Until then the resonance circuit had been assembled from existing parts to suit the particular requirements, and from whatever came to hand. Through the combination of a Köpsel's calibrated variable rotating capacitor and a number of calculated self-inductances, an apparatus was constructed which covered a large range of wavelengths both conveniently and continuously. The « current effect » was measured by means of a Riess's air thermometer which I had long used for intensity measure-

By means of this instrument the foundation of measuring techniques for wireless telegraphy were laid. It soon displaced our cumbersome laboratory equipment and gave us great help in our scientific investigations, whilst it became indispensible for rational technical work in the field of electrical oscillations.

In the summer of 1902 came the publication of a theoretical study of the coupled transmitter by Max Wien. This particularly concerned the effect of damping. Wien showed by calculation the versatility of the coupled transmitter. He summed up the qualitative results of his work as follows: « According to the kind of coupling, a powerful but quickly attenuated excitation can be attained which reaches far into the distance, or alternatively a slowly decreasing wave-train which is capable of exciting a similarly-tuned resonator while passing all others by – a cannon shot to be heard afar, or a soft, slowly declining tuning-fork tone ».

This theoretical investigation was most effective in clarifying the problem basically, and it will remain the foundation. It remains to be seen, however, how closely the data chosen for the numerical examples correspond to actual practice. Some calculated figures and a few laboratory figures were all that were available on the subject of damping. The field of measurements in relation to practice was beginning to be opened up. From then on, the work spread further and further outwards, branching into that of the scientific laboratories on the one hand, and the conversion of their results into practice with its complicated conditions and extensive requirements on the other. Success in the latter connection is due to Count Arco and Mr. Rendahl.

The circumstances which led me, more than ten years ago, to introduce the capacitor circuit, have altered greatly in the meantime. The Leyden-jar circuit is still to-day indispensible in wireless telegraphy. Two properties should be mentioned which I have not yet touched upon:

(1) For equal powers it is easier to design an inductor for use with high charging capacities and low voltages than vice versa. This was a determining factor at the time for the energy circuit mentioned earlier and remained so for this set-up.

(2) Insulation difficulties are practically non-existent in the Leyden-jar circuit, but the contrary is the case in the antenna circuit. If, for example, the insulators in a coupled transmitter are damp, the transmitter still works, whilst it can become impossible to charge it statically or with low frequency.

I illustrated the latter point in my lecture in November of 1900 by means of the following experiment. I allowed the transmitter to operate inductively upon a neighbouring receiver and so produced current in the latter which

brightly lit up an incandescent bulb. I touched the transmitter wire with a moist binding thread which was connected to earth. This had no effect on the operation in the case of the coupled transmitter, but the transmitter with direct inductor charging could not be operated once the damp thread was placed in contact with it.

Before I leave the subject of the coupled system I might perhaps recall an accessory which was of great use to me and other experimenters. I mean the cathode-ray tube which I described in 1897. It provided a visual picture of current- and voltage-waveforms up to 100 kc/s, and was the means by which investigations of period, waveform, intensity and thereby damping, as well as relative phases, could be made.

One of the first applications of this tube was Knut Ångström's neat method of showing directly the hysteresis curve. In a similar way, the permeability of iron up to 130 kc/s was investigated at the Strasbourg Institute, and a number of other problems concerned with electrical oscillations were also studied.

Three oscillograms made with the tube will serve to show its application. They illustrate the primary current pattern in the inductor, which interests us, and the significance of the capacitor therein.

I will show you now only the current oscillations in two coupled (but strongly damped) capacitor circuits. You will see that the tubes do in fact show actual beating or pulsating oscillations (Fig. 11).

In still another place, wireless telegraphy brought me into contact with earlier investigations which I had made, this time in connection with work in my youth. I found in 1874 that materials such as galena, pyrite, pyrolusite,

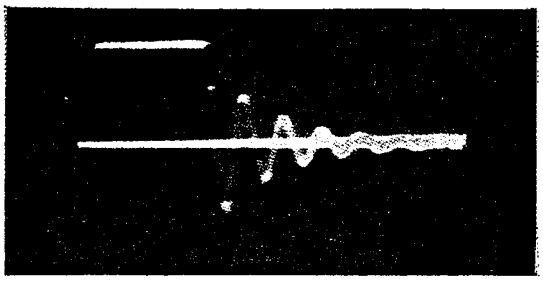


Fig. 9.

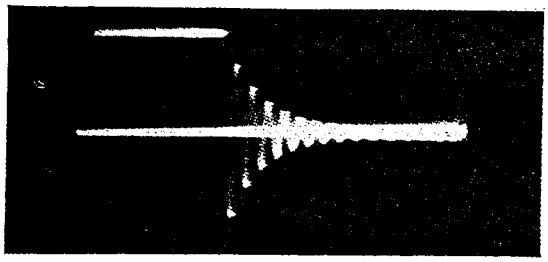


Fig. 10.

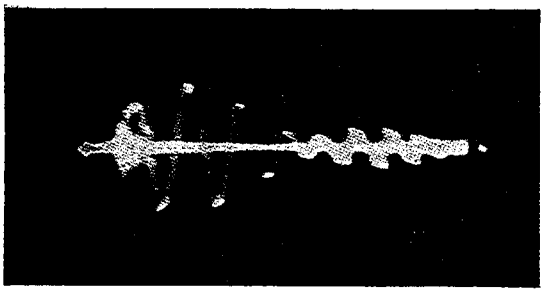


Fig. 11.

tetrahedrite, etc., departed from Ohm's Law, particularly when an electrode made contact over a small surface area. These materials greatly interested me since they conduct without electrolysis although they are binary compounds. The resistance appeared to be dependent upon the direction and intensity of the current and I could, for instance, separate the opening and closing currents of a small inductor by means of such materials, in a similar way to that of a Geisler's tube. I did not succeed in finding an « explanation » for the phenomena, for instance to what material unsymmetry the electrical unsymmetries (which without doubt existed) corresponded. I had to content myself with showing that the observed phenomena were not brought about through secondary effects such as heating. I was able to demonstrate that it appeared – at least qualitatively – even in 1/500 second, and I was convinced that – perhaps at the furthest limits – an inertialess process was concerned, a view which was supported by E. Cohn whilst carrying out some other experiments, when he found that the unsymmetrical d.c. resistance could follow current oscillations of 25 kc/s. But always there remained with me a feeling of dissatisfaction, and with it, a faint memory which had obviously never died, but remained half-somnolent at the back of my mind. Instinctively I was driven back to this valve effect (with which I had repeatedly, though in vain, attempted to obtain direct current from oscillations of light) when I began to occupy myself with wireless telegraphy in 1898. The elements showed the expected detector effect, but *at that time* offered no advantages over the coherer. As the swing to aural reception of messages took place, I came back to these materials, and recognized their usefulness for this purpose in 1901. In 1905 the *Gesellschaft für drahtlose Telegraphie* (Wireless Telegraphy Co.) decided, on my recommendation, to start up a technical project in this sphere of activity. Today, these detectors – including other combinations of a similar nature – are extensively used. Pierce, by means of the cathode-ray tube, has demonstrated for slow oscillations an almost complete separation of positive and negative current components in the case of molybdenite. It seems to me that it is still an open question whether this will hold also for rapid oscillations.

I will turn now to another series of experiments.

It had always seemed most desirable to me to transmit the waves, in the main, in one direction only. I will not concern myself with the successful experiments of this kind made at the Strasbourg Forts in 1901, since it came out later that similar proposals had already been made by others.

I found in 1902 that an antenna, inclined at somewhat less than 10° to the horizon, formed a kind of directional receiver. The receptivity showed a clearly defined maximum for waves passing through the vertical plane in which the antenna was situated. The results were published in March of 1903.

A directional *transmitter* is made up in the following way (Fig. 12). It is assumed that the antennae A and B, located at corners of an equilateral triangle, are equal in phase, but are *delayed* by a quarter of a cycle of oscillation relative to antenna C, which is in the third corner. The height CD of the triangle is to be a quarter wavelength. The radiation will then prefer the direction CD. The wave emanating from C will reach AB at the moment that A and B start to oscillate.

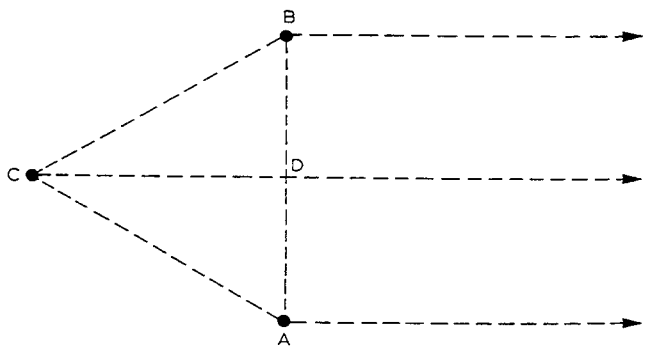


Fig. 12.

The task arose to attain this kind of phase difference for rapid oscillations, and prior to this, to measure such differences. A measuring method came easily to hand, one which has also proved itself in practical experiments. The solution of the other task did not go well using the scheme which I had thought out. On the other hand, two of my assistants found an ingenious solution when they took up the work, at my suggestion, in the Strasbourg Institute. Experiments were carried out on a big parade-ground in the vicinity of Strasbourg (spring of 1905).

In Fig. 13 is shown, schematically, the layout used. The field was measured at a fair distance away, that is to say, in the so-called wave-zone. There was satisfactory agreement between theory and observation, and the results were checked in various ways. It was further shown that the experimental layout functioned in the desired sense. By suitable distribution of the amplitudes in the three transmitters, a field as in Fig. 14 was calculated (the

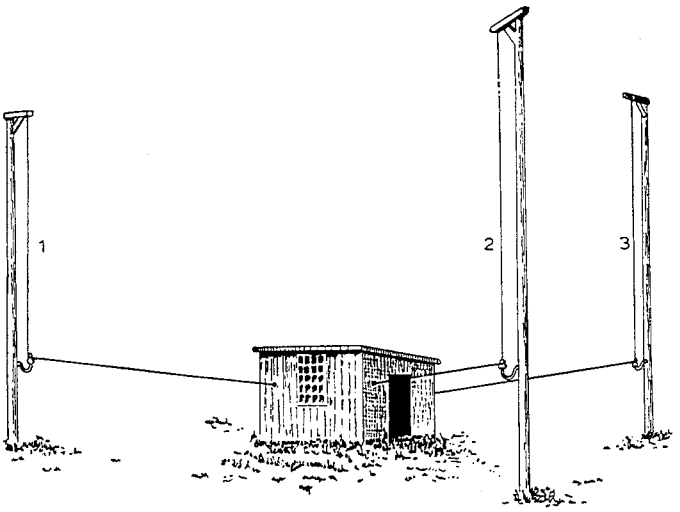
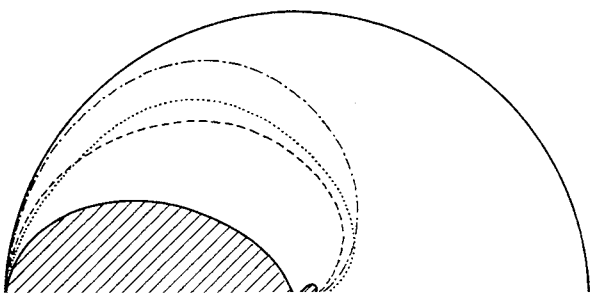


Fig. 13.



It would appear to be of general interest to remark that one is led to the conclusion that the radiation of a transmitter is reduced here by the oscillations in its neighbour, which are shifted in position and phase, a conclusion which could be proved experimentally.

If nowadays optical phenomena are ascribed to electrical molecular resonators, then electrical processes, as demonstrated here by a single example, can also be linked up with optical phenomena, though this can hardly be experimentally verified in this field.

Here, the study of electrical oscillations supplements that of optical oscillations, and since we are in the position to tackle a problem in either field by analogy with a phenomenon which is comprehended in the other field, the first attack on the problem can be made from the electrical or the optical standpoint according to whichever presents the easier concept to realize. I can perhaps illustrate this by means of two worked-out examples.

Elementary considerations led me to the conclusion that a medium, composed of layers of different dielectric constants, must behave as a uniaxial crystal if it is assumed that the layer thicknesses are only a fraction of a wavelength. I was able to confirm this conclusion in the following way (Fig. 15). A beam of practically parallel electrical rays emerges from the Hertzian reflector. It strikes a structure made of bricks in layers having the same breadth of air layers. These layers lie open to short waves, but if the wavelength is 12 times, or so, longer than the layer thickness, then the brick grating behaves towards it as a body which *homogeneously* occupies the space but exhibits double refraction. The electrical oscillations are linearly polarized and incident at an azimuth of 45° upon the brick layers. A brick structure which is

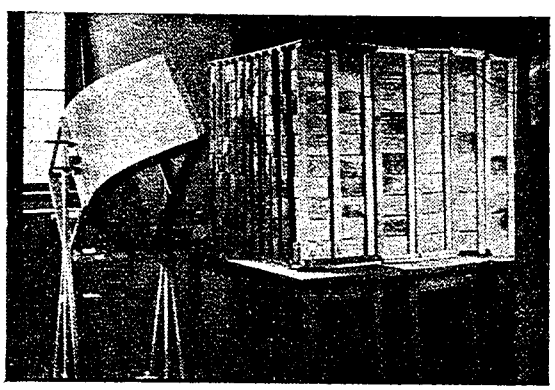


Fig. 15.

about $2\frac{1}{2}$ times the thickness of a single brick has the effect of a quarter-wave foil of mica, and the linearly-incident ray emerges circularly polarized as we deduce from the investigations with a Righi's resonator. Assume it is right-handed circular. If the layer thickness is now doubled, the emergent wave is again linearly polarized, though in the other quadrant. And so we can transform the ray, by continuous addition of further thicknesses, into a left-handed circular one, and finally back into a ray which is linearly polarized parallel to the incident ray. The double refraction of the brick grating surpasses that of calcite. Optically, this brick structure would correspond to a tiny crystal of a few thousandths of a millimetre length of edge, but electrically it is $2\frac{1}{2}$ metres thick, weighs 4000 kg, and its raw material is worth about 200 marks. The analogue of a corresponding optical phenomenon was also demonstrated by me at a later date.

This phenomenon of double refraction does not depend upon the use of rigid materials. Whether the double refraction occurring in cross-striated muscle results from a similar layer structure is thus a closely related question.

We have so far studied an electrically unknown, but optically conjectured phenomenon and both have been discovered to exist. The following example is concerned with demonstrating the unknown optical phenomenon corresponding to a known electrical phenomenon. It seemed to me to be of interest to reproduce the Hertzian grid experiment in the field of visible rays. For this to be realized, a very fine grating of metal wires was necessary and from 10,000 to 100,000 tiny wires, separated by air gaps, had to be located within a width of 1 mm. Mechanical methods of manufacture are impossible, but a Hertzian grid could be made in the following way. If a powerful discharge is passed through a thin metal wire on a glass plate, or between two such plates, the well-known sputtering or vaporization effect occurs, as you can see from Fig.16. The metal wire vaporizes (temperatures of up to 30,000°

are calculated). The metal vapour is driven outwards by the pressure arising from the explosive effect (Fig. 17) and is then again precipitated obviously in a kind of grid structure on the glass. If we allow linearly polarized light to fall upon the prepared surface, it will, if the oscillations are parallel to the lines of the grid, be strongly reflected and strongly absorbed – the preparation appears dark (Fig. 18). If the plane of the oscillations is turned so that it is perpendicular to the lines of the grid, the metal layer becomes transparent

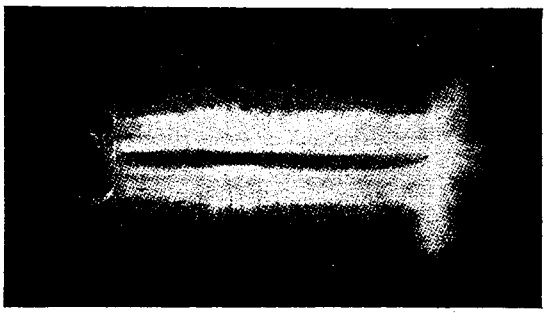


Fig. 17.

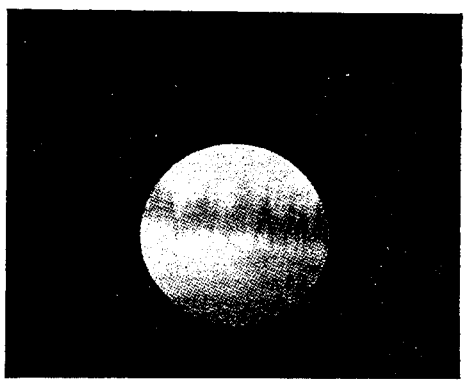


Fig. 18.

(Fig. 19). We have the complete optical analogue to a Hertzian grid made out of moderately good conductors.

This experiment permits a further development. If we imagine that in an organized fabric such as muscle tissue, plant fibres, etc. there exists a similar fine grating structure somewhat in the form of the finest possible channels, then if we could succeed in filling these with metal, the preparation would have the optical effect of a Hertzian grid. H. Ambronn, in 1896, treating the

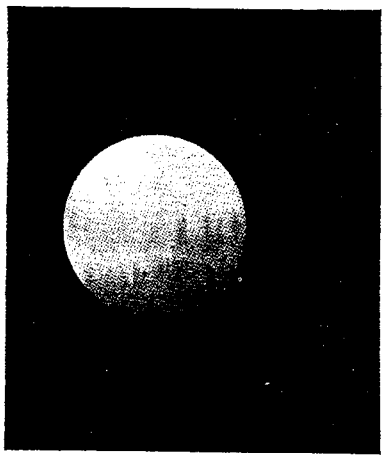


Fig. 19.

above mentioned substances with gold or silver salts, discovered phenomena which I explained in this way. In an exhaustive investigation into this matter I have everywhere found confirmation of my concept, and nowhere a contradiction. Yet a direct and incontrovertible proof would be extremely desirable because of the importance of its consequences. For if my idea is, as I believe, correct, then we would in this way not only discover sub-microscopic gratings, but, as a result of electrical imitation, we would even be able to some extent to make a picture of the material structure which is as yet invisible to the human eye. This method would augment those so far available in a most valuable manner, for it takes its place just where the microscope and – because of the density of the particles – even the ultramicroscope reach

Wien's discovery. Arising from this and through the agency of Rendahl and Arco, came the so-called tone-spark. The small hissing or quenched sparks of Wien of itself meet the conditions which I had hoped to produce artificially. The Leyden-jar circuit cuts itself out at the most suitable moment, and the greater part of the primary energy then oscillates in the highly conductive paths in the transmitter at its own natural frequency.

On the occasion of my first lecture in November 1900 I closed with the following words:

« Sometimes, wireless telegraphy has been described as spark telegraphy, and so far a spark in one place or another has been unavoidable. Here, however, it has been made as harmless as possible. This is *important*. For the spark which produces the waves also destroys them again as Saturn destroyed his own children. What was pursued here could be truthfully described as *sparkless telegraphy*. »

Finishing as I did with these words at that time, I feel happy to think that with the means I have described we have come appreciably nearer to this target, and have thereby made the coupled transmitter still more effective.

Biography

Carl Ferdinand Braun was born on June 6, 1850 at Fulda, where he was educated at the local « Gymnasium » (grammar school). He studied at the Universities of Marburg and Berlin and graduated in 1872 with a paper on the oscillations of elastic strings. He worked as assistant to Professor Quincke at Würzburg University and in 1874 accepted a teaching appointment to the St. Thomas Gymnasium in Leipzig. Two years later he was appointed Extraordinary Professor of Theoretical Physics at the University of Marburg, and in 1880 he was invited to fill a similar post at Strasbourg University. Braun was made Professor of Physics at the Technische Hochschule in Karlsruhe in 1883 and was finally invited by the University of Tübingen in 1885; one of his tasks there was to build a new Physics Institute. Ten years later, in 1895, he returned to Strasbourg as Principal of the Physics Institute, where he remained, in spite of an invitation from Leipzig University to succeed G. Wiedemann.

Braun's first investigations were concerned with oscillations of strings and elastic rods, especially with regard to the influence of the amplitude and environment of rods on their oscillations. Other studies were based on thermodynamic principles, such as those on the influence of pressure on the solubility of solids.

His most important works, however, were in the field of electricity. He published papers on deviations from Ohm's law and on the calculations of the electromotive force of reversible galvanic elements from thermal sources. His practical experiments led him to invent what is now called Braun's electrometer, and also a cathode-ray oscillograph, constructed in 1897.

In 1898 he started to occupy himself with wireless telegraphy, by attempting to transmit Morse signals through water by means of high-frequency currents. Subsequently he introduced the closed circuit of oscillation into wireless telegraphy, and was one of the first to send electric waves in definite

of a brochure under the title *Drahtlose Telegraphie durch Wasser und Luft* (Wireless telegraphy through water and air).

After the outbreak of the First World War, Braun was summoned to New York to attend as a witness in a lawsuit regarding a patent claim. Owing to his absence from his laboratory and due to illness he was unable to carry out further scientific work. Braun thus spent the last years of his life peacefully in the United States, where he died on April 20, 1918.

Physics 1910

JOHANNES DIDERIK VAN DER WAALS

«for his work on the equation of state for gases and liquids»

Physics 1910

Presentation Speech by the Rector General of National Antiquities, Professor O. Montelius, President of the Royal Swedish Academy of Sciences

Your Majesty, Your Royal Highnesses, Ladies and Gentlemen,

The Academy of Sciences has resolved to award this year's Nobel Prize for Physics to the world-famous Dutch physicist, Johannes Diderik van der Waals for his studies of the physical state of liquids and gases.

As far back as in his inaugural dissertation «The relationship between the liquid and the gaseous state», Van der Waals indicated the problem to which he was to devote his life's work and which still claims his attention today. In the dissertation to which I have referred he sought to account for the discrepancies from the simple gas laws which occur at fairly high pressures. He was led to the assumption that these discrepancies are partly associated with the space occupied by the gas molecules themselves, and partly with the attraction which the molecules exert on one another, owing to which the pressure acting on the interior of the gas is greater than the external pressure. These two factors become more and more pronounced with increasing compression of the gas. At a sufficiently high pressure, however, the gas becomes liquid, unless the temperature exceeds a certain value, the critical temperature as it is termed. Van der Waals showed that it is possible to apply the same considerations and calculations to liquids as to gases. When the temperature of a liquid is raised to beyond the critical temperature without the liquid being allowed to volatilize, it is in fact converted continuously from the liquid to the gaseous form; and close to the critical temperature it is impossible to distinguish whether it is liquid or gas.

The force preventing the separation of the molecules in a liquid is their mutual attraction, owing to which a high pressure prevails in the interior of the liquid. Van der Waals calculated this pressure, the existence of which had already been vaguely perceived by Laplace, for water. It amounts to not less than about 10,000 atmospheres at normal temperature. In other words the internal pressure, as it is called, of a drop of water would be about ten times greater than the water pressure at the greatest depth of the sea known to us.

However, this was not the most important result of Van der Waals'

studies. His calculations led him to consider that once we are acquainted with the behaviour of a single type of gas and the corresponding liquid, e.g. that of carbon dioxide, at all temperatures and pressures, we are able by simple proportioning to calculate for any gas or liquid its state at any temperature and pressure, provided that we know it at only one, i.e. the critical, temperature.

On the basis of this law of what are known as «corresponding states» for various gases and liquids Van der Waals was able to provide a complete description of the physical state of gases and, more important, of liquids under varying external conditions. He showed how certain regularities can be explained which had earlier been found by empirical means, and he devised a number of new, previously unknown laws for the behaviour of liquids.

It appeared, however, that not all liquids conformed precisely to the simple laws formulated by Van der Waals. A protracted controversy arose around these discrepancies which were ultimately found to be attributable to the molecules in these liquids not all being of the same character; the older Van der Waals laws apply only to liquids of uniform composition. Van der Waals then extended his studies to mixtures of two or more types of molecules and here too he managed to find the laws and these, of course, are more complex than those which apply to substances composed of molecules of a single type. Van der Waals is still occupied with working out the details of this great investigation.

Nevertheless, he has successfully surmounted the difficulties that were initially in his path.

Van der Waals' theory has also been brilliantly successful through its predictions which made it possible to calculate the conditions for converting gases to liquids. Two years ago Van der Waals' most prominent pupil, Kamerlingh Onnes, in this way succeeded in compelling helium – the last previously uncondensed gas – to assume the liquid state.

Yet Van der Waals' studies have been of the greatest importance not only for pure research. Modern refrigeration engineering, which is nowadays such a potent factor in our economy and industry, bases its vital methods mainly on Van der Waals' theoretical studies.

Professor Van der Waals. The Royal Academy of Sciences has awarded you this year's Nobel Prize for Physics in recognition of your pioneering studies on the physical state of liquids and gases.

Hamurabi's and Moses' laws are old and of great importance. The laws of Nature are older still and even more important. They apply not just to certain regions on this Earth, but to the whole world. However, they are difficult to interpret. You, Professor, have succeeded in deciphering a few paragraphs of these laws. You will now receive the Nobel Prize, the highest reward which our Academy can give you.

JOHANNES D. VAN DER WAALS

The equation of state for gases and liquids

Nobel Lecture, December 12, 1910

Now that I am privileged to appear before this distinguished gathering to speak of my theoretical studies on the nature of gases and liquids, I must overcome my diffidence to talk about myself and my own work. Yet the thought that you are entitled to expect that of me leads me to hope that you will forgive me if I state my views in this field with utter conviction, even in regard to aspects which are not yet universally known and which have so far not achieved universal recognition. I intend to discuss in sequence:
(1) the broad outlines of my equation of state and how I arrived at it;
(2) what my attitude was and still is to that equation;
(3) how in the last four years I have sought to account for the discrepancies which remained between the experimental results and this equation;
(4) how I have also sought to explain the behaviour of binary and ternary mixtures by means of the equation of state.

(1) The first incentive to this my life's work came to me when, after my studies at university, I learned of a treatise by Clausius (1857) on the nature of the motion which we call heat. In this treatise, which is now taught with negligible modifications in every highschool in Holland, he showed how Boyle's law can very readily be derived on the assumption that a gas consists of material points which move at high velocity, that this velocity is of the order of that of sound and increases in proportion to the square root of the absolute temperature. It was subsequently realized that this is only the root mean square of the velocities and that, as Maxwell stated, although he erroneously considered to have proved it, there is a law for the distribution of the velocities known as the Maxwell law. Boltzmann was the first to give the correct proof for this velocity distribution law. Clausius' treatise was a revelation for me although it occurred to me at the same time that if a gas in the extremely dilute state, where the volume is so large that the molecules can be regarded as points, consists of small moving particles, this is obviously still so when the volume is reduced; indeed, such must still be the case down to the maximum compression and also in liquids, which can only be re-

garded as compressed gases at low temperature. Thus I conceived the idea that there is no essential difference between the gaseous and the liquid state of matter – that the factors which, apart from the motion of the molecules, act to determine the pressure must be regarded as quantitatively different when the density changes and perhaps also when the temperature changes, but that they must be the very factors which exercise their influence throughout. And so the idea of continuity occurred to me. I then also asked myself how the solid state behaved. Although I have not as yet seriously gone into that question I do think that in the amorphous state the close proximity of the molecules impedes their mutual displacement. The crystalline state definitely behaves in a slightly different way. Actually I should still be silent on this question. Nevertheless it can now scarcely be doubted that continuity exists between the other two states of aggregation.

As you are aware the two factors which I specified as reasons why a non-dilute aggregate of moving particles fails to comply with Boyle's law are firstly the attraction between the particles, secondly their proper volume. Turning first to the second factor I should like to point out the following: originally I had anticipated that simply the total volume must be decreased with the total volume of the molecules to find the volume remaining for the motion. But closer examination showed me that matters were not so simple. To my surprise I realized that the amount by which the volume must be reduced is variable, that in the extremely dilute state this amount, which I have notated b, is fourfold the molecular volume – but that this amount decreases with decreasing external volume and gradually falls to about half. But the law governing this decrease has still not been found. This very point has proved to be the most difficult in the study of the equation of state. Korteweg, Lorentz, Boltzmann, Jeans, and of my pupils, Van Laar, my son, and Kohnstamm have subsequently worked on it. I had thought that it was simply a matter of studying how the mean free path decreases between the collisions of the molecules which are the consequence of their extensiveness, i.e. of the fact that they must not be considered material points but small particles with a real volume in common with all bodies known to us, and so I arrived at this formula:

$$b = b_g \left\{ 1 - \alpha \frac{b_g}{v} + \beta \left(\frac{b_g}{v} \right)^2 \text{etc.} \right\} \qquad \text{(A)}$$

where b_g is fourfold the molecular volume.

Boltzmann has shown, however, that this is inadequate and later on Kohnstamm proved that the formula will be more complex and that for b/b_g a quotient will be obtained of two series in which powers of b_g/v occur. It is so difficult to determine the coefficients α, β, etc. that Van Laar was compelled to carry out fearfully long calculations when determining the second coefficient β by my method (α gave rise to far less trouble). This prevented me from proceeding further. And here I have come to the weak point in the study of the equation of state. I still wonder whether there is a better way. In fact this question continually obsesses me, I can never free myself from it, it is with me even in my dreams.

As regards the other cause underlying the non-compliance of real gases and liquids with Boyle's law, i.e. the mutual attraction of the molecules, the situation is somewhat better although here again the last word has not yet been spoken. Using the procedure adopted by Laplace in his capillarity theory, in my continuous theory I have reduced this attraction, which acts in the whole volume, to a surface force which acts towards the interior and thus, together with the external pressure, holds together the moving molecules. Laplace regards his liquid actually as a continuum; at the time he was still unaware of molecules. And if we were dealing with stationary molecules it would have been inadmissible to reduce the attracting forces in the interior to just a surface force. Yet since the molecules are in motion each point in the interior will certainly not be filled with matter at every moment. But space may be regarded as continuously filled with matter of mean-normal-density. I shall return to this point however when I speak of my studies in recent years. The above considerations brought me to the following formula:

$$p = \frac{RT}{v-b} - \frac{a}{v^2} \qquad \text{(B)}$$

which became universally known only as a result of Eilhard Wiedemann's efforts.

(2) Having reached the second part of my lecture I must now mention my own attitude to this equation. It will be abundantly plain from my earlier comments that I never expected this equation, with a and b assigned a constant value, to give results numerically in agreement with experiment and yet people almost always act as though that were my opinion. This astonishes me as in my treatise of 1873 not only did I expressly emphasize the variability

of b but also quoted a series of b-values from Andrews' experiment in which for small volumes the change in b with the volume is calculated. For carbon dioxide I moved from the limiting value $b_g = 0.0023$ down to $b = 0.001565$. In this series of values the volumes of the liquid even go below the limiting value of b_g for infinite volume.

Perhaps the reason why my opinion that b is variable has rarely been regarded as seriously intended is to be found in the manner in which I calculated the critical parameters. In this calculation I had to assume b as invariable. But that was because I believed that Andrews' values justified still assigning the limiting value b_g to b in the critical volume. But as I later showed that was a mistake; the value of b in the critical volume has, of course, slightly decreased. Yet to determine the critical volume when b changes with the volume, the first and second differential quotients db/dv and d^2b/dv^2 also need to be known. To determine the critical volume, as I later showed, we obtain the equation:

$$\frac{v}{v-b}\left(1-\frac{db}{dv}\right)+\frac{1}{2}\frac{v\dfrac{d^2b}{dv^2}}{1-\dfrac{db}{dv}}=\frac{3}{2} \qquad (C)$$

Only when we neglect db/dv and d^2b/dv^2 do we find $v_k = 3b$. And it can again be seen from this equation that what I have termed the weak point of my theory is actually responsible for the theoretical impossibility of calculating accurately the critical volume. Using an approximation formula for b I was able to determine v_k as about 2.2 b_g. This equation for determining the critical volume I gave and used in my paper in honour of Boltzmann; it was repeated in part in my 1910 treatises. When I had realized this large deviation in the critical volume I also feared a large deviation in the other critical data, i.e. non-compliance with the formulae:

$$RT_k = \frac{8}{27}\frac{a}{b_g} \text{ and } p_k = \frac{1}{27}\frac{a}{b_g^2} \qquad (D)$$

To my great joy, however, and not without astonishment, I found that the two equations used to calculate a and b from the critical data remain unchanged. That I was able to conclude from the results of Sydney Young's admirable experiments on the volumes of co-existing vapour and liquid

phases and on the saturation pressure level at various temperatures. I showed this recently in a treatise to which I shall refer again in the present lecture. However I can explain it as follows: Sydney Young determines inter alia the value of

$$\frac{pv}{RT} \qquad (D')$$

at the critical point and finds $1/3.77$. If p and T have the same values as in my theory, providing b is imagined to be constant, the difference from $\frac{3}{8}$, the value which I found for pv/RT, can be ascribed wholly to the volume. And this, we can say, is entirely correct. The critical volume is not 3 b_g, but $(3/\sqrt{2})$ $b_g = 2.125\ b_g$ (approximately). And the product pv/RT is not $\frac{3}{8}$ but $\frac{3}{8}\sqrt{2}$. For the value of

$$\left(\frac{T}{p}\frac{dp}{dT}\right)_k - 1$$

my equation gives a/pv^2. A value of about 6 is found for this value from Sydney Young's experiments. Therefore for p_k we have the value: $p_k = a/6v_k^2$, and with $v_k = (3/\sqrt{2})b_g$ the value $p_k = (1/27)a/b_g^2$, as was originally found, is again obtained for p_k.

It will thus be seen that I have never been able to consider that the last word had been said about the equation of state and I have continually returned to it during other studies. As early as 1873 I recognized the possibility that a and b might vary with temperature, and it is well known that Clausius even assumed the value of a to be inversely proportional to the absolute temperature. Thus he thought he could with probability account for the equation

$$\left(\frac{T}{p}\frac{dp}{dT} - 1\right)_k = 6$$

only the half being found when a and b are held constant. The foregoing remarks signify that this value does not arise from a change in a with temperature, but merely from the change in b with the volume.

For a long time I searched for a definite characteristic to find whether just making b variable is sufficient to bring about complete agreement between

my formula and experiment, and in the case of remaining discrepancies, whether perhaps it is necessary to assume variability of a and b with temperature; and now I come to my latest studies on the equation of state.

(3) In 1906 before the Royal Academy of Sciences in Amsterdam I gave a lecture entitled: pseudo association. At the time, however, I contented myself with an oral communication. But one of my pupils, Dr. Hallo, took it down in shorthand and later Dr. Van Rij incorporated it in his inaugural dissertation and developed the theme further. As the criterion whether the variability of b with the volume would be sufficient to bring about agreement between my formula and Sydney Young's experimentally determined liquid and vapour volumes I used in my lecture the Clapeyron principle which, with b varying only with v and not with T, and with a constant, leads to the equation

$$\left(\frac{T}{p}\frac{\mathrm{d}p}{\mathrm{d}T} - 1\right) p = \frac{\frac{a}{v_1} - \frac{a}{v_2}}{v_2 - v_1} = \frac{a}{v_1 v_2}$$

or

$$\left(\frac{T}{p}\frac{\mathrm{d}p}{\mathrm{d}T} - 1\right) p v_1 v_2 = a$$

To ensure that only parameters are involved which are directly determinable by experiment, we may also write

$$\frac{\left(\frac{T}{p}\frac{\mathrm{d}p}{\mathrm{d}T} - 1\right) p v_1 v_2}{\left(\frac{T}{p}\frac{\mathrm{d}p}{\mathrm{d}T} - 1\right)_k p_k v_k^2} = 1 \qquad \text{(E)}$$

All the parameters on the left of this equation were accurately determined by Sydney Young for a series of substances. At the critical temperature the value of the left-hand side is, of course, equal to unity; but what is the value at temperatures that are only a fraction of T_k? Here it appears that as the temperature decreases the value of the left-hand side increases, very rapidly at first, then at an imperceptible rate.

Sydney Young's experiments only go as far as a value of $T/T_k = \frac{2}{3}$. The right-hand side has then increased to 1.4 and seems to be approaching asymptotically the value 1.5. The value of the right-hand side may very accurately be represented by the empirical formula

$$1 + \sqrt{1 - m} - \frac{1 - m}{2} \qquad (F)$$

where $T/T_k = m$.

At a temperature very close to the critical, e.g. $m = 0.99$, the increase in this expression is as much as 0.1 whereas if the square root of $1 - m$ did not occur in the formula and the increase were only to be represented by $1 - m$, it would be less by a factor of ten.

I then had to examine whether the assumption that a or b is temperature-dependent could account correctly for this increase of the right-hand side; and were such not the case, whether another expression for the internal pressure, which I have always written as a/v^2, could explain it. As a result of this examination it was found that each temperature function for a and b compatible with such a rapid initial increase of the right-hand side, must contain $\sqrt{1 - m}$. Yet above the critical temperature that would give imaginary values for a and b and is obviously at variance with the whole behaviour of gases. It also appeared that no other assumption for the internal pressure can lead to such a rapid initial increase as the one under discussion. It should not be forgotten that the critical temperature is actually not a special temperature. At this temperature the co-existing densities are equal in magnitude. That alone gives this temperature a meaning which under all other circumstances it does not have. An abrupt jump, a rapid increase either in a or b would make the temperature quite a special one at any level of compression and its determination would then be possible at any density level. In fact, bluntly speaking, the result would be: an equation of state compatible with experimental data is totally impossible. No such equation is possible, unless something is added, namely that the molecules associate to form larger complexes; this year, therefore, I have given two treatises at the Academy in Amsterdam on this possible association. I have termed it « pseudo association » to differentiate it from the association which is of chemical origin. The possible formation of larger molecular complexes, particularly in the liquid state, has frequently been emphasized and the finding that the assumption is necessary to achieve agreement between the state equation

and experiment will hence cause no surprise. Unfortunately my examination is still incomplete. I have found it arduous. And I have had to make use of every piece of evidence to derive something concrete. Nevertheless so much has emerged that it will have to be assumed that a large number of single molecules are required to form a new group which holds together and behaves as a new, larger unit in the molecular motion.

What is the origin of this complex formation, this pseudo association? I was compelled to assume it because it seemed to me the only way to make an equation of state – whichever it is – compatible with the results of measurements. However, as a result of a remark by Debye in last month's *Annalen der Physik*, I remembered a phrase of Boltzmann's. When a few years ago I was privileged to have him with me for some days, among the many matters which we discussed he told me in passing that he was unable to reduce the attraction of molecules to a surface force. At the time the significance of this remark of Boltzmann's did not dawn on me – only now do I think I appreciate what he meant. As far back as in my treatise of 1873 I came to the conclusion that the attraction of the molecules decreases extremely quickly with distance, indeed that the attraction only has an appreciable value at distances close to the size of the molecules. At the time this even prompted me to state that in the case of gases the collisions alone are responsible for their exhibiting an attraction. And it must have been Boltzmann's view that it is only admissible to adopt Laplace's procedure and assume a surface force as a consequence of the attraction provided it does not fall off so rapidly.* Debye's remark implied that Boltzmann had predicted the formation of a complex. Thus, so I believe, the assumption of pseudo association is justified from the theoretical standpoint. And now I think I may state how I proceeded in my latest treatise which appeared in November of this year. Pseudo association differs from true association in that the latter is the result of new chemical forces which arise only when molecules are combined to form e.g. double molecules, whereas pseudo association must be ascribed wholly to the normal molecular forces. Now, since this force diminishes so quickly, it has two consequences. Firstly it results in the formation of a complex, but that is not all. Secondly it leads to a surface pressure, although a lower one. And this is precisely what I was compelled to assume in my calculations were I to have some prospect of accounting for the cited differences. I thus sought to make this clear in the following manner. Let the number of molecules that have combined into a complex be so large that it

* Cf. L. Boltzmann, *Gastheorie*, II, p.176.

is possible to speak of a molecule at the centre surrounded by a single layer containing almost as many other molecules as is possible simultaneously. Then, for the surrounding molecules the attraction directed towards the interior acts only to maintain the complex; and this part of its attraction is lost for the surface pressure. Only the forces acting outwards from these molecules can contribute to the formation of the internal pressure. But of course, for pseudo association as for true association the number of formed complexes increases with decreasing temperature and volume. At the critical point, so I was compelled to conclude, only a very small part of the weight is present as complexes.

If pseudo association exists in a substance, there are at least two types of molecules, namely simple and complex. I say at least two types because it cannot be assumed that all complexes are of equal size. But as a first step I have assumed only two types, i.e. simple molecules and n-fold molecules. For a really scientific treatment, of course, it would be necessary to assume all values of n as possible and to seek the law of distribution for these values. For the time being, however, I have confined myself to assuming only a single type of complex. We then have a binary mixture. It was very fortunate that for many years I had made a serious study of the laws of binary mixtures – and so I come to the fourth point of my lecture. Not to demand too much of your attention, however, I promise to be quite brief.

(4) When I first conceived the idea of utilizing my equation to study the properties of binary mixtures I can no longer say. But even 20 years ago, at the insistence of my friend Kamerlingh Onnes, I was able to publish a complete theory for binary mixtures. My « Théorie moléculaire d'une substance composée de deux matières différentes » (Molecular theory of a substance made up of two different constituents) appeared in the *Archives Néerlandaises* for 1890. I had written it in Dutch but my esteemed friend Bosscha undertook the difficult task of rendering it into French, a task which was all the more exacting as I had written it in an extremely concise form and the mathematical treatment led to particular points (plait points) and particular curves which at the time had rarely been examined in detail. Previously, however, my friend Korteweg, to whom I had communicated in broad outlines the outcome of my examinations, had studied the mathematical properties of these points and curves, a study which I have often found of great use. The reasons why I hesitated so long over publication were many and it would serve little purpose to enumerate them here. But one of the

reasons of scientific importance was the question which I kept asking myself: is it any use, until the study of the equation of state has been completed, applying it to mixtures? I appreciated in advance that as long as I was ignorant of the law governing the variability of b and hence had to assume that b did not vary with the volume, the results for many parameters would yield values exhibiting numerically large differences as compared with the values of these parameters as determined by experiment. Nevertheless, the consideration that even with b constant my theory had not been unimportant in the case of a single substance gave me the hope that many phenomena would be explained qualitatively provided that a suitable value for a and b were introduced for mixtures. It was of great importance for me to be acquainted with Gibbs' treatises on the equilibrium of heterogeneous substances which he had sent me immediately after their appearance. I made use inter alia of his principle that for a given amount of substance equilibrium sets in if the free energy is minimum for the given temperature and volume. In his honour I named the equilibrium surface for a binary system the ψ-surface. For, the free energy whose significance for the equilibrium he was the first to recognize, he always represents by the sign ψ.

The phenomena are really not complex in the case of a simple substance and it is an easy matter to obtain a general picture of them. It is therefore very surprising that in a mere binary mixture they become so complex that they have often been compared with a labyrinth. This is particularly so where three-phase pressure can exist. And it has now become clear that they are at least qualitatively in agreement with the « Théorie moléculaire, etc. » from which they can be derived and indeed often predicted. Owing to the many experimental studies to which it has given rise I have frequently found the opportunity of discussing it closely in special communications. Thus in the years 1907 to 1909 about 15 treatises of this nature have appeared in the proceedings of the Academy at Amsterdam. It would be an impressive number were I just to mention the names of the physicists and chemists who, guided by this theory, have studied and still are studying binary mixtures. It begins with Kuenen and ends with Dr. Jean Timmermans of Belgium whose studies have still to be completed but which have already been published in part.

And if now I may be permitted to look back on the way I have come, I must confess that it may fairly be called a detour. Immediately the necessity of assuming association became clear to me I extended my equation of state

analogously to the formula for a binary mixture and introduced a new parameter, the degree of association. This degree of association is determined by means of Gibbs' equation to which I have referred. The degree of association thus determined must then be introduced into the equation of state. And I confess that this is a detour. Perhaps there is a direct way. That this way is seriously being sought I know from those in direct contact with me. In the search for this way Gibbs' *Elementary Principles of Statistical Mechanics* will be a necessary guide. In this lecture, I have only given the history of the origin and further elaboration of my theoretical studies and therefore had to speak of the difficulties that had to be surmounted. Consequently I could not, or only meagrely, discuss how they have assisted in correctly understanding the phenomena. One of the main conclusions, which I have termed the « Law of corresponding states », has, I may say, become universally known. Nor have I discussed how this law was a potent contributory factor in Dewar's determination of the method of liquefying hydrogen, and particularly in Kamerlingh Onnes' determination of the method of liquefying helium. I have also forgone detailed discussion of the temperature at which, to use Regnault's nomenclature, a gas starts to behave like a « pluperfect gas » (gaz plus que parfait), which temperature has been found to have a value of $(27/8)T_k$ – besides the temperature at which it may be stated that the Joule-Kelvin effect reverses: its value is found to be $(27/4)T_k$.

Neither have I spoken of my perhaps somewhat overhasty efforts to determine the equation of state of the molecule itself. I have even omitted to say why I thought I had to go to such trouble to determine the relation between p, v and T for a substance. The formulae of thermodynamics are effective and can actually be applied in all problems, even to determining thermal parameters, only when this relation is known, otherwise they can be regarded as just *one* equation between two unknowns. I have explicitly emphasised this in the book in commemoration of Kamerlingh Onnes published in 1904.

But it was not my intention to discuss all that and I would not have had sufficient time. Yet it does not seem to me superfluous, perhaps it is even necessary, to make a general observation. It will be perfectly clear that in all my studies I was quite convinced of the real existence of molecules, that I never regarded them as a figment of my imagination, nor even as mere centres of force effects. I considered them to be the actual bodies, thus what we term « body » in daily speech ought better to be called « pseudo body ». It is an aggregate of bodies and empty space. We do not know the nature of a

molecule consisting of a single chemical atom. It would be premature to seek to answer this question but to admit this ignorance in no way impairs the belief in its real existence. When I began my studies I had the feeling that I was almost alone in holding that view. And when, as occurred already in my 1873 treatise, I determined their number in one gram-mol, their size and the nature of their action, I was strengthened in my opinion, yet still there often arose within me the question whether in the final analysis a molecule is a figment of the imagination and the entire molecular theory too. And now I do not think it any exaggeration to state that the real existence of molecules is universally assumed by physicists. Many of those who opposed it most have ultimately been won over, and my theory may have been a contributory factor. And precisely this, I feel, is a step forward. Anyone acquainted with the writings of Boltzmann and Willard Gibbs will admit that physicists carrying great authority believe that the complex phenomena of the heat theory can only be interpreted in this way. It is a great pleasure for me that an increasing number of younger physicists find the inspiration for their work in studies and contemplations of the molecular theory. The crowning of my studies by the esteemed Royal Swedish Academy affords me satisfaction and fills me with gratitude, a gratitude which I cannot call eternal and in my old age I cannot even promise that it will be of long duration, but perhaps for this very reason my gratitude is all the more intense.

Biography

Johannes Diderik van der Waals was born on November 23, 1837 in Leyden, The Netherlands, the son of Jacobus van der Waals and Elisabeth van den Burg. After having finished elementary education at his birthplace he became a schoolteacher. Although he had no knowledge of classical languages, and thus was not allowed to take academic examinations, he continued studying at Leyden University in his spare time during 1862–65. In this way he also obtained teaching certificates in mathematics and physics.

In 1864 he was appointed teacher at a secondary school at Deventer; in 1866 he moved to The Hague, first as teacher and later as Director of one of the secondary schools in that town.

New legislation whereby university students in science were exempted from the conditions concerning prior classical education enabled Van der Waals to sit for university examinations. In 1873 he obtained his doctor's degree for a thesis entitled *Over de Continuïteit van den Gas- en Vloeistoftoestand* (On the continuity of the gas and liquid state), which put him at once in the foremost rank of physicists. In this thesis he put forward an «Equation of State» embracing both the gaseous and the liquid state; he could demonstrate that these two states of aggregation not only merge into each other in a continuous manner, but that they are in fact of the same nature. The importance of this conclusion from Van der Waals' very first paper can be judged from the remarks made by James Clerk Maxwell in *Nature*, «that there can be no doubt that the name of Van der Waals will soon be among the foremost in molecular science» and «It has certainly directed the attention of more than one inquirer to the study of the Low-Dutch language in which it is written» (Maxwell probably meant to say «Low-German», which would also be incorrect, since Dutch is a language in its own right). Subsequently, numerous papers on this and related subjects were published in the *Proceedings of the Royal Netherlands Academy of Sciences* and in the *Archives Néerlandaises*, and they were also translated into other languages.

When, in 1876, the new Law on Higher Education was established which promoted the old Athenaeum Illustre of Amsterdam to university status, Van

der Waals was appointed the first Professor of Physics. Together with Van 't Hoff and Hugo de Vries, the geneticist, he contributed to the fame of the University, and remained faithful to it until his retirement, in spite of enticing invitations from elsewhere.

The immediate cause of Van der Waals' interest in the subject of his thesis was R. Clausius' treatise considering heat as a phenomenon of motion, which led him to look for an explanation for T. Andrews' experiments (1869) revealing the existence of «critical temperatures» in gases. It was Van der Waals' genius that made him see the necessity of taking into account the volumes of molecules and the intermolecular forces («Van der Waals forces», as they are now generally called) in establishing the relationship between the pressure, volume and temperature of gases and liquids.

A second great discovery – arrived at after much arduous work – was published in 1880, when he enunciated the Law of Corresponding States. This showed that if pressure is expressed as a simple function of the critical pressure, volume as one of the critical volume, and temperature as one of the critical temperature, a general form of the equation of state is obtained which is applicable to all substances, since the three constants a, b, and R in the equation, which can be expressed in the critial quantities of a particular substance, will disappear. It was this law which served as a guide during experiments which ultimately led to the liquefaction of hydrogen by J. Dewar in 1898 and of helium by H. Kamerlingh Onnes in 1908. The latter, who in 1913 received the Nobel Prize for his low-temperature studies and his production of liquid helium, wrote in 1910 «that Van der Waals' studies have always been considered as a magic wand for carrying out experiments and that the Cryogenic Laboratory at Leyden has developed under the influence of his theories».

Ten years later, in 1890, the first treatise on the «Theory of Binary Solutions» appeared in the *Archives Néerlandaises* – another great achievement of Van der Waals. By relating his equation of state with the Second Law of Thermodynamics, in the form first proposed by W. Gibbs in his treatises on the equilibrium of heterogeneous substances, he was able to arrive at a graphical representation of his mathematical formulations in the form of a surface which he called «ψ-surface» in honour of Gibbs, who had chosen the Greek letter ψ as a symbol for the free energy, which he realised was significant for the equilibrium. The theory of binary mixtures gave rise to numerous series of experiments, one of the first being carried out by J. P. Kuenen, who found characteristics of critical phenomena fully predictable by the

theory. Lectures on this subject were subsequently assembled in the *Lehrbuch der Thermodynamik* (Textbook of thermodynamics) by Van der Waals and Ph. Kohnstamm.

Mention should also be made of Van der Waals' thermodynamic theory of capillarity, which in its basic form first appeared in 1893. In this, he accepted the existence of a gradual, though very rapid, change of density at the boundary layer between liquid and vapour – a view which differed from that of Gibbs, who assumed a sudden transition of the density of the fluid into that of the vapour. In contrast to Laplace, who had earlier formed a theory on these phenomena, Van der Waals also held the view that the molecules are in permanent, rapid motion. Experiments with regard to phenomena in the vicinity of the critical temperature decided in favour of Van der Waals' concepts.

Van der Waals was the recipient of numerous honours and distinctions, of which the following should be particularly mentioned. He received an honorary doctorate of the University of Cambridge; was made honorary member of the Imperial Society of Naturalists of Moscow, the Royal Irish Academy and the American Philosophical Society; corresponding member of the Institut de France and the Royal Academy of Sciences of Berlin; associate member of the Royal Academy of Sciences of Belgium; and foreign member of the Chemical Society of London, the National Academy of Sciences of the U.S.A., and of the Accademia dei Lincei of Rome.

In 1864, Van der Waals married Anna Magdalena Smit, who died early. He never married again. They had three daughters and one son. The daughters were Anne Madeleine who, after her mother's early death, ran the house and looked after her father; Jacqueline Elisabeth, who was a teacher of history and a well-known poetess; and Johanna Diderica, who was a teacher of English. The son, Johannes Diderik Jr., was Professor of Physics at Groningen University 1903–08, and subsequently succeeded his father in the Physics Chair of the University of Amsterdam.

Van der Waals' main recreations were walking, particularly in the country, and reading. He died in Amsterdam on March 8, 1923.

Physics 1911

WILHELM WIEN

«for his discoveries regarding the laws governing the radiation of heat»

Physics 1911

Presentation Speech by the Librarian of the National Library, Dr. E. W. Dahlgren, President of the Royal Swedish Academy of Sciences

Your Majesty, Your Royal Highnesses, Ladies and Gentlemen.

The Royal Academy of Sciences has awarded the Nobel Prize for Physics, for the year 1911, to Wilhelm Wien, Professor at the University of Würzburg, for his discoveries concerning the laws of heat radiation.

Ever since the beginning of the last century and, in particular, since spectrum analysis reached an advanced stage of development as a result of the fundamental work by Bunsen and Kirchhoff, the problem of the laws of heat radiation has occupied the attention of physicists to an exceptionally high degree.

The solution of this problem has presented immeasurable difficulties both in the theoretical and experimental respects, and it would hardly seem possible to solve this task without a knowledge of certain laws which embrace a wide diversity of radiating bodies.

One of these is the famous Kirchhoff law of the relationship between the ability of substances to emit and to absorb radiation energy. It relates the laws of radiation of all bodies so far as their radiation is dependent on temperature to those laws which are valid for the radiation emitted by a completely black body.

The search for the latter laws has therefore been one of the most fundamental problems of radiation theory. These laws have been discovered in the last decades and, by virtue of the great importance that attaches to them, belong to the major achievements of modern physics.

The difficulty in investigating the laws of radiation of black bodies was, firstly, that no completely black body exists in nature. In accordance with Kirchhoff's definition, such a body would reflect no light at all, nor allow light to pass. Even substances such as soot, platinum black etc. reflect part of the incident light.

This difficulty was only removed in 1895, when Wien and Lummer stated the principles according to which a completely black body could be constructed, and showed that the radiation which issues from a small hole in a hollow body whose walls have the same temperature behaves in the same

manner as the radiation emitted by a completely black body. The principle of this arrangement is based on the views of Kirchhoff and Boltzmann and had already been applied in part by Christiansen in 1884.

With the assistance of this apparatus it now became possible to investigate black body radiation. In this manner, Lummer, together with Pringsheim and Kurlbaum, succeeded in substantiating the so-called Stefan-Boltzmann law which indicates the relationship between the quantity of heat radiated by a black body and its temperature.

This solved in a highly satisfactory manner one of the major problems of radiation theory, i.e. that which touches total black body radiation.

However, the thermal energy that radiates from a body contains rays of different wavelengths whose intensities differ and change with the temperature of the body. It therefore remained to investigate the manner of change in intensity with wavelength and temperature.

An important step towards the solution of this question had been taken as early as 1886 by Langley who, with his famous spectrobolometer, investigated the distribution of radiation in the spectrum of a number of heat sources of high and low temperature. Inter alia these classical researches showed that the radiation had a maximum for a certain wavelength and that the maximum shifted in the direction of the shorter waves with increasing temperature.

In 1893 Wien published a theoretical paper which was destined to acquire the utmost importance in the development of radiation theory. In this paper he presented his so-called displacement law which provides a very simple relationship between the wavelength having the greatest radiation energy and the temperature of the radiating black body.

The importance of Wien's displacement law extends in various directions. As we shall see, it provides one of the conditions which are required for the determination of the relationships between energy radiation, wavelength and temperature for black bodies, and thus represents one of the most important laws in the theory of heat radiation. Wien's displacement law has however acquired the greatest possible importance in other contexts as well. Lummer and Pringsheim have shown that the radiation of bodies other than black bodies obeys the displacement law, with the sole difference that the constant which forms part of the formula has a different value.

Thus it became possible to determine the temperature of bodies, within fairly narrow limits, simply by seeking the wavelength at which radiation is greatest. The method has successfully been applied to the determination

of the temperature of our light sources, of the sun and of some of the fixed stars, and has yielded extremely interesting results.

The Stefan-Boltzmann law and the Wien displacement law are the most penetrating statements on a secure theoretical foundation that have been discovered with respect to thermal radiation. They do not solve the central problem, i.e. the question as to the distribution of radiation energy over the various wavelengths at different black body temperatures. We can however say that Wien's displacement law provides half the answer to the problem. We have one condition for determining the desired function. One more would be sufficient for solving the problem.

It was only natural that Wien who had contributed so much to the advancement of radiation theory should make an attempt to find an answer to the last remaining question also, i.e. that of the distribution of energy in radiation. In 1894 he indeed deduced a black body radiation law. This law has the virtue that, at short wavelengths, it agrees with the above-mentioned experimental investigations by Lummer and Pringsheim.

By a different approach from that used by Wien, Lord Rayleigh also succeeded in discovering a law of radiation. By contrast with that discovered by Wien, it agrees with experiment for long wavelengths.

The problem now became to bridge the gap between these two laws each of which had been shown to be valid in a specific context. It was Planck who solved this problem; as far as we are aware, his formula provides the long sought-after connecting link between radiation energy, wavelength and black body temperature.

These remarks show that we now know, with considerable accuracy, the laws that govern thermal black body radiation.

A magnificent and unique task has thus been undertaken and brought to a certain conclusion – a task which has claimed the liveliest interest and energy of the leading physicists of our time.

Among the researchers in this field now living it was Wilhelm Wien who made the greatest and most significant contribution, and the Academy of Sciences has therefore decided to award to him the Nobel Prize for Physics for the year 1911.

Professor Wien. The Swedish Academy of Sciences has awarded to you this year's Nobel Prize for Physics for your discoveries concerning the laws of thermal radiation. You have devoted your researches to one of the most difficult and spectacular problems of physics, and among the researchers now

living it is you who has succeeded in making the greatest and most significant contributions to the solution of the problem. In admiration of the completed task and with the wish that further success may be granted to you in future work, the Academy now calls upon you to receive the prize from the hands of his Majesty the King.

WILHELM WIEN

On the laws of thermal radiation

Nobel Lecture, December 11, 1911

The kind recognition which my work on thermal radiation has received in the views of your ancient and famous Academy of Sciences gives me particular pleasure to speak to you about this subject which is again attracting the attention of all physicists because of the difficulty of the problems involved. As soon as we step beyond the established boundaries of pure thermodynamic theory, we enter a trackless region confronting us with obstacles which even the most astute of us are almost at a loss to tackle.

If, as is the custom, I speak mainly about my own researches, I must say that I was fortunate in finding that not everything had yet been gleaned in the field of general thermodynamic radiation theory. Using known physical laws it was possible to derive a general law of radiation theory which has, under the name of the displacement law, been acclaimed by fellow workers. In applying thermodynamics to the theory of radiation, we make use of the ideal processes which have been found so fruitful elsewhere. These are mental experiments whose realization is frequently impracticable and which nevertheless lead to reliable results. Such deliberations can only be undertaken if all the processes on which, governed by laws, the mental experiments are based, are known, so that the effect of any change can be stated accurately and completely. Further, to be allowed to idealize, we must neglect all non-essential secondary phenomena, while considering only everything indissolubly connected with the processes under examination. In the application of mechanical heat theory, this method has proved to be extremely fruitful. Helmholtz used it in the theory of concentration flows, Van 't Hoff used it in applying thermodynamics to the theory of solutions. It is necessary, in these deliberations, to presuppose the existence of a so-called semi-permeable membrane which permits the solvent to pass, but not the substance dissolved. Although it is impossible to prepare membranes which strictly satisfy this requirement, we can assume them as possible in the ideal processes, because the laws of Nature set no limit to approximation to semipermeability. The conclusions drawn from these assumptions have in any case always been in agreement with experience. In radiation theory,

analogous deliberations can be made if we assume perfectly reflecting bodies as possible in the ideal processes. Kirchhoff used them for proving his famous theorem of the constancy of the ratio of emission and absorption power. This theorem has become one of the most general of radiation theory and expresses the existence of a certain temperature equilibrium for radiation. According to it, there must exist, in a cavity surrounded by bodies of equal temperature, a radiation energy that is independent of the nature of the bodies. If in the walls surrounding this cavity a small aperture is made through which radiation issues, we obtain a radiation which is independent of the nature of the emitting body, and is wholly determined by the temperature. The same radiation would also be emitted by a body which does not reflect any rays and which is therefore designated as completely black, and this radiation is called the radiation of a black body or black-body radiation.

The Kirchhoff theorem is not limited to radiation caused by thermal processes. It seems to be valid for most, if not all luminous processes. That the temperature concept can be applied to all luminous processes is beyond doubt. Since we can produce all types of light by means of hot bodies, we can ascribe, to the radiation in thermal equilibrium with hot bodies, the temperature of these bodies, and thus every radiation, even that issueing from a phosphorescent body, has a certain temperature for every colour. This temperature has however no connection whatever with that of the body, nor is it possible as yet to state how e.g. a phosphorescent body comes into equilibrium with radiation. These conditions are bound to be very complicated, in particular in the case of bodies which convert the absorbed radiation and emit it after a long interval of time.

Again using ideal processes and assuming radiation pressure, which at that time had been deduced from the electromagnetic theory of light, Boltzmann derived from thermodynamics the law, previously empirically formulated by Stefan, that the radiation of a black body is proportional to the fourth power of the absolute temperature.

This did not exhaust the conclusions to be drawn from thermodynamics. There remained the determination of the changes undergone by the colours present in radiation with changes of temperature. Computation of this change is again based on an ideal process. For this, we must assume wholly reflecting bodies as possible that scatter all incident radiation and which can therefore be described as completely white. If we allow the radiation coming from a black body to enter a space of this kind, it will in the end propagate

exactly as if the walls of the space were themselves radiant and had the same temperature as the black body. If we then seal off the black body from the white space, we obtain the unrealisable case of a radiation permanently reciprocated between mirroring walls. In our thoughts, we continue the experiment. We imagine the volume of our space to be reduced by movement of the walls, so that the entire radiation is concentrated in a smaller space. Since radiation exercises a certain pressure, the pressure of light, on the walls it strikes, it follows that some work must have been expended in size reduction, as if we had compressed a gas. Because of the low magnitude of the pressure of light, this work is very small, but it can be computed accurately, which is all that matters in the case under discussion. In accordance with the principle of the conservation of energy, this work cannot be lost, it is converted into radiation, which further increases the radiation concentration. This change of radiation density due to the movement of the white walls is not the only change to which the radiation is subjected. When a light ray is reflected by a moving mirror, it undergoes a change of the colour dictated by the oscillation frequency. This change in accordance with the so-called Doppler principle plays a substantial part in astrophysics. The spectrum line emitted by an approaching celestial body appears to be displaced in the direction of shorter wavelengths in the ratio, its velocity: the velocity of light. This is also the case when a ray is reflected by a moving mirror, except that the change is twice as great. We are therefore able to calculate completely the change undergone by the radiation as a result of the movement of the walls. The pressure of light which is essential to these deliberations was demonstrated at a much later date, Lebedev being the first to do so. Arrhenius used it to explain the formation of comet tails. Before that, it was only a conclusion drawn from Maxwell's electromagnetic theory. We now calculate both the change of radiation density due to movement, and the change of the various wavelengths. From this mental experiment, we can draw an important conclusion. We can conclude from the second law of mechanical heat theory that the spectral composition of the radiation which we have changed by compression in the space with mirror walls is exactly the same as it would be had we obtained the increased density of radiation by raising the temperature, because we would otherwise be able to produce, by means of colour filters, unequal radiation densities in the two spaces, and to generate work from heat without compensation. Since we can calculate the change of individual wavelengths due to compression, we can also derive the manner in which the spectral composition of black-body radiation varies

with temperature. Without discussing this calculation in detail, let me give you the result: the radiation energy of a certain wavelength varies with changing temperature so that the product of temperature and wavelength remains constant.

Using this displacement law it is easy to calculate the distribution of the intensity of thermal radiation over the various wavelengths for any temperature, as soon as it is known for one temperature.

The shift of the maximum of intensity in particular is directly accessible to observation. Since the wavelength at which the maximum intensity lies also defines the principal area of the wavelength which is most intense at this temperature, we can, by changing the temperature, shift the principal area of radiation in the direction of short or long wavelengths of any desired magnitude. Of the other derivations of the displacement law, I shall only mention that by H. A. Lorentz. If, in the electromagnetic equations of Maxwell, we imagine all spatial dimensions as being displaced in time in the same ratio, these equations show that the electromagnetic energy must decrease in proportion to the fourth power of displacement. Since, according to the Stefan-Boltzmann law, energy increases with the fourth power of absolute temperature, the linear dimensions must vary inversely proportionately to the absolute temperature. Each characteristic length must vary in this ratio, from which the displacement law follows.

From the displacement law, we can calculate the temperature of the sun if we are entitled to assume that the radiation of the sun must be ascribed to heat, and if we know the position of the maximum of the energy of solar radiation. Different figures are given for this position by different observers, i.e. 0.532 μ according to Very, and 0.433 μ according to Abbot and Fowle. Depending on the figure used, the temperature of the sun works out at 5,530° and 6,790°. However much the observers may differ, there can be no doubt that the maximum of solar radiation is situated in the visible range of wavelengths. This is to say that the temperature of the sun is the most favourable utilization of the radiant energy of a black body for our illumination and that, in our artificial light sources which utilize thermal radiation we must aim at achieving this temperature, from which we are admittedly far removed as yet.

I wish to discuss yet another application of the displacement law, i.e. the possibility of calculating the wavelength of X-rays. As we know, X-rays are produced by the impact of electrons on solid bodies, and their wavelength must be a function of the velocity of the electrons. According to the

kinetic theory of gases, the mean kinetic energy of a molecule is a measure of absolute temperature. If, as is done in the theory of electrons, we assume that this is also valid for the kinetic energy of the electrons, the electric energy of the cathode rays would be a measure of their temperature. If we substitute the temperature thus calculated in the displacement law, we find that the wavelength of the maximum of the intensity indicates a wavelength range of X-rays which agrees well with the wavelengths found by other arguments. It might be objected that we must not ascribe a temperature to the electrons. The permissibility of our procedure can however be justified by an inversion of the above argument. Radiation in an enclosed space must necessarily release electrons whose velocity according to Einstein's law is proportional to the oscillation frequency. The energy maximum of radiation generates electrons whose velocity is so great that their kinetic energy comes very close to the temperature associated with the maximum of energy.

The displacement law exhausts the conclusions that can be drawn from pure thermodynamics with respect to radiation theory. All these conclusions have been confirmed by experience. The individual colours present in the radiation are mutually wholly independent. The manner in which at a given temperature the intensity of radiation is distributed over the individual wavelengths cannot be determined from thermodynamics. For this, one must examine the mechanism of the radiation process in detail. Similar conditions obtain in the theory of gases. Thermodynamics can tell us nothing about the magnitude of the specific heat of the gases; what is required, is to examine molecular motion. But the kinetic theory of gases which is based on probability calculations has made much greater progress than the corresponding theory of radiation. The statistical theory of gases has set itself the task of accounting also for the laws of thermodynamics. I do not wish to discuss here the extent to which the task may be considered as having been solved, and whether we are entitled to consider the reduction of the second law to probability as a wholly satisfactory theory. It has in any case been very successful, in particular since a theoretical explanation has been found of the deviations from the thermodynamic state of equilibrium, the so-called fluctuations, e.g. in Brownian movement. None of the statistical theories of radiation has however as yet even attempted to derive the Stefan-Boltzmann law and the displacement law, which must always be introduced into theory from outside. Quite apart from this, we are as yet far removed from a satisfactory theory to account for the distribution of radiation energy over the individual wavelengths.

I myself made the first attempt in this direction. I endeavoured to bypass the problem of applying probability calculation to radiation theory by imagining radiation as resulting from gas molecules moving according to laws of probability. Instead of these we could also imagine electrons generating radiation on striking molecules. What is essential is the further assumption that such a particle will only emit radiation of a certain wavelength dictated by velocity, and that the velocity distribution of the particles obeys Maxwell's law. With the assistance of the radiation laws derived from thermodynamics we obtain a radiation law which shows good agreement with experience for a wide range of wavelengths, i.e. for the range in which the product of temperature and wavelength is not unduly large.

Imperfect as this first attempt was, a formula had been obtained which considerably deviates from reality for large wavelengths only. Since observations however establish these deviations beyond doubt, it was clear that the formula had to be modified.

Lord Rayleigh was the first to approach the problem from an entirely different angle. He made the attempt to apply to the radiation problem a very general theorem of statistical mechanics, i.e. the theorem of the uniform distribution of energy over the degrees of freedom of the system in the state of statistical equilibrium. The meaning of this theorem is as follows:

In the state of thermal equilibrium, all movements of the molecules are so completely irregular that there exists no movement which would be preferred over any other. The position of the moving parts can be established by geometrical parameters which are mutually independent and in the direction of which the movement falls. These are called the degrees of freedom of the system. As regards the kinetic energy of movement, no degree of freedom is preferred over another, so that each contains the same amount of the total energy.

Radiation present in an empty space can be represented so that a given number of degrees of freedom is allocated to it. If the waves are reflected back and forth by the walls, systems of standing waves are established which adapt themselves to the distances between two walls. This is most easily understood if we consider a vibrating string which can execute an arbitrary number of individual vibrations, but whose half wavelengths must be equal to the length of the string divided by an integer.

The individual standing waves possible represent the determinants of the processes and correspond to the degrees of freedom. If we allocate to each degree of freedom its proper amount of energy, we obtain the Rayleigh

radiation law, according to which the emission of radiation of a given wavelength is directly proportional to the absolute temperature, and inversely proportional to the fourth power of the wavelength. The law agrees with observation at exactly the point where the law discussed above failed, and it was at first considered to be a radiation law of limited validity. But if the process of radiation obeys the general laws of electromagnetic theory or of the theory of electrons, we must necessarily arrive at Rayleigh's radiation law, as Lorentz has shown. Viewed as a general radiation law, it directly contradicts all experience, because, according to it, energy would have to accumulate increasingly at the shortest wavelengths. The possibility that we are not dealing in reality with a true state of equilibrium of radiation, but that it very gradually approaches the state where all energy is only present in the shortest wavelengths, is also contradicted by experience. In the case of the visible rays, to which the Rayleigh formula no longer applies at attainable temperatures, we can easily calculate, according to the Kirchhoff law, that the state of equilibrium must be attained in the shortest time, which state however remains far removed from the Rayleigh law. We thus obtain an inkling of the extraordinary difficulties which confront exact definition of the radiation formula. The knowledge that current general electromagnetic theory is insufficient, that the theory of electrons is inadequate, to account for one of the most common of phenomena, i.e. the emission of light, remains purely negative as yet. We only know how the thing cannot be done, but we lack the signposts that would enable us to find our way. We do however know that none of the models whose mode of action is based on purely electromagnetic principles can lead to correct results.

It is the merit of Planck to have introduced new hypotheses which enable us to avoid Rayleigh's radiation law. For long waves, this law is undoubtedly correct, and the right radiation formula must have a form such that, for very long waves, it passes into Rayleigh's law, and for short waves into the law formulated by me. Planck therefore retains as starting point the distribution of energy over the degrees of freedom of the system, but he subjects this distribution of energy to a restriction by introducing the famous hypothesis of elements of energy, according to which energy is not infinitely divisible, but can only be distributed in rather large quantities which cannot be divided further. This hypothesis would probably have been accepted without difficulty, if unchangeable particles, e.g. atoms of energy had been involved. It is an assumption that has proved inevitable for matter and electricity. The energy elements of Planck are however no atoms of energy; on the contrary,

the displacement law requires that they are inversely proportional to the wavelength of a given vibration. This represents great difficulty for the understanding of these energy elements. Once we accept the hypothesis, we arrive at an entirely different distribution of energy over the radiating centres, if we search for them according to the laws of probability. This does not however give us the radiation law. All we know is how much energy the radiating molecules possess on average at a certain temperature, but not how much energy they emit. To derive emission at a given energy, we need a definite model which emits radiation. We can only construct such a model on the foundation of the known electromagnetic laws, and it is at this early point that the difficulty of the theory starts. On the one hand we relinquish the electromagnetic laws by introducing the energy elements; on the other hand we make use of these same laws for discovering the relationship between emission and energy. It could admittedly be argued that the electromagnetic laws are only valid for mean values taken over extended periods, whereas the energy elements relate to the elementary radiation process itself. An oscillator radiating in accordance with the electromagnetic laws will indeed have little similarity with the real atoms. Planck however rightly argues that this does not matter precisely because radiation in the equilibrium state is independent of the nature of the emitting bodies. It will however be required of a model which is to stand for the real atoms that it should lack none of the essential characteristics of the event under consideration. Every body that emits thermal rays has the characteristic that it is able to convert thermal rays of one wavelength into thermal rays of a different wavelength. It is on this that there rests the possibility of a specific spectral composition being established in the radiation at all times. The Planck oscillator lacks this ability, and doubts are bound to be raised as to whether it can properly be used for establishing the relationship between energy and emission. This difficulty can be avoided, and the oscillator can be done without, if, with Debye, we decompose the radiation energy in a hollow cube into Planck energy elements and distribute these energy elements over the oscillation frequencies of the standing waves formed in the cube according to the laws of probability. The logarithm of this probability will then be proportional to entropy, and the law of radiation results, if we search for the maximum of this entropy. This result is proof of the extremely general nature of Planck's concepts.

There are however further difficulties. The energy elements increase with decreasing wavelength, and an oscillator exposed to incident radiation will,

at low intensity, need a very long time before it absorbs a full energy element. What happens if the incident radiation ceases, before an entire energy element has been absorbed? The difficulties implicit in answering this question have recently induced Planck to reformulate his original theory. He now assumes of emission only that it can occur exclusively by whole energy elements. Absorption is assumed to occur continuously according to the electromagnetic laws, and the energy content of an oscillator is assumed to have energy values capable of continuous change. The difficulty of the long absorption time is indeed avoided in this manner. On the other hand the close relationship between emission and absorption for the elementary process is relinquished, and this relationship now becomes valid statistically only. Every atom which only emits whole energy elements and absorbs continuously would therefore in the event of emission suddenly expend energy from its own reserve and would supplement this but little in the event of short irradiation. The special hypothesis must be made that, taken as a whole for many atoms in the stationary state, the absorbed energy after all becomes equal to that emitted. Whereas in the original form of the Planck theory the introduction of the hypothesis of energy elements was sufficient to permit the radiation laws to be derived, the new theories include uncertainties which can only be removed by further hypotheses. On the other hand the new fundamental hypothesis provides the possibility of further application, e.g. to electron emission.

It will be seen from the few observations I am able to offer in this context how great are the difficulties that remain in radiation theory. But the reference to these difficulties which it is the duty of the scientific approach to emphasize must not prevent us from paying tribute to the great positive achievements which the Planck theory has already accomplished.

It has produced a law of radiation which accomodates all observed data and includes the Rayleigh formula and my own formula as limiting cases. In addition, it has thrown unexpected light on an entirely different subject, i.e. the theory of specific heats.

It has long been known that the specific heats do not strictly obey the Dulong-Petit law and that they decrease at low temperatures. Diamonds do not obey the Dulong-Petit law even at normal temperatures. This law can be derived from the theorem of the distribution of kinetic energy over the degrees of freedom and states that, in solid bodies, every atom possesses, in accordance with its three degrees of freedom, three times the amount of energy, and, because of the potential energy, altogether six times the amount

of energy of a degree of freedom. If however we apply Planck's distribution of energy by energy elements, we obtain, according to Einstein, a formula for the specific heat which does in fact show the drop of temperature. This result is characteristic of the Planck theory. This theory of specific heats is not derived from the radiation formula, but from the formula for the mean energy of an oscillator which is based directly on the hypothesis of the energy elements. Unfortunately, difficulties are beginning to appear. The exact measurements of specific heats at low temperatures made in Nernst's laboratory have shown that the Einstein formula does not agree with observation. The formula which satisfies the experimental data contains half energy elements in addition to the whole energy elements, which cannot be interpreted satisfactorily as yet. There can however be no doubt that the Planck radiation theory provides the first step to the theory of specific heats.

That the theory should remain in many respects incomplete and provisional, is in the nature of the problem which is perhaps the most difficult which has ever confronted theoretical physics. What is involved is to leave behind the laws of theoretical physics confirmed by direct observation, which alone have been applied in the past, and to enter areas which are beyond the reach of direct observation.

The difficulties which beset radiation theory also emerge in an entirely different approach. Einstein investigated the fluctuations to which radiation is continuously subjected even in the state of equilibrium as a result of the irregularities of the thermal processes. If we imagine a small plate in a cavity filled with radiation, this plate will be subjected to a radiation pressure which is the same on average on both sides of the plate. Since the radiation must contain irregularities, the pressure will alternately be greater on one or the other side so that the plate will execute small irregular movements, similar to the Brownian movement of a dust particle suspended in a liquid. These fluctuations can be derived from probability calculations. According to the Boltzmann theorem there is a simple relationship between entropy and probability. The entropy of radiation is known from the radiation law, so that the probability of state is also known, from which the fluctuations can be calculated. The mathematical expression for these fluctuations consists, in a peculiar manner, of two members. The first is readily understandable: it is due to irregularities which arise as a result of the mutual interference of the many independent beams which meet in one point. Where the density of radiation energy is high, this term alone predominates; it corresponds to the radiation range that obeys Rayleigh's law.

The other term, which cannot be directly explained by the undulation theory, predominates at low density of radiation energy, where the radiation obeys the law formulated by me. It would be understandable if the radiation consisted of the Planck energy elements which would be localized even in an empty space. We cannot however pursue this line of thought. We cannot shake the undulation theory of light, which is one of the most firmly established constructions in the whole of physics. Moreover, the term to be explained by localized energy elements, is not present by itself, and it is *a priori* impossible to introduce a dualistic approach into optics, e.g. to assume simultaneously Huyghens' wave theory and Newton's emanation theory. All we can do is to relinquish the Boltzmann method of applying probability calculations to this type of fluctuations, or to assume that a new irregularity is introduced into radiation with the process of reflection.

In view of the magnitude of the difficulties it is natural that opinions about the path to be pursued should differ greatly. Some are of the opinion that the fundamental principles of electrodynamics must be changed. And yet, previous theory embraces a vast range of facts, it accounts for events even in the most rapid movements of the β-rays, it has proved itself in the most precise optical measurements. In my view, all the signs suggest that the deviations from current theory are due to events within the atom. None of the processes in which the interior of the atom participates are amenable to current theory.

Sommerfeld made an attempt in this direction: He would ascribe to the constant h of the radiation law, which together with the oscillation frequency dictates the magnitude of the energy element, a simple significance for the interior of the atom. It is alleged to determine the period in which an electron entering the atom comes to a stop, as a function of its velocity. On this view, the constant h expresses a universal characteristic of the atoms. This theory permits calculation of the wavelength of X-rays, and it connects two previously independent attempts made by me to carry out this calculation. One method is based on the Planck theory of energy elements in that it assumes that the energy of the so-called secondary electrons released by X-rays is dictated by the energy element. The second method is based on the theory of electrons by means of which it calculates the energy radiated in the X-rays by sudden braking of an electron. From the determination of the energy of the cathode rays and the X-rays we can then calculate the brake path of the electrons and consequently the wavelength of the X-rays. The Sommerfeld theory connects these two theories. It has the great advantage

of explaining the generation of X-rays with the aid of electromagnetic theory. A number of conclusions can then be drawn from this which are in complete agreement with observation, e.g. the polarisation of X-rays, the diversity of emission and of hardness in various directions.

The Sommerfeld theory has the great advantage that it attempts to invest the universal constant h of the Planck radiation theory with physical significance. It has the disadvantage that it has been applied so far only to electron emission and absorption, but cannot yet solve the problem of thermal radiation.

We must admit that the result of radiation theory todate is not a very good one for theoretical physics. As we have seen, only the general thermodynamic theories have proved satisfactory as yet. The theory of electrons has come to grief over the radiation problem, the Planck theory has not yet been brought into a definite form. Research is faced with exceptional difficulties and we cannot discern when and how they can be overcome. In science, the redeeming idea often comes from an entirely different direction, investigations in an entirely different field often throw unexpected light on the dark aspects of unresolved problems. We must base our hope in the future in the expectation that the present era which has proved so fruitful for physics may not pass without a complete solution being found for the problem of thermal radiation. Far-reaching and new thoughts will have to set to work, but the result will be great, because we shall obtain a profound insight into the world of the atom and the elementary processes within it.

Biography

Wilhelm Wien was born on January 13, 1864 at Fischhausen, in East Prussia. He was the son of the landowner Carl Wien, and seemed destined for the life of a gentleman farmer, but an economic crisis and his own secret sense of vocation led him to University studies. When in 1866 his parents moved to Drachstein, in the Rastenburg district of East Prussia, Wien went to school in 1879 first at Rastenburg and later, from 1880 till 1882, at the City School at Heidelberg. After leaving school he went, in 1882, to the University of Göttingen to study mathematics and the natural sciences and in the same year also to the University of Berlin. From 1883 until 1885 he worked in the laboratory of Hermann von Helmholtz and in 1886 he took his doctorate with a thesis on his experiments on the diffraction of light on sections of metals and on the influence of materials on the colour of refracted light.

His studies were then interrupted by the illness of his father and, until 1890, he helped in the management of his father's land. He was, however, able to spend, during this period, one semester with Helmholtz and in 1887 he did experiments on the permeability of metals to light and heat rays. When his father's land was sold he returned to the laboratory of Helmholtz, who had been moved to, and had become President of, the Physikalisch-Technische Reichsanstalt, established for the study of industrial problems. Here he remained until 1896 when he was appointed Professor of Physics at Aix-la-Chapelle in succession to Philipp Lenard. In 1899, he was appointed Professor of Physics at the University of Giessen. In 1900 he became Professor of the same subject at Würzburg, in succession to W. C. Röntgen, and in this year he published his *Lehrbuch der Hydrodynamik* (Textbook of hydrodynamics).

In 1902 he was invited to succeed Ludwig Boltzmann as Professor of Physics at the University of Leipzig and in 1906 to succeed Drude as Professor of Physics at the University of Berlin; but he refused both these invitations.

In 1920 he was appointed Professor of Physics at Munich, where he remained throughout the rest of his life.

In addition to the early work already mentioned, Wien worked, at the

Physikalisch-Technische Reichsanstalt, with Holborn on methods of measuring high temperatures with the Le Chatelier thermoelements and at the same time did theoretical work on thermodynamics, especially on the laws governing the radiation of heat.

In 1893 he announced the law which states that the wavelength changes with the temperature, a law which later became the law of displacement.

In 1894 he published a paper on temperature and the entropy of radiation, in which the terms temperature and entropy were extended to radiation in empty space. In this work he was led to define an ideal body, which he called the black body, which completely absorbs all radiations. In 1896 he published the formula of Wien, which was the result of work undertaken to find a formula for the composition of the radiation of such a black body. Later it was proved that this formula is valid only for the short waves, but Wien's work enabled Max Planck to resolve the problem of radiation in thermal equilibrium by means of quantum physics. For this work Wien was awarded the Nobel Prize for Physics for 1911. An interesting point about it is that this theoretical work came from an Institute devoted to technical problems and it led to new techniques for illumination and the measurement of high temperatures.

When Wien moved, in 1896, to Aix-la-Chapelle to succeed Lenard, he found there a laboratory equipped for the study of electrical discharges *in vacuo* and in 1897 he began to work on the nature of cathode rays. Using a very high vacuum tube with a Lenard window, he confirmed the discovery that Jean Perrin had made two years earlier, that cathode rays are composed of rapidly-moving, negatively-charged particles (electrons). And then, almost at the same time as Sir J. J. Thomson in Cambridge, but by a different method, he measured the relation of the electric charge on these particles to their mass and found, as Thomson did, that they are about two thousand times lighter than the atoms of hydrogen.

In 1898 Wien studied the canal rays discovered by Goldstein and concluded that they were the positive equivalent of the negatively-charged cathode rays. He measured their deviation by magnetic and electric fields and concluded that they are composed of positively-charged particles never heavier than electrons.

The method used by Wien resulted some 20 years later in the spectrography of masses, which has made possible the precise measurement of the masses of various atoms and their isotopes, necessary for the calculation of the energies released by nuclear reactions.

In 1900 Wien published a theoretical paper on the possibility of an electromagnetic basis for mechanics. Subsequently he did further work on the canal rays, showing, in 1912, that, if the pressure is not extremely weak, these rays lose and regain, by collision with atoms of residual gas, their electric charge along their course of travel. In 1918 he published further work on these rays on the measurement of the progressive decrease of their luminosity after they leave the cathode and from these experiments he deduced what classical physics calls the decay of the luminous vibrations in the atoms, which corresponds in quantum physics to the limited duration of excited states of atoms.

In this, and other, respects Wien's work contributed to the transition from Newtonian to quantum physics. As Max von Laue wrote of him, his «immortal glory» was that «he led us to the very gates of quantum physics».

Wien was a member of the Academies of Sciences of Berlin, Göttingen, Vienna, Stockholm, Christiania and Washington, and an Honorary member of the Physical Society of Frankfurt-on-Main.

In 1898 he married Luise Mehler of Aix-la-Chapelle. They had four children. He died in Munich on August 30, 1928.

Physics 1912

NILS GUSTAF DALÉN

«for his invention of automatic regulators for use in conjunction with gas accumulators for illuminating lighthouses and buoys»

Physics 1912

Presentation Speech by Professor H. G. Söderbaum, President of the Royal Swedish Academy of Sciences

Your Majesty, Your Royal Highnesses, Ladies and Gentlemen.

The Royal Academy of Sciences believes it is acting in strict accordance with Alfred Nobel's will in awarding the Physics Prize to Chief Engineer Gustaf Dalén in recognition of his remarkable invention of automatic valves designed to be used in combination with gas accumulators in lighthouses and light-buoys.

The ever-growing use of maritime communication creates an increased demand for navigational safety devices. Amongst these devices lighthouses and light-buoys are of great importance, and their number has become several times greater during the last few decades. At the same time efforts have been made to find ways of making their lights more powerful and their different lights more easily distinguishable. As far as possible, work has been directed towards finding a system which would regulate these lights automatically. This is a very important point, for no country is wealthy enough to maintain a continuous inspection of all its necessary lighting equipment.

In Sweden, a country with a long coastline and large archipelagos, the problem of a reliable and fairly inexpensive lighting organisation has for a long time been more pressing than elsewhere.

About 1895, for the first time it was discovered how to prepare acetylene from calcium carbide on a commercial scale. Acetylene is a gaseous hydrocarbon which, when burnt, produces an extremely bright and white light.

The first attempt to use this gas for lighting in lighthouses did not have very satisfactory results. The petroleum gas in use for similar purposes up to this time had been compressed and enclosed in large iron containers. It was found extremely dangerous to treat acetylene in the same manner, as this gas, when under the pressure of one or more atmospheres, explodes at the slightest shock. It was also attempted to store calcium carbide in light-buoys and to let the acetylene escape under the action of water supplied automatically. Unfortunately this method proved to be inconvenient, not very reliable, and unusable in cold weather.

In 1896, two French chemists, Claude and Hess, discovered that acetone

possesses the property of dissolving large quantities of acetylene. This solution is not explosive. However, it cannot be used as it is to store acetylene, because, even if the container is filled to the brim with a saturated solution under high pressure, the volume of the liquid is reduced by consumption or by cooling, and explosive acetylene gas is produced in the space above the surface of the liquid.

It was then discovered that its explosive nature disappears if the acetylene solution is compressed in a porous mass. Numerous unsuccessful attempts were made to prepare such a porous mass which would be sufficiently resistant and elastic to withstand the shocks encountered in transportation, without cracking and crumbling and thus producing cavities filled with explosive acetylene gas.

The credit for the eventual discovery of such a mass, called *aga* or porous substance, belongs to Gustaf Dalén.

By a complicated and carefully developed process, this substance is enclosed in steel containers which thus become practical accumulators for the acetylene gas. The porous mass in the container is half-filled with acetone, and acetylene is then introduced by compressing it to a pressure of ten atmospheres. Under this pressure, and at a temperature of 15° C, the container contains one hundred times its own volume of acetylene. The container is then ready for supplying to a lighthouse or light-buoy the acetylene necessary for lighting.

The advantage of this arrangement would not be great if the acetylene light had to burn uninterruptedly. On the one hand, this form of lighting would be quite costly, and, on the other hand, it would be difficult to distinguish the lights of various lighthouses from one another and from other light-sources. True, several methods of producing an intermittent light were already known. For example, the flame can be surrounded by moving screens, or the lighting device itself can be made to rotate. But such arrangements need continual inspection and consequently involve considerable expense.

Where compressed petroleum gas was used as the source of light, eclipsing or flashing lighting devices had also been constructed using the escaping gas as motive power. The flashes lasted 5 to 7 seconds, which was perhaps necessary because of the weak light output of petroleum gas. But with the intense brightness of acetylene light, such a long flash becomes unnecessary. Furthermore, long flashes offer insufficient variation of the signal. Accordingly the big lighthouses have generally replaced them with lights giving flashes lasting from $\frac{1}{10}$ to $\frac{3}{10}$ of a second.

It was about 1904 that Dalén started to study this problem. With a petroleum gas apparatus it was impossible to divide one litre of gas into more than fifty flashes. So Dalén constructed an apparatus, based on an entirely new principle, which by instantaneous opening and closing of the gas pipe, enabled one litre of gas to provide several thousand very rapid but distinct flashes. After a considerable trial period, this ingenious device proved to be extremely reliable. Dalén then provided a brilliant solution for the supplementary problems arising from the use of aga light in the lighthouse service, and an ever increasing number of lighthouses and light-buoys in Sweden have been adapted to this form of lighting. The burner is fitted with a small permanent flame which, in the most usual arrangement, lights a flash every three seconds lasting $\frac{3}{10}$ of a second.

In 1907 Dalén crowned his achievement with a further refinement by designing a kind of valve, called the «solar valve», which extinguishes the light at sunrise and re-lights it when night falls. This valve is controlled by four metal rods enclosed in a glass tube. The lower one is blackened, while the others are gilded and highly polished. Daylight is absorbed by the blackened rod which is heated and consequently expands, closing the gas valve. As the daylight decreases, the black rod reaches the temperature of the other three rods: it contracts and allows the gas valve to reopen.

The device can be regulated in such a way as to act with more or less sensitivity. To be on the safe side, it is usually regulated so that it lights as soon as mist or clouds cover the sun.

The solar valve combined with an intermittent light produces a saving of gas of 93 per cent, and even greater economy might be achieved by prolonging the periods between the flashes.

The use of aga light facilitates the placing of lighthouses and lightbuoys in the most inaccessible places such as archipelagos and seas with dangerous reefs. With the use of one or more of the easily transportable gas accumulators, such lights can give their warning or guiding signals for a whole year or more without the need of inspection or the fear of failure.

The result is an entirely new standard of safety in navigation and an enormous economy. For example, one shoal in Swedish waters previously required a lightship costing approximately 200,000 Kronor and maintained at a cost of about 25,000 Kronor a year. Now, in many cases, navigation is adequately served by establishing an aga buoy with optical and audio-signalling apparatus, the cost of which is 9,000 Kronor, and the annual maintenance of which costs about 60 Kronor.

Most of the maritime nations have now started to install these Dalén devices, and they are to be found operating from Spitzberg, the Varanger Fjord, Iceland and Alaska in the north, to the Straits of Magellan and Kerguelen Island in the south. The annual benefit to navigation can be expressed in terms of saving of thousands of human lives and of hundreds of millions of Kronor.

The aga flame has proved to be extremely useful in other fields, such as the lighting of railway coaches, railway signalling apparatus, car head-lights, soldering, the casting and cutting of metals and so on.

The Academy of Sciences recognizes the true value of all these applications and wishes to emphasize those which contribute to the progress of navigation, because it is uncontestably these that have rendered the greatest benefit to humanity.

The sciences that were especially favoured in the will of the great explosives technician Alfred Nobel, i.e. Physics, Chemistry, and Medicine, have one common feature of involving and sometimes demanding the sacrifice of the experimenter's personal safety. We all know that this year's Physics Prize winner was the victim of a serious accident which prevents him from being here to receive the award from the hands of his King.

He is represented by his brother, Professor Albin Dalén, of the Caroline Institute. Professor, when handing over to your brother the medal and the diploma, I beg you also to convey to him from the Royal Academy of Sciences, our sincere congratulations on the distinction he has merited, and our best wishes for a complete and speedy recovery.

No Lecture was delivered by Mr. N. G. Dalén

Biography

Nils Gustaf Dalén was born at Stenstorp in Skaraborg, Sweden on November 30, 1869, the son of a farmer. After his preliminary education, he entered a School of Agriculture to study dairy farming but he was later advised by Gustaf de Laval, who recognized his natural gift for mechanics, to seek a technical education. He prepared himself for the Chalmers Institute at Gothenburg and gained admission in 1892. He graduated as an engineer in 1896 and spent a year in Switzerland, studying under Professor Stodola at the Eidgenössisches Polytechnikum.

On his return to Sweden, Dalén carried out some research at Gothenburg and set up as a consulting engineer. He became Technical Chief of the Svenska Karbid- och Acetylen A.B. (Swedish Carbide and Acetylene, Ltd.) in 1901 and he later joined the Gas Accumulator Company where he became Chief Engineer in 1906. In 1909, the company was reorganized as Svenska Aktiebolaget Gasaccumulator (AGA) (Swedish Gas Accumulator Ltd.) with Dalén as Managing Director.

Dalén's inventiveness first showed in his early days on his father's farm when he built a threshing machine powered by an old spinning wheel. He contrived a device to indicate the butterfat content of milk and thereby made his contact with de Laval. On completion of his advanced education, he worked on the construction of a hot-air turbine and related air compressors and pumps. He also invented a pasteurization apparatus and a milking machine.

In 1901, Dalén's company purchased the patent rights of the French invention of dissolved acetylene and he began his work on automatic flashing beacons for lighthouses. His subsequent invention of the sun-valve, which causes a beacon to light automatically at dusk and extinguish itself at dawn, enabled lighthouses to function perfectly and unattended for periods of up to a year. His invention of cylinder filled with a porous mass of asbestos and diatomaceous earth for storage of acetylene reduced considerably the hazards in handling this material and its use in welding became safe. He also invented a mixer for providing a constant and correct balance of gas and

air for use in the incandescent mantle and a device for removing broken mantles and replacing them by new ones.

In 1912, whilst testing safety devices on cylinders of acetylene in an outdoor location, and when satisfactory safety precautions had been taken, a sudden explosion seriously injured Dalén and caused the loss of his eyesight. He recovered from his other injuries and overcoming his great incapacity, continued his researches. He was awarded the contract for lighting the Panama Canal and later turned to the field of thermal technics to invent a stove, now in universal use, which maintains cooking heat for 24 hours using only eight pounds of coal.

Dalén's writings were few, but he left his mark in a practical way by the provision of light, and therefore safety, for the benefit of travellers by land, sea and air.

Amongst the many distinctions conferred upon Dalén are membership of the Swedish Royal Academy of Sciences, 1913, and the Academy of Science and Engineering, 1919. He was made Honorary Doctor of Lund University in 1918 and received the Morehead Medal of the International Acetylene Association. He took part in debates at the National Society of Economics and served on the Lidingö City Council for almost twenty years.

Dalén married Elma Persson in 1901. They had two sons and two daughters. Their eldest son, Gunnar, qualified as an engineer and followed his father as a Director of AGA; their younger son, Anders, became a Doctor of Medicine; Gustaf's brother Albin, a famous ophthalmologist, was a Professor at the Caroline Institute.

Dalén died on December 9, 1937, in his villa at Lidingö.

Physics 1913

HEIKE KAMERLINGH ONNES

«for his investigations on the properties of matter at low temperatures which led, inter alia, to the production of liquid helium»

Physics 1913

Presentation Speech by former Councillor Th. Nordström, President of the Royal Swedish Academy of Sciences

Your Majesty, Your Royal Highnesses, Ladies and Gentlemen.

At its meeting on the 11th November the Royal Academy of Sciences decided to award the Nobel Prize for Physics for the year 1913 to Dr. Heike Kamerlingh Onnes, Professor at the University of Leyden « for his investigations on the properties of matter at low temperatures which led, *inter alia*, to the production of liquid helium ».

As early as 100 years ago research into the behaviour of gases at various pressures and temperatures gave a great impetus to physics. Since this time the study of the connection between the pressure, the volume and the temperature of gases has played a very important part in physics, and particularly in thermodynamics – one of the most important disciplines of modern physics.

In the years 1873 and 1880 Van der Waals presented his famous laws governing gases which, owing to their great importance for thermodynamics, were rewarded by the Royal Academy of Sciences in 1910 with the Nobel Prize for Physics.

The thermodynamic laws of Van der Waals were laid down on a theoretical basis under the assumption that certain properties could be attributed to molecules and molecular forces. In the case of gases the properties of which are changed by pressure and temperature, or in one way or another do not agree with Van der Waals' hypothesis, deviations from these laws occur.

A systematic experimental study of these deviations and the changes they undergo due to temperature and the molecular structure of the gas must therefore contribute greatly to our knowledge of the properties of the molecules and of the phenomena associated with them.

It was for this research that Kamerlingh Onnes set up his famous laboratory at the beginning of the 1880's, and in it he designed and improved, with unusual success, the physical apparatus needed for his experiments.

It is impossible to report briefly here on the many important results of this work. They embrace the thermodynamic properties at low temperatures of a series of monatomic and diatomic gases and their mixtures, and have

contributed to the development of modern thermodynamics and to an elucidation of those associated phenomena which are so difficult to explain. They have also made very important contributions to our knowledge of the structure of matter and of phenomena related to it.

Whilst important on its own account, this research has gained greater significance because it has led to the attainment of the lowest temperatures so far reached. These lie in the vicinity of so-called absolute zero, the lowest temperature in thermodynamics.

The attainment of low temperatures in general was not possible until we learnt to condense the so-called permanent gases, which, since Faraday's pioneer work in this field in the middle of the 1820's, has been one of the most important tasks of thermodynamics.

After Olszewski, Linde, and Hampson had prepared liquid oxygen and air in a variety of ways, and after Dewar, having overcome great experimental difficulties, had succeeded in condensing nitrogen, all temperatures down to -259° C, i.e. all temperatures down to 14° from absolute zero, could be attained.

At these low temperatures all known gases can easily be condensed, except for helium, which was discovered in the atmosphere in the year 1895.

Thus, by condensing this it would be possible to reach still lower temperatures. After both Olszewski and Dewar, Travers, and Jacquerod had tried in vain to prepare liquid helium, using a variety of methods, it was generally assumed that it was impossible.

The question was solved in 1908, however, by Kamerlingh Onnes, who then prepared liquid helium for the first time.

I should have to cover too much ground if I were to report here on the experimental equipment with which Kamerlingh Onnes was at last successful in liquefying helium, and on the enormous experimental difficulties which had to be overcome. I would only mention here that the liquefaction of helium represented a continuation of the long series of investigations into the properties of gases and liquids at low temperatures which Kamerlingh Onnes has carried out in so praiseworthy a manner. These investigations finally led to the determination of the so-called isotherms of helium and the knowledge gained here was the first step towards the liquefaction of helium. Kamerlingh Onnes has constructed cold baths with liquid helium which permit research to be done into the properties of substances at temperatures which lie between 4.3° and 1.15° from absolute zero.

The attainment of these low temperatures is of the greatest importance to

physics research, for at these temperatures both the properties of the substances and also the course followed by physical phenomena, are generally quite different from those at our normal and higher temperatures, and a knowledge of these changes is of fundamental importance in answering many of the questions of modern physics.

Let me mention one of these particularly here.

Various principles borrowed from gas thermodynamics have been transferred to the so-called theory of electrons, which is the guiding principle in physics in explaining all electrical, magnetic, optical, and many heat phenomena.

The laws which have been arrived at in this way also seem to be confirmed by measurements at our normal and higher temperatures. That the situation is at very low temperatures not the same, however, has, amongst other things, been shown by Kamerlingh Onnes' experiments on resistance to electrical conduction at helium temperatures and by the determinations which Nernst and his students have carried out in relation to specific heat at liquid temperatures.

It has become more and more clear that a change in the whole theory of electrons is necessary. Theoretical work in this direction has already been begun by a number of research workers, particularly by Planck and Einstein.

In the meantime new supports had to be created for these investigations. These could only be obtained by a continued experimental study of the properties of substances at low temperatures, particularly at helium temperatures, which are the most suitable for throwing light upon phenomena in the world of electrons. Kamerlingh Onnes' merit lies in the fact that he has created these possibilities and at the same time opened up a field of the greatest consequence and significance to physical science.

Owing to the great importance which Kamerlingh Onnes' work has been seen to have for research in physics, the Royal Academy of Sciences has found ample grounds for bestowing upon him the Nobel Prize for Physics for the year 1913.

HEIKE KAMERLINGH ONNES

Investigations into the properties of substances at low temperatures, which have led, amongst other things, to the preparation of liquid helium

Nobel Lecture, December 11, 1913

Since you have done me the honour of describing to you my investigations into the properties of substances at low temperatures, which have also led me, amongst other things, to the preparation of liquid helium, I must first of all express my deepest thanks to your old and famous Academy for distinguishing me in this manner. This has happened at a time when the continuation of my work will make great demands upon me. Nothing could make me more able than your good will does to meet new problems with the same hopeful confidence with which, 30 years ago, I met difficulties now overcome.

The main aim in investigations at low temperatures has greatly changed since then. When I first turned to this field of work the aim was still to liquefy statically the gases which up to then had not been mastered and to pour into open containers those gases which it is most difficult to liquefy.

What has given a character of its own to the Leyden work from the very beginning is that I allowed myself to be led by Van der Waals' theories, particularly by the law of corresponding states which at that time had just been deduced by Van der Waals.

This law had a particular attraction for me because I thought to find the basis for it in the stationary mechanical similarity of substances and from this point of view the study of deviations in substances of simple chemical structure with low critical temperatures seemed particularly important.

I was greatly drawn to making the necessary precision measurements at low temperatures. To this end large measuring instruments had to be maintained at constant temperatures, and one had also to construct suitable temperature baths – which would also be very useful for other investigations – to cover the whole field of low temperatures.

Just as I was thinking about how to do this, the basic classic work of Wroblewski and Olszewski appeared on the static liquefaction of oxygen. This meant that the determination of isotherms of hydrogen in a bath of

liquid oxygen was of great value, particularly with regard to liquefaction of hydrogen. These determinations were not carried out until the second ten-year period of the cryogenic institute and the results could not be published until many years after Dewar had already liquefied hydrogen. The first great stage in the development of the cryogenic laboratory, which made measurements in a bath of liquid oxygen possible, was likewise not reached until after ten years. Olszewski and Dewar had by then already solved the problem of pouring oxygen into open containers.

The Leyden process differed from the others as follows: in order to have a permanent bath available, the evaporated oxygen was liquefied again and conducted through a closed circulation system. When the apparatus in the Leyden laboratory was further developed the production of circulation systems of the same type was considered, although the investigations to be carried out were directed towards still lower temperatures. Earlier experience could thus be used in attacking a later problem and this was decisive for the liquefaction of helium. Thus the whole cryogenic laboratory still bears the marks left on it by its early problems.

The range of low temperatures now available to us far exceeds what I ever dreamt of at the beginning, for helium was not discovered until later and this allows us to get five to ten times nearer to absolute zero than is possible with hydrogen.

At the same time the aim of the cryogenic laboratory, which had originally been a limited one, became much more diverse.

This was the case as early as ten years ago, when I discussed in my rectorial address the programme of the laboratory at that time. Besides thermodynamic questions, questions relating to other fields were dealt with.

Thermodynamic problems were then in the forefront as far as the cryogenic section of the laboratory was concerned, because it was particularly work on the equation of state which was concentrated on low temperatures. But questions of other types, which demanded investigations at low temperatures, had become so numerous and important that I had felt it indicated that I should continue the other groups of work also exclusively at low temperatures, and dedicate the Leyden institute entirely to a study of the general character of the influence of low temperatures.

It is due to having specialized in this way that I was able to co-operate with a number of learned men who accepted the hospitality of the laboratory. The decision to make the Leyden institute an institute which co-operated with other larger ones, being a special institute for the more difficult

work in the field of low temperatures, has been found to be well worth as the scientific basis of this collaboration.

During the specialization in the Leyden institute, which is now completed, I was of course convinced that the investigations into the properties of substances at low temperatures would provide explanations for many of these properties. But my expectations were far surpassed by the reality. Not only has it been generally desirable – and to a far greater extent than was foreseen – to simplify as far as possible the conditions under which the phenomena occur, via extensive suppression of thermal motion, but it has also been shown that with liquid helium one can take an unexpectedly long step in this direction, as it permits temperatures to be reached which, for certain phenomena, represent practically absolute zero. Further discoveries in another field have made necessary, in an unexpected connection, the examination of many properties in the neighbourhood of absolute zero. The Nernst heat theorem and, especially, Planck's quantum theory and zero-point energy theory have made measurements at low temperatures of prime interest to physicists.

It is true that Faraday's problem as to whether all gases can be liquefied has been solved step by step in the sense of Van der Waals' words « matter will always show attraction », and thus a fundamental problem has been removed. At the same time, however, the question asked by Planck introduces a problem which is probably no less fundamental, to the solution of which investigations into the properties of substances at low temperatures can contribute.

I would now ask you to follow me into the Leyden laboratory.

As I have already briefly explained, the measurements are made in baths of liquid gases, which are maintained as long as is required in cryostats (equipment for maintaining constant low temperatures), the evaporated gas being allowed to circulate – it is sucked up and liquefied again.

The cycles for the various gases, as in Pictet's famous experiment, are joined in cascades, so that the cold bath, which can be maintained by a previous cycle, also serves to liquefy at a lower temperature the gas which circulates in the following cycle. In Fig. 1 is shown diagrammatically the various cycles with the exception of that for helium. They are shown by various sorts of lines.

The first cycle contains methyl chloride. The liquid methyl chloride is in a condenser cooled by water. From the condenser it passes into a refrigerator, where it evaporates under a pressure corresponding to the temperature necessary to liquefy ethylene. The vapour is sucked up by a large vacuum pump

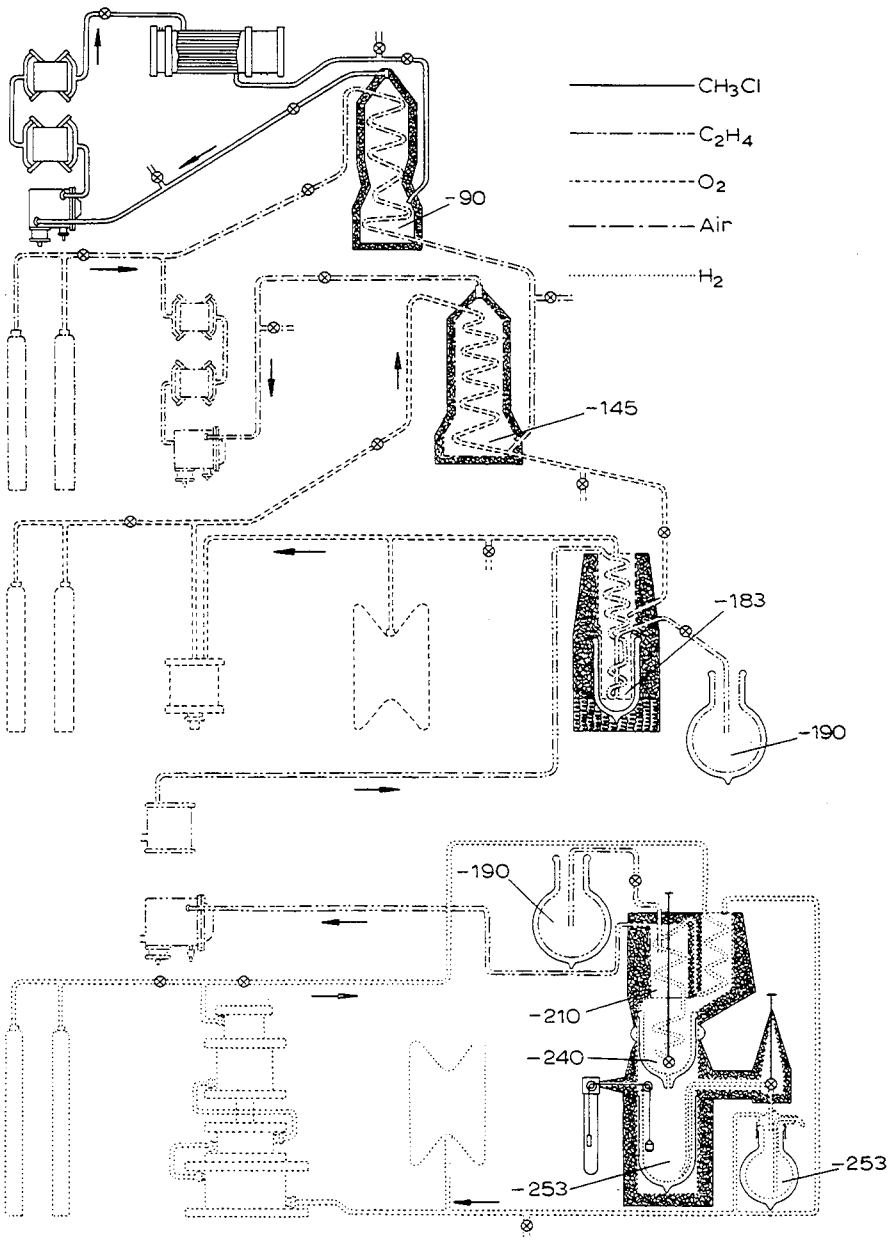


Fig. 1.

through a regenerator (in which, according to the counter-current principle, it flows past the ethylene which is being cooled), and is led by a conjugated compressor back to the condenser where it is liquefied.

In the second cycle is the ethylene. Circulation proceeds in the same way as in the first cycle.

In the third cycle the oxygen circulates in the same way. The liquefied oxygen can also serve to produce liquid air in the oxygen refrigerator.

The cryostat with the bath of liquid oxygen, of which I spoke before, was erected without using vacuum glass vessels. It was connected to the liquefying apparatus and the measurements could thus only be made in the immediate neighbourhood of this apparatus. Dewar revolutionized working at low temperatures by introducing his silvered vacuum glass vessels. The liquid oxygen could be stored in these vessels and transported in them from the liquefying apparatus to the point where one wished to use it. When working the cycles and in my subsequent work I found Dewar's ingenious idea very useful.

The liquid air is prepared in the fourth cycle. The other cycles are closed but this can be broken into. Air is sucked up from the room and a supply of liquid air can be stored in open vacuum glass vessels.

Shortly after success had been achieved in making available a permanent bath of liquid oxygen, the great master of refrigeration technique, Linde, discovered the process by means of which he created the liquid-air industry. This ingenious discovery permits the air, starting from normal temperature, to be liquefied without intermediate stages. You will see that the preparation of liquid hydrogen and liquid helium in the following cycles, which demands each time a similarly large temperature drop without an intermediate stage, is based on the principles of the Linde process. We, however, still always prepare the liquid air by means of cycles joined in cascade, as already described. When considering this arrangement, which is also very economical and supplies about 14 litres per hour, it must be borne in mind that all the relevant cycles here must be ready for continuous work so that during experiments in the laboratory the whole temperature range down to -217 °C is always freely available.

Fig. 2 shows a view of the four first cycles of the Leyden cascade. The conjugated pumps for the methyl chloride and ethylene circulation systems and the Brotherhood compressor for the oxygen cycle can clearly be seen. (Many pieces of equipment can also be seen which are used for taking various measurements; the figure does not show either the Brotherhood air compressor

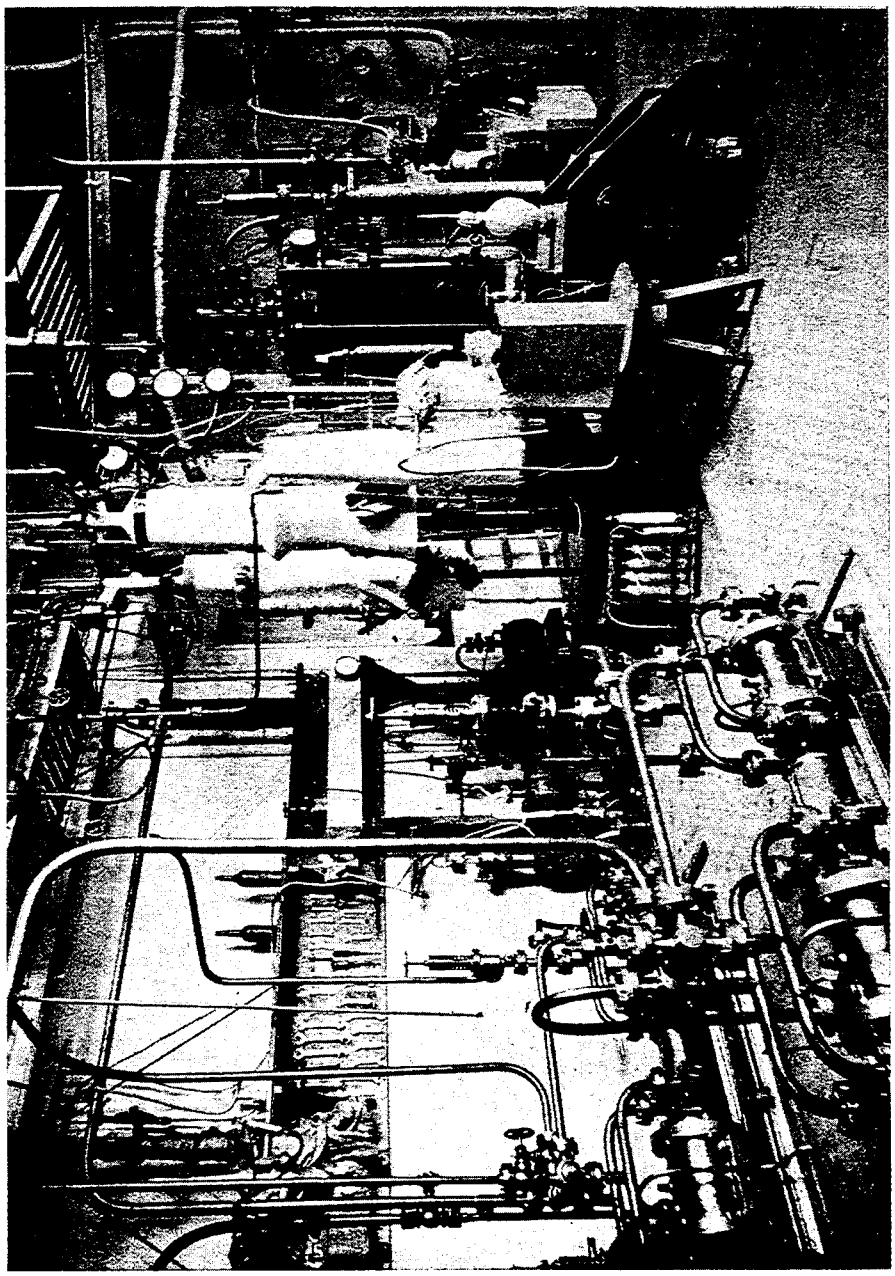


Fig. 2.

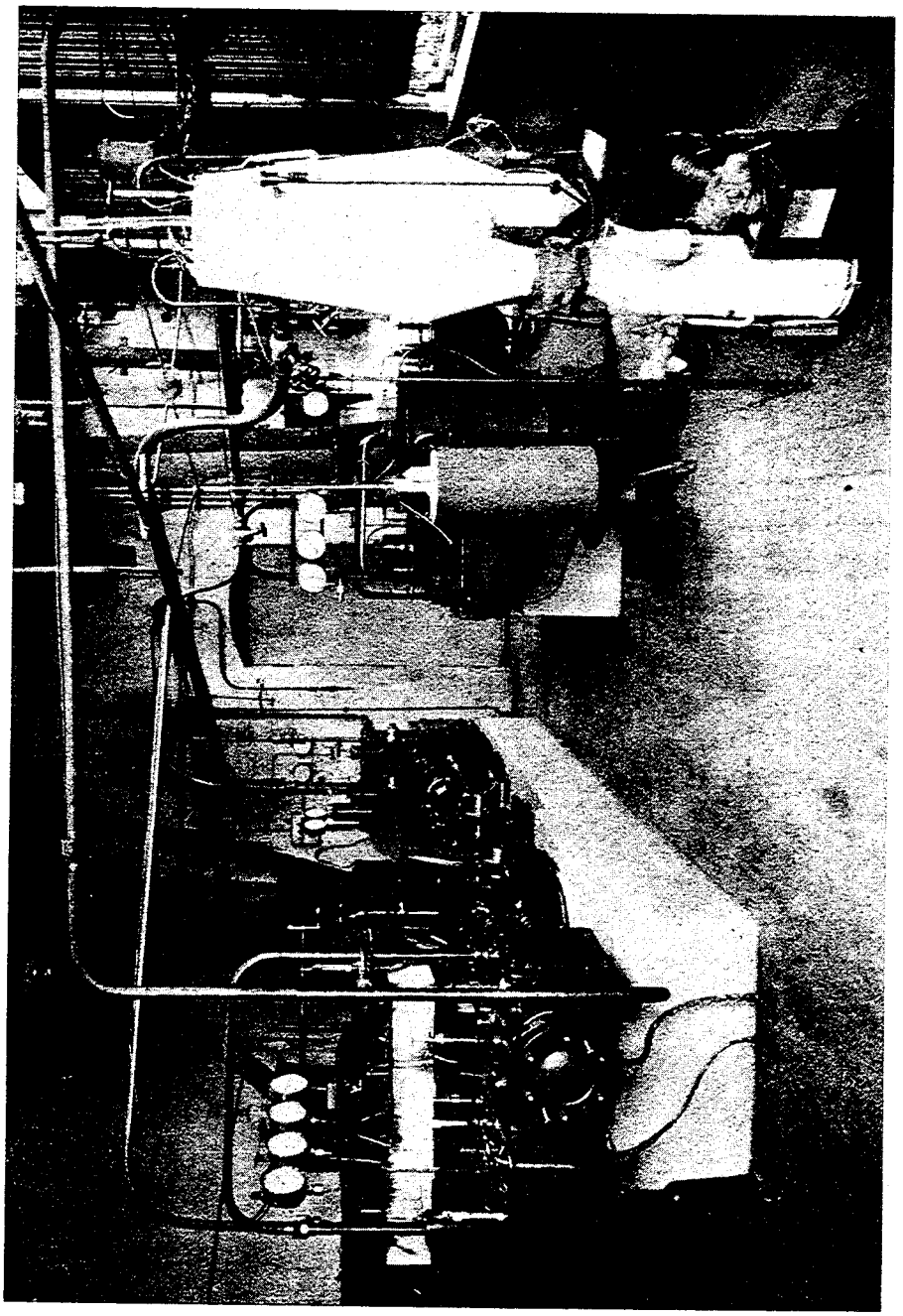


Fig. 3.

or the vacuum pumps which are so important to the working of the apparatus and which make evaporation possible under very low pressure. These are particularly important to the measurements because it is with their help that a wide temperature range can be covered with the same liquid gas.)

Let us now consider the hydrogen cycle. When I began to consider liquefying hydrogen, I started from the principle of mechanical similarity, already mentioned. From this and the critical data determined in such a masterly way by Olszewski it could be concluded that if one were successful in constructing an apparatus suitable as a model, which liquefies *oxygen* starting from a *normal* temperature, it would also be possible, with apparatus based on this model, to liquefy *hydrogen* starting from the temperature *of the liquid oxygen*. Long before I had the opportunity of using this principle, hydrogen had already been liquefied by Dewar. I have already discussed the great importance of Dewar's brilliant triumph for physics measurements in the rectorial address referred to above. In the liquid hydrogen we have at our disposal a freezing agent which leads us five times nearer to absolute zero than does liquid air. In Leyden a cycle which worked continuously was set up to make use of this. (Owing to the great demands made by the precision measurements which were envisaged at hydrogen temperatures, this did not come into being until many years later.) The circulation system is shown diagrammatically in Fig. 1.

The compressed hydrogen comes out of the refrigerator in which air is evaporating at the air pump, into a regenerator spiral with a throttle valve, which, following Dewar's example, is placed in a silvered vacuum glass. The spiral itself is modelled on the extremely practical Hampson apparatus for air liquefaction.

The liquid hydrogen collects in a Dewar glass and (as indicated by a float) is siphoned over into transportable, silvered vacuum glass vessels, which are connected to the container for pure hydrogen. The liquid hydrogen is siphoned out of the vacuum glass vessels in a similar way into the cryostats. It is clear that very pure hydrogen, a carefully closed cycle and special pumps must be used to circulate 40 m^3 per hour continuously, since only a little admixture of air will block up the fine ducts and valves through which the hydrogen has to flow.

The circulation is shown in the accompanying Fig. 3. The compressors, the liquefying apparatus and the vacuum pump for evaporating the liquid air in the refrigerator can easily be recognized. Much more apparatus is necessary to ensure the regular supply of 10 litres of liquid hydrogen per hour

which we now obtain and to enable the larger supplies, necessary when working with liquid helium, to be collected.

Tubing to suck the gas out of the cryostats connects the cycles described with all the working rooms. Thus, by means of liquefied gases, which evaporate under suitable pressure and which are, so to speak, as freely available as water, it is possible to maintain constant temperatures down to the freezing point of hydrogen in all parts of the laboratory.

Let us now turn to the experiments which have been carried out by this method.

Generally speaking the first task was to suit the measuring apparatuses to the method of working chosen. They had to be enclosed in an air-tight chamber, which was not always easy. It was, however, often important. For, once a cryostat of this type suitable for the experiments had been constructed, there was the advantage that one could pour the various gases one after another into the same cryostat and thus run through the whole range of low temperatures without the measuring apparatus having to be altered at all.

I cannot spend much time over this equipment (nor can I linger over that for the handling of baths, although this was often decisive for the success of difficult measurements, since it permitted the observer to concentrate all his attention on the measurements). I would prefer to put forward that the investigations in which they were used have led to a knowledge of the temperature dependence of properties of very different type. In mentioning some of these I will limit myself to those investigations which went down as far as the freezing point of hydrogen. May I first mention a few people who have collaborated with me.

Mr. J. Becquerel came to examine the absorption spectra of rare earths with and without magnetic field (Zeeman phenomenon), Messrs. Becquerel, father and son, to examine the phosphorescence of the uranyl compounds, Mr. Lenard and Mr. Pauli to examine the long-lasting phosphorescence of the alkaline earth sulphides, Mme. Curie to examine the penetrating radiation of radium, Mr. Weber to determine the viscosity of gaseous hydrogen and helium. I am particularly glad to mention that the deserving Swedish research worker Mr. Bengt Beckman and his wife came to Leyden to work on a number of problems mainly connected with his Uppsala experiments, from amongst which I name the Hall effect of alloys and the increase in resistance brought about by pressure in lead. I would like to show here (in Fig. 4) a diagram taken from a work by Dr. Beckman, in which baths of ethylene,

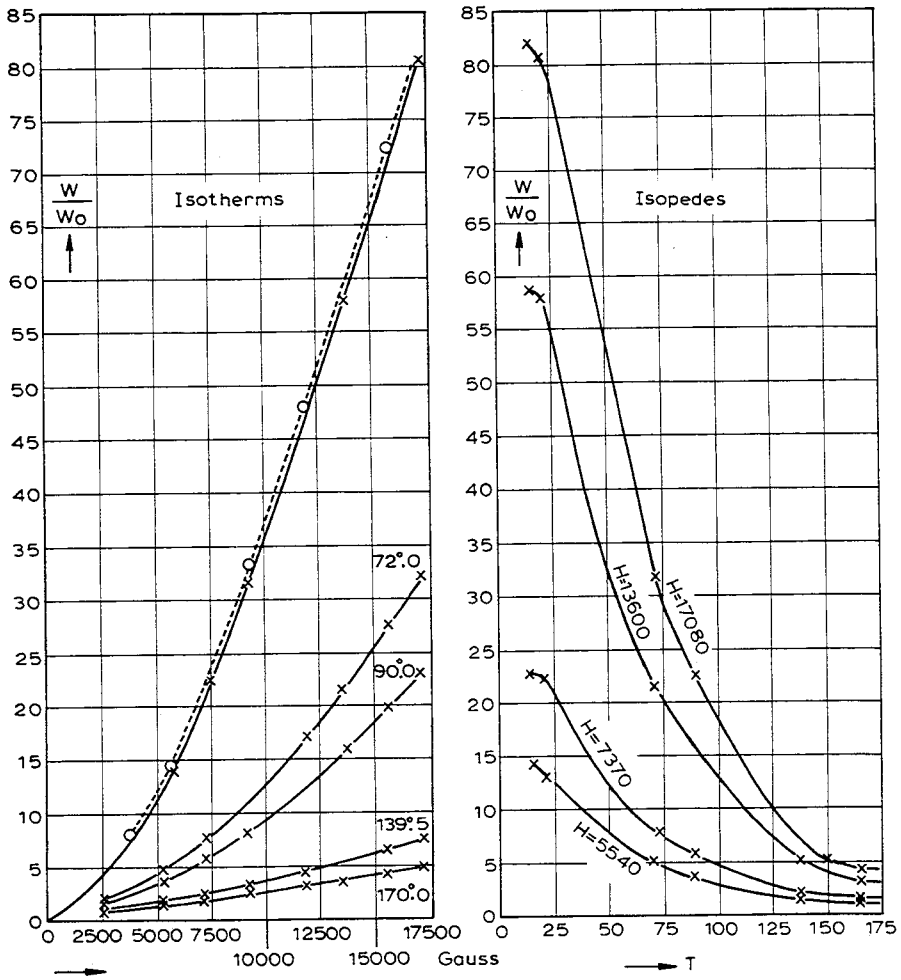


Fig. 4.

oxygen and hydrogen were used. The diagram shows to what high values the magnetic increase in resistance of bismuth rises at hydrogen temperatures.

A group particularly closely connected with the experiments last mentioned is made up of those concerned with magnetism.

Mr. Weiss came to determine the magnetic moment of the molecular magnets of ferromagnetic substances by means of saturation measurements at the melting point of hydrogen. Later, when Weiss discovered the magneton,

the results contributed to the determination of its value, which is probably fundamental to the magnetic moments of all atoms. The experiment undertaken by Mr. Perrier and myself on liquid and solid oxygen led to deviations from the Curie law, which we then found also with paramagnetic salts. During the continuation of the work with Mr. Oosterhuis it was found that one class of these deviations can be explained by assuming a Weiss molecular field with a negative sign, which then – according to investigations by Mr. Perrier and myself into the susceptibility of liquid mixtures of oxygen and nitrogen and following up Weiss' theory – could be conceived as a function of the spacing of the molecules.

The continuation of measurements on paramagnetism at helium temperatures, which has been in course of preparation for some time, is also important in connection with the theory of zero-point energy. The fact is that attempts have been made to explain deviations from the Curie law without assuming a negative field. This was on the part of Oosterhuis, in that he introduces into the Langevin theory of rotational energy, which Langevin puts as proportional to the temperature, the expression of Einstein and Stern which contains zero-point energy. If the unchanged theory of Langevin is right, however, this would be shown very clearly at helium temperatures. Also the influence of the external field on the susceptibility, which is according to the Langevin theory inversely proportional to temperature, would become clearly visible at helium temperatures in attainable fields, whilst at hydrogen temperatures it cannot be expected to the same extent until fields are reached which are ten times greater and thus far exceeding what it is possible to expect. (See Addendum 1.)

The study of the equation of state related to the monumental theories of Van der Waals has always been a main item of research in the Leyden laboratory. In particular, the exact study of the equation of state of simply constructed gases belongs to the low temperature field. Here also belong the determination of the absolute scale of temperatures below the freezing point of water. For help with the extensive precision measurements which are dealt with here I must thank my many talented co-workers. In recent times particularly Messrs. Braak, Clay, Crommelin and Dorsman, Mr. and Mrs. de Haas-Lorentz, and Messrs. Holst and Keesom have supported me and of these I have been indebted to Mr. Crommelin also for the technical running of the laboratory.

I owe much to the distinguished theoretical knowledge of Mr. Keesom and to the thorough and ingenious way in which he has worked on the the-

oretical problems connected with the equation of state, the study of which also fell mostly to his lot.

As far as the experimental work is concerned, various conditions needed to be fulfilled, each with great precision. (The law of corresponding states is roughly valid and thus only those observations are of real value the accuracy of which makes it possible to determine deviations from this law.) The pressure under which the gas is compressed, the volume which it occupies and the temperature at which the experiment takes place must be known with suitable accuracy. The volume which had to be assigned to the measuring apparatus demanded that the baths in the cryostats should sometimes have a capacity of up to $1\frac{1}{2}$ litres, even when working with liquid hydrogen. The temperature also had to be held evenly and constantly to within 1/100 degree in such cases.

(The layout of a piezometer for high pressures is shown diagramatically in

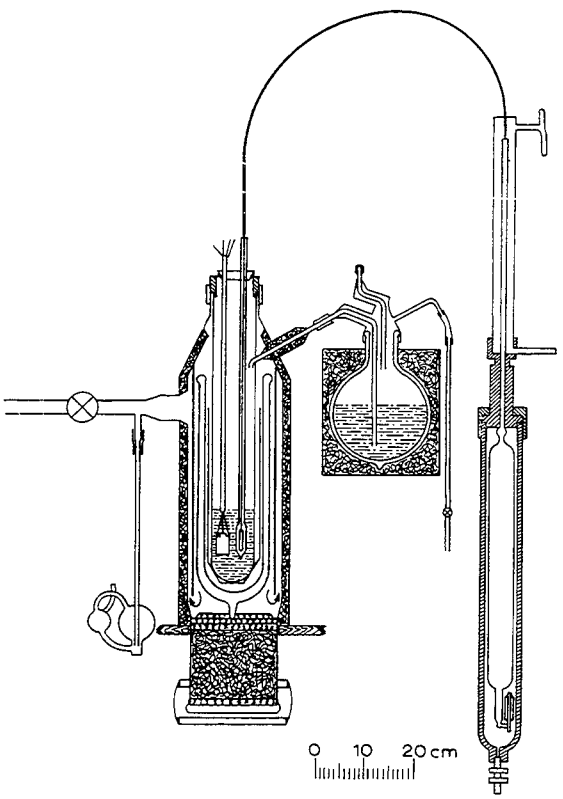


Fig. 5.

Fig. 5. The large reservoir filled with the gas to be examined is dipped into a Cailletet block and connected by a steel capillary tube to the small reservoir. The latter is in the cryostat in which the bath is maintained by one of the cycles. The gas in the piezometer is pressed by the mercury into the small reservoir. The amount of gas which is transferred is measured by the mercury rising in the calibrated column.)

In order to have a basis for pressure measurement, which would guarantee the necessary accuracy, the pressure was transferred from one to the next and so on in a series of mercury differential manometers, as shown in Fig. 6, by suitably compressed gas, so that a height of 45 metres of mercury column can be accommodated in the working room, and pressures up to 60 atmospheres measured to the nearest millimetre. (The apparatus was later extended to 90 atmospheres.)

As far as thermometry is concerned I must limit myself to mentioning that its standard was a hydrogen thermometer to which more recently a helium thermometer was also added. From the simultaneous study of all types of auxiliary thermometers down to the melting point of hydrogen it seems certain that in the absolute determination of temperatures an accuracy of 1/50 degrees is reached.

The volume gauge which was used amongst other things in the investigations into rectilinear diameter for which Mr. Mathias came to Leyden, belongs to the apparatus which serves to determine the equation of state. I must pass this by, along with much other auxiliary equipment, although I should have liked to give a better picture than is possible in this short report of the work which had to be done to determine an equation of state. Still less can I linger over the evaluation of observations. I should like, however, to mention that I obtained the virial coefficients for hydrogen with the help of Mr. Braak and Mr. de Haas, and those for argon with the help of Mr. Crommelin. Using these coefficients Mr. Keesom then did some fine work in testing various theoretical conceptions and found, amongst other things, that, as far as deviations from Boyle's law at low pressures are concerned, hydrogen behaves at low temperatures in a similar way to monatomic argon, just as was found for specific heat by Eucken.

Determination of the isotherms of helium was of the greatest importance in the liquefaction of this gas. From them information could be obtained as to whether helium, like other substances, follows the law of corresponding states, and if so what the critical data of helium are. With this information it

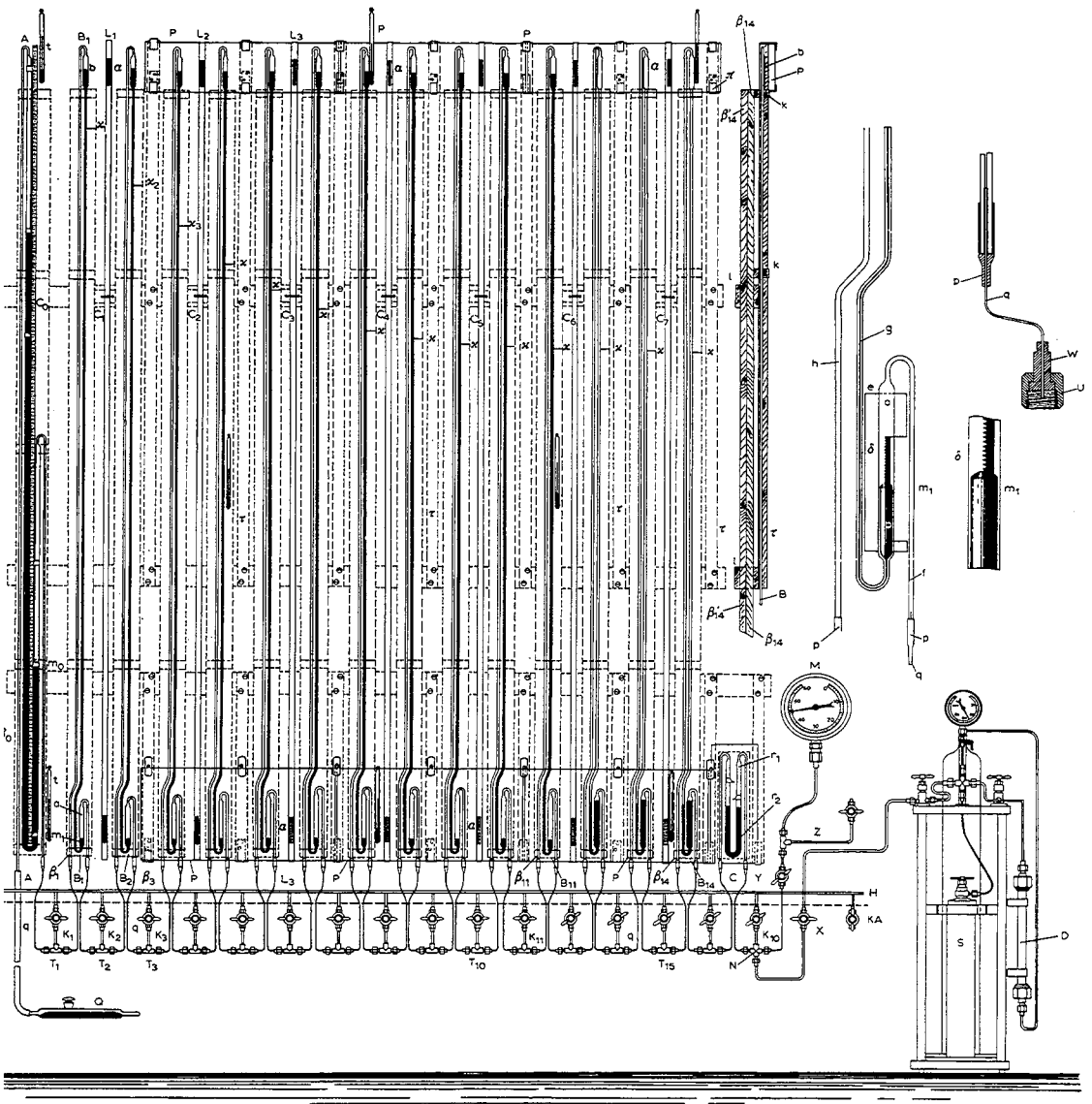


Fig. 6.

would be possible to judge whether the gas, starting from the freezing point of hydrogen, the lowest temperature to which it could be cooled, could be liquefied by means of Linde's process. If so then the principle of similarity could again be applied to the setting up of a helium cycle, just as the hydrogen liquefier had been built according to Hampson's model for an air liquefier. It became clear that it had to be found out whether the temperature at which the initial compression of the helium follows Boyle's law – the Boyle point – lies above the melting point of hydrogen. In this case one could hope for static liquefaction, if a closed helium cycle on the model of the hydrogen cycle were set up and operated in a suitable way. The question was answered in the affirmative by isotherm determination. Moreover, the compression pressure, the time the experiment would take, the cooling action to be brought about by the hydrogen cycle and also the quantity of helium necessary, were assessed as being not beyond what might be attained. As far as the helium was concerned, my brother Mr. O. Kamerlingh Onnes, the Director of the Department of Foreign Trade Relations in Amsterdam, was able to help me to obtain the necessary monazite. I am glad to be able to thank him here also.

Carrying out the experiment in the manner stated meant that the usual intermediate stage in the liquefaction of a gas would have to be by-passed by one great effort, although this bordered on the impossible. This seemed, however, the only thing to do since helium, then still such a mystery, had already so greatly disappointed me.

The accompanying Fig. 7 gives a diagram of the equipment for the experiment. The compressed helium comes, after suitable pre-cooling, into the refrigerator, where hydrogen is evaporating at the air pump, runs through the regenerator spiral and expands at the throttle valve. The liquid formed according to the Linde process gathers in the lower portion of the vacuum glass vessel. In order to be able to observe the liquid helium this portion is transparent and is surrounded by a vacuum glass vessel which is kept filled with liquid hydrogen, which is again protected by a transparent vacuum glass vessel containing liquid air, and this again by a vessel in which heated alcohol circulates.

The helium cycle, as set up, is shown again in Fig. 8 in more detail, but still in diagrammatic form, and here we find again the liquefaction apparatus; the compressors, storage flasks and regulating gas bells floating on oil are also shown. The liquefier itself is shown in more detail in Fig. 9, where the liquefying spiral built on the Hampson model can again easily be recognized.

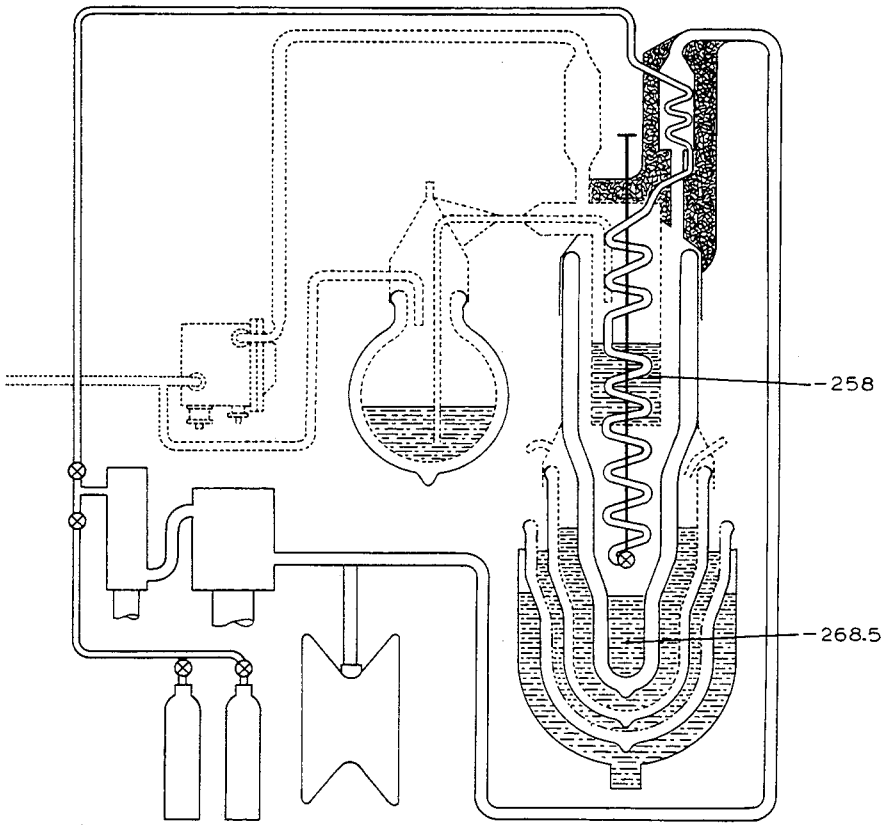


Fig. 7.

The refrigerator is supplied with liquid hydrogen from a transparent intermediate reservoir, into which the liquid hydrogen is siphoned from the storage flasks. The helium, before being cooled in the hydrogen refrigerator, runs through a tube cooled in liquid air, with carbon to free it from traces of air which might have contaminated it during circulation. We owe the application of this property of carbon to Dewar. I have already stressed the great importance of his work, but would like to emphasize here that it has been of greatest use to me in the liquefaction of helium.

The temperature of the helium is measured with a helium gas thermometer, the reservoir of which is in the liquefying chamber and is connected by a steel capillary tube to the manometer portion.

The accompanying Fig. 10 gives a view of the whole apparatus. The trans-

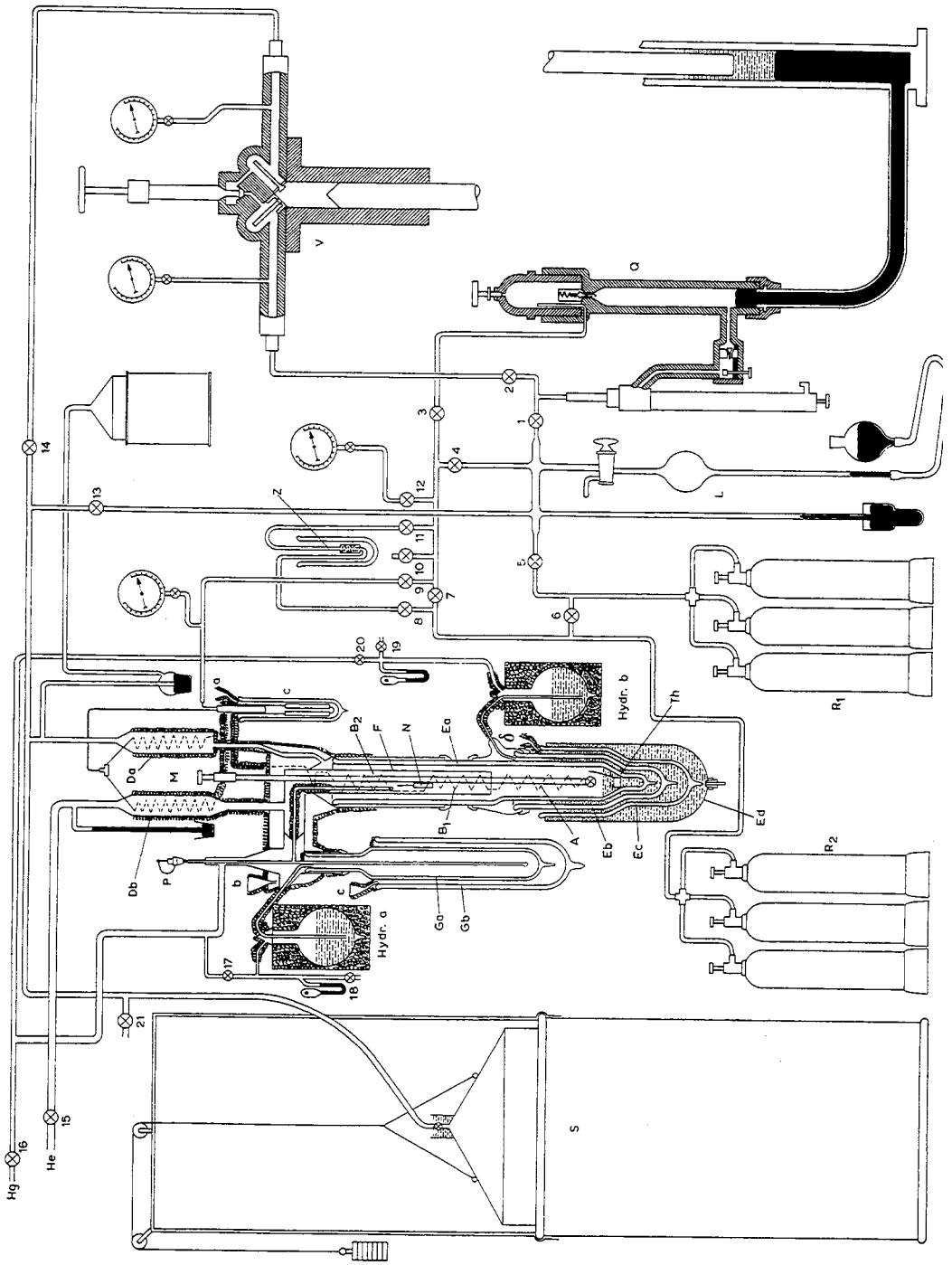

Hg
16
He
15
14
13
12
11
10
9
8
7
6
5
4
3
2
1
20
19
21
17
18
V
Q
L
Z
S
M
P
A
F
N
Th
Hydr. a
Hydr. b
R1
R2
Da
Db
Ea
Eb
Ec
Ed
Ga
Gb
B1
B2

Fig. 8.

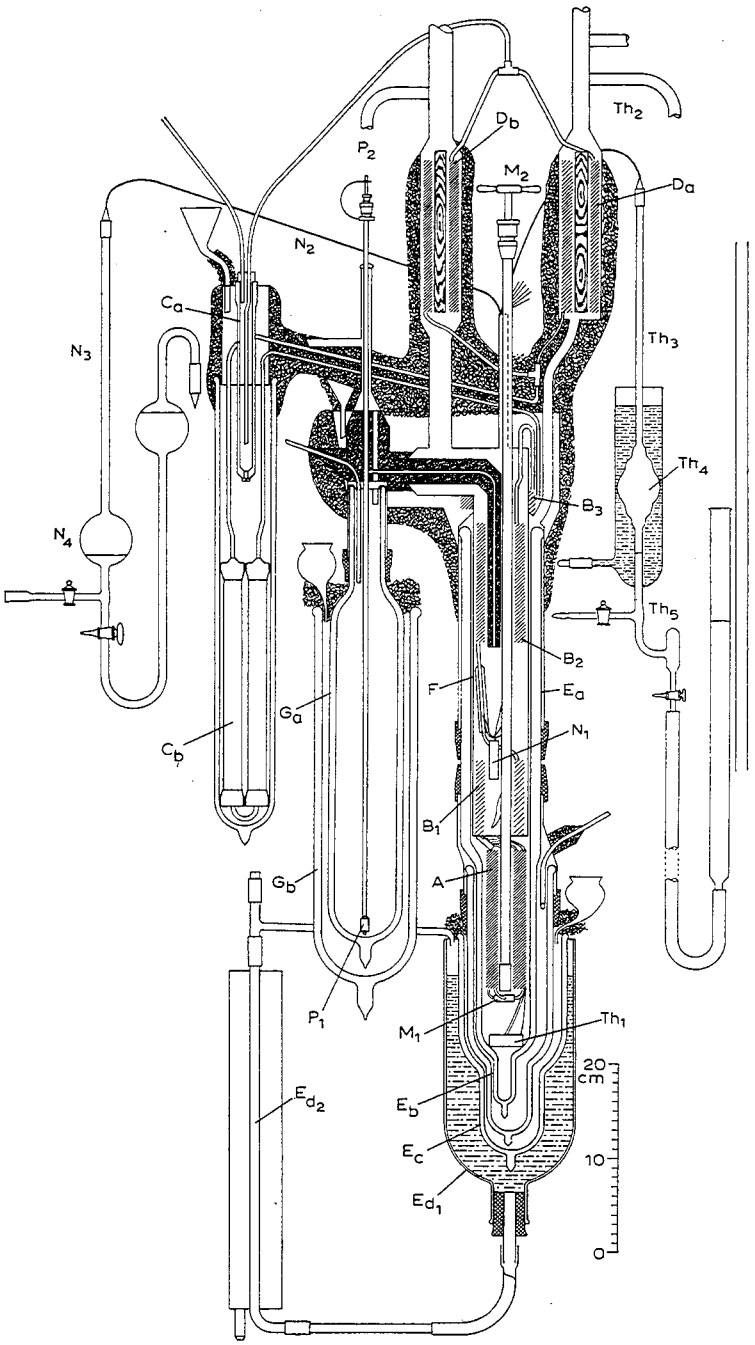

Th2
Db
P2
M2
Da
N2
Ca
N3
Th3
Th4
B3
N4
Th5
B2
F
Ea
Ga
N1
Cb
B1
A
Gb
P1
M1
Th1
20
cm
Ed2
Eb
10
Ec
Ed1
0

Fig. 9.

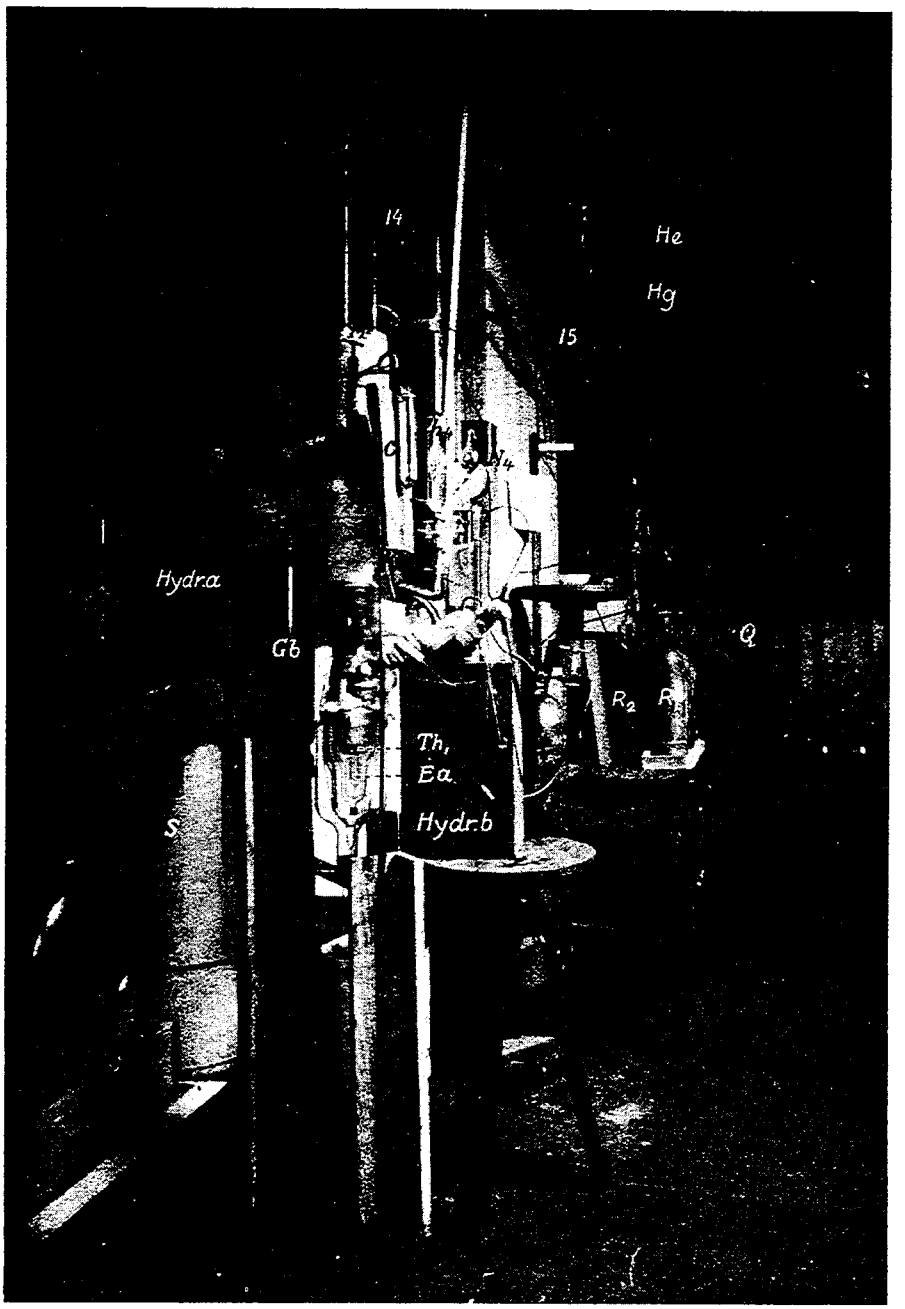


Fig. 10.

parent liquefying chamber, with three-fold protection, can be easily recognized.

As had been foreseen when the experiments were planned, their execution bordered on the impossible. By the time the experiment, which began at half past five in the morning, had ended at half past nine at night, the first helium having been seen at half past six, everyone had done everything that could possibly be demanded of him. This is particularly true of my loyal co-worker G. J. Flim, the mechanic, to whom I am also deeply grateful for help in setting up the other experiments and apparatuses discussed here.

It was a wonderful sight when the liquid, which looked almost unreal, was seen for the first time. It was not noticed when it flowed in. Its presence could not be confirmed until it had already filled up the vessel. Its surface stood sharply against the vessel like the edge of a knife.

How happy I was to be able to show condensed helium to my distinguished friend Van der Waals, whose theory had guided me to the end of my work on the liquefaction of gases.

The boiling temperature of the helium was found at 4.25°K, the critical temperature assessed as 5°K. The low critical pressure to which we had to conclude and which fell between 2 and 3 atmospheres was remarkably small.

Now that we had arrived at this elucidation of the properties of helium the next target was already indicated.

Using our knowledge, we had to make measurements and to this end, on the basis of the information obtained, to decide on arrangements which would simplify the preparation of liquid helium and make it available in a cryostat. This problem I also solved step by step. I will indicate these steps by mentioning some of the investigations already carried out with liquid helium. In these I was supported by Mr. Dorsman in the thermodynamic work and by Mr. Holst in the electrical work. I am most grateful to them.

First of all somewhat difficult conditions were encountered in the liquefying chamber. In the experiments shown in Fig. 11 considerable progress had already been made in various directions.

The experimental chamber also for these experiments is still closed off at the top by the liquefying spiral and the experiments are thus made difficult (since it is only possible to penetrate into the experimental chamber with the help of steel capillary tubes or insulated wires led past the liquefying spiral), yet a whole collection of instruments is already placed in the liquid helium. We see here a helium thermometer, the pressure of which (at 0° C 145 mm)

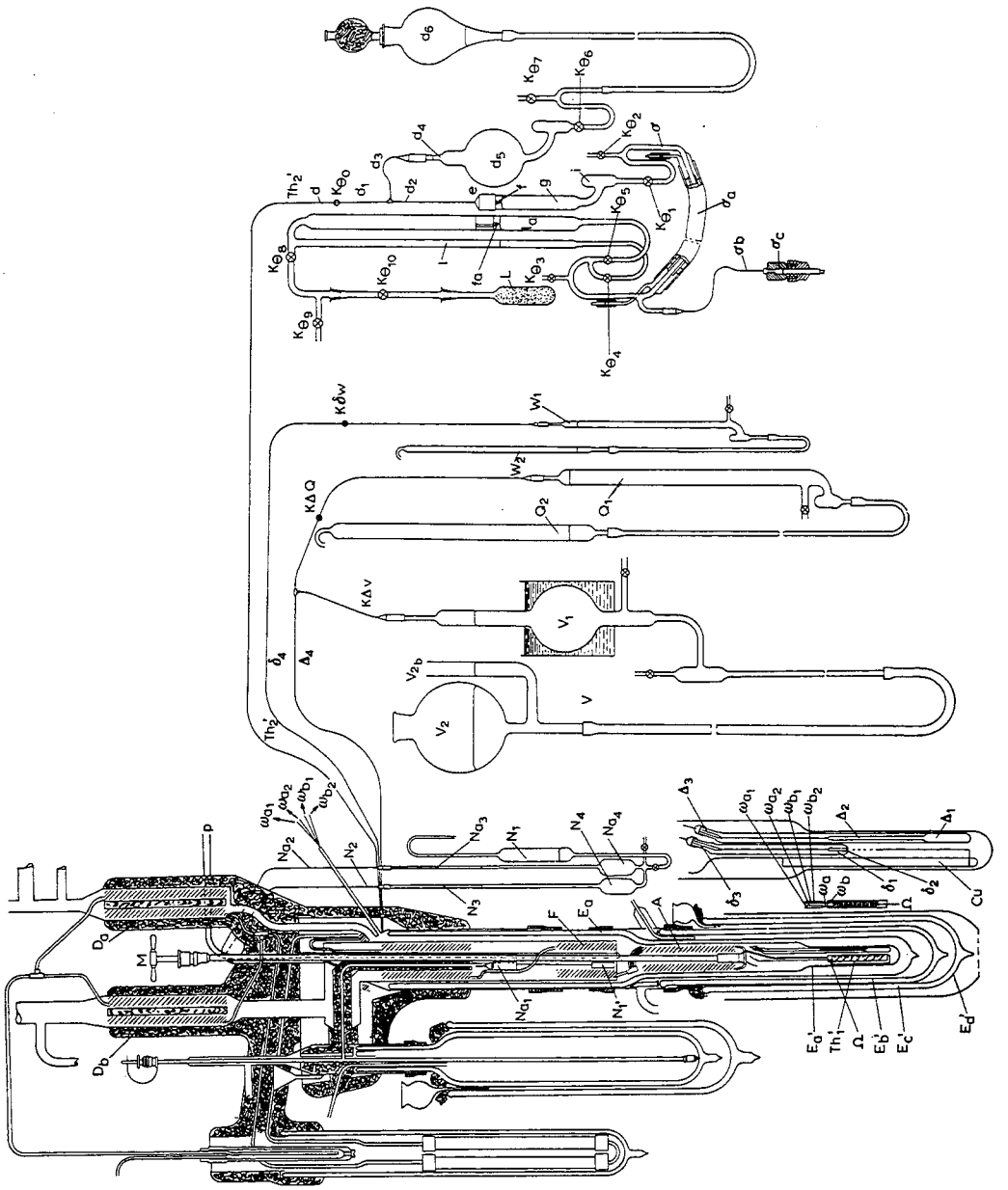

d6
Kθ7
Kθ6
Kθ2
σ
d4
d5
d3
d2
d1
Th'2
d
Kθ0
e
f
g
Kθ5
Kθ1
σa
σb
σc
Kθ8
Kθ10
l
fa
L
Kθ3
Kθ9
Kθ4
Kδw
W1
W2
KΔQ
Q2
Q1
KΔv
V1
δ4
Δ4
V2b
V
V2
Th'2
ωa1
ωa2
ωb1
ωb2
Na2
N2
Na3
N1
N4
Na4
N3
P
Da
M
Db
Na1
N1'
F
Ea
A
Δ3
Δ2
Δ1
δ3
δ1
δ2
Cu
Ea'
Th'1
Ω
Eb'
Ec'
Ed'

Fig. 11.

amounts to only 3 mm at the boiling point of helium, so that temperatures down to 1.5°K approximately could be measured with it. Here the accuracy of the measurement was suited to the smallness of the pressure.

The simultaneous measurement of the pressure in the chamber in which the liquid helium evaporates yielded the law of vapour pressure. To measure how the density of the liquid helium changes with the temperature there was also in the helium bath a differential dilatometer (into which volumetrically measured quantities of helium were brought through two identical steel capillary tubes).

It is very noticeable that the experiments indicate that the density of the helium, which at first quickly drops with the temperature, reaches a maximum at 2.2°K approximately, and if one goes down further even drops again. Such an extreme could possibly be connected with the quantum theory. At any rate, closer examination of the equation of state of the helium in relation to deviations from the law of corresponding states is very promising, as it is the critical temperature precisely of this gas which approaches absolute zero so closely.

Naturally the question arose as to whether helium can also be converted into the solid state. An experiment aimed at lowering the temperature of helium sufficiently by evaporating it without supply of heat was not successful, and only served to reach the lowest temperature recorded up to that time. The helium (see Fig. 12) evaporating in a vacuum glass vessel was protected against heat supply, the vessel itself being dipped in a bath of liquid helium. The bath was obtained, as the figure shows, by the liquid helium flowing out of the liquefying apparatus into a cryostatic vessel.

The evaporation of even a very small quantity, when the pressure of the vapour is small, demands the continuous carrying away of colossal volumes of vapour. With vacuum pumps of very large capacity we succeeded in lowering the pressure to 0.2 millimetre. The temperature then reached was 1.15°K according to the law of vapour pressure found. (Of course we can only make an estimate here. The working out of the thermometry of these low temperatures with, amongst other things, the aid of the Knudsen hot wire manometer is still in its initial stages.) Since it would have needed new equipment, I deferred the question as to whether helium can be made to freeze in favour of other, more urgent problems, which could be tackled with the equipment available.

The handling of these questions was very considerably facilitated when we succeeded in getting the cryostat shown in Fig. 13 to work. The liquid he-

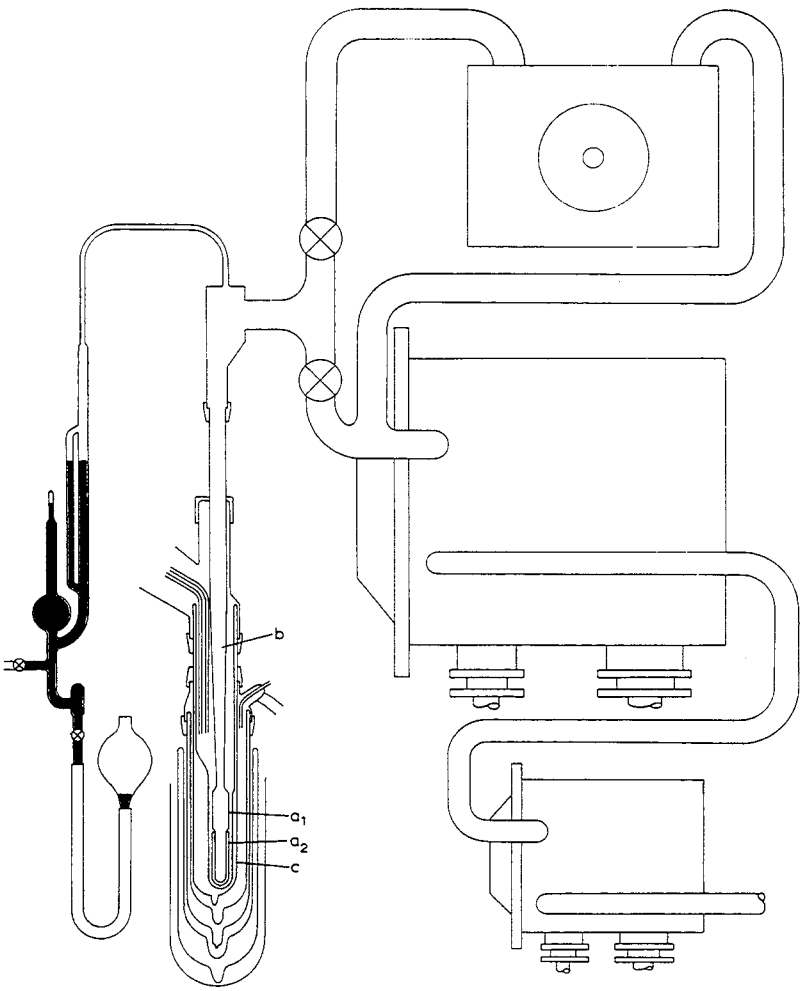


Fig. 12.

lium is siphoned over from the liquefier into a cryostatic vessel open at the top (through a vacuum tube cooled with liquid air). The liquefier can be shut off from the cryostat by a valve. In this way continuous working of the liquefier was possible. The bath is stirred vigorously by an electromagnetically-driven pump.

With this cryostat, investigations into the extraordinarily great conductivity of some metals at low temperatures were possible, and I wish now to report on these.

Even the very first experiments on the resistance of metals had brought

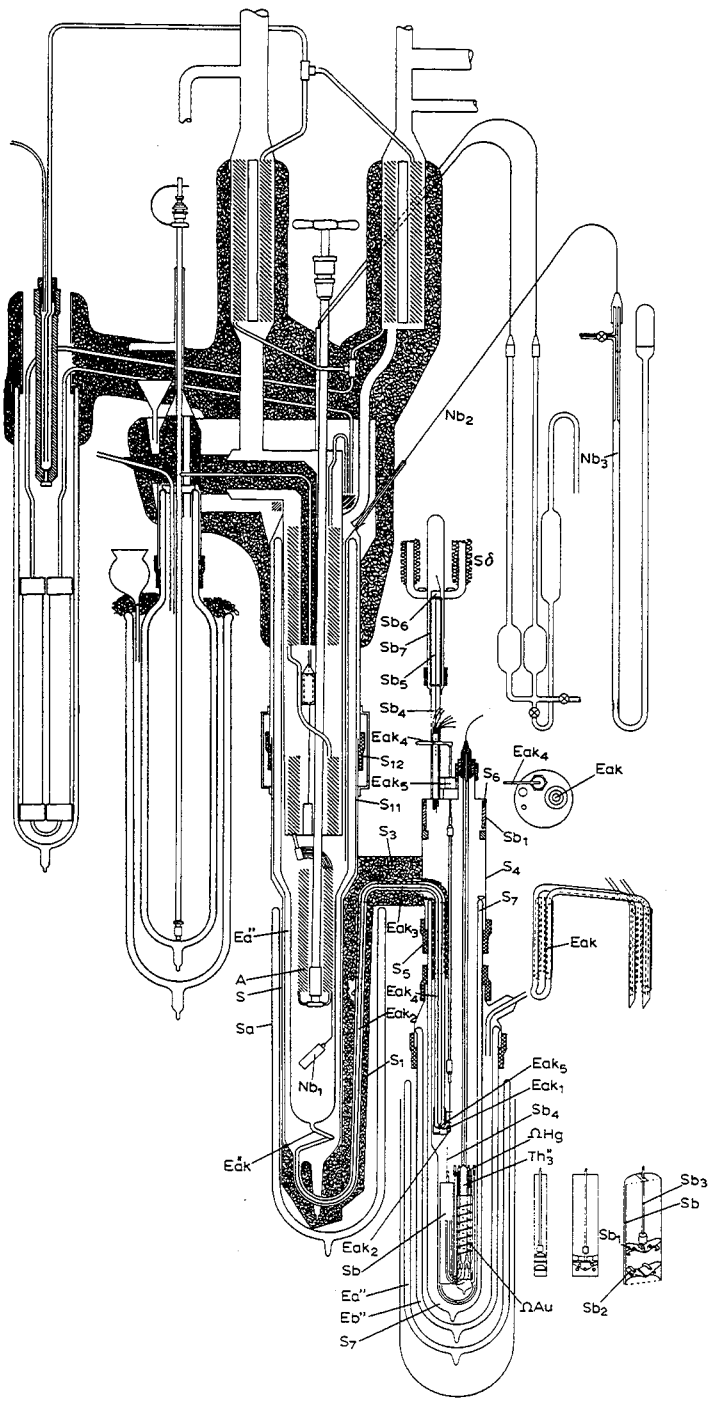

Nb2
Nb3
Sδ
Sb6
Sb7
Sb5
Sb4
Eak4
S12
Eak5
S11
S3
S6
Eak4
Eak
Sb1
S4
S7
Eak
Eak3
S5
Eak4
Eak2
S1
Ea''
A
S
Sa
Nb1
Eak
Eak5
Eak1
Sb4
ΩHg
Th3
Sb3
Sb
Sb1
Sb2
Eak2
Sb
Ea''
Eb''
S7
ΩAu

Fig. 13.

very surprising results. With platinum wire there was no re-rise in resistance after reaching a minimum, as would be expected, if the electrons which provide the conduction were firmly frozen to the atoms at low temperatures, but on the contrary resistance became independent of temperature. The conclusion drawn from the results of an experiment carried out with Mr. Clay down to the freezing point of hydrogen was that for absolutely pure platinum resistance would disappear at the boiling point of helium. At the lowest temperature therefore the conduction electrons are not bound but the factors which hinder their movement disappear. The obvious thing was to look for these hindering factors in the energy of Planck's vibrators, so that here the theory of resistance agrees very well with that of specific heat.

I attributed the fact that the resistance had become constant and not zero to the slight impurities which remain even in pure gold. I could only hope to get the mercury purer than gold, and the observations just made permit one to conclude that its resistance at the boiling point of helium would still be observable but that at the lowest temperature that could be reached with helium it would disappear. The possibility of confirming this caused me to think of little account all the difficulties which were attached to the experiment. These were overcome and the results are, as Fig. 14 shows, convincing. The resistance, in fractions of the resistance at zero Centigrade, is shown as the ordinate and the temperature as the abscissa. The figure contains, as well as the results obtained in helium, which are marked II, also those, marked I,

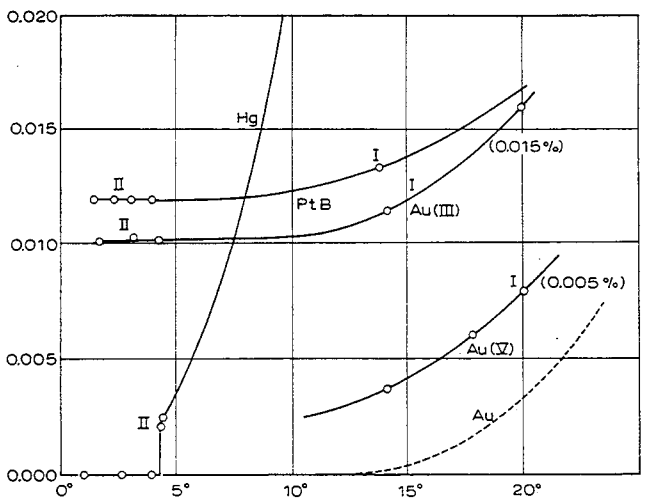


Fig. 14

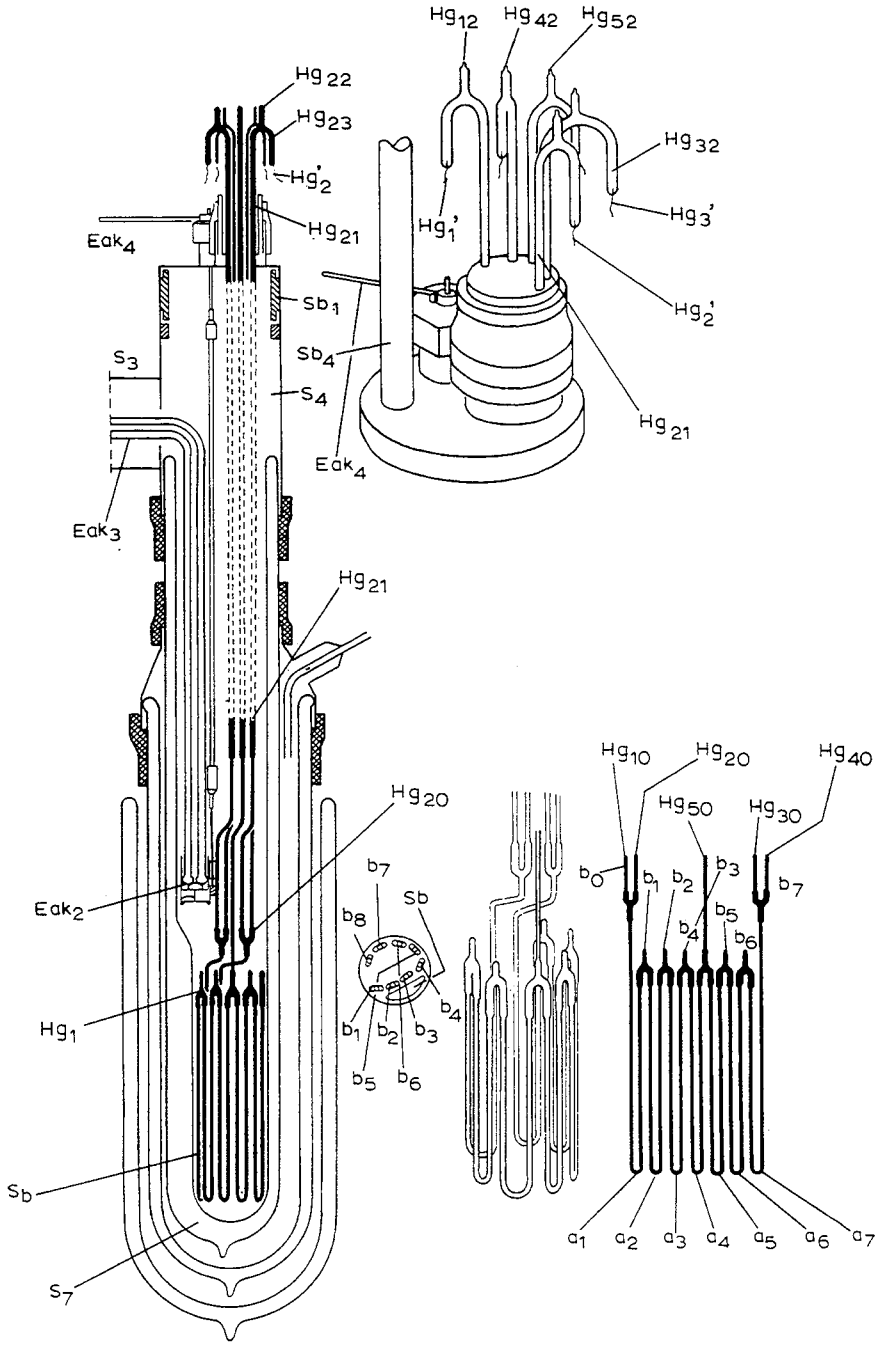

Hg12
Hg42
Hg52
Hg22
Hg23
Hg'2
Hg32
Hg3'
Eak4
Hg21
Hg1'
Sb1
Hg2'
Sb4
S3
S4
Hg21
Eak4
Eak3
Hg21
Hg10
Hg20
Hg40
Hg50
Hg30
Hg20
b7
Sb
b8
Eak2
b0
b1
b2
b3
b4
b5
b6
b7
Hg1
b1
b2
b3
b4
b5
b6
Sb
S7
a1
a2
a3
a4
a5
a6
a7

Fig. 15.

which had been found in the course of investigations carried out by Mr. Clay down to the melting point of hydrogen. The resistance of the mercury is indicated by *Hg*. The results for platinum and for gold are also given in the figure, as well as the hypothetical curve for pure gold, the latter shown as a dashed line.

I would like to spend a little time on the arrangement of the experiment.

The thread of mercury, the resistance of which was measured, is shown in the accompanying Fig. 15. The mercury, which had been painstakingly purified, is poured off into a series of U-shaped capillary tubes of only 1/20 millimetre diameter in vacuo and frozen in them during the experiment. At the upper ends of the U there are (see Fig. 16) reservoirs to deal with the

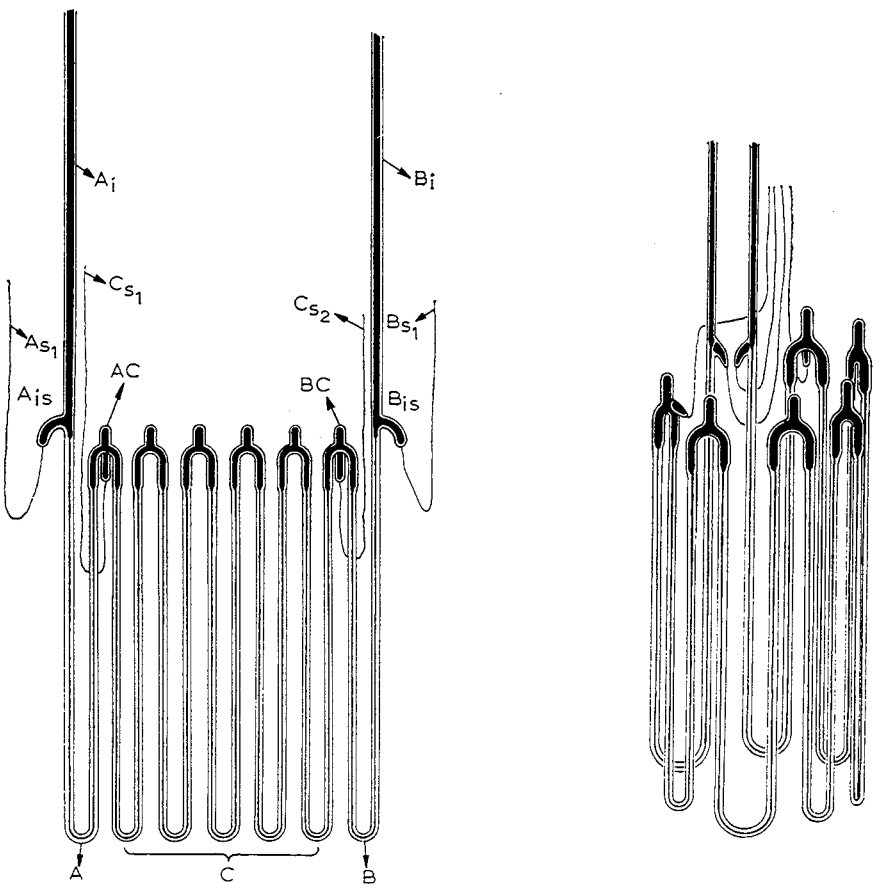


Fig. 16.

tubes bursting or the thread breaking either during thawing or freezing. At the ends of the resistances there are mercury wires to conduct the current and to measure potential differences. (This is the most simple model theoretically: a branched wire system all of the same metal. In the following model, see Fig. 16, the lower portions of the voltage wires are replaced by platinum wires.)

As has been said, the experiment left no doubt that, as far as accuracy of measurement went, the resistance disappeared. At the same time, however, something unexpected occurred. The disappearance did not take place gradually but (compare Fig. 17) *abruptly*. From 1/500 the resistance at 4.2°K drops to a millionth part. At the lowest temperature, 1.5°K, it could be established that the resistance had become less than a thousand-millionth part of that at normal temperature.

Thus the mercury at 4.2°K has entered a new state, which, owing to its particular electrical properties, can be called the state of superconductivity.

It is very important to the study of the properties of this state that tin and lead can also become superconductive. The transition point of tin lies at 3.8°K, that of lead probably at 6°K. Now that we are able to use these metals, which are easy to work, all types of electrical experiments with resistance-free apparatus have become possible. To take one example: a self-contained coil, cooled in the magnetic field, should, when the field is removed,

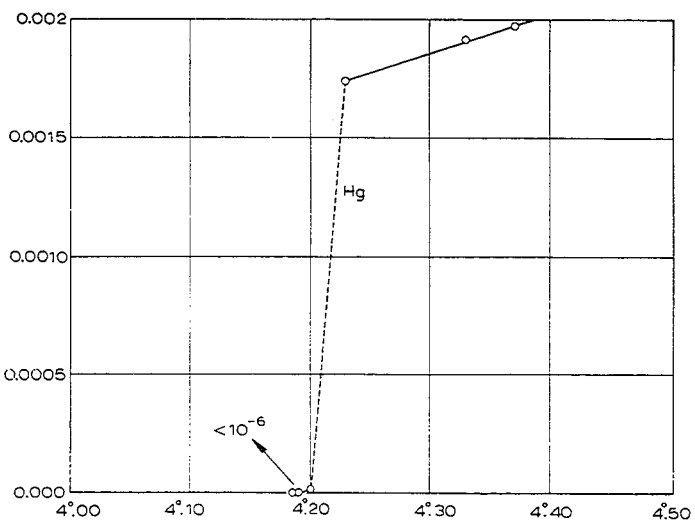


Fig. 17.

be able to simulate for some time an Ampère molecular current. (See Addendum 2.) There is also the question as to whether the absence of Joule heat makes feasible the production of strong magnetic fields using coils without iron,* for a current of very great density can be sent through very fine, closely wound wire spirals. Thus we were successful in sending a current of 0.8 amperes, i.e. of 56 amperes per square millimetre, through a coil, which contained 1,000 turns of a diameter of 1/70 square mm per square centimetre at right angles to the turns.

All that I have said so far about superconductors applies only to currents below a certain threshold value, which lies all the higher the lower one drops in temperature. Up to this threshold value of current intensities no potential differences are found at the ends of the resistance within the degree of accuracy reached so far. The wire has at the most only a residual micro-resistance. As soon, however, as the threshold value is exceeded, a potential difference is necessary to maintain the current. Then, in a way as yet unexplained, normal resistance occurs in the wire, and at the great rate at which the potential difference is increased at the ends, there can be a considerable rise in temperature. Thus a bright lead wire which had been dipped in liquid helium and which remained superconductive up to a current density of 420 amps. per square millimetre, melted at a current density of 940. It appears

* Shortly before this lecture I still felt that further experiments would lead to this question being answered in the affirmative. For if the contrary were the case new phenomena would have to be found. It seems possible that the potential phenomena which will be further discussed in the text can be suppressed by improving the material and the construction of the coil. Then only the resistance would be need to be considered which, owing to the characteristic magnetic field of the coil, can be produced in the resistanceless wire. After the experiment with the small coil mentioned in the text, it seemed that even this resistance would not come into the picture very much, even when strong fields are produced. From analogy it was to be assumed that it rises regularly with the field and even when it is proportional to the square of the field it would not be of very great importance even in fields of 100,000 Gauss, as the resistance produced in the coil by its own field was anyway still unnoticeable. Only experiments, however, could make certain of this. The experiments then planned were carried out after this lecture was given and produced surprising results. In fields below a threshold value (for lead at the boiling point of helium 600 Gauss), which was not reached during the experiment with the small coil mentioned in the text, there is no magnetic resistance at all. In fields above this threshold value a relatively large resistance arises at once, and grows considerably with the field. Thus in an unexpected way a difficulty in the production of intensive magnetic fields with coils without iron faced us. The discovery of the strange property which causes this made up for the difficulties involved.

that, generally speaking, when the threshold value of the current intensity is exceeded, local heating of the conductor through which the current is passing occurs above the transition temperature and this is why normal resistance at this point originates. When the current intensity increases the Joule heat produced at this point soon brings the whole resistance above the transition point.

The great question still not solved is whether this first impulse proceeds from bad sections of the wire or is also produced in pure, tension-free, evenly crystallized metal. If the potential phenomena are to be attributed to bad spots, we shall learn to eliminate them and then it is perhaps possible that the conductivity could be increased still higher than to the value reached at present, which is thousand million times that at normal temperature. The potential phenomena may, however, depend upon the nature of the metal in that the resting vibrators of the now superconductive metal are not made to move until the electron wind which blows past them has reached a certain strength, just as the surface of water is not ruffled until the wind has reached a certain strength. Ohm's law would have lost its validity for these phenomena.

Rather than immersing myself in a possible explanation based on the quantum theory, I should like to consider one of the experimental questions raised by the superconductors. According to ordinary theory of electrons one finds, for the free paths of the electrons, which at normal temperature are about of molecular size, values for the superconductive state going up to a metre. Will superconductive foils be passed through more easily, or with less deflection, by electric rays of various types than are ordinary conductive ones? This question has been raised by Mr. Lenard and myself. Mr. Becker and I are already busy with the relevant preliminary experiments.

I have already said that this is only one of the many questions which the superconductors raise. From each field of physics further questions push their way to the fore which are waiting to be solved by measurements at helium temperatures.

In the future I see all over the Leyden laboratory measurements being made in cryostats, to which liquid helium is transported just as the other liquid gases now are, and in which this gas also, one might say, will be as freely available as water.

In the meantime there is plenty of work which can already be done, albeit with difficulty (as when we began to use liquid oxygen), in the neighbourhood of the liquefying apparatus, and which can contribute towards lifting the veil which thermal motion at normal temperature spreads over the inner world of atoms and electrons.

Added in proof

1. In May 1914 the qualitative validity of the Langevin theory in both the connections mentioned in the text was established in gadolinium sulphate by experiments at helium temperatures.

2. The following was achieved during the experiments carried out in April-June 1914 relating to currents persisting in superconductors in the absence of electromotive force. The current, once produced, lasted for hours practically unchanged in the superconductive coil. Heating the latter above the transition point immediately destroys the electromagnetic field borne by the circuit. The experiments evidently demonstrate the frictionless mechanism as envisaged by Maxwell in the ether around a circuit and the absorption of its kinetic energy by a resistance introduced into the circuit.

Biography

Heike Kamerlingh Onnes was born on September 21, 1853, at Groningen, The Netherlands. His father, Harm Kamerlingh Onnes, was the owner of a brickworks near Groningen; his mother was Anna Gerdina Coers of Arnhem, the daughter of an architect.

After spending the allotted time at the «Hoogere Burgerschool» in his native town (secondary school without classical languages), the director of which was the later Professor of Chemistry at Leyden J. M. van Bemmelen, he received supplementary teaching in Greek and Latin. In 1870 he entered the University of Groningen, obtained his «candidaats» degree (approx. B. Sc.) the following year, and then went to Heidelberg as a student of Bunsen and Kirchhoff from October 1871 until April 1873. Thereafter he returned to Groningen, where he passed his «doctoraal» examination (approx. M.Sc.) in 1878 and obtained the doctor's degree in 1879 with a remarkable thesis *Nieuwe bewijzen voor de aswenteling der aarde* (New proofs of the rotation of the earth).

Meanwhile in 1878 he had become assistant at the Polytechnicum at Delft, working under Bosscha, in whose place he also lectured in 1881 and 1882, the year in which he was appointed Professor of Experimental Physics and Meteorology at Leyden University, in succession to P. L. Rijke.

Kamerlingh Onnes' talents for solving scientific problems was already apparent in 1871, when at the age of 18 he was awarded a Gold Medal for a competition sponsored by the Natural Sciences Faculty of the University of Utrecht, followed the next year by a Silver Medal for a similar event at the University of Groningen. When working with Kirchhoff he also won the «Seminarpreis», entitling him to occupy one of the two existing assistantships under Kirchhoff.

In his doctor's thesis theoretical as well as experimental proof was given that Foucault's well-known pendulum experiment should be considered as a special case of a large group of phenomena which in a much simpler fashion can be used to prove the rotational movement of the earth. In 1881 he published a paper *Algemeene theorie der vloeistoffen* (General theory of liquids),

which dealt with the kinetic theory of the liquid state, approaching Van der Waals' law of corresponding states from a mechanistic point of view. This work can be considered as the beginning of his life-long investigations into the properties of matter at low temperatures. In his inaugural address *De beteekenis van het quantitatief onderzoek in de natuurkunde* (The importance of quantitative research in physics) he arrived at his well-known motto «Door meten tot weten» (Knowledge through measurement), an appreciation of the value of measurements which concerned him throughout his scientific career.

After his appointment to the Physics Chair at Leyden, Kamerlingh Onnes reorganized the Physical Laboratory (now known as the Kamerlingh Onnes Laboratory) in a way to suit his own programme. His researches were mainly based on the theories of his two great compatriots J. D. van der Waals and H. A. Lorentz. In particular he had in mind the establishment of a cryogenic laboratory which would enable him to verify Van der Waals' law of corresponding states over a large range of temperatures. His efforts to reach extremely low temperatures culminated in the liquefaction of helium in 1908. Bringing the temperature of the helium down to 0.9°K, he reached the nearest approach to absolute zero then achieved, thus justifying the saying that the coldest spot on earth was situated at Leyden. It was on account of these low-temperature studies that he was awarded the Nobel Prize. Later, his pupils W. H. Keesom and W. J. de Haas (Lorentz' son-in-law) conducted experiments in the same laboratory which led them still closer to absolute zero.

Other investigations in his laboratory which gradually gained in importance and international fame, included thermodynamics, the radioactivity law, and observations on optical, magnetic and electrical phenomena, such as the study of fluorescence and phosphorescence, the magnetic rotation of the polarization plane, absorption spectra of crystals in the magnetic field; also the Hall effect, dielectric constants, and especially the resistance of metals. A momentous discovery (1911) was that of the *superconductivity* of pure metals such as mercury, tin and lead at very low temperatures, and following from this the observation of *persisting currents*.

The results of Kamerlingh Onnes' investigations were published in the *Proceedings of the Royal Academy of Sciences of Amsterdam* and also in the *Communications from the Physical Laboratory at Leyden*. Many foreign scientists came to Leyden to work in his laboratory for shorter or longer periods, not only those mentioned on pp. 314 and 315 but others as well.

The laboratory gained additional fame throughout the world through the training school for instrument-makers and glass-blowers housed in it, founded by Kamerlingh Onnes in 1901.

At the early age of 30, Kamerlingh Onnes was appointed a member of the Royal Academy of Sciences of Amsterdam. He was one of the founders of the Association (now Institut) International du Froid. He was a Commander in the Order of the Netherlands Lion, the Order of Orange-Nassau of the Netherlands, the Order of St. Olaf of Norway, and the Order of Polonia Restituta of Poland. He held an honorary doctorate of the University of Berlin, and was awarded the Matteucci Medal, the Rumford Medal, the Baumgarten Preis and the Franklin Medal. He was Member of the Society of Friends of Science in Moscow, and of the Academies of Sciences in Copenhagen, Uppsala, Turin, Vienna, Göttingen and Halle; Foreign Associate of the Académie des Sciences of Paris; Foreign Member of the Accademia dei Lincei of Rome and the Royal Society of London; and Honorary Member of the Physical Society of Stockholm, the Société Helvétique des Sciences Naturelles, the Royal Institution of London, the Sociedad Española da Física y Química of Madrid, and the Franklin Institute of Philadelphia.

Outside his scientific work, Kamerlingh Onnes' favourite recreations were his family life and helpfulness to those who needed it. Although his work was his hobby, he was far from being a pompous scholar. A man of great personal charm and philanthropic humanity, he was very active during and after the First World War in smoothing out political differences between scientists and in succouring starving children in countries suffering from food shortage. In 1887 he married Maria Adriana Wilhelmina Elisabeth Bijleveld, who was a great help to him in these activities and who created a home widely known for its hospitality. They had one son, Albert, who became a high-ranking civil servant at The Hague.

Kamerlingh Onnes' health had always been somewhat delicate, and, after a short illness, he died at Leyden on February 21, 1926.

Physics 1914

MAX VON LAUE

«for his discovery of the diffraction of Röntgen rays by crystals»

Physics 1914

Presentation Speech by Professor G. Granqvist, Chairman of the Nobel Committee for Physics of the Royal Swedish Academy of Sciences

Your Majesty, Your Royal Highnesses, Ladies and Gentlemen.

Seldom indeed can a discovery in the field of physics have given rise to such intensive research work as did that of Röntgen in 1896, when he proved the existence of a new form of rays which had hitherto been unknown and which, owing to their remarkable characteristics, have since achieved a position of the greatest importance, not only in the field of pure physics but also in connection with research work throughout the other sciences.

Notwithstanding the considerable number of tests which have been carried out since their discovery and directed toward investigation of the true nature of X-rays, it was not until over a decade had passed that their true nature had finally been elucidated.

Already during the first tests it was established that not even the strongest magnetic fields were able to alter the direction of the rays. It was equally impossible to prove the existence of a refraction on transfer of the rays from one medium to another. If the X-rays were of a corpuscular nature they could not, therefore, be carriers of an electrical charge, as is the case with other known rays of corpuscular nature. If, therefore, we wish to disregard matter which has no electrical charge, it is necessary to assume that the particles, whose motion is characteristic for the X-rays, bear two charges of opposite sign, one of which neutralizes the other. On the other hand, from the fact that there was no evidence of refraction of the X-rays, it was possible to assume that, should they consist of a transverse wave motion – as is the case with light waves – the relevant wavelength would have to be very small, as for very small wavelengths, according to the theory of dispersion of light, the refractive index would approach unity.

After hurriedly discarding an hypothesis which had been expounded initially, according to which X-rays were believed to consist of longitudinal wave motions in ether, opinions as to their actual nature were divided according to the above two alternatives. Nevertheless an objective presentation could only describe them as a type of impulse of an unknown nature.

On the basis of an hypothesis expounded as early as 1896 by Stokes and

Wiechert this impulse was believed to consist of a disturbance which occurs in the ether when the cathode-ray particle, i.e. a forward-rushing electron, is impeded on colliding with molecules of matter. This disturbance or impulse was believed to propagate in all directions at the speed of light from the ether surrounding the electron. In each part of the space this disturbance was maintained for a period of identical duration to that in which the electron was impeded. This period of time, multiplied by the speed of light, was described as the impulse width, a quantity which, if the nature of the X-rays were the same as that of the light rays, would coincide with the wavelength.

According to that theory the X-ray impulse, which originates perpendicular to the cathode-ray bundle by which it is excited, is alleged to be completely polarized. The evidence of this type of polarization was first produced by Barkla in 1905, but, contrary to the theory, the polarization was not complete but only partial. While it was possible to explain the causative factors of this aberration the characteristics of the polarization were not adequate to prove the existence of a transverse undulation.

Once Dorn had succeeded, in 1897, in determining the fraction of the energy of the impeded electrons which is converted to X-rays, W. Wien was able to calculate the impulse width which, according to his figures, amounted to approximately 10^{-10} cm, or only one hundred-thousandth of the shortest known wavelengths of light. The short impulse width thus determined could explain the lack of success with previous diffraction tests which had been carried out on slits with X-rays, for even with the narrowest slit the diffraction phenomenon, which is produced by such small impulse widths or wavelengths, would have to lie just about at the limits of possible observation. And it may, in actual fact, only be said even of the most accurate of these tests conducted by Walter and Pohl that they render diffraction highly probable. From the research carried out by these scientists it would meanwhile seem to follow that the upper limit for the impulse width of X-rays lies at 4×10^{-9}.

This was the situation when von Laue placed a research medium of the highest import at the disposal of science by virtue of his epoch-making discovery of the interference of X-rays and, at the same time, proved that X-rays, as is the case with light rays, consist of progressive transversal waves.

Previous research had indicated, as is mentioned in the foregoing, that it was highly probable that, if X-rays are wave motions of the same type as light rays, then their wavelengths would have to be of an order of 10^{-9} cm. In order to obtain clear interference phenomena of the same type as those

which are caused when light rays pass a grating it was necessary for the distance between the grating slits to be of an order of 10^{-8} cm. But this is approximately the distance between the molecules of a solid body and it was in this manner that von Laue arrived at the idea of employing, as a diffraction grating, a solid body with regularly-arranged molecules, e.g. a crystal. As early as 1850 Bravais had introduced into crystallography the assumption that the atoms composing the various crystals are arranged in regular groups, so-called three-dimensional lattices or space-lattices, whose constants could be calculated with the aid of crystallographic data.

However, the theoretical basis of a space-lattice was unknown and thus it was first necessary for von Laue to develop this theory if else the investigation were to have a value. This he did mainly according to the same approximations as those conventional to the science of optics as applied to normal one-dimensional lattices.

Von Laue left the execution of the experimental work in the hands of W. Friedrich and P. Knipping. The apparatus which they employed consisted of a lead box into which they admitted a thin bundle of X-rays which they directed so as to fall upon a precisely oriented crystal. Sensitized film was positioned both behind and at the sides of the crystal. Already the preparatory tests showed that the intensity maxima which had been anticipated by von Laue became evident in the form of blackened spots on the film positioned behind the crystal.

From the grouping shown by these intensity maxima in accordance with the requirements of the theory, as established, for such photograms of various crystals and from the degree of clarity with which they have been reproduced, it follows that they are an interference phenomenon. Absorption tests have shown that the rays which give rise to the points of interference are actually X-rays, and from this von Laue has deduced with a high degree of certainty that the X-rays which cause intensity maxima on irradiation of a crystal have the character of a wave motion. However, the same is required also for those rays employed for irradiation purposes, for, as he says, were they of a corpuscular nature, coherent oscillations could only arise from those atoms set into motion by the identical corpuscle and these atoms would have to form together one whole agglomerate whose dimensions would be largest in the direction of radiation. However, contrary to what was indicated in the experiment, this would result in the intensity maxima consisting of irregular concentric circles.

As a result of von Laue's discovery of the diffraction of X-rays in crystals

proof was thus established that these light waves are of very small wavelengths. However, this discovery also resulted in the most important discoveries in the field of crystallography. It is now possible to determine the position of atoms in crystals and much important knowledge has been gained in this connection. We can anticipate further discoveries of equal note in the future. It is thus rendered likely that experimental research into the influence of temperature upon diffraction will provide the solution to the question of a zero-point energy, or will at least be of some assistance in arriving at a solution to this problem, as the temperature factor assumes a different value according to whether a zero-point energy exists or not. However, the direct results of this discovery of diffraction are of no less importance: it is now possible to subject the X-ray spectra to direct examination, their line spectra can even be photographed, and science has thus been enriched by a method of research whose full implications can not yet be fully appreciated.

If it is permissible to evaluate a human discovery according to the fruits which it bears then there are not many discoveries ranking on a par with that made by von Laue. If one reflects further on the fact that only a few years have passed since his discovery was first published it may surely be said that, when awarding the Nobel Prize for Physics, the Royal Academy of Sciences will presumably seldom, if ever, be in a position of such close agreement with the letter of the Testament as on this occasion in deciding to award the Nobel Prize for Physics for the year 1914 to Professor Max von Laue, for his discovery of the diffraction of X-rays in crystals.

MAX VON LAUE

Concerning the detection of X-ray interferences

Nobel Lecture, November 12, 1915

If other Nobel Prize winners express their thanks for this high honour at this juncture by recounting the history of their discovery they are able to report how, at the outset, they envisaged a high, but nevertheless attainable, objective and how they strove toward that objective by many paths, most of which proved initially to be false and how then, after many years of protracted endeavour, they finally achieved their objective. In my eyes the credit to which they are entitled increases in direct proportion to the magnitude of the difficulties which they finally surmounted. By comparison, what I have to say here differs somewhat from those examples. There is no doubt that I, also, had long been aware of the problem, i.e. producing X-ray interferences, before the inherent difficulties had finally been surmounted. But I never believed that it would be my personal good fortune to make a contribution in that direction, and it was for that reason that I did not concern myself unduly in that respect until suddenly I perceived the way which subsequently proved to be the shortest path to success. I am not, therefore, able to recount many details in connection with my personal preparatory work, so I shall confine my comments to explaining the combination of scientific and personal circumstances from which the idea first arose.

In the case of X-rays their discoverer had already made efforts to locate diffraction or interference phenomena in order to solve the question of whether or not they represent a wave phenomenon or the ejection of any small particles. But in this quest his research, which had otherwise been so successful, met with failure. Nevertheless, from the outset there were supporters for the former opinion; for Stokes and Wiechert could point out with complete justification that, according to Maxwell-Lorentz electrodynamics, electromagnetic waves must be produced whenever electricity carriers alter their velocity; one was also aware that cathode rays, which induce X-rays on striking an obstacle, are made up of electrons. W. Wien endeavoured to reconcile this consideration with what was, at that time, the very young quantum theory in order thus to arrive at an estimate of the relevant wavelengths. He arrived

at a value lying between 10^{-10} and 10^{-9} cm, which was in accordance with the fact that, if they were at all made up of waves, the X-rays could only have a shorter wavelength than visible light. For in 1900 all electromagnetic radiation of longer wavelengths was already known at least to the extent that one could not seek in it the more striking characteristics of X-rays such as, for example, the strong penetrating power.

If diffraction or interference phenomena were to be sought it was therefore necessary, in accordance with the basic principles of wave theory, to select for the test arrangement far smaller decisive dimensions than those employed in corresponding tests with visible light. Thus Haga and Wind, as well as Walter and Pohl, selected a wedge-shaped pointed slit at the pointed end of which they were able to prove a broadening of the transmitted X-ray bundles which could be attributed to diffraction. The conclusive force of these tests has frequently been a subject of dispute; for example there was one school of thought which tried to explain the broadening as a subjective optical illusion. However, the photometric measurement carried out by P. P. Koch on the best photograms of this type and the estimated wavelength of 4×10^{-9} cm which Sommerfeld connected with those results would appear to prove that there is an objective basis for the broadening theory. For now that the relevant wavelengths have been confirmed we must regard this value as representing a very good determination for such tests.

Further evidence in support of the wave theory of X-rays was provided by Barkla's discovery of polarisation. Barkla permits rays, such as those originating from the anticathode of an X-ray tube, to fall upon a substance such as, for example, carbon, after which he then carries on working with those secondary X-rays as result from scattering, which emanate from the substance in question perpendicular to the original direction of radiation. If the secondary radiation is again permitted to fall upon such a substance this causes a production, as a result of repeated scattering, of a tertiary radiation of X-rays ; but this has, at the plane perpendicular to the secondary radiation, an intensity maximum in one direction, and perpendicular to that a minimum at which it may even disappear completely. This indicates most clearly that, for the secondary radiation, the direction of propagation is not a symmetry axis and that it is thus polarised. This fact can be explained only with extreme difficulty by a corpuscular theory – of this we are only too well aware from the history of optics. Conversely it is quite possible to explain this by wave theory based upon the fact that all electromagnetic waves oscillate transversally and are therefore generally polarised. This theory also readily

explains the further fact that the direction of the vanishing intensity is parallel to the direction of the primary rays.

Notwithstanding these major arguments the wave theory initially did not meet with complete acceptance. For experts like W. H. Bragg maintained until 1912 the corpuscular view and, in support of that view, they were able to quote a series of phenomena which, in actual fact, have remained inaccessible to explanation by the wave theory. If X-rays do, in fact, fall upon a body, they not only excite the aforementioned secondary X-rays, they also free electrons. The velocities of these electrons do not now depend upon the temperature and other conditions obtaining with the body in question and one might therefore assume that their kinetic energy is derived from the incident X-ray radiation. But this is where the difficulty is encountered. For the velocities which have been observed are not dependent upon the intensity of radiation but, in fact, upon their degree of hardness, and specifically in such a manner that these velocities increase with increasing hardness. If the intensity is weakened this will result in a decrease in the number of electrons released, but it will have no effect upon their velocities. The greatest velocity which occurs is always that which was possessed by the cathode radiation by which the X-rays have been excited; it is almost as if X-ray radiation retains a «memory» of the process of its creation and expresses this fact when, for its part, it releases electrons.

This would be quite easy to comprehend if the energy of the X-ray radiation were to accumulate and remain accumulated at certain points, as would, for example, be the case with the motion of individual corpuscles. But in agreement with the wave theory it expands progressively over increasing areas. This poses us with a complete puzzle, which is merely rendered the more complex by the further observation that the electrons which have been released would appear to prefer the direction taken by the incident X-rays. And it is a source of only qualified comfort if we make clear to ourselves that with the short-wave visible and ultraviolet light rays the same phenomena occur in the photoelectric effect. In this case also, where we most certainly have before us a wave process, they are simply incomprehensible.

We can only understand the reasons why our understanding fails at this juncture. Einstein has provided us with a rule on the dependency of the greatest existing electron velocity upon the frequency of light waves; in this the decisive rôle is played by the universal constant h, designated by Planck, its discoverer, as the elementary quantum of action. Conversely, more recent measurements – among which particular accuracy was achieved in those

made by Ernst Wagner in Munich – have shown that with the reverse process, specifically the excitation of X-rays by cathode rays, the same constant appears. This same Einstein law, namely, also links the greatest frequency in the X-ray spectrum with the kinetic energy of cathode rays. We are confronted here with just one special case of the great quantum mystery, the solution of which one may justly term the most important objective currently confronting the entire field of physics. However, it was quite understandable that before this relationship with the quantum theory had been ascertained researchers had based their arguments against the wave theory on the facts of electron emission.

But let us turn once again to the situation which obtained before 1912. For as far back as I can recall having had an interest in physics – and that goes back to the days in which I was privileged to receive first-class instruction at the famous old Protestant Gymnasium in Strassburg, Alsatia – my particular attention was drawn to the field of optics, and within that field the wave theory of light. The lectures which I heard during my student days from Voigt, Planck and Lummer provided me at that time, and especially in the afore-mentioned field, with thorough experimental and theoretical knowledge; and as I had finally been able to cultivate what one could almost term a special feeling or intuition for wave processes, it was quite natural that my first independent work should be concerned with the propagation of natural radiation in dispersing substances and the thermodynamics of interference phenomena. It turned out to be a matter of great good fortune that Sommerfeld passed to me the article «Wellenoptik» (Wave optics) at that time to work upon for the *Encyclopedia of Mathematical Sciences*. For it was during that project that I was obliged to seek a mathematical presentation of the lattice theory which – while it was not exactly what one might term an innovation – could be applied with considerable simplicity to lattice grids. Already at that juncture I was devoting considerable thought to the further transfer to space-lattices; but I did not pursue the matter further owing to the fact that they do not, after all, play any actual part in the field of optics.

On my arrival in Munich in 1909 my attention was drawn constantly – first owing to the influence of Röntgen's work at this University and subsequently by Sommerfeld's active interest in X-rays and γ-rays, which he had also testified in several works – back to the question of their actual nature. To this was added a further important circumstance. Since the times of Hauy and Bravais the basic crystallographic law of rational indices had been explained simply and visually by the mineralogists through space-lattice arrangement of

the atoms. Sohncke, Federow and Schoenflies had brought the mathematical theory of possible space-lattices to the greatest possible degree of perfection. But no more far-reaching physical conclusion had evolved from this line of thought and thus, in the form of a questionable hypothesis, it remained a somewhat unknown quantity to physicists. But in Munich, where models of the Sohncke space-lattices were to be found in more than one university institute, it was P. Groth who expressed his defence of it, both orally and in writing, and I, also, thus learned from him. My interest in the theories which he was expounding increased progressively as, already at an early time, I had made sure – and contrary to the doubts which, in my day, were maintained by only a few philosophers concerning the reality of atoms – that there existed no watertight epistemological arguments which might contradict this fact, while practical experience was constantly providing fresh confirmation in support of it.

Such was the state of affairs as, one evening in February 1912, P. P. Ewald came to visit me. On Sommerfeld's instigation he was working on a mathematical investigation into the behaviour of long electromagnetic waves in a space-lattice and subsequently he published a dissertation on the theory of crystal optics which was based upon that work. But he was faced at that time with certain difficulties and came to me with a request for advice. Now it was not, however, possible for me to assist him at that time. But during the conversation I was suddenly struck by the obvious question of the behaviour of waves which are short by comparison with the lattice-constants of the space-lattice. And it was at that point that my intuition for optics suddenly gave me the answer: lattice spectra would have to ensue. The fact that the lattice-constant in crystals is of an order of 10^{-8} cm was sufficiently known from the analogy with other interatomic distances in solid and liquid substances, and, in addition, this could easily be argued from the density, molecular weight and the mass of the hydrogen atom which, just at that time, had been particularly well determined. The order of X-ray wavelengths was estimated by Wien and Sommerfeld at 10^{-9} cm. Thus the ratio of wavelengths and lattice-constants was extremely favourable if X-rays were to be transmitted through a crystal. I immediately told Ewald that I anticipated the occurrence of interference phenomena with X-rays.

It was not long before W. Friedrich also heard of this. While he immediately expressed his willingness to carry out a relevant test, the acknowledged masters of our science, to whom I had the opportunity of submitting it, entertained certain doubts about this viewpoint. A certain amount of diplomacy

was necessary before Friedrich and Knipping were finally permitted to carry out the experiment according to my plan, using very simple equipment at the outset. Copper sulphate served as the crystal, since large and regular pieces of it can easily be obtained. The irradiation direction was left to chance. Immediately from the outset the photographic plate located behind the crystal betrayed the presence of a considerable number of deflected rays, together with a trace of the primary ray coming directly from the anticathode. These were the lattice spectra which had been anticipated (Fig. 1).

In continuation of the work, Friedrich and Knipping, with the aid of the considerable amount of equipment then placed at their disposal by the Institute of Theoretical Physics of the Munich University, substituted random irradiation of triclinic copper sulphate with irradiation, in the crystallographically-indicated axial directions, of crystals of maximum possible symmetry, i.e. regular crystals. In principle the theory had already been completed by transfer from the ordinary and from the grid lattice, and, on June 8, 1912, Sommerfeld was able to submit to the Munich Academy the joint work of Friedrich, Knipping, and myself on X-ray interferences, which work, apart from the theory itself, also contained a series of very characteristic exposures (Figs. 2 and 3). Four weeks later followed the first application of the theory for interpreting the points of interference, produced by a regular crystal after irradiation along a four-fold axis (Fig. 2). Each point of interference corresponds with three integral numbers, originating from the three space-lattice periodicities, the ratios of which determine its position. I have attributed these numbers to the points of interference in the second publication which appeared simultaneously with the first. And said numbers, or their ratios, have proven thoroughly reliable, even after re-checking by the Braggs and other researchers. Thus the initial probe into the rightness of the interference theory proved favourable for this new phenomenon. This benefitted not only the wave conception of X-rays but also the lattice theory of crystals.

The wavelength can also be determined from the three specified numbers and the latter publication contains therefore also information in that connection. Nevertheless I made no secret of the fact that I could not attribute to these values the same degree of reliability as to the ratios of the numbers. For this there were two reasons. On the hand there may occur in each interference maximum – as has been adequately known in the science of optics – apart from a basic wavelength, in addition its half, third, etc. For as long as one has no further point of reference, apart from the position of the maximum, the wavelength thus remains uncertain by an integral factor. The wavelengths

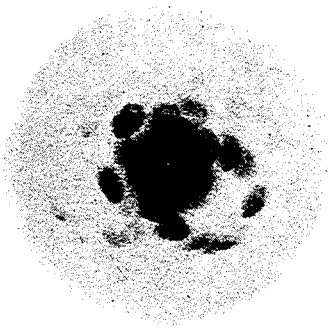


Fig. 1.

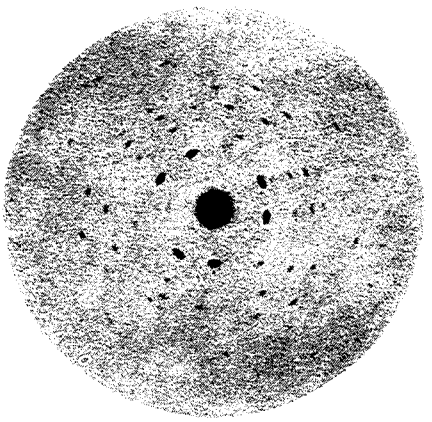


Fig. 2.

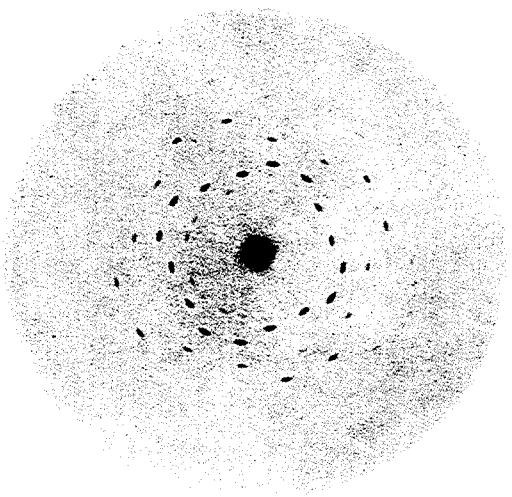


Fig. 3.

were then measured by the dimensions of the space-lattice. As has already been mentioned, these could certainly be estimated by the order of magnitude; but in order to compute them with some degree of accuracy it would be necessary to know the molecular structure of the crystals down to the last detail. Instead of that one was aware, solely from the research carried out by the mathematicians, that, apart from the simple space-lattices, many other types of lattice were possible, for example, face- and body-centred lattices. The type of structure which is found in certain crystals was first determined by the Braggs after they had made a daring guess in producing an accurate

hypothesis as to the structure of sodium chloride. And it was thus that only they were also able to carry out the final measurements on the wavelengths.

But in reference to the manner in which the mathematical theory is to be generalised for such more complex types of structure, this I set forth almost one year later, when the necessity of that step had been shown with all urgency. The changes are minimal. The position of the points of interference remains the same as with a simple lattice of identical lattice-constants; it is only in the expression of their intensity that a new factor appears which is now designated as the structural factor. However, this factor can occasionally reduce to zero and then the relevant point is suppressed completely. It is due to this factor when an irradiation exposure plate gives some indication of the lower symmetry of the actual crystal instead of the full symmetry of the simple space-lattice.

The considerable interest which had been caused by the initial investigations was soon reflected in a large number of subsequent works. At first certain doubts were expressed as to whether, in this instance, an interference phenomenon really did exist. But these were soon overcome. In my opinion the most complete rebuff was embodied in the proof produced by Wagner and Glocker that the X-ray radiation at the points of interference contained only one wavelength, even if many of them should exist in the original ray. The experimental research diverged essentially into two branches. The crystals were either investigated with the aid of the interferences, which examined the crystals' space-lattices, or they were employed in spectroscopy in the X-ray range. Since 1912 much has been done in both fields, and in both sectors W. L. Bragg and W. H. Bragg have taken the first important step beyond the investigations carried out at Munich. It would range far too wide if I were, at this juncture, to compile a fairly comprehensive list of all the researchers who have gained distinction in that research work. I can mention here only the transformation of the process, which proved to be of great importance for the further conduct of the experiments and in which Debye transferred the examination of beautiful, well-formed crystal fragments – which sometimes are obtainable only with considerable difficulty – to research into the finest possible crystal powder. Mention should also be made here of the extremely painstaking and accurate determination of the characteristic spectra which, commenced by Moseley, have been carried on – especially here in Sweden by Manne Siegbahn and Stenström – on the great majority of chemical elements. This research, as is known, has become of fundamental importance for the further knowledge of the structure of the atom. Neither

should I neglect to mention the fact that also the theory has been successfully extended. The important question as to why the thermal motion of the atoms does not result in any great amount of disturbance to the interference phenomena, has been settled by Debye, while H. A. Lorentz showed the manner in which the interference points which originate from the continuous spectrum obtain their intensity from the energy of many neighbouring wavelengths. For both reasons, owing to the thermal motion and to the working together of various wavelengths, factors arise which, in a similar manner to the structural factor, exert some influence upon the brightness of the interference points but not upon their location.

I would finally like to deal with a question which is still pending and to which, I trust, X-rays will shortly bring the answer. There are two contradictory opinions in respect of the mixed crystals. Opinions are united in respect of the fact that atoms form space-lattices, as is the case with chemically uniform crystals. There is also agreement in respect of the fact that in a mixed crystal of, e.g., potassium chloride and rubidium chloride the chlorine atoms lie in precisely the same position in relation to each other as in pure potassium chloride or pure rubidium chloride. (From the work carried out by the Braggs we are already aware of the fact that these two salts have the identical space-lattice.) But how are the potassium and rubidium atoms distributed over the positions reserved for the metal atoms? According to the one viewpoint, which probably originated from Van 't Hoff, this occurs entirely by chance, while the other viewpoint maintains that it occurs regularly, thus permitting the space-lattices to be composed of equal-sized parallelepipeds of molecular dimensions which are identical in every respect, including the atomic occupation. For purely theoretical reasons the former opinion would appear to me to be the more probable. If, as in the example given, every mixing ratio is possible, it is indeed difficult to conceive, in every case, of atomic distributions which observe crystallographic symmetry, and, in addition, leave sufficiently small dimensions for the specified elementary parallelepipeds. Also the irradiation tests conducted by Vegard and Skjeldrup on the above-mentioned mixtures would appear to have resulted in an even stronger contradiction of the second opinion than these researchers themselves had originally thought. Nevertheless during recent years Tammann has conducted chemical tests on mixed crystals of various metals such as, for example, copper and gold, which appear at first sight to support the second view. This should provide a further field of endeavour for a new application of X-ray interferences.

Biography

Max Theodor Felix von Laue was born on October 9, 1879 at Pfaffendorf, near Koblenz. He was the son of Julius von Laue, an official in the German military administration, who was raised to hereditary nobility in 1913 and who was often sent to various towns, so that von Laue spent his youth in Brandenburg, Altona, Posen, Berlin and Strassburg, going to school in the three last-named cities. At the Protestant school at Strassburg he came under the influence of Professor Goering, who introduced him to the exact sciences.

In 1898 he left school and for a year did his military service. He then went to the University of Strassburg where he studied mathematics, physics and chemistry; but soon he moved to the University of Göttingen, where he worked under Professor W. Voigt and Professor W. Abraham, who greatly influenced him. After a semester at the University of Munich he went, in 1902, to the University of Berlin to work under Professor Max Planck. Here he attended lectures by O. Lummer on interference spectroscopy and heat radiation, the influence of which was shown in von Laue's dissertation on interference phenomena in plane-parallel plates.

After obtaining his doctorate at Berlin in 1903, von Laue went for two years to the University of Göttingen. In 1905 he was offered the post of assistant to Max Planck at the Institute for Theoretical Physics at Berlin. Here he worked on the application of entropy to radiation fields and on the thermodynamic significance of the coherence of light waves.

In 1909 he went as Privatdozent to the University of Munich, where he lectured on optics, thermodynamics and the theory of relativity and in 1912 he became Professor of Physics at the University of Zurich. In 1914 he moved, as Professor of Physics, to Frankfurt-on-Mains and from 1916 he was engaged in war work at the University of Würzburg on high vacuum tubes used for telephony and wireless communication. In 1919 he was appointed Professor of Physics at the University of Berlin, a post which he held until 1943. From 1934 onwards he acted as consultant to the Physikalisch-Technische Reichsanstalt at Berlin-Charlottenburg.

In 1917, when the Institute for Physics was established at Berlin-Dahlem with Einstein as its Director, von Laue had charge, as Second Director, of most of the administrative work of this Institute, which was in close touch with German scientific research. Von Laue exerted, during this period and also later, considerable influence on the development of scientific research in Germany. When Berlin was bombed, this Institute moved to Hechingen, in Württemberg and von Laue accompanied it there. He remained at Hechingen from 1944 until 1945 and here, to distract his thoughts from the war, he wrote a History of Physics, which went into four editions and was translated into seven other languages. Here he welcomed the arrival of the French troops and was taken by an Anglo-American mission, together with nine other German scientists, to England where he remained until 1946. During his confinement in England he wrote a paper on the low absorption of X-rays during diffraction, which he contributed in 1948, to the International Union of Crystallographers at Harvard University. In 1946 he went to Göttingen as Acting Director of the Max Planck Institute and Titular Professor in the University there.

In 1951 he was elected Director of the Fritz Haber Institute for Physical Chemistry at Berlin-Dahlem and here he did much work on X-ray optics in collaboration with Borrmann and others.

In 1958 he retired and in 1959 his 80th birthday was celebrated in Berlin-Dahlem. He lived on, still actively at work, for another six months.

Apart from his earlier work already mentioned, von Laue's scientific work extended over a wide field. Early in his career he was greatly excited by Einstein's theory of relativity and between 1907 and 1911 he published eight papers on the application of this theory. In 1911 he published a book on the restricted theory and in 1921 another on the general theory, both books going into several editions.

His best known work, however, for which he received the Nobel Prize for Physics for 1914, was his discovery of the diffraction of X-rays on crystals. This discovery originated, as he related in his Nobel Lecture, when he was discussing problems related to the passage of waves of light through a periodic, crystalline arrangement of particles. The idea then came to him that the much shorter electromagnetic rays, which X-rays were supposed to be, would cause in such a medium some kind of diffraction or interference phenomena and that a crystal would provide such a medium. Although his colleagues Sommerfeld, W. Wien and others, with whom he discussed the idea on a skiing expedition, raised objections to the idea, W. Friedrich, one of

Sommerfeld's assistants and P. Knipping tested it out experimentally and, after some failures, succeeded in proving it to be correct. Von Laue worked out the mathematical formulation of it and the discovery was published in 1912. It established the fact that X-rays are electromagnetic in nature and it opened the way to the later work of Sir William and Sir Lawrence Bragg. Subsequently von Laue made other contributions to this subject.

Also prominent in von Laue's work were his contributions to the problems of superconductivity which he made when he was Professor of Theoretical Physics at Berlin University. At this time Walther Meissner was studying at the Physikalisch-Technische Reichsanstalt in Berlin, the remarkable disappearance of ohmic resistance shown by many metals at temperatures of the order of that of liquid helium. An especially valuable contribution then made by von Laue was his explanation, in 1932, of the fact that the threshold of the applied magnetic field which destroys superconductivity varies with the shape of the body because, when the magnetic field is established after the state of superconductivity has been established, the magnetic field is deformed by the supercurrents induced at the surface of the metal being used. This explanation was confirmed and it opened the way to Meissner's subsequent discovery that a superconductor eliminates the whole magnetic field in its interior and this became the basic idea of F. and H. London's theory of superconductivity. Von Laue published one paper in collaboration with F. and H. London and between 1937 and 1947 he published a total of 12 papers and a book on this subject.

Among the many honours and distinctions which he was awarded were the Ladenburg Medal, the Max-Planck Medal and the Bimala-Churn-Law Gold Medal of the Indian Association at Calcutta. He held Honorary Doctorates of the Universities of Bonn, Stuttgart, Munich, Berlin, Manchester and Chicago, was a member of the Russian Academy and the Academy of Sciences of Berlin, the German Physical Society and Mathematical Society, the Kant Society, the Academy of Sciences of Vienna, the American Physical Society, the Société Française de Physique and the Société Française de Minéralogie et Crystallographie. He was also Honorary Senator of the Max-Planck Society and Honorary Member of the German Röntgen Society, and Corresponding Member of the Academies of Sciences of Göttingen, Munich, Turin, Stockholm, Rome (Papal), Madrid, the Academia dei Lincei of Rome, and the Royal Society of London. In 1948 he became Honorary President of the International Union of Crystallographers, in 1952 he was made a Knight of the Order Pour le Mérite, in 1953 he received the Grand Cross with Star

for Federal Services, and in 1957 he became an Officer of the Legion of Honour of France.

Much esteemed by his contemporaries for his character and sound judgment, von Laue's opinions were often sought and during his life he exerted great influence on the direction and development of German scientific work. Among his characteristics were a deep love and admiration of Prussia and a strong sense of justice and fair play. When Hitler and the National Socialist Party were in power, he defended, even at the risk of reprimand or personal injury, scientific views, such as the theory of relativity, which were not approved by the Party or by such strong adherents to it as the physicist Lenard. When Einstein resigned from the Berlin Academy and the Vice-President of this Academy stated that this was no loss, von Laue was the only member of the Academy who protested.

Chief among his recreations were sailing, skiing, mountaineering and motoring. Von Laue was not a rock-climber, but preferred to tour the Alpine glaciers with his scientific friends. As a motorist he was famous in Berlin, first on the motor bicycle on which he went at high speed to his lectures; and later in a car. He loved high speeds, but never, until the fatal collision that ended his life, had any accident.

In his later years he suffered from attacks of depression and a feeling of being persecuted by scientists and by the military authorities, whom he disliked intensely. Usually, however, he successfully overcame these attacks and regained his sense of humour and joy in life. He did not practise any art, but he took an interest in many arts, especially in classical music; and he read widely history and the philosophy of science. He thought of the stars, the mountain peaks and the achievements of the human with awe and humility and was at heart a deeply religious man. He asked that his tombstone should bear the statement that he died trusting firmly in the mercy of God.

In 1910 von Laue married Magdalena Degen.

On April 8, 1960, when he was driving alone to his laboratory, a motor cyclist, who had only received his licence two days previously, collided with von Laue's car. The motor cyclist was instantly killed and von Laue's car overturned in the Berlin speedway and he was taken from beneath it by the Fire Brigade. Although he showed at first some signs of recovery from his injuries, he died of them on April 24, at the age of 80.

Physics 1915

WILLIAM HENRY BRAGG

WILLIAM LAWRENCE BRAGG

«for their services in the analysis of crystal structure by means of Röntgen rays»

Physics 1915

The following account of the work of the Braggs is by Professor G. Granqvist, Chairman of the Nobel Committee for Physics of the Royal Swedish Academy of Sciences*

Von Laue's epoch-making discovery of the diffraction of the X-rays in crystals, on the one hand established wave motion as the essential quality of those rays and, on the other, afforded the experimental proof of the existence of molecular gratings in the crystals. The problem, however, of calculating the crystal structures from von Laue's formulae was an exceedingly complicated one, inasmuch as not only the space lattices, but also the wavelengths and the intensity-distribution over the various wavelengths in the spectra of the X-rays, were unknown quantities. It was consequently a discovery of epoch-making significance when W. L. Bragg found out that the phenomenon could be treated mathematically as a reflection by the successive parallel planes that may be placed so as to pass through the lattice points, and that in this way the ratio between the wavelengths and the distances of the said planes from each other can be calculated by a simple formula from the angle of reflection.

It was only by means of that simplification of the mathematical method that it became possible to attack the problem of the crystal structures, but to attain the end in view it was further necessary that the photographic method employed by von Laue should be replaced by an experimental one, based on the reflection principle, which admitted of a definite, even though at first unknown, wavelength being made use of. The instrument requisite for the said purpose, the so-called X-ray spectrometer, was constructed by Professor W. H. Bragg, W. L. Bragg's father, and it has been with the aid of that instrument that father and son have carried out, in part conjointly, in part each on his own account, a series of extremely important investigations respecting the structure of crystals.

If a number of cubes are laid on and beside each other in such a way that one cube face coincides in every case with the face of an adjoining cube, whereby consequently eight vertices always meet in one point, those angular points give a visual picture of the lattice points in the so-called simple cubic

* Owing to war conditions Professor Bragg and his son were unable to attend the ceremony.

lattice. If again a lattice point is placed so as to coincide with the central point of each cube face, the so-called face-centred cubic lattice is obtained, whereas the centred cubic lattice has one lattice point in every cube-centre. With the exception of these three cases there is no cubic lattice that fulfils the condition that parallel planes placed in any direction whatever so as to pass through all the lattice points, shall also be at a constant distance from each other. The space lattice in the regular or cubic system must therefore coincide with one of those three, or constitute combinations of them. In such lattice combinations, on the other hand, in which the condition just mentioned is not fulfilled, where consequently parallel planes placed to pass through all the lattice points in certain directions are not equidistant, that circumstance is revealed by an abnormal intensity distribution among spectra of different orders, when the reflection takes place by those planes.

From crystallographical data it is always known how the face of a cube is situated in any given regular crystal, and there is consequently no difficulty in fixing the crystal on the spectrometer table in such a way that the reflection shall take place by planes with any prescribed orientation.

The rays falling on the crystal were produced by X-ray tubes, platinum being at first used for the anticathode. The characteristic X-radiation of the metals consists, as is well known, of a few strong lines or narrow bands, and the very first experiments with the spectrometer revealed the X-radiation that is characteristic of platinum. However, in the research undertaken to find out the nature of complicated space lattices, in which an abnormal intensity distribution among spectra of varying orders constitutes one of the most important of the results observed, it soon proved desirable to have available an X-radiation of approximately half the wavelength of the strongest platinum-line. From theoretical considerations W. H. Bragg regarded it as probable that a metal whose atomic weight was somewhere near the figure 100, would give a characteristic radiation of the desired wavelength. Accordingly anticathodes of palladium and rhodium were produced, which fully answered the purpose in view, so that spectra even of the fifth order could be obtained and measured. In order to take practical advantage, however, of those results, it was essential to have a method for calculating the intensity in the case of a complicated space lattice, that would prove simpler than the one given by von Laue's theory, and W. L. Bragg developed one.

The above is a brief sketch of the methods discovered by the two Braggs for investigating crystal structures. The results of their investigations em-

brace a large number of crystals belonging to various systems and can only be cursorily summarized in this place.

To begin with, the two investigators applied themselves to the simplest types of the regular system, represented by the alkaline haloid salts. It then proved that potassium bromide and potassium iodide showed the spectra that are characteristic of a face-centred cubic lattice, while the spectra of potassium chloride represented a simple cubic lattice, sodium chloride occupying an intermediate position. As it must be assumed, on the strength of the analogy of these salts, both in a chemical and a crystallographical sense, that they are possessed of a corresponding space lattice, which could also be corroborated in another way, it was proved by those researchers that the lattice of the crystals in question consists of two face-centred cubic lattices corresponding to the two atoms, which interpenetrate in such a way that they together constitute one single cubic lattice.

From these investigations it follows that a metal atom in the crystals of the alkaloid salts is situated at one and the same distance from the six haloid atoms nearest to it, and vice versa – a relationship that was found to prevail, *mutatis mutandis*, in all the crystals examined. That means the exceedingly important discovery, both for molecular physics and chemistry, that the crystals consist of atomic lattices and not, as has been always imagined, of molecular ones.

Two face-centred cubic lattices can also interpenetrate in such a way that every point belonging to the one lattice is at the centre of gravity of a tetrahedron whose vertices are points belonging to the other lattice. That structure was found by the two Braggs in the diamond, and afforded an experimental support for the tetrahedral arrangement that chemists postulate for the four-co-ordinate carbon. On the other hand, the explanation became evident of why crystallographers have not been able to agree regarding the class in the regular system to which the diamond should be referred.

It would carry us too far and be quite too complicated a proceeding to give an account here of the further investigations into the space lattices of the crystals. It will suffice to add that, in the course of their investigations, the two Braggs have also discovered important relations between the amplitude and the phase difference of the diffracted rays on the one hand and the atomic weights on the other, and have also shown experimentally the influence of heat on the space lattice.

Finally it may be mentioned that the two investigators have also determined the wavelengths of the X-rays and the distances between the succes-

sive planes placed to pass through the lattice points with such exactitude, that the error, if any, is probably at most some few units per cent and is more due to the general physical constant entering into the calculations than to the measurements themselves.

Thanks to the methods that the Braggs, father and son, have devised for investigating crystal structures, an entirely new world has been opened and has already in part been explored with marvellous exactitude. The significance of these methods, and of the results attained by their means, cannot as yet be gauged in its entirety, however imposing its dimensions already appear to be. In consideration of the great importance that these methods possess for research in the realm of physics, the Swedish Royal Academy of Sciences decided that the 1915 Nobel Prize in Physics should be divided between Professor W. H. Bragg and his son W. L. Bragg, in recognition of their services in promoting the investigation of crystal structures by means of X-rays.

No Lecture was delivered by Professor W. H. Bragg

Biography

William Henry Bragg was born at Westward, Cumberland, on July 2, 1862. He was educated at Market Harborough Grammar School and afterwards at King William's College, Isle of Man. Elected a minor scholar of Trinity College, Cambridge, in 1881, he studied mathematics under the well-known teacher, Dr. E. J. Routh. He was Third Wrangler in the Mathematical Tripos, Part I, in June 1884, and was placed in the first class in Part II in the following January. He studied physics in the Cavendish Laboratory during part of 1885, and at the end of that year was elected to the Professorship of Mathematics and Physics in the University of Adelaide, South Australia. Subsequently he became successively Cavendish Professor of Physics at Leeds (1909–1915), Quain Professor of Physics at University College, London (1915–1925), and Fullerian Professor of Chemistry in the Royal Institution.

His research interests embraced a great many topics and he was an adept at picking up a subject, almost casually making an important contribution, then dropping it again. However, the work of Bragg and his son Lawrence in 1913–1914 founded a new branch of science of the greatest importance and significance, the analysis of crystal structure by means of X-rays. If the fundamental discovery of the wave aspect of X-rays, as evidenced by their diffraction in crystals, was due to von Laue and his collaborators, it is equally true that the use of X-rays as an instrument for the systematic revelation of the way in which crystals are built was entirely due to the Braggs. This was recognized by the award of the Nobel Prize jointly to father and son in 1915.

During the First World War, Bragg was put in charge of research on the detection and measurement of underwater sounds in connection with the location of submarines. It was probably in acknowledgement of his work, as well as of his scientific eminence, that Bragg was made a C.B.E. in 1917 and was knighted in 1920. The Order of Merit followed in 1931. After having been a Fellow since 1907, he was elected President of the Royal Society in 1935.

He was an honorary doctor of some sixteen universities, and a member of the leading foreign societies. Many other medals and awards were bestowed

upon him among which may be mentioned the Rumford Medal in 1916 and the Copley Medal (its premier award) in 1930.

He was the author of many books, including *Studies in Radioactivity; X-Rays and Crystal Structure; The World of Sound; Concerning the Nature of Things; Old Trades and New Knowledge; An Introduction to Crystal Analysis;* and *The Universe of Light.* His favorite recreation was golf.

In 1889 he married Gwendoline Todd, daughter of Sir Charles Todd, F.R.S., Postmaster General and Government Astronomer of South Australia. Their son William Lawrence Bragg shared the Nobel Prize with his father.

After a life of astonishing productiveness, Sir William Bragg died on March 10, 1942.

WILLIAM LAWRENCE BRAGG

The diffraction of X-rays by crystals

*Nobel Lecture, September 6, 1922**

It is with the very greatest pleasure that I take this opportunity of expressing my gratitude to you for the great honour which you bestowed upon me, when you awarded my father and myself the Nobel Prize for Physics in the year 1915. In other years scientists have come here to express their thanks to you, who have received this great distinction for the work of an illustrious career devoted to research. That you should have given me, at the very outset of my scientific career, even the most humble place amongst their ranks, is an honour of which I cannot but be very proud.

You invited me here two years ago, after the end of the war, but a series of unfortunate circumstances made it impossible for me to accept your invitation. I have always profoundly regretted this, and it was therefore with the very greatest satisfaction that I received the invitation of Prof. Arrhenius a few months ago, and arranged for this visit. I am at last able to tell you how deeply grateful I am to you, and to give you my thanks in person.

You have already honoured with the Nobel Prize Prof. von Laue, to whom we owe the great discovery which has made possible all progress in a new realm of science, the study of the structure of matter by the diffraction of X-rays. Prof. von Laue, in his Nobel Lecture, has described to you how he was led to make his epochal discovery. In trying to think of some way in which diffraction effects with X-rays might be found, and the question of their true nature answered, he came to the realization that Nature had provided, in a crystal, a diffraction grating exactly suited for the purpose. It had already been surmised that the X-rays, if they were in truth electromagnetic waves, would be found to have a very short wavelength, of the order of 10^{-9} centimetre. The work of Sohnke, Fedorov, and others on the geometry of crystal structure had shown that the atoms or molecules in a crystal must be arranged in a geometrical pattern with perfect regularity, and it was possible to calculate that the spacing of the pattern must be of the order of 10^{-8}

* On November 12, 1915, Professor W.L. Bragg was awarded the Nobel Prize in Physics by the Royal Swedish Academy of Sciences. On September 6, 1922 he made the following speech in the hall of the Technical University of Stockholm.

centimetre. The relation between this spacing and the conjectured wavelength of the X-rays was precisely that required to give diffraction effects. Von Laue, in collaboration with Friedrich and Knipping, tried the experiment of passing a narrow beam of X-rays through a crystal, and they were immediately successful in obtaining undoubted evidence of regular diffraction. The lines of investigation which this discovery opened up have been so numerous and fruitful that Prof. von Laue could only give the briefest review of them in his address to you. When we consider the advances in our knowledge of the structure of matter which have been made by means of the von Laue effect, this discovery must surely be regarded as occupying a unique position in the history of science.

In saying something about the work for which we were awarded the Nobel Prize, I feel that I cannot but speak for both my father and myself. It was with his inspiration and under his guidance that any contributions of my own were made, and it was one of the proudest moments of my life when I heard that you had associated my name with his and awarded the prize to us jointly.

Prof. von Laue had made some of his earliest experiments with a crystal of zinc sulphide, and had obtained results which proved that the diffracted pencils showed the symmetry of the underlying crystal structure, which in this case was cubic. He developed a mathematical theory of diffraction by a space lattice, and proved that these diffracted pencils were in directions which were to be expected for a series of diffracting points arranged on a cubic space lattice. In pursuing the analysis still further, he tried to account for the fact that, whereas there were a large number of directions in which one would expect to find a diffracted beam, only a certain number of these appeared on the photographic plate used to record the effect. He suggested that this might be accounted for by the existence, in the X-ray beam, of certain wavelengths alone; and that a diffracted beam only appeared when conditions were right for diffracting these wavelengths. In studying his work, it occurred to me that perhaps we ought to look for the origin of this selection of certain directions of diffraction in the peculiarities of the crystal structure, and not in the constitution of the X-ray beam; this might be of the nature of white light and be composed of a continuous range of wavelengths. I tried to attack the problem from a slightly different point of view, and to see what would happen if a series of irregular pulses fell on diffracting points arranged on a regular space lattice. This led naturally to the consideration of the diffraction effects as a reflexion of the pulses by the planes of the crystal structure. The

points of a space lattice may be arranged in series of planes, parallel and equidistant from each other. As a pulse passes over each diffracting point, it scatters a wave, and if a number of points are arranged on a plane the diffracted wavelets will combine together to form a reflected wave front, according to the well-known Huygens construction.

The pulses reflected by successive planes build up a wave train, which analysis shows to be composed of the wavelengths given by the formula

$$n\lambda = 2d \sin \vartheta$$

In this expression, n is an integer, λ is the wavelength of the X-rays, d the spacing of the planes, and ϑ the glancing angle at which the X-rays are reflected.

To regard the diffraction as a reflexion of the X-rays involved no new principle that was not already contained in von Laue's mathematical treatment. It still left open the question why certain planes in the zinc blende structure appeared to reflect strongly, while others which were apparently equally well adapted to do so were not represented by diffracted pencils. In analysing von Laue's result, however, I found that the selection of the effective planes could be accounted for by assuming that the diffracting centres were arranged in a face-centred cubic lattice, and not on a simple cubic lattice. The structural unit of the face-centred cubic lattice is a cube with a point at each corner and at the centre of each face. When the planes of such a lattice are arranged in the order of those most densely packed with atoms, and so most effective for reflexion, this order is rather different to that for a simple cubic lattice. By assigning a face-centred cubic structure to zinc blende, it seemed possible to explain satisfactorily the von Laue photograph as due to the diffraction of white radiation with a maximum intensity in a certain part of the spectrum. I made a further test of two simple cubic crystals, sodium chloride and potassium chloride. While the von Laue photographs obtained with sodium chloride indicated a face-centred lattice, those obtained with potassium chloride were of a simpler nature, and were such as one would expect from an arrangement of points at the corners of cubes. Since it seemed probable that these two crystals had a similar structure, I was led to conjecture that the atoms were arranged as shown in Fig. 1, where every corner of the cube is occupied by an atom, whereas the atoms of one kind considered alone are arranged on a face-centred lattice. In potassium chloride the atoms are so nearly equal in their weight that they act as equivalent diffracting centres and the structure may be regarded as a simple cubic one.

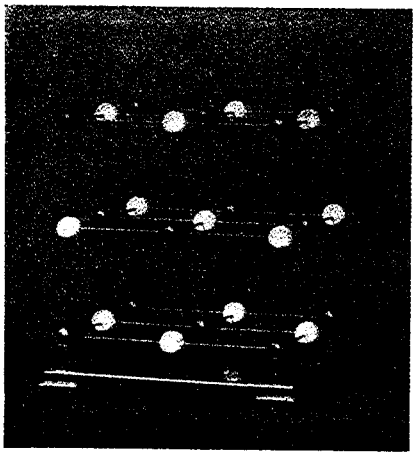


Fig. 1.

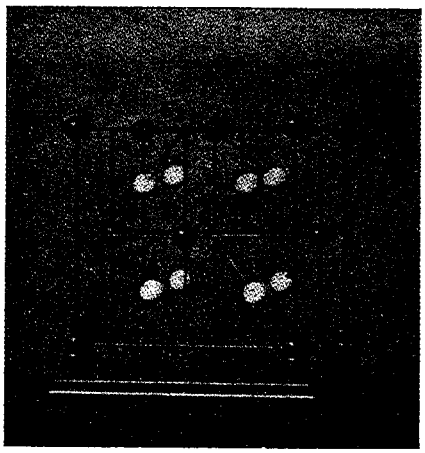


Fig. 2.

Fig. 1. *Rocksalt type (NaCl, KCl, LiF, etc.).* The *white balls* represent sodium (or chlorine); the *black balls*, chlorine (or sodium).

Fig. 2. *Fluorspar (CaF_2).* The *black balls* represent calcium; the *white balls*, fluorine.

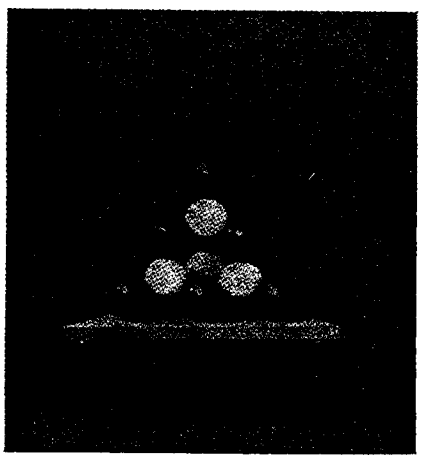


Fig. 3.

Fig. 3. *Diamond.* Both *black* and *white balls* represent carbon atoms. Each ball is at the centre of gravity of four others.

Although it seemed certain that the von Laue effect was due to the diffraction of very short waves, there remained the possibility that there might not be the X-rays. My father, in order to test this, examined whether the beam reflected by a crystal ionized a gas; this he found to be the case. He examined the strength of the reflexion at various angles, and the instrument which was first used for the purpose was developed later into the X-ray spectrometer with which we have done the greater part of our work. In this instrument the X-rays coming from a tube are limited to a narrow beam by slits, and fall on a crystal at the centre of the spectrometer table by which they are

reflected; the reflected beam is received and measured in an ionization chamber.

In examining the effect for varying angles of incidence my father discovered that a very strong reflexion appeared when a given crystal face was set at certain definite angles. Since the relationship which has been mentioned above must hold between glancing angle and wavelength, this constituted the first evidence of the existence of characteristic «lines» in the radiation given off by the anticathode. These same lines could be recognized in the reflexions from other faces, and the measurement of the angles at which they appeared proved a most powerful method of finding the arrangement of the atoms in the crystal. The structures of sodium chloride and potassium chloride, which had been suggested by the von Laue photographs, were established on a firm basis by means of the spectrometer, and more complex struc-

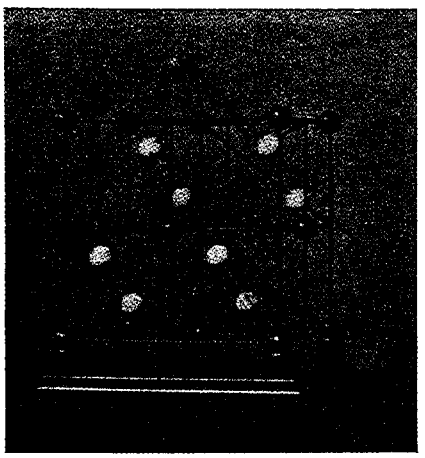


Fig. 4.

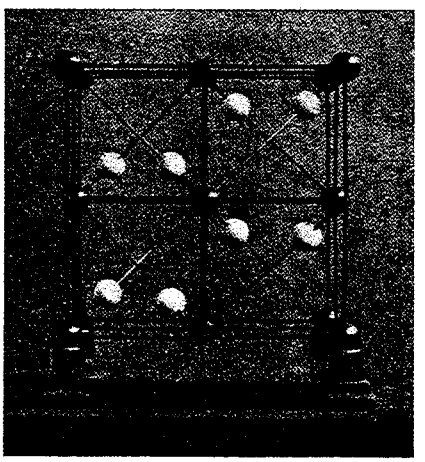


Fig. 5.

Fig. 4. *Iron pyrites (FeS_2).* The arrangement may be considered as a modification of CaF_2. The *black balls* are the iron atoms. Each *white ball*, now representing a sulphur atom is displaced from the centre of the cube as in CaF_2 along a diagonal: the diagonal is chosen in a special way, best grasped from the model. A trigonal axis passes through the two white balls which lie close together and near the centre of the whole figure.

Fig. 5. *Iron pyrites.* Viewed from a different angle, which shows the disposition of the sulphur atoms in horizontal lines. These are parallel to the well-known markings on the FeS_2 crystal. If the viewpoint is altered, as by turning the model through a right angle about a vertical axis, or an axis in the plane of the paper, the disposition of the sulphur atoms appears vertical instead of horizontal: just as the marking do.

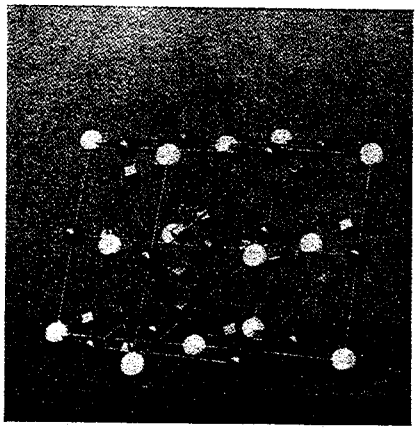


Fig. 6.

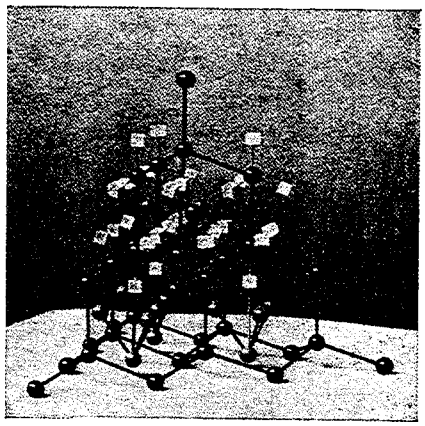


Fig. 7.

Fig. 6. *Calcite ($CaCO_3$).* The *white balls* represent calcium; the *black balls*, carbon; and the *cubes*, oxygen.

Fig. 7. *Spinel structure (spinel $MgAl_2O_4$; magnetite $FeFe_2O_4$; gahnite $ZnAl_2O_4$; etc.).* The divalent atoms (Mg, Fe, Zn, etc.) are placed with respect to each other in the same way as the carbon atoms in a diamond and are represented by the *black balls*. The *grey balls* represent the trivalent atoms (Al, Fe, etc.) A grey ball lies at the centre of every hexagonal (benzene) ring of black balls. The *light cubes* are the oxygen atoms. Every divalent atom touches four oxygen atoms and nothing else. Every trivalent atom touches six oxygen atoms and nothing else. Every oxygen atom touches one divalent and three trivalent atoms. The distance between the centres of two neighbouring divalent atoms in magnetite is 3.60 Å.

tures such as calcite, zinc blende, fluorspar, and iron pyrites, were analysed. On the other hand, since the arrangement of atoms in these crystals was known, it was possible to calculate the spacings of the reflecting planes and so to assign a definite wavelength to the characteristic lines in the spectrum. By using tubes with anticathodes of platinum, osmium, tungsten, nickel, and other metals, it became clear that each gave off a radiation containing characteristic lines which agreed in their properties with the K- or L-radiations first discovered by Barkla. Further, Whiddington had measured the energy of the cathode rays required to excite Barkla's K- and L-radiations. The results obtained with the spectrometer gave the frequency of these radiations, and my father was able to show that the product, of the frequency and Planck's constant h, was equal to the cathode-ray energy required to excite the rays; this was a first proof of the quantum law as applied to X-rays.

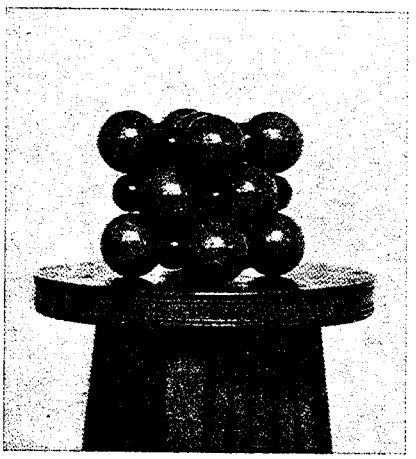


Fig. 8.

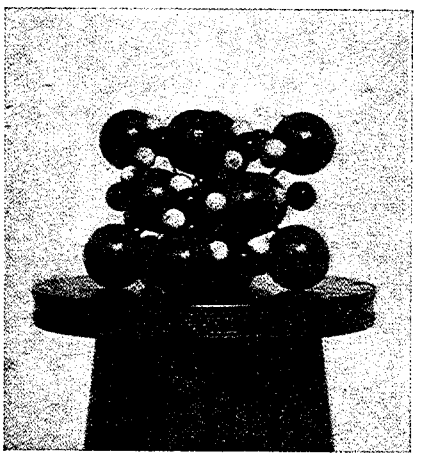


Fig. 9.

Fig. 8. *Rocksalt* (See also Fig. 1).
Fig. 9. *Calcite.* The *large dark ball* is the calcium atom; the *small dark ball*, carbon; and the *white ball*, oxygen.

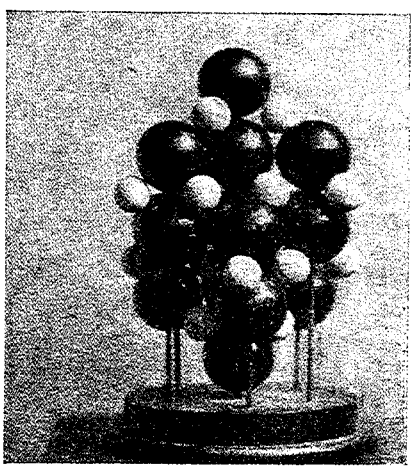


Fig. 10.

Fig. 10. *Corundum* (Al_2O_3). The type of symmetry is the same as that of calcite. The axis is vertical. The *dark balls* represent aluminium, the *white balls*, oxygen.

These two lines of investigation, into X-ray spectra and into crystal structure, are the two great branches of research to which von Laue's discovery led. Moseley was the first to make a survey of X-ray spectra. His famous researches established the law which governs the number and order of the elements. He found that, in passing from one element to the next in the Periodic Table, there was a regular progression of the frequency in the K- and L-radiations given out by the elements, and that its regularity was so great that it was possible to assign with absolute certainty each element to its proper place in the list, and to find the gaps where an element was not known. Moseley's death in the war cut short his brilliant career at its outset. Since

then the technique of X-ray spectrometry has been very highly developed. No one has done more in this line than Prof. Siegbahn at your University of Lund, who has attained such a marvellous accuracy and delicacy in his measurement of X-ray spectra. The establishment of the origin of the K-, L-, and M-radiations first indicated by Kossel's relationships, and the examination of the absorption edges, where the absorbing power of an element, for different wavelengths, passes through a critical value, have been of vital importance in the study of atomic structure since they furnish direct evidence as to the amount of energy-change, when an electron passes from one stationary state to another in the interior of the atom. The refinement to which wavelength measurement can now be pushed has even made it possible to discover that there are small differences in the position of these absorption edges, when different chemical compounds of an element are used as absorbers, as Dr. Lindh has shown, and this suggests a new and most powerful method of examining the chemical composition of substances and the potential energy of valency bonds.

The examination of crystal structure, with the aid of X-rays has given us for the first time an insight into the actual arrangement of the atoms in solid bodies. The study of structure by means of a microscope is limited by the coarseness of the light which illuminates the object, for we can never hope

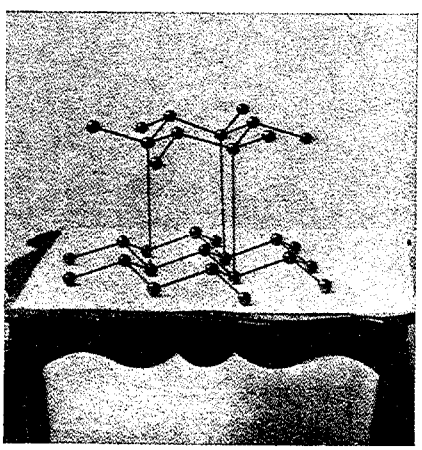


Fig. 11.

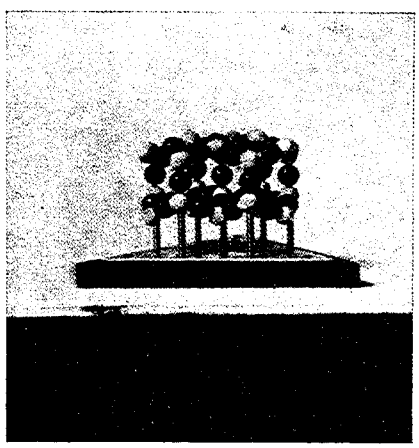


Fig. 12.

Fig. 11. *Graphite.*

Fig. 12. *Ice (H_2O).* The *white balls* represent the oxygen atoms, the *black balls* the hydrogen atoms. The oxygen atoms are arranged like those of carbon in diamond. Each hydrogen atom is situated midway between two oxygen atoms.

to see details smaller than the wavelength of the light. By using X-rays with their very short wavelengths, this limit of minuteness has at one step been decreased ten thousand times, for the wavelength of the X-rays is of a smaller order than the dimensions of the atomic structure. We are actually looking into the interior of the molecule and the atom with this fine-grained form of light.

The limits of the crystal pattern are arranged on a space lattice. Each limit is repeated over and over again in an identical fashion throughout the structure, at regular intervals in all directions. In analysing a crystal structure by X-rays, it is first necessary to find the dimensions of the unit cell of this space lattice, which has for its sides the primitive translation which the structure may be supposed to undergo in order so be brought into self-coincidence. It is a simple matter to achieve this. We find the angle at which a monochromatic beam of X-rays of known wavelength is reflected by the various faces of the crystal. Reflexion takes place only when the relation

$$n\lambda = 2d \sin \vartheta$$

is satisfied, and so the spacing d of the planes parallel to any face under examination can be found by measuring the angle ϑ. The dimensions of the unit cell can be found by measuring d for several faces. Having determined the dimensions of the unit cell, we can calculate how many atoms or molecules are contained within it, using as data the density of the crystal and the mass of each molecule.

The second step in analysis consists in the determination of the manner in which the atoms are grouped together to form each unit of the structure. It is here that the principal difficulty is experienced. The way in which the analysis is pursued may be illustrated by an analogy with the spectra obtained with a diffraction grating. It is well known that the form of the lines ruled on a grating has an influence on the relative intensity of the spectra which it yields. Some spectra may be enhanced, or reduced, in intensity as compared with others. Indeed, gratings are sometimes ruled in such a way that most of the energy is thrown into those spectra which it is most desirable to examine. The form of the line on the grating does not influence the positions of the spectra, which depend on the number of lines to the centimetre, but the individual lines scatter more light in some directions than others, and this enhances the spectra which lie in those directions.

The structure of the group of atoms which composes the unit of the crystal

grating influences the strength of the various reflexions in exactly the same way. The rays are diffracted by the electrons grouped around the centre of each atom. In some directions the atoms conspire to give a strong scattered beam, in others their effects almost annul each other by interference. The exact arrangement of the atoms is to be deduced by comparing the strength of the reflexions from different faces and in different orders.

In our early work we made certain assumptions in order to unravel the complex tangle which the reflexions from some crystals represented. We supposed that the diffraction took place at the centre of each atom, as if the whole effect were localized there. Further, we supposed that the effect of an atom was proportional to its atomic weight. Neither of these assumptions is more than very approximately true, as deeper analysis has shown. However, theory shows that the strength of the scattered beams varies so greatly with slight changes in the relative positions of the atoms, that these assumptions will serve to give an accurate determination of these positions. We analysed a number of simple crystals in this way, examples of which are given in the plates at the end of this address.

A single crystal is employed in the X-ray spectrometer. We used large crystals in our first experiments, reflecting from natural or artificially prepared faces. It is not essential to do this, however, for the ionization chamber is so sensitive a detector of X-radiation that small crystals a millimetre or two in breadth can be examined. In this case the rays are reflected throughout the whole volume of the crystal, which is not sufficiently thick to absorb them too much. A small crystal can be set at the centre of the instrument, adjusted so that a zone is parallel to the axis, and then turned round so that the reflexion of various faces is observed in turn. The X-ray spectrometer may be thus used in a manner very similar to that employed by a crystallographer when he measures a crystal with a goniometer. It is a more searching method, however. The goniometer can only measure the angles between faces which have been developed on the crystal, whereas the X-ray spectrometer measures the angles between planes of the crystal structure, and the spacings of the planes, although the crystal may have no regular external form.

In many cases it is impossible to obtain even small perfect crystals. The beautiful method of analysis initiated by Debye and Scherrer in Germany, and by Hull in America, has overcome this difficulty. In their method a monochromatic beam of X-rays is allowed to fall on a mass of crystalline powder. Some of the minute crystals will be set so as to reflect the rays for

every face and order, and so to give rise to a series of diffracted haloes which are recorded on a photographic plate. By means of this method, Hull has analysed the structure of a very large number of elements, including most of the metals. The method is admirably adapted to the study of compounds which do not form large crystals, but whose structure is very simple. By itself, it is not so well adapted to the analysis of complex crystals, since there is nothing to indicate which planes have given rise to the diffracted beams observed on the plate. Very often, however, a clue to the unravelling of the data can be got by examining the reflexion from an identified plane of a single small crystal; this gives us the key which enables us to assign the lines on the Debye photograph to the correct crystal planes.

The Debye method of crystal analysis has possibilities of development which have hardly been touched on as yet. A complex mixture of different crystals can be analysed, since each substance gives its characteristic series of haloes. This is an entirely new way of attacking such problems as the structure of alloys, or of minerals, since it tells us, not only what are the constituents, but how they are built up into solids; their crystallographic as well as their chemical structure.

A further development of the powder method which has proved to be most interesting is the study of colloidal particles. Scherrer has shown that particles of metals in the colloidal state consist of minute crystals, with a structure like that of large crystals; he has even formed an estimate of the size of the particles by observing the broadening of the haloes, this being due to a loss of resolving power owing to the small number of reflecting planes in any one particle.

There seems to be hardly any type of matter in the condition of a true solid which we cannot attempt to analyse by means of X-rays. For the first time the exact arrangement of the atoms in solids has become known; we can see how far the atoms are apart and how they are grouped. The importance of this consists in the light it throws on chemical composition. Very few crystals have yet been analysed by means of X-rays, and those that have been done are of simple composition, but as the knowledge of crystal structure increases we ought to get a far deeper insight into the nature of the forces which bind atoms together. One type of chemical binding has been already confirmed. In a structure such as that of sodium chloride (Fig. 1) the atoms of sodium and chlorine are not associated into chemical molecules, but are arranged alternately equidistant from several neighbours. This is explained by supposing that these atoms are ionized in the crystal structure, and that

the whole is held together by the attraction of oppositely charged ions. This is in accordance with the theories of Kossel on the constitution of compounds, and the work of Born and Landé has shown that we can make quantitative comparisons between the electrical potential energy of such an assemblage of ions, and the heat of formation of the solid from its elements. This is one of the first steps towards an explanation of chemical affinity in terms of familiar physical forces.

On the other hand, the chemical composition of a compound aids in the determination of its crystal structure. So many dispositions of the atoms are possible, in the more complex crystals, that one is at a loss where to begin in trying various arrangements in order to explain the X-ray diffraction effects. A knowledge of the most probable configuration of the atoms, and of the distances between them, is of the greatest assistance, however approximate it may be. This may perhaps be illustrated by the structures of the aromatic organic compounds, some of which my father has lately succeeded in analysing. The symmetry of the crystals of such substances as naphtalene, anthracene, and their derivatives is low, and there are a large number of atoms in the molecule, so that the analysis of their structures would be very difficult from first principles. However, it was assumed that the size of the benzene

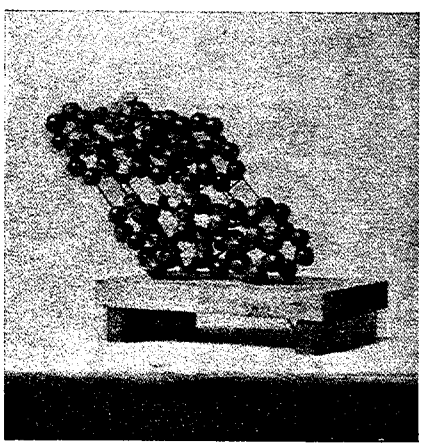


Fig. 13.

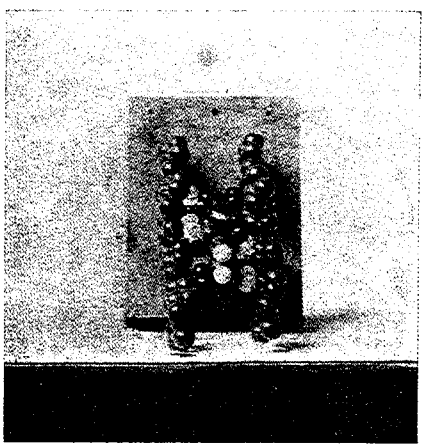


Fig. 14.

Fig. 13. *Naphthalene* ($C_{10}H_8$). The double rings of *grey balls* represent the double benzene rings of carbon atoms. The positions of the hydrogen atoms are indicated by the *black* and *white balls*.

Fig. 14. *Naphthalene* ($C_{10}H_8$). The same model as in Fig. 13, viewed perpendicularly to the C-axis.

rings in these crystals was approximately the same as that of the corresponding rings in diamond and graphite. With this assumption, the data on the size of the unit cell for all these compounds fell into relationship with each other. The benzene ring could be treated as a single unit, and space had to be found in the crystal structure for one or more to be packed into each unit cell. Probably the solutions of many other complex crystals, which appear almost hopeless at present, will be made along similar lines.

Finally, there is a still deeper application of the X-ray analysis, to the structure of the atom itself. Since the wavelength of the X-rays is less than the « atomic diameter », as it is somewhat vaguely termed, and since the rays are presumably diffracted by the electrons in the atom, we ought to be able to get some idea of the distribution of these electrons in the same way that we draw conclusions as to the grouping of the atoms. Interference between the waves scattered by the electrons must take place. In order to study this question, a much closer analysis of the mechanism of X-ray reflexion must be made. Many workers have attacked this problem, amongst them Darwin in England, Debye in Germany, Compton in America. We have been trying to make some accurate quantitative determinations of the strength of X-ray reflection, the object in view being the measurement of the amount of radiation scattered by an atom in all directions when an X-ray beam passes over it. The whole subject bristles with difficulties, and the assumptions on which the theory of reflexion rests have not been exhaustively proved. On the other hand there seems to be a prospect that the method will prove a valid way of testing various models proposed for the atom, since the way, in which these models should diffract the rays, can be calculated, and compared with that which is actually observed.

I have only given in this account a very brief description of the many lines of investigation which have arisen from von Laue's discovery. I wish that there were more contributions of my own to this subject which I could have described to you, to justify the supreme compliment which you paid me in giving me your award, but the war interrupted work for so many years, and since then there has been so much reorganization of all university work in England that research has been difficult to achieve. I will conclude, then, in expressing my thanks to you for giving me this opportunity of addressing you, and my gratitude for all which I owe to you.

Biography

William Lawrence Bragg, son of William Henry Bragg, was born in Adelaide, South Australia, on March 31, 1890. He received his early education at St. Peter's College in his birthplace, proceeding to Adelaide University to take his degree in mathematics with first-class honours in 1908. He came to England with his father in 1909 and entered Trinity College, Cambridge, as an Allen Scholar, taking first-class honours in the Natural Science Tripos in 1912. In the autumn of this year he commenced his examination of the von Laue phenomenon and published his first paper on the subject in the *Proceedings of the Cambridge Philosophical Society* in November.

In 1914 he was appointed as Fellow and Lecturer in Natural Sciences at Trinity College and the same year he was awarded the Barnard Medal. From 1912 to 1914 he had been working with his father, and the results of their work were published in an abridged form in *X-rays and Crystal Structure* (1915). It was this work which earned them jointly the Nobel Prize for Physics in 1915, and from this year to 1919 W. L. Bragg served as Technical Advisor on Sound Ranging to the Map Section, G.H.Q., France, receiving the O.B.E. and the M.C. in 1918. He was appointed Langworthy Professor of Physics at Manchester University in 1919, and held this post till 1937.

W. Lawrence Bragg, who had been elected Fellow of the Royal Society in 1921, was Director of the National Physical Laboratory in 1937–1938 and Cavendish Professor of Experimental Physics, Cambridge, from 1938 to 1953. He was Chairman of the Frequency Advisory Committee from 1958 to 1960.

Knighted in 1941, Sir Lawrence holds the degree of M.A. (Cambridge), Honorary D.Sc. (Dublin, Leeds, Manchester, Lisbon, Paris, Brussels, Liege, and Durham), honorary Ph.D. (Cologne), and honorary LL.D. (St. Andrews). He has many honorary fellowships and is an honorary or foreign member of American, French, Swedish, Chinese, Dutch, and Belgian Scientific Academies, besides being Membre d'Honneur de la Société Française de Minéralogie et Cristallographie.

He was awarded the Hughes Medal of the Royal Society in 1931; the

Royal Medal of the same Society in 1946, and the Roebling Medal of the Mineral Society of America in 1948.

Together with his father, he has published various scientific papers on crystal structure after their joint publication of 1915: *The Crystalline State* (1934), *Electricity* (1936), and *Atomic Structure of Minerals* (1937).

Sir Lawrence's chief interests at the present time are the application of X-ray analysis to the structure of protein molecules, which are being investigated in the Davy-Faraday Laboratory of the Royal Institution, in continuation of similar work at the Cavendish Laboratory, Cambridge. This collaboration has succeeded in determining for the first time the structure of the highly complex molecules of living matter.

Having been awarded the Nobel Prize at the very early age of 25, W. Lawrence Bragg was the youngest-ever laureate. The very rare opportunity of celebrating a golden jubilee as a Nobel Laureate was given special attention during the December ceremonies at Stockholm in 1965, when Sir Lawrence, at the invitation of the Nobel Foundation, delivered a lecture – the first Nobel Guest Lecture – in retrospect on developments in his field of interest during the last fifty years.

In 1921 he married Alice Grace Jenny (*née* Hopkinson) of Cambridge, and they have two sons (the elder of whom became chief scientist with Rolls Royce, while the younger entered a Cambridge instrument-making firm), and two daughters (the elder of whom married an official of the Foreign Office, while the second married the son of the Master of Corpus Christi College, Cambridge).

Physics 1916

Prize not awarded

Physics 1917

CHARLES GLOVER BARKLA

«for his discovery of the characteristic Röntgen radiation of the elements»

Physics 1917

The following account of Barkla's work is by Professor G. Granqvist, Chairman of the Nobel Committee for Physics of the Royal Swedish Academy of Sciences*

When X-rays fall on any substance, whether solid, liquid, or gaseous, they cause a secondary radiation. That was discovered by Sagnac in 1897 and it has since then been studied by a long series of investigators. Professor Barkla of Edinburgh, however, has carried out the most fundamental and exhaustive researches into the nature of this radiation, and in doing so he has discovered a new and unanticipated phenomenon, which has turned out to possess the utmost importance for physical investigation.

The secondary radiation consists of two varieties of radiation wholly different from each other. One is a corpuscular radiation of the same character as cathode rays and as the analogous β-rays of radioactive substances, being an emission of electrons. The other, on the contrary, is of the same character as X-rays.

Barkla has made a long series of very careful investigations into the nature of the latter of the two types of radiation. In the first place he discovered that there are two different kinds of X-rays in the secondary radiation. The absorption coefficients of one of these two varieties are the same as those of the incident X-rays. Thus the rays have the same penetrability as the primary rays, and, as they prove in other respects to have the same qualities as the primary rays, they must be regarded as a diffused primary radiation.

The intensity of this diffuse radiation varies in different directions in relation to that of the incident primary radiation. By measuring the distribution of intensity of the diffuse radiation, Barkla was able to determine the total emission of a series of substances under varying conditions. One very important result among others that his investigations led to, enabled Barkla at an early stage to estimate approximately the number of electrons contained in an atom.

The other variety of X-rays is wholly independent of the character of the incident radiation. Barkla showed that this radiation is homogeneous, that its absorption coefficient is not dependent on the incident radiation, but is determined by the irradiated substance. Further, he made the important dis-

* Owing to war conditions Professor Barkla was unable to attend the ceremony.

covery that the character of the rays is solely dependent on the qualities of the atoms constituting the substance, irrespective of their grouping and influence upon each other, that is to say, independent of the chemical composition of the substance. Every chemical element yields a secondary radiation that is characteristic of that element. Hence Barkla named this variety of radiation the characteristic X-radiation.

This variety of radiation may be most conveniently studied in elements of relatively high atomic weight, for in them it is stronger than the diffuse radiation. The characteristic radiation, however, being, in contrast to the diffuse radiation, perfectly homogeneous, can be distinguished from the latter, and thus Barkla could trace the characteristic radiation down to elements of the atomic weight of 27.

The characteristic radiation is furthermore, in contrast to the diffuse radiation, wholly unpolarized and is not dissymmetrical in respect to the direction of incidence of the primary radiation, but spreads uniformly in all directions.

As the characteristic radiation originates from X-rays, the emission of this radiation must be accompanied by an absorption of X-rays. For this reason Barkla made a very thorough investigation of the absorption of X-rays by various substances. The result was that all such factors as density, temperature, state of aggregation and chemical composition proved to be without essential importance. The qualities of the atoms alone determine the amount of the absorption. The absorption is furthermore selective, and, just as with regard to light, those rays are by preference absorbed that the substance emits at the same temperature, so it is here, the atoms absorbing by preference those X-rays that they themselves emit in the form of characteristic radiation.

Another striking resemblance between light and the X-rays has been discovered by Barkla. Just as, in agreement with Stokes' law, fluorescence can only be generated by light of a higher frequency, so too the characteristic radiation requires for its origination a greater penetrability of the primary rays.

Barkla's discovery that two domains of differing hardness are to be differentiated in the characteristic radiation, is of fundamental importance as regards the modern conception of the structure of atoms. Barkla has named the two domains the K-series and the L-series respectively. Thus every chemical element, when irradiated by X-rays, emits two rays of different penetrability, that is to say, every element can by fluorescence emit an X-ray spectrum of two lines or line-groups, the so-called K-series and L-series. Of these, the K-series have the greater penetrability. Barkla succeeded in fol-

lowing the K-series from calcium to cerium, the L-series from silver to bismuth.

If these rays are defined, for instance, in terms of their absorption in aluminium, the said absorption for rays belonging to one and the same series proves to be, approximately, a linear function of the atomic weight of the element that emits the radiation. By arriving at that conclusion Barkla had on the one hand furnished the most indisputable proof that every chemical element possesses a characteristic X-ray spectrum of its own, while on the other hand it was made manifest by these investigations that the said characteristic spectra, in contrast to all other spectra hitherto known, do not display any periodic qualities in connection with the places of the elements in the periodic system.

Barkla's discovery of the characteristic X-radiation has proved to be a phenomenon of extraordinary importance as regards physical research, a fact which has been made increasingly manifest by the subsequent researches of other investigators.

The discovery of the diffraction of X-rays in crystals gave a means of measuring their wavelengths, and since then the further investigation of the K-series and the L-series has yielded results of fundamental importance as regards our conception of the inner structure of atoms. It would, however, carry us too far to enter upon that topic here. Suffice it to say that those investigations have shown that it is the electrical charge of the nucleus of the atom that determines its place in the periodic system, not, as has been hitherto assumed, its atomic weight. The former is in general half the latter, but the divergences from the rule are due to irregularities in the distribution of the atomic weight amongst the elements, the charge of the nucleus being the factor that determines the chemical attributes of the atom. It has also been possible to establish the fact that of the chemical elements there are now not more than six that are unknown. Barkla's discovery of the characteristic X-radiation of the elements reveals a phenomenon of the utmost importance for the study of the inner structure of atoms and has already led to such far-reaching and significant conclusions, that it may with justice be asserted that no such important discovery in spectroscopy has been made since that of the discontinuous spectra from flames and electric sparks and the subsequent differentiation of those spectra in series, line and band spectra.

In consideration of this fact the Royal Swedish Academy of Sciences decided to award the 1917 Nobel Prize in Physics to Professor Barkla for his discovery of the characteristic X-radiation of the elements.

CHARLES G. BARKLA

Characteristic Röntgen radiation

Nobel Lecture, June 3, 1920

I have had some difficulty in deciding what to speak about to-day. Much of my work on characteristic radiation is already comparatively old and may be familiar to many of you, as it has been summarized in such works as the *Jahrbuch der Radioaktivität und Elektronik* and Professor Stark's *Atomdynamik*. I shall therefore choose two, and only two, points which are of more recent interest – firstly, the results of my own experiments which have a bearing on the Quantum Theory of Radiation; secondly, the evidence for another series of characteristic radiations – a *J* series*.

The quantum theory of radiation

Phenomena of scattering – The results of experiments on the scattering of X-rays, which have an obvious bearing on the Quantum Theory of Radiation, may be briefly summarized.

When X-rays traverse matter of any kind, this matter becomes a source of a radiation similar in character to that of the primary radiation falling upon it. A variation in the intensity of this scattered radiation with direction around the primary beam shows slight polarization of the primary radiation proceeding direct from an X-ray tube.

The scattered radiation proceeding in a direction at right angles to that of propagation of the primary radiation is highly polarized in the manner of light scattered from the sky.

The variation in intensity of the scattered radiation with direction relative to the axis of the primary radiation agrees within certain limits very closely with the theoretical distribution as given by the equation

$$I_o = I_{\pi/2}\,(1 + \cos^2\theta)$$

* References to the original papers by the author will be found in the Bakerian Lecture, 1916, published in *Philosophical Transactions of the Royal Society*, 1917.

an equation readily derived on the orthodox electromagnetic wave theory. These experimental results were looked for as necessarily following from such a theory.

Difficult as the first two results would be to explain on any entity or quantum theory, that is, on any theory assuming radiation itself to exist in definite indivisible bundles or quanta, perhaps the strongest evidence against this is provided by the experimental verification, within limits, of the above equation. The derivation of this equation depends essentially on a steady variation with direction, not of a number of indivisible entities, but of the energy density around a single radiating charge – an electron in this case.

Further, measurement of the energy of radiation scattered furnishes one of the most searching and critical of tests that could be applied to any theory. The writer early concluded that neither atoms, molecules nor gaseous ions were the scattering units, but that these were the constituent electrons in matter, and that in general the number of electrons per atom, for light atoms at any rate, was proportional to the atomic weight.* The fraction of a beam lost by scattering per centimetre of substance traversed was shown by J.J. Thomson to be given by the expression

$$f = \frac{8\pi}{3} N \frac{e^4}{m^2} \mu^2$$

where N is the number per cubic centimetre of particles of charge e and mass m. Applying this to the experimental data, it at first appeared that the number of electrons per atom was several times the atomic weight. As the data available for the values of N, e/m, and e were more accurately determined, the calculated value for the number of electrons per atom became smaller, until with the most recent values the number indicated is one electron per atom of hydrogen, 6 per atom of carbon, 7 per atom of nitrogen, 8 for oxygen, 15 or 16 for sulphur, and so on.

As these conclusions regarding the number of electrons (outer electrons) within the atom have been confirmed by the researches of Rutherford, Bohr and Moseley, it is perfectly legitimate to use the agreement as evidence in support of the theory of radiation upon which it was based. The chances of such an agreement being arrived at accidentally are almost infinitesimal, for, apart from the orthodox theory of electromagnetic radiation, the intensity of scattered radiation might have been anything between – say – one thou-

* Hydrogen excepted.

sandth part and a thousand times what was experimentally observed. Yet the value experimentally determined in 1904 agrees with the calculated value to a degree of accuracy as close as it is now possible to estimate the pressure of the air upon which the experiments were performed!

This confirmation is very remarkable. The theory on which it is based is the spreading wave theory. It assumes that the scattered radiation is the radiation resulting from the disturbance in electrons while under the influence of the electrostatic field in the primary radiation.

The theory assumes that radiation can take place from these electrons in any quantity whatever, and is not confined to units or quanta; that the radiation is a continuous process not depending on any limiting or critical condition.

Again, I have found that the intensity of the radiation scattered from light elements over considerable ranges varies little with the wavelength of the primary radiation. This is in perfect agreement with the theory based on the assumption of independent action of the electrons. Further experiments have shown that the intensity of scattered radiation from the heavy atoms in which the constituent electrons are more closely packed, increases rapidly and apparently continuously with the wavelength of the radiation, unless this is fairly small. Also, in general, the rate of increase of intensity with wavelength is greater, the heavier the atom from which the scattered radiation proceeds. Such results are, again to be expected on the wave theory when the wavelength becomes comparable with the size of the atom, for there is very close agreement in phase of the radiations set up by neighbouring electrons; ultimately a group of electrons and not an individual electron moves as a whole and becomes the scattering unit. (When the group comprises all the electrons in the atom, the scattering per atom becomes on this theory proportional to the atomic number squared, instead of to the atomic number, when the electrons scatter independently.)

There is thus in the phenomena of scattering, not only no suggestion of a quantum or entity of radiation, or of any discontinuity in the process of radiation involved, but there is some of the strongest positive evidence against any such theory. The results appear conclusive, for the tests which have been applied are the most searching and sensitive. The phenomena observed become meaningless on any quantum or entity theory. This conclusion is true also of absorption, for in the transmission of X-rays, particularly of short wavelength through matter consisting of light elements only, the energy absorbed is practically all re-emitted as scattered radiation. The quantities

radiated by each electron are approximately identical with those absorbed. It follows that *this* process of absorption is also a process which takes place in any quantity whatever, and is unlimited by any critical condition.

Characteristic radiation – Each element when traversed by X-rays emits X-radiations characteristic of the element; each characteristic radiation is unaffected by changes in the physical condition or state of chemical combination of the radiating element, and its quality is independent of that of the exciting primary radiation. But only primary radiations of shorter wavelength are able to excite the characteristic X-radiations – an extension of Stokes's fluorescence law.

All the radiations hitherto definitely observed have fallen into three series, the *K*, *L*, and *M* series (the *M* series was discovered by Siegbahn and his collaborators). There is also strong evidence of a higher frequency series – a *J* series. This will be discussed later. The absorption method of analysing a radiation showed the radiation of the *K* series from a particular element to be so homogeneous, that it was regarded as giving a spectral line, the *K* line; but the possibility of the *L* radiation consisting of more than one line was suggested by an obvious heterogeneity in the *L* radiation.

The interference experiments of Bragg, Moseley and others have shown, however, that both the *K* and *L* radiations give spectra consisting of a number of neighbouring lines.

The uniformity in the distribution of the characteristic radiation around the radiating substance, even when the primary beam is polarized, shows that, in contrast with the process of emission of scattered radiation, the emission of a characteristic radiation is absolutely uncontrolled by the primary radiation exciting it. The phenomena of emission is not an immediate consequence of the passage of the primary beam, but arises only indirectly from it; the process is dependent on some critical condition, as evidenced by Stokes' law. Here we see the possibility of the applicability of some kind of quantum theory. The most significant evidence as to the origin of the characteristic radiation comes from the study of the accompanying phenomena of the absorption of the exciting primary radiation and the emission of electrons by the radiating substance in the form of a corpuscular radiation. I have shown that the total absorption of a primary radiation in the substance traversed can be analysed into what are apparently independent absorptions, each – with the exception of that due to the process of scattering – definitely associated with the emission of a characteristic X-radiation. Thus there are

the *J*, *K*, *L*, *M* absorptions. Similarly, a corpuscular radiation may be analysed into *J*, *K*, *L*, *M* corpuscular radiations, each associated* with the emission of the corresponding characteristic X-radiation.

The results obtained from a study of the energy of the primary beam absorbed, of the energy of the characteristic radiation emitted, and of the corpuscular radiation emitted, are very significant. In certain substances – bromine and probably substances of high atomic weight – nearly all the energy of the primary beam absorbed in association with the emission of *K* characteristic radiation is re-emitted partly as characteristic X-radiation of the *K* series, and partly as corpuscular radiation of the *K* series. Not only this, but there is a definite relation between the intensity of the *K* radiation and the number of electrons emitted in the associated corpuscular radiation. For various wavelengths of the primary radiation it appears that the number of quanta of *K* fluorescent radiation per *K* electron emitted is approximately 1. The numbers actually obtained are 1.09, 0.95, 0.85, 0.81, 0.90. The maximum variation of 19% from unity is exceedingly small, considering the number and nature of the experimental determinations involved. Apart from the quantum theory, the range of possible values is so enormous, in comparison with the variation observed, that the emission of one quantum of characteristic X-radiation for each electron in the associated corpuscular radiation must be regarded as an experimentally established fact. If the two *K* secondary radiations, the corpuscular and the characteristic X-radiations, together accounted for the whole of the *K* energy absorbed, we should have of the energy of the primary beam absorbed (*K* absorption) the fraction $n/(n + n_k)$, re-emitted as *K* corpuscular radiation, and fraction $n_k/(n + n_k)$, re-emitted as *K* fluorescent X-radiation, where n and n_k are the frequencies of the primary and the characteristic radiation respectively. As a matter of fact, when n is slightly greater than n_k, the two secondary radiations together account for about 88% of the primary radiation. As n increases the two energies of fluorescent and corpuscular radiations remain approximately complementary, the energy of the former diminishing while that of the other increases. There is indeed very close agreement between the fractions observed and those given by the above expressions. It may be, however, that one quantum of *L* radiation, in addition to one quantum of *L* radiation, is emitted for each *K* electron ejected. There is even closer agreement between the observed and the calculated values, based on this assumption. Whatever the process of radiation may be, there can be little doubt that

* Probably in the case of *T* radiation.

characteristic radiation is emitted in quanta by those atoms merely from which an electron has been ejected.

The same energy relations, however, show us that absorption is *not* in quanta of primary radiation. Each absorbing atom, that is each atom which ultimately emits an electron, absorbs the energy of one quantum of primary radiation plus the energy of one quantum of characteristic radiation, which is $(1 + n_k/n)$ quanta of primary radiation. This may be anything from 1 to 2 quanta. All the evidence suggests that the characteristic radiation is emitted immediately after the ejection of the electron from the atom.

Summarizing, we may say that all the available evidence shows that X-radiation may be, and is, emitted by electrons, probably in certain cases by groups of electrons, or even atoms – as a continuous process and in any quantity whatever. It is frequently emitted in quantities almost infinitesimal in comparison with a quantum. It is, however, emitted in quanta from atoms, when certain critical conditions resulting in the ejection of certain electrons are reached, the process of radiation then taking place in a perfectly definite manner, involving the radiation of a definite amount of energy which is proportional to the frequency of vibration (Planck's Law: $E = hn$).

Absorption, too, normally takes place in very minute quantities – very small in comparison with a quantum. But in certain processes, which usually account for nearly the whole absorption, the radiation is absorbed in quantities greater than a quantum of the primary radiation, quantities varying with the conditions from one to two quanta approximately. There is no evidence of absorption of X-radiation in whole quanta, though the conditions are frequently such as to give an approximation to this.

All this evidence seems to indicate that a quantum of radiation in the sense in which it has frequently been used, i.e. as an indivisible bundle of radiation energy, does not exist. The process of radiation may be, and is, continuous – at any rate within limits extending to far smaller quantities than the quantum. The quantum is a unit of atomic energy which must be absorbed in order to change the configuration of the atom, and is radiated when that configuration returns to its original state. It thus of necessity appears in certain processes of absorption and radiation.

Probable J series of radiation

Recent investigations have led me to the conclusion that a characteristic radiation of higher frequency than the K radiation (forming a J series in the various elements) is probably emitted by each of the light elements. For experiments on (1) the absorption of X-rays, (2) the ionization of gases by X-rays, and (3) the intensity of corpuscular (electronic) radiation from plates exposed to X-rays, all show that a decrease in the wavelength of the primary X-radiation is accompanied by a sudden increase in the particular effect measured – an increase such as has invariably been found to accompany the emission of a characteristic X-radiation, and such as has hitherto not been observed except in association with such emission.

There is also close agreement between the values of the critical wavelengths found by the three methods.

Further, as with the phenomena accompanying K, L or M characteristic radiation, an increase in the atomic weight of the element is accompanied by a decrease in the critical wavelength in that element.

Direct evidence of the emission of J characteristic radiations has proved to be less easily obtained than appeared probable from preliminary experiments. Certain experimental results appeared to indicate that, though the J radiations might not be separable from the body of scattered radiation, their existence could readily be demonstrated. They have, however, proved more elusive than early experiments led us to believe. Experiments at present in progress are being gradually improved, and will, we hope, succeed in providing the direct evidence for which we are looking. It should, however, be pointed out that the evidence obtained from experiments on the K characteristic radiation would lead us to expect only a very weak characteristic radiation from these light elements, for of the energy of a primary beam absorbed in association with the emission of a particular K fluorescent characteristic radiation – i.e. K absorption – the fraction transformed into that characteristic X-radiation becomes rapidly smaller, as the atomic weight of the element decreases, so that even of the « K absorption » only a very small fraction appears as K characteristic radiation from these light elements. Thus the fraction K radiation (energy)/K absorption (energy) at its maximum value in copper is about 40%, in iron 30%, in chromium 20%. There is every indication of it becoming very small indeed in the lightest elements. In addition, the « J absorption » in these light elements is only a small fraction of the absorption by scattering – say of the order of magnitude of 15%.

Consequently, from aluminium, oxygen, nitrogen and carbon we might reasonably expect the energy of the fluorescent radiation to be something much less than 3 % – 15 % of 20 % – of the energy of the scattered radiation; how much less it would be impossible to predict, but the probability is however, that it is less than 1 %. We are, however, reasoning from our experience of « *K* radiations »: this might be regarded as unsafe ground if it did not find confirmation in our experimental results, obtained in investigations of *J* radiations.

Duane and others have been unable to detect any *J* characteristic radiation excited in aluminium when bombarded by cathode rays. This may mean either that the *J* characteristic radiation is very weak in comparison with the heterogeneous X-radiation produced in aluminium – for it must be remembered that by analogy with the other characteristic radiations, *J* radiation is produced only in association with the emission of *J* electrons, and these may be very few or difficult to displace – or it may just possibly indicate that *J* radiation cannot be excited at all or only in the most exceptional cases by the impact of cathode particles. This is not at all unlikely, if the *J* electrons are closely bound with other electrons, positive and negative, as they may be in a nucleus. For the cathode particle may come under the influence of this minute system as a whole and be deflected by it, without ever coming into such intimate relationship with any constituent electron as to disturb its stability. Thus it is quite conceivable that the negative and positive components of such a system are influenced separately by X-radiation, and that equilibrium is disturbed and *J* electrons are ejected and succeeded by the emission of *J* characteristic X-radiation; whereas an electron of the corpuscular radiation is unable to produce the necessary displacement of the *J* electron. Further experiments alone can decide this point.

One important feature of the discovery of a *J* series of characteristic X-radiations lies in the fact that there is no room for it in Bohr's theory of radiation as applied to *K* and *L* radiations. It may, however, be that a *J* radiation is emitted by electrons in a system of different kind, as for instance in the nucleus itself instead of outside it. But the association of other phenomena – an association similar to that observed with *K* and *L* characteristic radiations – suggests a close similarity in the whole process involved. Experiments on this subject are at present in progress.

Biography

Charles Glover Barkla was born on June 7, 1877 at Widnes, Lancashire, England, where his father, J. M. Barkla, was Secretary to the Atlas Chemical Company. He was educated at the Liverpool Institute and entered University College, Liverpool, in 1894 to study mathematics and physics, the latter under Oliver Lodge. He graduated with First Class Honours in Physics in 1898 and in the following year he obtained his master's degree. Also in 1899, he was awarded a research scholarship by the Royal Commissioners for the Exhibition of 1851 and he proceeded to Trinity College, Cambridge, to work in the Cavendish Laboratory with J. J. Thomson. He migrated to King's College during 1900 and in 1902 returned to Liverpool as Oliver Lodge Fellow. From 1905 to 1909 he was successively demonstrator, assistant lecturer in physics and special lecturer in advanced electricity at the University, and in 1909 he succeeded H. A. Wilson as Wheatstone Professor of Physics in the University of London. In 1913, Barkla accepted the Chair in Natural Philosophy in the University of Edinburgh and he held this position until his death.

Barkla's first researches concerned the velocity of electric waves along wires but in 1902 he commenced his investigations on Röntgen radiation which were to occupy almost his whole life. His discovery of homogeneous radiations characteristic of the elements showed that these elements had their characteristic line spectra in X-ray and he was the first to show that secondary emission is of two kinds, one consisting of X-rays scattered unchanged, and the other a fluorescent radiation peculiar to the particular substance. He discovered the polarisation of X-rays, an experimental result of considerable importance for it meant that X-radiation could be regarded as similar to ordinary light. Barkla made valuable contributions to present knowledge on the absorption and photographic action of X-rays and his later work demonstrated the relation between the characteristic X-radiation and the corpuscular radiation accompanying it. He has also shown both the applicability and the limitation of the quantum theory in relation to Röntgen radiation. The results of his findings are recorded in various papers which have ap-

peared mainly in the *Transactions* and *Proceedings of the Royal Society*. He had a considerable reputation as an examiner in physics.

Barkla, a Fellow of the Royal Society, had several honorary degrees. He was appointed Bakerian Lecturer (Royal Society) in 1916 and he was awarded the Hughes Medal in the following year.

Charles Glover Barkla married Mary Esther, the eldest daughter of John T. Cowell of Douglas, Receiver-General of the Isle of Man, in 1907. They had two sons and one daughter. Their youngest son, Flight Lieutenant Michael Barkla, a brilliant scholar, was killed in action in 1943. Barkla's chief recreation was singing – he had a powerful baritone voice and he was a member of the King's College Chapel Choir, 1901–1902. Latterly, he had also become fond of golf.

Barkla died at his home, Braidwood, Edinburgh, on October 23, 1944.

Physics 1918

MAX PLANCK

«in recognition of the services he rendered to the advancement of Physics by his discovery of energy quanta»

Physics 1918

Presentation Speech by Dr. Å. G. Ekstrand, President of the Royal Swedish Academy of Sciences

Ladies and Gentlemen.*

The Royal Academy of Sciences has decided to award the Nobel Prize for Physics, for the year 1918, to Geheimrat Dr. Max Planck, professor at Berlin University, for his work on the establishment and development of the theory of elementary quanta.

From the time that Kirchhoff enunciated the principle «that the intensity of radiation from a black body is dependent only upon the wavelength of the radiation and the temperature of the radiating body, a relationship worth while investigation», the theoretical treatment of the radiation problem has provided a rich, fertile source of fresh discoveries. It is only necessary here to recall the fertile Doppler principle, and further, the transformation of our concept of the nature of light as seen now in the electromagnetic theory of light formulated by that great man, Maxwell, the deduction of Stefan's Law by Boltzmann, and Wien's Law of Radiation. Since it was clear, however, that this did not correspond exactly with the reality, but was rather, like a radiation law propounded by Lord Rayleigh, only a special case of the general radiation law, Planck sought for, and in 1900 found, a mathematical formula for the latter, which he derived theoretically later on. The formula contained two constants; one, as was demonstrated, gave the number of molecules in a gram molecule of matter. Planck was also the first to succeed in getting, by means of the said relation, a highly accurate value for the number in question, the so-called Avogadro constant. The other constant, the so-called Planck constant, proved, as it turned out, to be of still greater significance, perhaps, than the first. The product $h\nu$, where ν is the frequency of vibration of a radiation, is actually the smallest amount of heat which can be radiated at the vibration frequency ν. This theoretical conclusion stands in very sharp opposition to our earlier concept of the radiation phenomenon. Experience had to provide powerful confirmation, therefore, before Planck's

* Owing to the sudden decease of the Royal Princess, no member of the Royal Family was present at the ceremony.

radiation theory could be accepted. In the meantime this theory has had unheard-of success. The specific heat of substances, Stokes' Law for phosphorescence and fluorescence phenomena and the photoelectric effect provide, as Einstein has first suggested, most powerful support for Planck's radiation theory as against the older, usual concept. A still greater triumph was enjoyed by Planck's theory in the field of spectral analysis, where Bohr's basic work, the work of Sommerfeld and Epstein, and other complementary efforts provided an explanation for the enigmatic laws ruling within this part of science. Recently, basic physico-chemical phenomena, such as the effect of temperature upon speed of reaction and the heat of reaction, have also had a new light shed upon them as a result of the work of W. C. McCullagh, Lewis, Perrin and others, using Planck's theory.

Planck's radiation theory is, in truth, the most significant lodestar for modern physical research, and it seems that it will be a long time before the treasures will be exhausted which have been unearthed as a result of Planck's genius.

Professor Planck. The Swedish Academy of Sciences has awarded you the Nobel Prize for 1918 in recognition of your epoch-making investigations into the quantum theory. This theory, which was originally connected with black-body radiation, has now demonstrated its validity for other fields and relationships of Nature, and the constant number, named after you, is a proportionality factor which describes a common, but until now unknown, property of matter. The Academy now begs you, Professor, to receive the prize from the President of the Nobel Foundation.

MAX PLANCK

The genesis and present state of development of the quantum theory

Nobel Lecture, June 2, 1920

If I take it correctly that the duty imposed upon me today is to give a public lecture on my writings, then I believe that this task, the importance of which I am well aware through the gratitude felt towards the noble-minded founder of our Foundation, cannot be more suitably fulfilled than by my trying to give you the story of the origin of the quantum theory in broad outlines and to couple with this, a picture in a small frame, of the development of this theory up to now, and its present-day significance for physics.

When I look back to the time, already twenty years ago, when the concept and magnitude of the physical quantum of action began, for the first time, to unfold from the mass of experimental facts, and again, to the long and ever tortuous path which led, finally, to its disclosure, the whole development seems to me to provide a fresh illustration of the long-since proved saying of Goethe's that man errs as long as he strives. And the whole strenuous intellectual work of an industrious research worker would appear, after all, in vain and hopeless, if he were not occasionally through some striking facts to find that he had, at the end of all his criss-cross journeys, at last accomplished at least one step which was conclusively nearer the truth. An indispensable hypothesis, even though still far from being a guarantee of success, is however the pursuit of a specific aim, whose lighted beacon, even by initial failures, is not betrayed.

For many years, such an aim for me was to find the solution to the problem of the distribution of energy in the normal spectrum of radiating heat. Since Gustav Kirchhoff has shown that the state of the heat radiation which takes place in a cavity bounded by any emitting and absorbing substances of uniform temperature is entirely independent upon the nature of the substances, a universal function was demonstrated which was dependent only upon temperature and wavelength, but in no way upon the properties of any substance. And the discovery of this remarkable function promised deeper insight into the connection between energy and temperature which is, in fact, the major problem in thermodynamics and thus in the whole of molecular physics. To

attain this there was no other way but to seek out from all the different substances existing in Nature one of known emissive and absorptive power, and to calculate the properties of the heat radiation in stationary energy exchange with it. According to Kirchhoff's Law, this would have to prove independent of the nature of the body.

Heinrich Hertz's linear oscillator, whose laws of emission, for a given frequency, Hertz had just previously completely developed, seemed to me to be a particularly suitable device for this purpose. If a number of such Hertzian oscillators are set up within a cavity surrounded by a sphere of reflecting walls, then by analogy with audio oscillators and resonators, energy will be exchanged between them by the output and absorption of electromagnetic waves, and finally stationary radiation corresponding to Kirchhoff's Law, the so-called black-body radiation, should be set up within the cavity. I was filled at that time with what would be thought today naively charming and agreeable expectations, that the laws of classical electrodynamics would, if approached in a sufficiently general manner with the avoidance of special hypotheses, be sufficient to enable us to grasp the most significant part of the process to be expected, and thus to achieve the desired aim. I, therefore, developed first the laws of emission and absorption of a linear resonator on the most general basis, in fact I proceeded on such a detour which could well have been avoided had I made use of the existing electron theory of H. A. Lorentz, already basically complete. But since I did not quite trust the electron hypothesis, I preferred to observe that energy which flowed in and out through an enclosing spherical surface around the resonator at a suitable distance from it. By this method, only processes in a pure vacuum came into account, but a knowledge of these was sufficient to draw the necessary conclusions however, about the energy changes in the resonator.

The fruit of this long series of investigations, of which some, by comparison with existing observations, mainly the vapour measurements by V. Bjerknes, were susceptible to checking, and were thereby confirmed, was the establishment of the general connection between the energy of a resonator of specific natural period of vibration and the energy radiation of the corresponding spectral region in the surrounding field under conditions of stationary energy exchange. The noteworthy result was found that this connection was in no way dependent upon the nature of the resonator, particularly its attenuation constants – a circumstance which I welcomed happily since the whole problem thus became simpler, for instead of the energy of radiation, the energy of the resonator could be taken and, thereby, a complex

system, composed of many degrees of freedom, could be replaced by a simple system of one degree of freedom.

Nevertheless, the result meant no more than a preparatory step towards the initial onslaught on the particular problem which now towered with all its fearsome height even steeper before me. The first attempt upon it went wrong, for my original secret hope that the radiation emitted from the resonator can be in some characteristic way or other distinguished from the absorbed radiation and thereby allow a differential equation to be set up, from the integration of which one could gain some special condition for the properties of stationary radiation, proved false. The resonator reacted only to those rays which it also emitted, and was not in the slightest bit sensitive to the adjacent spectral regions.

Furthermore, my hypothesis that the resonator could exercise a unilateral, i.e. irreversible, effect upon the energy in the surrounding radiation field, was strongly contested by Ludwig Boltzmann, who, with his riper experience in these problems, proved that according to the laws of classical dynamics each of the processes observed by me can proceed in exactly the opposite direction, in such a way, that a spherical wave emitted from the resonator, returns and contracts in steadily diminishing concentric spherical surfaces inwards to the resonator, and is again absorbed by it, thereby allowing the formerly absorbed energy to be re-transmitted into space in the direction from which it came. And when I excluded this kind of singular process, such as an inwardly directed wave, by means of the introduction of a limiting definition, the hypothesis of natural radiation, all these analyses still showed ever more clearly that an important connecting element or term, essential for the complete grasp of the core of the problem, must be missing.

So there was nothing left for me but to tackle the problem from the opposite side, that of thermodynamics, in which field I felt, moreover, more confident. In fact my earlier studies of the Second Law of Heat Theory stood me in good stead, so that from the start I tried to get a connection, not between the temperature but rather the entropy of the resonator and its energy, and in fact, not its entropy exactly but the second derivative with respect to the energy since this has a direct physical meaning for the irreversibility of the energy exchange between resonator and radiation. Since I was, however, at that time still too far oriented towards the phenomenological aspect to come to closer quarters with the connection between entropy and probability, I saw myself, at first, relying solely upon the existing results of experience. In the foreground of interest at that time, in 1899, was the energy

distribution law established by W. Wien shortly before, whose experimental proof was taken up, on the one hand, by F. Paschen at the Technische Hochschule in Hannover, and, on the other hand, by O. Lummer and E. Pringsheim at the State Institution in Charlottenburg. This law brought out the dependence of the radiation intensity on the temperature, representing it by an exponential function. If one calculates the connection between the entropy and the energy of a resonator, determined by the above law, the remarkable result is obtained that the reciprocal value of the above-mentioned differential coefficient, which I will call R, is proportional to the energy. This extremely simple relationship can be considered as the completely adequate expression of Wien's energy distribution law; for with the dependence upon the energy, the dependence upon the wavelength is always directly given through the general, well-established displacement law by Wien.

Since the whole problem concerned a universal law of Nature, and since at that time, as still today, I held the unshakeable opinion that the simpler the presentation of a particular law of Nature, the more general it is – though at the same time, which formula to take as the simpler, is a problem which cannot always be confidently and finally decided – I believed for a long time that the law that the quantity R is proportional to the energy, should be looked upon as the basis for the whole energy distribution law. This concept could not be maintained for long in the face of fresh measurements. Whilst for small values of the energy and for short waves, Wien's law was satisfactorily confirmed, noteworthy deviations for larger wavelengths were found, first by O. Lummer and E. Pringsheim, and finally by H. Rubens and F. Kurlbaum, whose measurements on the infrared residual rays of fluorite and rock salt revealed a totally different, though still extremely simple relationship, characterized by the fact that the quantity R is not proportional to the energy, but to the square of the energy, and in fact this holds with increasing accuracy for greater energies and wavelengths.

So, through direct experiment, two simple limits were determined for the function R: for small energies, proportionality with the energy; for greater energies, proportionality with the square of the energy. There was no better alternative but to make, for the general case, the quantity R equal to the sum of two terms, one of the first power, and one of the second power of the energy, so that for small energies the first is predominant, whilst for the greater energies the second is dominant. Thus the new radiation formula was found, which, in the face of its experimental proof, has stood firm to a reasonable

extent until now. Even today, admittedly, we cannot talk of final exact confirmation. In fact, a fresh attempt at proof is urgently required.

However, even if the radiation formula should prove itself to be absolutely accurate, it would still only have, within the significance of a happily chosen interpolation formula, a strictly limited value. For this reason, I busied myself, from then on, that is, from the day of its establishment, with the task of elucidating a true physical character for the formula, and this problem led me automatically to a consideration of the connection between entropy and probability, that is, Boltzmann's trend of ideas; until after some weeks of the most strenuous work of my life, light came into the darkness, and a new undreamed-of perspective opened up before me.

I must make a small intercalation at this point. According to Boltzmann, entropy is a measure for physical probability, and the nature and essence of the Second Law of Heat Theory is that in Nature a state occurs more frequently, the more probable it is. Now one always measures in Nature the difference in entropies, never the entropy itself, and to this extent one cannot speak of the absolute entropy of a state, without a certain arbitrariness. Nevertheless, it is useful to introduce the suitably defined absolute value of entropy, namely for the reason that with its help certain general laws can be particularly easily formulated. The case seems to be parallel, as I see it, with that of energy. Energy itself cannot be measured, only its difference. For that reason one used to deal, not with energy, but with work, and even Ernst Mach, who had so much to do with the Law of Conservation of Energy, and who in principle kept away from all speculations beyond the field of observation, has always avoided speaking of energy itself. Likewise, in thermochemistry, one has always stuck to the thermal effect, that is, to energy differences, until Wilhelm Ostwald in particular emphatically showed that many detailed considerations could be significantly abbreviated if one dealt with energy itself instead of with calorimetric numbers. The additive constant which was at first still undetermined in the expression for energy, has later been finally determined through the relativistic law of the proportionality between energy and inertia.

In a similar way to that for energy, an absolute value can be defined also for entropy and, as a result thereof, for the physical probability too, e.g. by so fixing the additive constant that energy and entropy disappear together. On the basis of a consideration of this kind a specific, relatively simple combinatorial method was obtained for the calculation of the physical probability of a specified energy distribution in a system of resonators, which led

exactly to that entropy expression determined by the radiation law, and it brought me much-valued satisfaction for the many disappointments when Ludwig Boltzmann, in the letter returning my essay, expressed his interest and basic agreement with the train of thoughts expounded in it.

For the numerical treatment of the indicated consideration of probability, knowledge of two universal constants is required, both of which have an independent physical meaning, and whose subsequent evaluation from the law of radiation must provide proof as to whether the whole method is to be looked upon as a mere artifice for calculation, or whether it has an inherent real physical sense and interpretation. The first constant is of a more formal nature and is connected with the definition of temperature. If temperature were to be defined as the average kinetic energy of a molecule in an ideal gas, that is, as a tiny, little quantity, then the constant would have the value $\frac{2}{3}$. In conventional temperature measure, on the contrary, the constant has an extremely small value which stands, naturally, in close association with the energy of a single molecule, and an exact knowledge of which leads, therefore, to the calculation of the mass of a molecule and those parameters related to it. This constant is often referred to as Boltzmann's constant, although, to my knowledge, Boltzmann himself never introduced it – a peculiar state of affairs, which can be explained by the fact that Boltzmann, as appears from his occasional utterances, never gave thought to the possibility of carrying out an exact measurement of the constant. Nothing can better illustrate the positive and hectic pace of progress which the art of experimenters has made over the past twenty years, than the fact that since that time, not only one, but a great number of methods have been discovered for measuring the mass of a molecule with practically the same accuracy as that attained for a planet.

At the time when I carried out the corresponding calculation from the radiation law, an exact proof of the number obtained was quite impossible, and not much more could be done than to determine the order of magnitude which was admissible. It was shortly afterward that E. Rutherford and H. Geiger succeeded in determining, by direct counting of the alpha particles, the value of the electrical elementary charge, which they found to be 4.65×10^{-10} electrostatic units; and the agreement of this figure with the number calculated by me, 4.69×10^{-10}, could be taken as decisive confirmation of the usefulness of my theory. Since then, more sophisticated methods have led to a slightly higher value, these measurements being carried out by E. Regener, R. A. Millikan, and others.

The explanation of the second universal constant of the radiation law was not so easy. Because it represents the product of energy and time (according to the first calculation it was 6.55×10^{-27} erg sec), I described it as the elementary quantum of action. Whilst it was completely indispensable for obtaining the correct expression for entropy – since only with its help could the magnitude of the «elementary regions» or «free rooms for action» of the probability, decisive for the assigned probability consideration, be determined – it proved elusive and resistant to all efforts to fit it into the framework of classical theory. As long as it was looked upon as infinitely small, that is, for large energies or long periods of time, everything went well; but in the general case, however, a gap yawned open in some place or other, which was the more striking, the weaker and faster the vibrations that were considered. The foundering of all efforts to bridge the chasm soon left little doubt. Either the quantum of action was a fictional quantity, then the whole deduction of the radiation law was in the main illusory and represented nothing more than an empty non-significant play on formulae, or the derivation of the radiation law was based on a sound physical conception. In this case the quantum of action must play a fundamental role in physics, and here was something entirely new, never before heard of, which seemed called upon to basically revise all our physical thinking, built as this was, since the establishment of the infinitesimal calculus by Leibniz and Newton, upon the acceptance of the continuity of all causative connections.

Experiment has decided for the second alternative. That the decision could be made so soon and so definitely was due not to the proving of the energy distribution law of heat radiation, still less to the special derivation of that law devised by me, but rather should it be attributed to the restless forward-thrusting work of those research workers who used the quantum of action to help them in their own investigations and experiments. The first impact in this field was made by A. Einstein who, on the one hand, pointed out that the introduction of the energy quanta, determined by the quantum of action, appeared suitable for obtaining a simple explanation for a series of noteworthy observations during the action of light, such as Stokes' Law, electron emission, and gas ionization, and, on the other hand, derived a formula for the specific heat of a solid body through the identification of the expression for the energy of a system of resonators with that of the energy of a solid body, and this formula expresses, more or less correctly, the changes in specific heat, particularly its reduction with falling temperature. The result was the emergence, in all directions, of a number of problems whose more accurate

and extensive elaboration in the course of time brought to light a mass of valuable material. I cannot give here even an approximate report on the abundance of the work carried out. Only the most important and characteristic steps along the path of progressive knowledge can be high-lighted here.

First come thermal and chemical processes. As far as the specific heat of solid bodies is concerned, Einstein's theory, which rested upon the assumption of a single natural vibration of the atom, was extended by M. Born and Th. von Kármán to the case of various kinds of natural vibrations, which approached more nearly to the truth. P. Debye succeeded, by means of a bold simplification of the stipulations for the character of natural vibrations, in producing a relatively simple formula for the specific heat of solid bodies which, particularly for low temperatures, not only satisfactorily reproduces the measurements obtained by W. Nernst and his pupils, but is also compatible with the elastic and optical properties of these substances. The quantum of action also comes to the fore in considering the specific heat of gases. W. Nernst had earlier suggested that to the quantum of energy of a vibration there must also correspond a quantum of energy of a rotation, and accordingly it was to be expected that the rotational energy of the gas molecules would disappear with falling temperature. The measurements by A. Eucken on the specific heat of hydrogen confirmed this conclusion, and if the calculations of A. Einstein and O. Stern, P. Ehrenfest and others have not until now afforded any completely satisfactory agreement, this lies understandably in our, as yet, incomplete knowledge of the model of a hydrogen molecule. The fact that the rotations of the gas molecules, as specified by quantum conditions, do really exist in Nature, can no longer be doubted in view of the work on absorption bands in the infrared by N. Bjerrum, E. von Bahr, H. Rubens, G. Hettner and others, even though it has not been possible to give an all-round exhaustive explanation of this remarkable rotation spectra up to now.

Since, ultimately, all affinity properties of a substance are determined by its entropy, the quantum-theoretical calculation of the entropy opens up the way to all the problems of chemical relationships. The Nernst chemical constant, which O. Sackur calculated directly through a combinatorial method as applied to oscillators, is characteristic for the absolute value of the entropy of a gas. H. Tetrode, in close association with the data to be obtained by measurement, determined the difference in entropy values between vapour and solid state by studying an evaporation process.

Whilst in the cases so far considered, states of thermodynamic equilibrium are concerned, for which therefore the measurements can only yield statis-

tically average values relating to many particles and lengthy periods of time, the observation of electron impacts leads directly to the dynamic details of the process under examination. Thus the determination of the so-called resonance potential carried out by J. Franck and G. Hertz, or that concerning the critical velocity is the minimum an electron must possess in order to cause emission of a light quantum or photon by impact with a neutral atom, supplied a method of measuring the quantum of action which was as direct as could be wished for. The experiments by D. L. Webster and E. Wagner and others resulted in the development of methods suitable for the Röntgen spectrum which also gave completely compatible results.

The production of photons by electron impact appears as the reverse process to that of electron emission through irradiation by light-, Röntgen-, or gamma-rays and again here, the energy quanta, determined by the quantum of action and by the vibration frequency, play a characteristic role, as could be recognized, already at an early time, from the striking fact that the velocity of the emitted electrons is not determined by the intensity of radiation, but only by the colour of the light incident upon the substance. Also from the quantitative aspect, Einstein's equations with respect to the light quantum have proved true in every way, as established by R. A. Millikan, in particular, by measurements of the escape velocity of emitted electrons, whilst the significance of the photon for the initiation of photochemical reactions was discovered by E. Warburg.

If the various experiments and experiences gathered together by me up to now, from the different fields of physics, provide impressive proof in favour of the existence of the quantum of action, the quantum hypothesis has, nevertheless, its greatest support from the establishment and development of the atom theory by Niels Bohr. For it fell to this theory to discover, in the quantum of action, the long-sought key to the entrance gate into the wonderland of spectroscopy, which since the discovery of spectral analysis had obstinately defied all efforts to breach it. And now that the way was opened, a sudden flood of new-won knowledge poured out over the whole field including the neighbouring fields in physics and chemistry. The first brilliant acquisition was the derivation of Balmer's series formula for hydrogen and helium including the reduction of the universal Rydberg constant to merely known numerical quantities, whereby even the small discrepancies for hydrogen and helium were recognized as essentially determined by the weak motion of the heavy atom nucleus. Investigation then turned to other series in the optical and the Röntgen spectrum using the extremely fruitful Ritz

combination principle, which was at last revealed clearly in all its fundamental significance.

Whoever, in view of the numerous agreements which in the case of the special accuracy of spectroscopic measurements could lay claim to particularly striking confirmatory power, might have been still inclined to feel that it was all attributable to the play of chance, would been forced, finally, to discard even his last doubt, as A. Sommerfeld showed that from a logical extension of the laws of quantum distribution in systems with several degrees of freedom, and out of consideration of the variability of the inertial mass in accordance with the relativity theory, that magic formula arose before which both the hydrogen and the helium spectrum had to reveal the riddle of their fine structure, to such an extent that the finest present-day measurements, those of F. Paschen, could be explained generally through it – an achievement fully comparable with that of the famous discovery of the planet Neptune whose existence and orbit was calculated by Leverrier before the human eye had seen it. Progressing further along the same path, P. Epstein succeeded in fully explaining the Stark effect of the electrical splitting up of the spectral lines, P. Debye produced a simple explanation of the K-series of the Röntgen spectrum, which had been investigated by Manne Siegbahn, and now followed a great number of further experiments, which illuminated with more or less success the dark secrets of the construction of the atom.

After all these results, towards whose complete establishment still many reputable names ought essentially to have been mentioned here, there is no other decision left for a critic who does not intend to resist the facts, than to award to the quantum of action, which by each different process in the colourful show of processes, has ever-again yielded the same result, namely, 6.52×10^{-27} erg sec, for its magnitude, full citizenship in the system of universal physical constants. It must certainly appear a unique coincidence that just in that time when the ideas of general relativity have broken through, and have led to fantastic results, Nature should have revealed an «absolute» in a place where it could be least expected, an invariable unit, in fact, by means of which the action quantity, contained in a space-time element, can be represented by a completely definite non-arbitrary number, and thereby divested itself of its (until now) relative character.

To be sure, the introduction of the quantum of action has not yet produced a genuine quantum theory. In fact, the path the research worker must yet tread to it is not less than that from the discovery of the velocity of light by Olaf Römer to the establishment of Maxwell's theory of light. The difficul-

ties which the introduction of the quantum of action into the well-tried classical theory has posed right from the start have already been mentioned by me. During the course of the years they have increased rather than diminished, and if, in the meantime, the impetuous forward-driving research has passed to the order of the day for some of these, temporarily, the gaps left behind, awaiting subsequent filling, react even harder upon the conscientious systematologist. What serves in Bohr's theory as a basis to build up the laws of action, is assembled out of specific hypotheses which, up to a generation ago, would undoubtedly have been flatly rejected altogether by every physicist. The fact that in the atom, certain quite definite quantum-selected orbits play a special role, might be taken still as acceptable, less easily however, that the electrons, circulating in these orbits with definite acceleration, radiate no energy at all. The fact that the quite sharply defined frequency of an emitted photon should be different from the frequency of the emitting electron must seem to a theoretical physicist, brought up in the classical school, at first sight to be a monstrous and, for the purpose of a mental picture, a practically intolerable demand.

But numbers decide, and the result is that the roles, compared with earlier times, have gradually changed. What initially was a problem of fitting a new and strange element, with more or less gentle pressure, into what was generally regarded as a fixed frame has become a question of coping with an intruder who, after appropriating an assured place, has gone over to the offensive; and today it has become obvious that the old framework must somehow or other be burst asunder. It is merely a question of where and to what degree. If one may make a conjecture about the expected escape from this tight corner, then one could remark that all the signs suggest that the main principles of thermodynamics from the classical theory will not only rule unchallenged but will more probably become correspondingly extended. What the armchair experiments meant for the foundation of classical thermodynamics, the adiabatic hypothesis of P. Ehrenfest means, provisionally, to the quantum theory; and in the same way as R. Clausius, as a starting point for the measurement of entropy, introduced the principle that, when treated appropriately, any two states of a material system can, by a reversible process, undergo a transition from one to the other, now the new ideas of Bohr's open up a very similar path into the interior of a wonderland hitherto hidden from him.

There is in particular one problem whose exhaustive solution could provide considerable elucidation. What becomes of the energy of a photon after

complete emission? Does it spread out in all directions with further propagation in the sense of Huygens' wave theory, so constantly taking up more space, in boundless progressive attenuation? Or does it fly out like a projectile in one direction in the sense of Newton's emanation theory? In the first case, the quantum would no longer be in the position to concentrate energy upon a single point in space in such a way as to release an electron from its atomic bond, and in the second case, the main triumph of the Maxwell theory – the continuity between the static and the dynamic fields and, with it, the complete understanding we have enjoyed, until now, of the fully investigated interference phenomena – would have to be sacrificed, both being very unhappy consequences for today's theoreticians.

Be that as it may, in any case no doubt can arise that science will master the dilemma, serious as it is, and that which appears today so unsatisfactory will in fact eventually, seen from a higher vantage point, be distinguished by its special harmony and simplicity. Until this aim is achieved, the problem of the quantum of action will not cease to inspire research and fructify it, and the greater the difficulties which oppose its solution, the more significant it finally will show itself to be for the broadening and deepening of our whole knowledge in physics.

Biography

Max Karl Ernst Ludwig Planck was born in Kiel, Germany, on April 23, 1858, the son of Julius Wilhelm and Emma (*née* Patzig) Planck. His father was Professor of Constitutional Law in the University of Kiel, and later in Göttingen.

Planck studied at the Universities of Munich and Berlin, where his teachers included Kirchhoff and Helmholtz, and received his doctorate of philosophy at Munich in 1879. He was Privatdozent in Munich from 1880 to 1885, then Associate Professor of Theoretical Physics at Kiel until 1889, in which year he succeeded Kirchhoff as Professor at Berlin University, where he remained until his retirement in 1926. Afterwards he became President of the Kaiser Wilhelm Society for the Promotion of Science, a post he held until 1937. The Prussian Academy of Sciences appointed him a member in 1894 and Permanent Secretary in 1912.

Planck's earliest work was on the subject of thermodynamics, an interest he acquired from his studies under Kirchhoff, whom he greatly admired, and very considerably from reading R. Clausius' publications. He published papers on entropy, on thermo-electricity and on the theory of dilute solutions.

At the same time also the problems of radiation processes engaged his attention and he showed that these were to be considered as electromagnetic in nature. From these studies he was led to the problem of the distribution of energy in the spectrum of full radiation. Experimental observations on the wavelength distribution of the energy emitted by a black body as a function of temperature were at variance with the predictions of classical physics. Planck was able to deduce the relationship between the energy and the frequency of radiation. In a paper published in 1900, he announced his derivation of the relationship: this was based on the revolutionary idea that the energy emitted by a resonator could only take on discrete values or quanta. The energy for a resonator of frequency ν is $h\nu$ where h is a universal constant, now called Planck's constant.

This was not only Planck's most important work but also marked a turning point in the history of physics. The importance of the discovery, with

its far-reaching effect on classical physics, was not appreciated at first. However the evidence for its validity gradually became overwhelming as its application accounted for many discrepancies between observed phenomena and classical theory. Among these applications and developments may be mentioned Einstein's explanation of the photoelectric effect.

Planck's work on the quantum theory, as it came to be known, was published in the *Annalen der Physik*. His work is summarized in two books *Thermodynamik* (Thermodynamics) (1897) and *Theorie der Wärmestrahlung* (Theory of heat radiation) (1906).

He was elected to Foreign Membership of the Royal Society in 1926, being awarded the Society's Copley Medal in 1928.

Planck faced a troubled and tragic period in his life during the period of the Nazi government in Germany, when he felt it his duty to remain in his country but was openly opposed to some of the Government's policies, particularly as regards the persecution of the Jews. In the last weeks of the war he suffered great hardship after his home was destroyed by bombing.

He was revered by his colleagues not only for the importance of his discoveries but for his great personal qualities. He was also a gifted pianist and is said to have at one time considered music as a career.

Planck was twice married. Upon his appointment, in 1885, to Associate Professor in his native town Kiel he married a friend of his childhood, Marie Merck, who died in 1909. He remarried her cousin Marga von Hösslin. Three of his children died young, leaving him with two sons.

He suffered a personal tragedy when one of them was executed for his part in an unsuccessful attempt to assassinate Hitler in 1944.

He died at Göttingen on October 3, 1947.

Physics 1919

JOHANNES STARK

«for his discovery of the Doppler effect in canal rays and the splitting of spectral lines in electric fields»

Physics 1919

Presentation Speech by Dr. Å. G. Ekstrand, President of the Royal Swedish Academy of Sciences

Ladies and Gentlemen.*

The Royal Academy of Sciences has decided to award the Nobel Prize in Physics for 1919 to Dr. Johannes Stark, professor in the University of Greifswald, for his discovery of the Doppler effect in canal rays and of the splitting of spectral lines in electric fields.

It is only rarely that the study of a physical phenomenon has led to such a brilliant series of important discoveries as that which follows the conducting of an electrical current through a rarefied gas. As long ago as 1869 Hittorf discovered that if a low pressure is set up in a discharge tube, rays are emitted from the negative electrode, the so-called cathode. Although invisible to the eye, they can nevertheless be observed through certain effects peculiar to them. The continued study of these cathode rays, in which Lenard in particular earned great merit, showed that they are composed of a stream of negatively charged particles, the mass of which amounts only to 1/1,800 of the mass of the hydrogen atom. We call these minute particles electrons, and gradually one of the principal theories of modern physics grew from the study of the properties of electrons and of their relationship with matter. The electron theory with its concept of the constitution of matter has become of radical importance to both physics and chemistry.

When cathode rays strike an object, this becomes the source of a new radiation, namely that discovered by Röntgen in 1895 and named by him X-rays, the study of which has led to so many important results for major branches of science, not only within physics. Through von Laue's discovery of the diffraction of X-rays in crystals it was demonstrated that these rays are light waves of very short wavelength. It is now even possible to photograph the spectra of these rays, and science has by this been enriched with a means of research the implications of which cannot yet be fully realized.

Von Laue's discovery also occasioned important discoveries in the field of crystallography. It is possible, now that W. H. Bragg and his son have worked

* Owing to the sudden decease of the Royal Princess, no member of the Royal Family was present at the ceremony.

out theoretic and experimental methods for that purpose, to determine the positions of the atoms in crystals. By these methods a whole new world has been opened up, and has already been partly explored.

Of not less importance was Barkla's discovery in the year 1906 that every chemical element when irradiated with X-rays emits an X-ray spectrum, characteristic of the element in question. This discovery has become of outstanding importance for the theoretical study of the structure of the atom.

In the year 1886 Goldstein discovered a new kind of rays in discharge tubes containing rarefied gas, the study of which has become extremely important to our knowledge of the physical properties of atoms and molecules. In view of the manner of their formation Goldstein called them canal rays. It was proved by the research of W. Wien and J. J. Thomson that the majority of these are composed of positively charged atoms of the gas in the discharge tube, which move along the beam at a very high velocity.

In their course along the beam these canal-ray particles are continually colliding with the gas molecules which are contained in the tube, and thus it may be expected that light is emitted, if the kinetic energy is sufficiently great. As long ago as 1902 Stark predicted that the moving canal-ray particles thus become luminous, and that consequently the lines in the spectrum emitted by them must be displaced to the violet end of the spectrum if the rays are sighted approaching the observer. This takes place in the same way as the displacement of the lines in the spectra of those stars which are moving towards us, and as this displacement, the so-called Doppler effect, increases with the velocity of the light source, it must thus also be possible to determine the velocity of the canal-ray particles.

In 1905 Stark succeeded for the first time in detecting this phenomenon in a canal-ray tube containing hydrogen.

Beside each of the single hydrogen lines belonging to the familiar, so-called Balmer series, a new, broader line appeared, which lay beside the original line, on the violet side of the spectrum if the canal rays were observed approaching the observer, but on the red side of the spectrum if observed from behind. The effect mentioned here has been established for the canal rays of all chemical elements which, in addition to hydrogen, have been investigated in this respect.

This discovery, by which a Doppler effect was recorded for the first time in the case of a terrestrial light source, was instrumental in the proof that canal-ray particles are luminous atoms, or atomic ions. The further study of the Doppler effect in their spectra, which has been pursued principally by

Stark and his pupils, has led to extremely important results, not only concerning the canal rays themselves, their formation, etc., but also concerning the nature of the different spectra which one and the same chemical element can emit in different circumstances.

In the course of an investigation of canal rays in a tube containing hydrogen gas, which passed through a strong electric field, Stark observed, in 1913, a broadening of the lines in the spectrum of the hydrogen. A more thorough examination of this broadening showed that the lines decomposed into several components with characteristic polarization conditions. Although this splitting can best be observed in canal rays, it has nevertheless nothing to do with the movement of the atoms, but depends solely on the fact that these are present in an extremely strong electric field.

In this, a discovery was made analogous to Zeeman's discovery of the splitting of serial lines by means of an extremely strong magnetic field, which was also in its time crowned with the Nobel Prize by this Academy.

This splitting of lines in electric fields has been detected and measured by Stark in the line spectrum not only of hydrogen, but also of that of a great number of other substances, and the result of these investigations was that (the effect named after him turned out to be in several respects quite different from the Zeeman effect, and that thus) the optical dynamics of the atoms alters, under the influence of an *electric* field, in a manner quite different from that under the influence of a *magnetic* field.

The effect discovered by Stark has become extraordinarily significant for modern research into the structure of atoms, and has opened up new fields for the study of the effect of atomic ions on each other and on molecules. The extremely complicated conditions which this effect manifests in the spectral series of hydrogen and of helium were successfully explained by a theory which forms one of the strongest pillars on which the modern concept of the internal structure of the atom rests.

In view of the great significance which Stark's work so obviously has for physical research within various fields of great importance, the Royal Academy of Sciences considers it well warranted that the Nobel Prize in Physics for 1919 should be bestowed on this scientist.

Professor Stark. Our Academy of Sciences has awarded you the Nobel Prize in Physics for 1919 in recognition of your epoch-making research into the so-called Doppler effect in canal rays, which has given us an insight into the reality of the internal structure of atoms and molecules. The Nobel Prize

relates also to your discovery of the splitting of spectral lines in electric fields – a discovery which is of the greatest scientific importance.

I ask you now, Professor, to receive the Nobel Prize from the President of the Nobel Foundation.

JOHANNES STARK

Structural and spectral changes of chemical atoms

Nobel Lecture, June 3, 1920

The question of the composition of perceptible objects is one which already occupied the mind of the ancient Greeks. They formed the concept, as a philosophical speculation, of the indivisible particle, the atom, as the smallest component of perceptible objects. However, they did not pass beyond this stage of the hypothesis; they did not bring it to productivity through experimental research.

It is otherwise with the mind of the Germanic peoples. They proceeded from the experience of their chemical dealings with matter, established the existence of a number of basic substances, or chemical elements, which could not be decomposed further, and proposed the hypothesis that a chemical element consists of homogeneous individuals, or atoms, which are responsible for the peculiar properties of the element, and which with the aid of chemical methods can neither be broken down further nor distinguished from one another.

Towards the end of the last century a certain torpidity fell upon this concept of the chemical atom. Its verification in thousands of chemical experiments led to the belief that the chemical atom not only could not be decomposed into further parts by familiar chemical methods, but that it was completely and absolutely indivisible. Moreover, the abundance of chemical compounds and their importance in daily life hindered the chemist from investigating the question, in what does the individuality of the atoms of different elements consist. In the last three decades the concept of the chemical atom has been set free from this torpidity by our experiences in physics. The discovery of various phenomena has led to a recognition of the fact that the chemical atom is an individual which again is itself made up of several units into a selfcontained whole.

At the head of these new discoveries and insights comes the establishment of the facts that electricity is composed of discrete particles of equal size, or quanta, and that light is an electromagnetic wave motion. It followed necessarily from this that single separate electric quanta must be present in the

composition of a chemical atom. For under certain conditions the chemical atoms emit light waves of a specific length or oscillation frequency – their familiar characteristic spectra – and these can come in the form of electromagnetic waves only from accelerated electric quanta.

Moreover, the discovery that the negative electron is a component of the chemical atom is of fundamental importance. In cathode rays the physicist became acquainted with free negative electric quanta capable of independent movement, the mass of which is smaller than a thousandth of the mass of the hydrogen atom. In the process of ionization he saw the liberation of such negative quanta or electrons from the chemical atoms. By exerting the influence of a magnetic field on the spectral lines of chemical elements, Zeeman and Lorentz even succeeded in detecting the negative electron in its place in, and as a component of, the atomic whole.

Furthermore, in these last three decades of great physical discoveries, as never before in history, Nature has drawn back the veil from a third undreamt-of secret before the eyes of the physicist. The discovery and investigation of radioactivity has made clear even for the most sceptical not only the separability of the parts of an atom, but also the chemical and physical individuality of a chemical atom – particularly of a parent atom, but equally of the atoms arising from its decay.

By recognizing that the chemical atom is composed of single separable electric quanta, humanity has taken a great step forward in the investigation of the natural world. However, this advance has faced us with a new, even greater problem – that of the structure of the atom. How many electric quanta are present in the atom of a chemical element? What are their fields of force? What are their mutual distances? What are their movements? What forces are roused on them if their state of equilibrium is disturbed by external interference?

We have been faced with these questions for a decade and a half. It is improbable that speculations will succeed in providing the answer to all these questions at one stroke, by one bold vision. It is more likely that more than a century will pass before we know the structure of the chemical atoms as thoroughly as we do our solar system. The path to this goal will lead, as it has so far, through the difficulties and surprises of experimental research. Many scientists will have to contribute to the solution of the great problem; they will have to follow up and measure all those phenomena in which the atomic structure is directly expressed.

With this in mind, for some twenty years I have set myself as my partic-

ular task the experimental investigation of the connexion between change in the structure and change in the spectra of chemical atoms. First of all, two questions may be posed in this connexion.

The first of them is tied up with the phenomenon of change in the structure of the surface of the atom. In order that we may have clearly set out before us every possibility in this respect, we shall proceed from the single atom, the parts of which are all arranged according to mutual equilibrium. We have learnt through experience that when an electrical ray strikes the surface of an atom, an electron, and in some circumstances a second and even a third electron, can be detached. In place of the structure of the neutral atom we are left with the structure of the corresponding monovalent, divalent, or trivalent atomic ion. We ask ourselves: what are the two spectra which belong to the two atomic structures – to neutral atoms and to positive atomic ions? And this question may be supplemented by the second question: is a specific spectrum emitted if the positive atomic ion is changed into a neutral atom?

To these questions I had given certain answers, at first in the form of working hypotheses, after a tentative examination of all previous observations known to me, in order to be able to think out a specific series of experiments to test the hypotheses.

The answer to the first of these questions was as follows: the spectral series of a chemical element are peculiar to the structure of the positive atomic ions, and are observed principally in arcs and sparks, and their lines, as Rydberg showed most successfully, can be grouped together, the lines of each group being a function of the integers.

The second hypothesis was as follows: during the attachment of negative electrons on the positive atomic ions of a chemical element its fine-structure band-spectra are emitted, as in numerous different orbits of attachment the potential energy is emitted in multiples of Planck's quantum constant.

These two working hypotheses have met with very different fates. Not long after its postulation I realized that the second of them was wrong, and neither bore it experimental fruit. However, what has been extraordinarily fruitful theoretically is the nucleus of it – that is, the assumption that energy is emitted in accordance with Planck's quantum law through an electron changing orbits about a positive charge. This assumption forms the starting-point of Bohr's theory of the emission of serial lines. Although I myself once stood on the threshold of this theory, and although the final formulae give a series of frequency relationships in the spectral series which agree well with

observed facts, I am nevertheless unable to believe it, because in its provisions it postulates suppositions which contradict, not only Maxwell's theory, but the very spirit of physics. This criticism is directed not at Planck's quantum of action, but at the hypotheses of Bohr which are bound up with it.

But let us return to the hypothesis that the positive atomic ions are responsible for the spectral series. Shortly after it was formed, I was able to make it bear experimental fruit through the following reflection.

By allowing the positive ions to pass through an electric field and thus giving them a certain velocity, it is possible to distinguish them from the neutral, stationary atoms. If it is possible to deduce their velocity from the spectral lines emitted by them, then this deduction implies the assignment of the displaced spectral lines to the moving atomic ions as emitters. The movement of the emitters of the spectral lines may be deduced on the basis of the Doppler principle.

We can in fact first place the beam of rays of moving positive atomic ions in a plane perpendicular to the axis in which we see the spectral lines emitted by them. These only appear in the places in which they are normally situated in the spectrum when their emitters are stationary. Secondly we can allow the beam of positive atomic ions in our axis of vision to approach us, and then their spectral lines appear to us displaced from their normal place in the spectrum towards the shorter wavelengths, by an amount which is proportional to the velocity of the emitting system. And thirdly, if we make the atomic ions in our axis of vision travel away from us, then their spectral lines appear to us displaced from their normal position towards the opposite side.

In the year 1905 I set about proving experimentally the phenomenon just described. The state of research at that time meant that one had to regard as positive ions the canal rays, which approach the cathode of the glow current and emerge on the other side through perforations in it. I directed the axis of the collimator of my spectrograph first perpendicular to the axis of a beam of hydrogen canal rays, and on a second occasion I allowed the canal rays in the axis of the collimator to approach it. During the comparison of the two spectrograms so obtained, the anticipated Doppler effect in the serial lines of hydrogen appeared, and the same result was later obtained on the serial lines of numerous other chemical elements.

Thus at the beginning of 1906 it seemed to be established that the emitters of the spectral series of chemical elements are their positive atomic ions. This interpretation of my observations, it is true, was soon questioned. For, as

particularly W. Wien and J. J. Thomson have shown, canal rays usually contain, beside the positive atomic-ion rays, also neutral rays, so that it was not possible to determine accurately whether the spectral lines showing a Doppler effect should be attributed to the former or the latter. However, cases came to light later in which canal rays containing only positive ions showed a Doppler effect in the spectral lines emitted by them. If even today I still regard the positive atomic ions of a chemical element as the emitters of their spectral series, nevertheless I do not take this concept now as restricted as I did when I only considered free positive atomic ions which owing to their positive total charge are accelerated by the electric field. Rather, in my present opinion, it is possible that the serial lines can in addition be emitted from a positive atomic ion which is not free, and to which a negative electron has already begun to attach itself; only the electron must not yet have come so close to the atomic ion that emission from the latter will already be sensitive to and disturbed by the electric field of the electron.

In this connexion the ultraviolet continuous spectrum of hydrogen may be mentioned. I expected that a continuous spectrum would be emitted when in the course of the attachment of the negative electron to the positive atomic hydrogen ion its encroachment has continued so far that the emission of the serial lines is noticeably disturbed. I therefore looked for – and found – a continuous spectrum in hydrogen canal rays.

Since the discovery of the Doppler effect in canal rays a great number of experiments on this phenomenon have taken place. The following results may, briefly, be deduced from them.

The emitters of the spectral series are without exception single atoms, not compounds of atoms. The spectra of atomic ions of higher valence are different from the spectra of atomic ions of the same element, but of lower valence.

The band-spectra – for example, those of hydrogen and nitrogen – do not as a rule show a canal-ray Doppler effect. Because I expected on the one hand that their emitters were positive molecular ions consisting of more than one atom, and because on the other hand positive molecular ions are also found as canal rays under low pressure, I was led to explain the absence of the Doppler effect in bands by the theory that when stimulation by collision takes place, so that light is emitted, the molecular ions of more than one atom are dissociated after a short period of existence. This concept also makes the canal-ray Doppler effect appear possible in bands, at a small velocity and low pressure. And in fact Mr. Rau of the Würzburg Institute of Phys-

ics has recently succeeded in proving this in the case of negative-nitrogen bands. All his observations, moreover, lead to the conclusion that the emitters of these bands are positive diatomic nitrogen molecular ions.

The announcement of this new step forward bids us recognize that research work on the canal-ray Doppler effect is by no means complete and the question of where the various spectra of chemical elements originate has not yet been conclusively answered.

If the experimental physicist has already done a great deal of work in this field, nevertheless the theoretical physicist has still hardly begun to evaluate the experimental material which may lead him to conclusions about the structure of the atom. It has, however, been established that the conversion of the structure of the neutral atom into the structure of the ion involves fundamental changes in the oscillation of the electric quanta which remain in the structure. What conclusions, however, may be drawn from this about the structure of the neutral atom and of the ion remains an unanswered question.

The removal of an electron from the surface of an atom – that is, the ionization of the atom – means a fundamental structural change in its surface layer. That this is accompanied by an equally fundamental change in the spectrum of the surface layer was to be expected from the very first. Matters are different in the case of the second sort of structural change which I made the subject of my experimental research.

We must always bear firmly in mind that the chemical atom is an individual, self-contained structure of positive and negative electric quanta. An external electric field, meeting it and passing through it, affects the negative as much as the positive quanta of the atom, and pushes the former to one side, and the latter in the other direction. Certainly this displacement is soon stopped by the opposing forces which it awakens among the quanta, which are displaced relative to one another, but nevertheless the displacement does take place, and it means a deformation, an alteration of the atomic structure in comparison with its form before the influence of the external electric field. And the question arises whether this sort of alteration of the atomic structure, this deformation by an electric field, manifests itself in an alteration of the spectrum of the atom. In other words, the question of the effect of an electric field on spectral lines has thus arisen.

At the time when I became engaged on this question Voigt had already developed and worked out mathematically a theory of an electric analogue to the Zeeman effect. The result of this theory was not encouraging; because

it implied that the alteration to the oscillation frequency, or wavelength, of spectral lines by an electric field would be so small that it would be immeasurable. And this result seemed to be confirmed by the complete absence of success in research to find this effect extending over a number of years.

However, I was unable to accept the presupposition of the theory – namely, the assumption that the emission of a spectral line on the part of an atom was the work of only one single independently moving electron in the atom. In my view the structure of the whole atom was that of an individual, with all its parts interconnected, and the emission of a spectral line appeared to me to be the result of the coherence and co-operation of several electric quanta. Therefore I expected that from the alteration of the atomic structure by an external electric field there would arise also an alteration of the spectrum of the atomic structure. And I tried to solve my problem by producing a strong electric field in a luminous gas. I achieved this by making canal rays, passing through perforations in the cathode, on the other side travel in a strong electric field between the cathode and a second supplementary electrode placed opposite it.

My very first spectrogram of canal rays in hydrogen and helium revealed the effect of the electric field on a number of spectral lines and gave some hint of the wealth of phenomena in the newly opened-up field. Moreover this discovery also showed again how much richer and more original are the works of Nature than the ideas of Man. In the event of the axis on which the observer views the luminous electric field being perpendicular to the axis of the field, the theory had postulated that every single spectral line would be splitted under the influence of the electric field into two components, both of which would appear, relative to the normal line, displaced towards the longer wavelengths, and of which one would vibrate parallel, the other perpendicular, to the field. How different the reality! For example, the red hydrogen line is splitted symmetrically to the normal line into nine components, of which the electric vibrations of six are parallel, and three perpendicular, to the field.

Since the discovery of the effect of an electrical field on spectral lines in the year 1913, already a great number of experiments on it have taken place. The most common and most important result of them is that the nature and size of the effect on corresponding series of different elements are largely an expression of the peculiarity of their atomic structure – or, at least, of the structure of the surface. Thus the effects on the hydrogen series, on the series

of mono- and bivalent positive atomic helium ions, on the series of lithium, mercury, and aluminium, differ from one another in characteristic ways.

The following results may be of interest in detail.

Along a series of lines running from longer to shorter wavelengths the effect of the electric field becomes greater as the serial numbers increase – that is, as the wavelength decreases.

The proportion of the intensity of various series within a serial system is, in the case of several elements, dependent on the strength of the deforming electric field. Thus under its influence new series may become visible, which outside the electric field possess so little intensity that they are not observable.

The broadening and displacement of spectral lines which accompanies an increase in the pressure of the gas or in the density of ions, originates in the effect of the electric fields of single atoms on neighbouring light-emitting atoms. Connected with this is Haber's highly promising idea of tracing the heterogeneous catalysis in solid surfaces back to the deformation of the molecular structure by the molecular electric field. Perhaps it will be possible to spectro-analyse accurately such electric fields.

But let us leave these details and return to the problem of the structure of the atom. We must ask ourselves whether a conclusion about the structure of the atom, or at least about the process of the emission of serial lines, may be drawn from the effects of an electric field on serial lines. Now, Bohr and Epstein have developed a theory for the effect in question which gives the number and interval for the component lines in electric fields which agree surprisingly well with observed facts, at least in the case of the series of the monovalent atomic hydrogen ion and of the bivalent atomic helium ion – or, rather, in the case of the electron adhering to these ions. This agreement strongly supports Bohr's theory of the emission of spectral lines, and thus also his presupposition, originating from Rutherford, about the structure of the hydrogen and helium atoms.

In spite of my high estimation of this achievement by the theory, nevertheless I am unable to accept it as definitive; apart from the fact that I am unable to believe in some of its presuppositions, it does not fully correspond to our experience. It cannot explain the following observation, which seems to me very important to research into the structure of the atom.

A beam of luminous hydrogen canal rays has, owing to its velocity, exactly the same direction as that of the electric field in which it may be made to move. If the axis of the beam is placed perpendicular to the axis of the field, the intensities of the components of every single line which is splitted

Biography

Johannes Stark was born on April 15, 1874 in Schickenhof, Bavaria; his father was a landed proprietor. He was educated at the Gymnasium (grammar school) in Bayreuth and later in Regensburg and proceeded to Munich University in 1894 to read physics, mathematics, chemistry and crystallography. Stark graduated in 1897 on the basis of his doctoral dissertation on Newton's electrochronic rings in a certain type of dim media. He worked as assistant to von Lommel at the Physics Institute at Munich University from 1897 until 1900 and then became unsalaried university lecturer of physics at the University of Göttingen. In 1906 he was appointed extraordinary professor at the Technische Hochschule in Hannover and in 1909 he followed the invitation of the Technische Hochschule in Aachen to become Professor there. A similar appointment at the University of Greifswald followed in 1917. Three years later he moved to the Physics Institute of the University of Würzburg, where he stayed until 1922.

Stark's scientific works cover three large fields: the electric currents in gases, spectroscopic analysis, and chemical valency. His spectroscopic work deals with the connection between the alteration in the structure and in the spectrum of chemical atoms. In 1919 Stark was awarded the Nobel Prize for Physics for his « discovery of the Doppler effect in canal rays and the splitting of spectral lines in electric fields ». The prize enabled him to set up his own private laboratory.

In 1933 Stark was elected President of the Physikalisch-Technische Reichsanstalt (Physico-Technical Institute) as successor to von Paschen, where he remained until his retirement in 1939. At the same time he held the post of President of the Deutsche Forschungsgemeinschaft (German Research Association).

Stark was a prolific writer and published more than 300 scientific papers. His book *Die Elektrizität in Gasen* (Electricity in gases) was published in 1902. This was followed by works on elementary radiation and electrical spectroscopic analysis of chemical atoms. In connection with his work on chemical valency he wrote a book *Die Elektrizität im chemischen Atom* (Elec-

tricity in the chemical atom). Stark founded the *Jahrbuch der Radioaktivität und Elektronik* (The Year-Book of Radioactivity and Electronics) and edited this publication from 1904 until 1913.

Johannes Stark was a corresponding member of the Academies in Göttingen, Rome, Leyden, Vienna and Calcutta, and was awarded the Baumgartner Prize of the Vienna Academy of Sciences in 1910 and the Vahlbruch Prize of the Göttingen Academy of Sciences in 1914, and also the Matteucci Medal of the Rome Academy.

During the last years of his life Stark, in his private laboratory on his country estate Eppenstatt near Traunstein in Upper Bavaria, investigated the effect of light deflection in an unhomogeneous electric field.

He was married to Luise Uepler. They had five children. His recreations were forestry and cultivation of fruit trees.

Stark died on June 21, 1957.

Physics 1920

CHARLES-ÉDOUARD GUILLAUME

«in recognition of the service he has rendered to precision measurements in Physics by his discovery of anomalies in nickel steel alloys»

Physics 1920

Presentation Speech by Dr. Å. G. Ekstrand, President of the Royal Swedish Academy of Sciences

Your Majesty, Your Royal Highnesses, Ladies and Gentlemen.

The Swedish Academy of Sciences has decided to award the Nobel Prize for Physics 1920 to Ch. É. Guillaume, Director of the International Bureau of Weights and Measures, for the services he has rendered to the physical precision technique by his discovery of the properties of nickel steel.

One of Greece's greatest thinkers said that « things are numbers » and attempted to explain the origin of everything by numbers. The scientists of today do not take the cult of numbers to quite that extent; yet they recognize nevertheless that every exact knowledge of Nature begins only when we succeed in expressing the phenomena in measures and weights. The development of science has always been in step with the progress in measuring precision. This applies to astronomy, geodesy, chemistry and above all to physics, the special growth of which dates from the time when modern precision began to be applied in observations.

This was the point which had been grasped by the French National Assembly when, in 1790, it instructed the Academy of Sciences of Paris to lay down an invariable base for weights and measures. A committee was set up for that purpose, consisting of Borda, Lagrange, Laplace, Monge and Condorcet, and on their suggestion the National Assembly adopted a decimal system based on a certain part of a quadrant of the Earth's meridian. Thus the principle of the metric system was introduced into France which was then established by a law passed by the Convention held on August 1, 1793.

In the other countries progress was slower. It was not until after a few decades that people in Europe began to realize the advantages of the metric system and that mainly because of the large international exhibitions. During the 1867 international exhibition in Paris a committee was formed by most of the countries represented at the exhibition with a view to preparing the adoption of a single international system for weights and measures. The proposal to that effect, approved by the emperor on September 1, 1869, was submitted to all the states and thus was subsequently founded the International Bureau of Weights and Measures at Breteuil, near Paris.

It was the French nation which not only conceived the idea of this great reform, but which, by its diplomatic skill, was also able to bring about its adoption in the whole civilized world; on this account, therefore, mankind owes France a great debt of gratitude.

All the copies of the standard metre and the standard kilogramme intended for the various countries are meticulously examined and compared in this International Bureau, the head of which, Charles-Édouard Guillaume, is undeniably the foremost metrologist of today. By devoting his entire life to the service of science, this scientist has made a powerful contribution to the progress of the metric system; during his long and painstaking studies he discovered a metal with the most excellent metrological properties. That is the discovery which the Swedish Academy of Sciences has sought to reward by conferring this year's Nobel Prize for Physics, since the discovery is of great significance for the precision of scientific measurements and thereby even for the development of science in general.

Actually the mere fact of possessing an international system for weights and measures and an International Bureau for the application of that system had not done away with the difficulties entailed in each measuring or weighing operation unless it is possible to achieve here the maximum precision. With measurements of length in particular the chief source of errors was dependent on temperature as a result of the well-known property of materials to change their volume with variations in temperature.

It was thus basic to examine with the greatest precision the expansibility of all metals and alloys under the action of heat. During these delicate examinations, and particularly while studying the properties of certain types of steel, Guillaume hit on the apparently paradoxical idea that it should be possible to produce an alloy free from this universal property of materials to change their volume at various temperatures. The long and difficult experiments performed by Guillaume year after year on numerous alloys and above all on nickel steel to determine their expansibility, elasticity, hardness, changeability with age, and stability ultimately led him to the important discovery of the nickel steel alloy known as invar, the temperature coefficient of which is practically zero.

These studies and discoveries by Guillaume have continued to give rise to new and significant practical applications. Instances are the use of invar in the design of physical instruments, and especially in geodesy where Guillaume's discovery has completely transformed the methods of measuring base lines; nickel steel has also supplanted platinum in the manufacture of

incandescent lamps and on the basis of the current price of platinum this represents an annual saving of twenty million francs; lastly chronometry is indebted to Guillaume's discoveries and investigations for a new refinement – the use of the new alloys enables watches to be adjusted more accurately and at less cost than formerly.

From the theoretical standpoint, too, Guillaume's penetrating and systematic studies on the properties of nickel steel have had the greatest significance because they have confirmed Le Chatelier's allotropic theory for binary and ternary alloys. He has thus made an important contribution to our knowledge of the composition of solid matter.

In consideration of the great importance of Mr. Guillaume's work for precision metrology and thus for the development of all modern science and engineering, the Swedish Academy of Sciences has awarded this year's Nobel Prize for Physics to Charles-Édouard Guillaume in recognition of the services which he has rendered to the physical precision technique by his discovery of the properties of nickel steel.

Monsieur Guillaume. By your persevering studies in thermometry you have deserved well of physics and chemistry; but you have gained your scientific laurels mainly in a different sector. By your studies of metal alloys and their sensitivity to temperature influences, you established that a few of those alloys possess remarkable properties; some scarcely expand on heating which suggested to you the idea of making them into measuring standards. One of the nickel steel alloys in particular, the one containing thirty-six per cent nickel, you considered to fulfil the necessary conditions. Since it is almost invariable under the action of heat and under other influences, you have called it invar. Its potential benefit to science for the construction of standards and of various instruments can readily be appreciated. In geodesy, invar wires give much more accurate base-line values than those formerly obtained.

On behalf of the Royal Swedish Academy of Sciences, I congratulate you on your studies and on your discoveries which have been of the greatest utility and for that very reason deemed worthy of the Nobel Prize. I would now ask you to receive the prize from the hands of His Majesty the King who has been pleased to make the presentation to you.

CHARLES-É. GUILLAUME

Invar and elinvar

Nobel Lecture, December 11, 1920

The anomaly of nickel steels

Discovery of the anomaly – In 1889 the General Conference on Weights and Measures met at Sèvres, the seat of the International Bureau. It performed the first great deed dictated by the motto inscribed in the pediment of the splendid edifice that is the metric system: «*A tous les temps, a tous les peuples*» (For all times, to all peoples); and this deed consisted in the approval and distribution, among the governments of the states supporting the Metre Convention, of prototype standards of hitherto unknown precision intended to propagate the metric unit throughout the whole world.

These prototypes were indeed noteworthy. They were made of a platinum-iridium alloy developed by Henri Sainte-Claire-Deville which combined all the qualities of hardness, permanence, and resistance to chemical agents which rendered it suitable for making into standards required to last for centuries. Yet their high price excluded them from the ordinary field of science; at that time a single metre actually cost 7,000 crowns – and how much more today!

A less costly answer had to be sought since between these precious prototypes and standards affording only uncertain guarantees there was a gap which nothing could fill.

I first examined this problem in 1891 and soon discovered the really excellent properties of pure nickel and still today this is the metal used to make a non-oxidizable standard, unaffected by the passage of time, rigid and of average expansibility. However, one difficulty prevented me generalizing its use. A geodesic standard 4 metres long was required, and no nickel-producing factory would undertake to supply, perfectly sound and crack-free, a suitable bar.

Subsequent studies were guided by a few strokes of good luck. In 1895, at the request of the Ordnance Technical Department (Section technique de l'artillerie) in Paris, J. R. Benoît had undertaken the study of a standard made from an iron alloy containing 22% nickel and 3% chromium and found its

expansibility to be close to that of brass. It was a non-magnetic alloy and thus was doubly anomalous.

The very curious phenomena discovered by John Hopkinson had already been known for a few years, notably that after forging certain alloys of iron and nickel containing about 25% of the latter are non-magnetic and not very hard; however, once they have been cooled in, say, solid carbon dioxide, they are hard and strongly magnetic; furthermore, during this transformation, their volume has increased by about 2%.

The phenomena discovered by Hopkinson and Benoît were clearly related. But however interesting they might be for the physical chemist, they were strictly taboo for the metrologist. An alloy that is changeable, and another of high expansibility, are unsuitable for designing length standards.

The matter took on a different aspect when in 1896 I was put on the trail of a new and quite unexpected fact closely connected with those just reported. A bar of steel containing 30% nickel arrived at the International Bureau and I found its expansibility to be about one third less than that of platinum. The continuation of a study so begun augured well and I pursued it with stubborn obstinacy.

For metrology the matter of expansibility is fundamental; as a matter of fact the temperature measuring error relates to the length measurement in proportion to the expansibility of the standard and the constantly renewed efforts of metrologists to protect their measuring instruments against the interfering influence of temperature reveal clearly the importance they attach to the expansion-induced errors.

It is common knowledge, for instance, that effective measurements are possible only inside a building, the rooms of which are well protected against the changes in outside temperature, and the very presence of the observer creates an interference against which it is often necessary to take strict precautions.

Prior to the discovery of the anomaly to which I have just referred, any physicist would have sworn that there was no hope of remedy by means of metals or alloys whose expansibility was much lower than the known values because it had always been considered that the rule of mixtures was complied with in practice.

My first care was to check the direction of the expansions as a function of the composition of the alloys. This was not a vain precaution since between the non-magnetic 22% alloy and the magnetic 30% alloy a discontinuity was apt to occur. Experiments conducted on two alloys, enclosing

the second, and which were reported to me by the Société de Commentry-Fourchambault & Decazeville, established the continuity.*

Classification by magnetic properties. – The accurate measurement of expansions is long and difficult. Since there could be no doubt that the anomaly involved affected all the properties of the new alloys, more readily applicable methods had to be used. It was self-evident to study the magnetic susceptibility, for if it is merely a question of ascertaining the presence or absence of ferro-magnetism, the experiments are elementary.

In its early stages this study showed me that there were two distinct transformations. The one is irreversible and this is the one discovered by Hopkinson; the other is reversible and its discovery was new. A number of observers, notably Osmond, Louis Dumas, Pierre Weiss and his pupils, Nagaoka and Honda have established its characteristics.

A very simple diagram shows at a glance the over-all pattern of the transformations of ferro-nickels referred to their magnetic properties.

Starting from iron these transformations split into two groups, AB and AC, which progressively diverge (Fig. 1a). When we cross downwards the lower curve, magnetism appears, then increases up to a certain limit. When the alloy is reheated the magnetism decreases from a given temperature and ultimately disappears at the intersection with the upper curve. On the other hand the behaviour of the single curve indicates, in alloys with a higher nickel content, both the appearance, on cooling, and the disappearance, on reheating, of the ferromagnetic properties.

The intersection of the curves for the two categories has a precise significance: additions of carbon, chromium, and manganese appreciably lower the temperature of the irreversible transformations but have far less effect on the reversible transformations. It is thus possible to follow the reversible transformation into the region of the normally irreversible alloys.

Moreover, to the right of the intersection moderate cooling leaves the magnetism completely reversible; more intense cooling sets the transformation and renders it irreversible.

Let us now assume a third axis at right angles to the other two. Along it we shall plot the susceptibility and in the solid diagram thus obtained we

* Inspired initially by the managing director, Henry Fayol, this firm was not content to stop there; when submitting to me its analytical reports the firm supplied me with alloys numbering more than six hundred. It is due to this unstinting collaboration that my studies were able to carry on for almost a quarter of a century.

make two orthogonal sections, *ab* and *cd*. In the irreversible region the previously cooled alloy retains along ABC (Fig. 1b) the susceptibility it has acquired, which reverts to zero along CD. On cooling it will remain zero until point F is reached where it will start to rise again along line FB.

Cooling may be interrupted at any time, then the alloy reheated and it will retain the constant magnetic properties shown by the line B'C'.

In the reversible region the susceptibility is represented by the single curve ABCDEF (Fig. 1c).

All the properties of the alloys with which we are concerned are associated with these transformations, and the characteristics of those which we have just traced will also be found in the curves representing the changes in volume or elasticity modulus.

Having thus found the guide I devoted my full energies to studying the

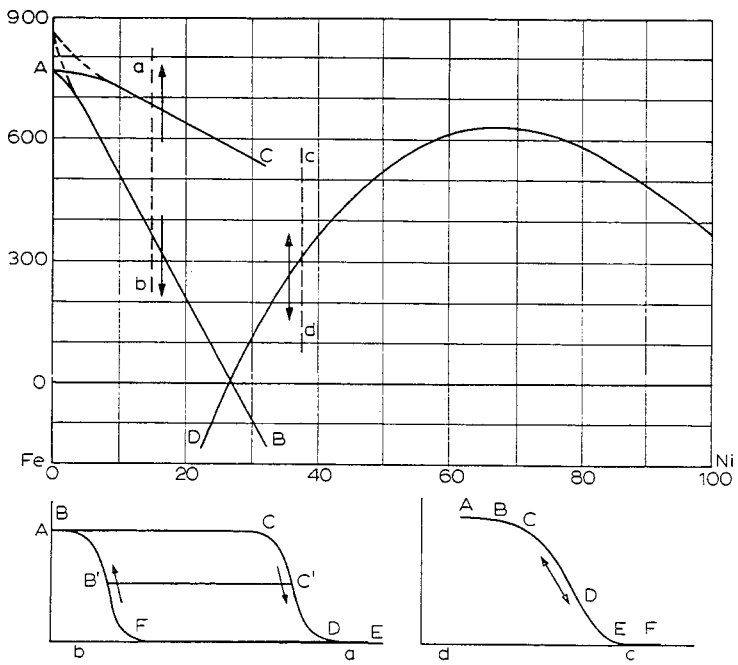


Fig. 1a. Magnetic transformation temperature of nickel steels as a function of their composition. The branches AB and AC refer to irreversible alloys, branch DE to reversible alloys.

Figs. 1b, 1c. Variations in magnetic susceptibility of nickel steels as a function of temperature in the irreversible and reversible alloy regions (temperature along abscissae, susceptibilities along the ordinates).

changes in volume which constitute the main metrological factor associated with the existence of these alloys.

Changes in volume

Methods–It was during the 18th century that the expansibility of solid materials was clearly demonstrated. The famous French physicist and geodesist Bouguer once wished to exhibit its effects to a large assembly and for this purpose he suspended under the dome of the Hôtel des Invalides a metal wire supporting a telescope balanced at two points. The telescope was sighted on a distant levelling rod and when, during the course of the day, the temperature rose then fell, the variations in length of the wire were revealed by the movement of the point on the rod seen in the telescope.

In that case, however, it was merely a verification and not a measurement. Various methods of determining expansions were used during the 19th century; the one which I have used almost exclusively is that of the comparator which Baron Wrede recommended to the International Bureau to be used in the form in which he himself used it and which, perfected over the years, particularly by J. R. Benoît, has resulted in the methods in use today.

Two micrometer microscopes fixed to stone supports are vertically sighted on the marks on a rod immersed in water and measure the changes in its length when it is brought to various temperatures in succession. However, as the distance between the microscopes is not fixed, the procedure is to measure alternately the rod under test and a standard kept at a practically constant temperature in a second trough. This is termed an *absolute* method. For routine work it is replaced by the *relative* method in which the reference standard is placed in the same bath as the rod under test. The expansibility of the latter is the algebraic sum of the relative expansibility given by the experiment and of that of the standard rod determined beforehand by the absolute method. I used the comparator method in its relative form with an iridium platinum rod as the reference standard.

But the comparator method can be used only over a restricted temperature range the limits of which, in the case of my experiments, were 0° and 38° C. To attain temperatures higher than 200° C and thus better characterize the transformations, I secured the sample to be studied to a brass rod, the two being coupled at one extremity and free to expand over the remainder of their length. The relative extensions were measured at the free end with a microscope.

Later P. Chevenard designed a dilatometer whereby the relative extensions of a small sample of the metal under test (25 to 50 mm) and of a reference made of a suitably chosen alloy can be measured with high accuracy. His dilatometer, with photographic recording, enabled the whole range of temperatures from that of liquid air to 1,000° C to be covered. In their common parts our diagrams coincide, but Chevenard's go far beyond mine.

Irreversible changes – Knowledge of the irreversible changes in volume of the alloys was necessary, especially to ascertain their limit. I studied the various aspects in binary and ternary ferro-nickel alloys with chromium, copper, etc. I was only able to trace the low part of the cycle. The complete cycles were traced much later by P. Chevenard.

When a bar of an irreversible alloy is heated from a low temperature, it expands in accordance with the almost rectilinear curve ABC (Fig. 2b), then, at a certain temperature, the rate of extension falls off, then progressive con-

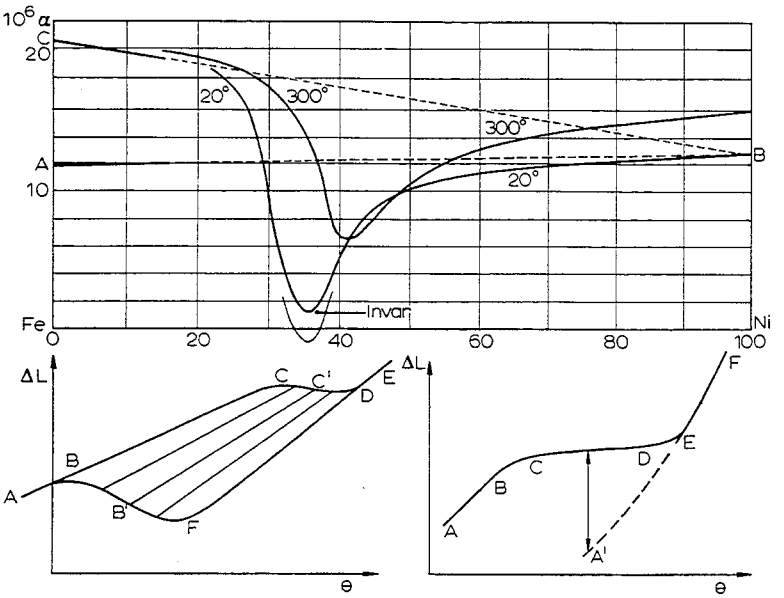


Fig. 2a. Diagram showing the expansibilities of nickel steels as a function of their composition. The two straight lines starting from A and C limit the range of the irreversible alloys, the curves represent the true expansibilities of the reversible alloys at 20° and at 300° C (α_{20}, α_{300}).

Figs. 2b, 2c. Diagrams for a reversible and an irreversible alloy.

traction sets in and continues regularly until completion of the transformation, whereupon the expansion resumes its regular course DE.

If the bar is allowed to cool it will be found to contract in accordance with EDF, at which point an expansion sets in which ceases at B and gives place to a contraction along BA.

If the cooling has been suspended at B′ and if the bar is reheated, the movement follows the line B′C′ and continues along the curve C′DE as in the preceding case. The slope of the straight line AC is between 10 and 11 $\times$ 10^{-6}, that of EF is about 18 $\times$ 10^{-6}.

The former shows the expansibilities of the ordinary irons or steels, the second those of the non-magnetic iron-nickel-chromium alloy mentioned above. Between the two all the intermediate expansibilities can be attained by suspending the transformation at any point along one of the curves CD or FB.

Along the curve ABC the iron is in a state which we shall term «cold stable»; along the curve EDF it is in the «hot stable», non-magnetic state.

Collating the expansibilities of the irreversible alloys, we find that they lie between the segments of the straight lines AB and CB in Fig. 2a, proper to their particular zone. Contrary to all appearances the rule of mixtures is obeyed but it must be applied to the association of the nickel with that of the varieties of iron present in the alloy within the limits of the zone covered, or else with a proportion of each of the two states of iron which depends on the degree attained by the transformation.

The zone which can be occupied by the high expansibilities is progressively extended to ordinary temperatures by retarding the transformation by additions of manganese, chromium, or carbon, as mentioned when discussing magnetism.

Reversible changes – The experiments with the reversible alloys were much more extensive.

A first examination consisted in determining the anomaly curve as a function of the nickel, entirely neglecting the manganese, carbon and silicon additions present in varying amounts in the alloys and which left the curve slightly ambiguous. Next I studied a series of alloys containing up to the maximum possible proportions of manganese and carbon. Having thus determined the coefficients relating to these additions for all the contents of nickel, I was able to convert the results to constant proportions of additions: 0.1% Mn, 0.4% C, and the alloys containing them I have termed *standard*

alloys. Moreover, after hot rolling and cooling in air, these alloys will be referred to as being *in the natural state*.

Since the expansibility of a material is given by the equation

$$l_\theta = l_0 (1 + \alpha\theta + 2\beta\theta^2)$$

the expression

$$\alpha_\theta = \alpha + 2\beta\theta$$

will be termed the *true* coefficient at θ, which is at the same time the mean coefficient of expansion between 0°C and 2 θ ; β will be the *quadratic coefficient*.

The expansibility of most metals is well represented, over a wide range of temperatures, by an equation of the above form; for the alloys under discussion the same rule is adequately applicable over the narrow range where I used the comparator; but, when it is extended, it soon becomes apparent that a second degree equation is no longer sufficient to represent the phenomenon. Nevertheless it is convenient to retain the same form of the equation, attributing to β a value which varies with the temperature and which indicates the semi-curvature of the expansibility curve at each point.

For normal, reversible nickel steels the values of α_{20} and β_{20} are represented by the curves in Figs. 2a and 3. The lines AB link together the values of the same coefficients for nickel and steel in the cold stable state and so reveal the magnitude of the anomaly in expansibility. For β this anomaly is undeniably positive, then negative. It appears to be likewise for α but the line CB which starts from the value of the coefficient in gamma iron and limits

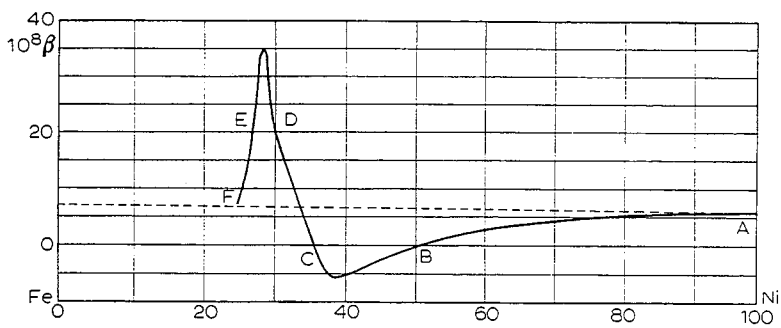


Fig. 3. Values for coefficient β in reversible alloys. The letters marked on the curve correspond to those in Figs. 1c and 2c.

the zone of large expansibilities of the irreversible alloys, shows the anomaly as entirely negative. And in fact it has to be referred to this representation.

The anomaly is very great since the expansibility of the alloys varies in the ratio of about 1 to 15 and reaches a value which is only one quarter of the lowest expansibility found in one metal. From a practical standpoint, moreover, it will be noticed that the low expansibilities are achieved here by inexpensive metals and constitute a completely continuous scale in contrast with the discontinuous and costly sequence of the less expansible metals, iridium, tantalum, tungsten.

The generic name *invar*, short for invariable, has been given to the alloy whose expansibility differs little from the minimum. The coordinates of the minimum for the standard alloys in the natural state are: Ni = 35.6; $\alpha = 1.2 \times 10^{-6}$.

Great importance need not be attached to the exact value of these coordinates. Actually they relate to an alloy in which the additions have been fixed arbitrarily at about the mean amounts contained in industrial alloys, and we shall soon see that these additions have a considerable effect on the position of the minimum. Moreover, every heat treatment or mechanical treatment modifies the expansibilities, raising them in the case of annealing with slow cooling, reducing them when the cooling is rapid, and more still when the alloys are hammer hardened. By superimposing one reducing treatment on the other it is thus possible to bring the expansibility of an alloy to 1.5×10^{-6} below its value corresponding to the natural state and give it a negative value. Then, after this has been achieved, heating for a few hours at 100°C, for instance, raises it again and may bring it very close to zero. In this way a method was elaborated for making several kilometres of invar wire, the expansibility of which can be detected only by very accurate measurements. This result is of great practical importance, as we shall soon see.

The wide margin of variations of the coefficient β is sufficient to suggest that the general curve representing the true expansibility of the alloys as a function of their composition will deform rapidly when the temperature to which it corresponds is altered. Furthermore, comparison of the curves in Figs. 1a and 3 shows that the maximum of β largely corresponds, for the particular case of ordinary temperatures, to the region in which the magnetism appears or disappears. This coincidence is universal, as I was able to demonstrate in 1896, up to the limit of the temperatures which it was possible for me to attain and I immediately concluded that the complete curve for the changes in length specific to a given alloy, transfers its characteristics

to the neighbouring alloys with a simple shift in the scale of temperatures. From that I deduced a rule of corresponding states which assuredly is only a coarse approximation to reality but nevertheless permits generalisation and, consequently, prediction.

It may be formulated by stating that, at a certain temperature, a given alloy is in the same state as another alloy at a temperature which differs from that at which magnetism first appears by the same margin as in the first alloy.

To apply this rule the concomitant values of α and β for the series of alloys which are considered at the same temperature will be compared as belonging to the same alloy for the series of temperatures. In this way it is possible to trace *a priori* the extension curve of a given alloy which has the general shape shown in Fig. 2c. Essentially five periods are distinguishable in this curve, the two extremes, AB and EF, with β weakly positive, giving the result obtained from the rule of nickel-iron mixtures in the cold and hot stable states, and three intermediate regions, BC and DE with β weakly negative or very strongly positive, lastly CD which is the region of inflexion where β changes from negative to positive, and where, as a result, the expansibility reaches its minimum.

Between 150° and 200° C the expansibility of invar proper starts to increase, and between 250° and 300° C its expansibility becomes normal.

Since the entire curve shifts towards the high temperatures with increasing nickel content, the alloy will be matched to the temperature range specific to the intended applications. It should not be forgotten, however, that the curve deforms slowly and that at high temperatures there are no alloys with very low expansibility.

The top curve in Fig. 2a represents, by way of an example, the expansibilities of nickel steels at 300° C as found in Chevenard's experiments. The anomaly is still very marked but the minimum has shifted considerably towards the high nickel contents as foreshadowed by the trend of the β-values.

The distance between the curve FEA′ and the curve EDCBA (Fig. 2c) is the amplitude of the anomaly at each temperature.

Elastic properties

Elasticity moduli – The irreversibility and the reversibility revealed by the study of the magnetic properties of the alloys, or of the variations in their volume, recur, as was to be expected, in their elastic properties. Here again the transformations whose effects we have ascertained bring about changes

which are a function of the prevailing temperature or else which are largely dependent on the thermal influences to which they have previously been exposed.

Generally speaking any expansion of materials lowers the modulus by reducing the intensity of the intermolecular reactions. The irreversible changes in volume by hardening have the result and owing to the transformation undergone by irreversible nickel steels at low temperatures, their elasticity modulus decreases by about one-tenth of its original value, the decrease being along the curve FB (Fig. 4b). Lastly, I would like to add that the system of which this curve is a part represents all the changes of the modulus in this class of alloys and its analogy with the changes in volume illustrated in Figure 2b is clearly apparent.

Compared with the values specific to iron and nickel, the curve of the moduli as a function of the content is depressed, the maximum depression

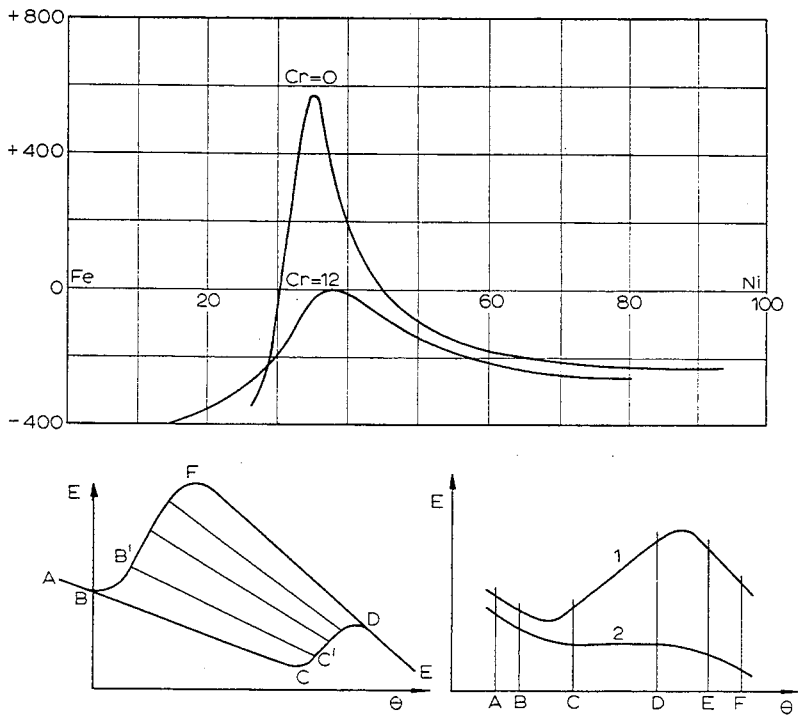


Fig. 4a. Thermoelastic coefficients of standard iron-nickel alloys and of alloys with 12% additions of chromium.

Figs. 4b, 4c. Changes in elasticity modulus of an irreversible and a reversible alloy. The letters in this diagram correspond to those in diagrams 1, 2 and 3.

occurring in the invar region, where it reaches about one quarter of the value that would be indicated by the rule of mixtures. The minimum modulus at ordinary temperature is thus close to 1.4×10^{12} c.g.s. units.

Variations of modulus with temperature – Interest centres mainly on studying the variations of the modulus with temperature, whether flexion or torsion is involved.

In the case of the latter I merely performed a rapid examination which showed me how, by an adaptation, it was possible to deduce the thermal coefficients from those which govern the flexion. P. Chevenard went into it very thoroughly. In all cases the torsional pendulum method was used.

To study the thermoelastic coefficient in the case of flexion the instrument chosen is the chronometer, this being readily available, very small, and yielding directly a result that can be used in practice. Its drawback is that successful manufacture and fitting of the balance spring require an expert. For this study I had the valuable assistance of a skilled regulator, Paul Perret, who had taken the initiative in offering me his services soon after my initial publications on invar; later, the experiments were conducted under my control by the technical laboratories of the Société des Fabriques de Spiraux Réunies.

The balance spring is made of the alloy under examination and mounted on a balance of a known metal and the assembly constitutes the regulator of the chronometer.

The thermal changes in the period of oscillation of the balance driven by the balance spring involve the expansions and the variations in bending modulus simultaneously.

Contrary to a very widespread idea, the expansion of the balance spring alone would reduce the oscillation period, i.e. the chronometer would be fast at high temperatures, the reason being that the transverse dimensional changes occur four times, and the changes in length only once, in determining the elastic moment. For its part, the expansion of the balance causes the chronometer to go slowly at high temperatures in accordance with the generally accepted idea. In the case of a steel balance spring and a brass balance the effects of the expansion largely cancel out each other. But a chronometer so equipped loses 11 seconds a day for each degree rise in temperature; this loss is due almost entirely to the temperature-induced change in modulus, the coefficient of which is negative.

For the reversible alloys the values of the thermoelastic coefficient, $dE/d\theta$, exhibit equally as strange an anomaly as that of the expansibility. The trend

is shown by the top curve (Cr = 0) in Fig. 4a. Beginning at a negative, i.e. normal value, the thermoelastic coefficient climbs very steeply, concurrently with increasing nickel content, crosses the zero axis, continues to climb, passes through a pronounced maximum in the invar region, then turns down and carries on more slowly to the value proper to nickel. Thus, for a whole category of alloys, the thermoelastic coefficient is positive; when bent at ordinary temperature, these alloys tend to straighten out when heated.

If not Young's modulus but the coefficient of deformability of the alloys had been plotted as a function of temperature, the sign would have altered and the curve, starting from positive values, would have exhibited a minimum. Its shape would then not have been very different from that of the expansibility curve and the common origin of the two anomalies would have been evident.

Owing to the low elastic limit of the metals at high temperatures the study of their bending deformations is very delicate there. Nevertheless, the application of the rule of corresponding states enables us to presume the characteristic of the modulus. Its general trend is shown by curve 1, Fig. 4c, where the minimum and maximum relate to the two points at which the over-all curve intersects the zero axis; the regions AB, etc..., which have already appeared in the study of the magnetic properties and expansibility, also occur in this curve.

Ternary alloys

The alloys whose properties have just been described are not pure mixtures of iron and nickel free from additions. They are, in fact, complexes in which solely the main constituents are entirely predominant since they form more than 99% of the total mass of the alloy. The additions are close to the indispensable minimum although it is possible to raise the quantity to a second limit and thus obtain ternary or quaternary alloys proper whose properties it is interesting to know.

Apart from the importance of this study considered from the physico-chemical point of view, these alloys may possess properties which render them particularly suitable for practical problems. Thus a sufficient addition of manganese makes the alloys suitable for casting, whereas chromium or carbon, or the two together, raise the elastic limit and make them better for manufacturing into springs.

My studies dealt in most detail with iron-nickel-manganese alloys and they

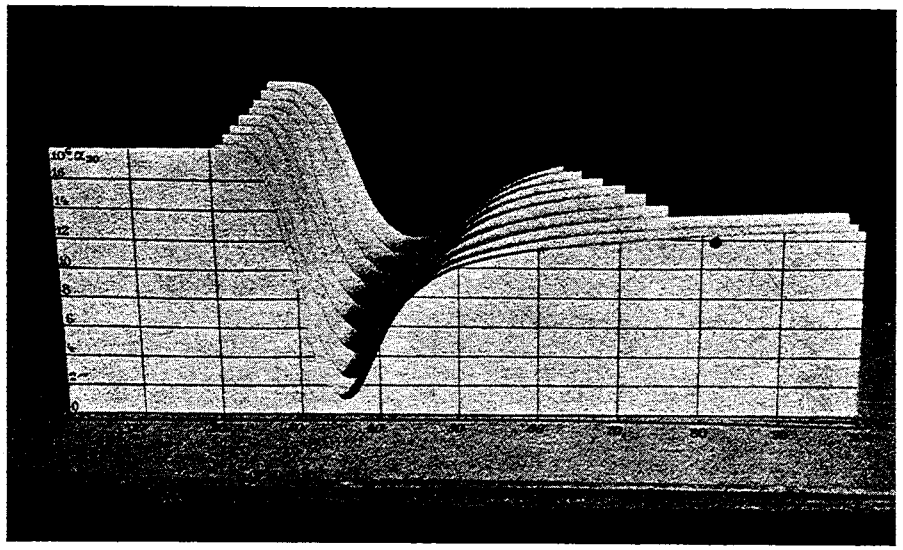


Fig. 5. Triangular diagram of the iron-nickel-manganese alloys.

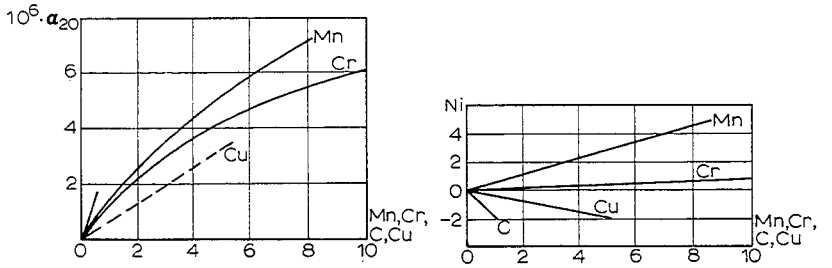
were sufficiently thorough to enable a family of curves Fig. 5 to be plotted on the principle of Guthrie's triangular diagrams.

The first curve is that of the standard alloys containing 0.4% manganese, the others refer to whole per cents, from 1 to 8. This three-dimensional diagram shows that the minimum rises rapidly, at the same time shifting towards the higher nickel contents.

Studies of the action of chromium, copper and carbon, although less extensive, have enabled an accurate plot to be obtained showing the trend of both value and position of the minimum. The two diagrams Figs. 6a and 6b illustrate this trend. Slight extrapolation to higher manganese content shows that when this metal is present to the extent of 10% in the alloy of least expansibility, it reduces the anomaly by about a half.

The curves in Fig. 7 represent the true expansibilities at 20° C for the limit contents of the additional constituent. The study of their action was of very special significance in connection with a practical problem, the solution of which I was searching for. Essentially the problem was to improve the conditions under which it was possible to prepare an alloy with a zero thermoelastic coefficient, an alloy with invariable elasticity modulus to which I had given the name *elinvar* in advance to define the programme of its preparation.

The binary alloys provide a double solution to the problem. However,



Figs. 6a, 6b. Trend of minimum for nickel steels containing varying additions of manganese, chromium, copper and carbon.

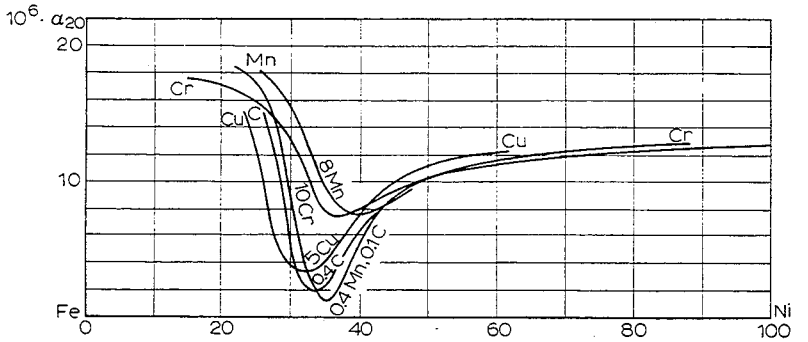


Fig. 7. True expansibilities at 20° C of nickel steels with additions of manganese, chromium, copper and carbon to the limits attained by the experiments.

on the one hand the thermoelastic coefficient curve cuts the zero axis at a very steep angle so that the smallest errors in composition, and even mere defects in the homogeneity of the alloy, lead to a property different from that sought; on the other hand the zero value is nothing other than the minimum or maximum of a variation curve and this value exists practically only over a small range of temperature.

Yet if we consider the raising of the expansibility minimum in ternary alloys and the analogy between the elastic and expansibility anomalies, we can be certain beforehand that suitable additions will give a thermoelastic coefficient curve tangential to the zero axis and, hence, leading to the ready preparation of elinvar. Furthermore, the rule of corresponding states shows that the whole of the modulus values for one and the same alloy will exhibit neither a maximum nor a minimum, but rather an inflexion, probably very extensive and almost horizontal.

These two conditions are indicated in the bottom curve (Cr = 12) in Fig. 4a and in curve 2 of Fig. 4c (region CD); they characterize elinvar.

Direct experiments have established the amount of the additions needed to make elinvar. The elastic limit conditions directed research mainly towards the hardening additions: chromium tungsten and carbon. The number of solutions is infinite since to reduce the anomaly the additions can replace one another continuously. In his torsion experiments, Chevenard found true elinvar with a 12% addition of chromium, the other auxiliary substances being kept close to the indispensable minimum.

Progressive or transient changes

The quite remarkable properties of nickel steels hold much promise for metrology. However, the hopes that had been founded on their use were for long restricted by a real defect which they have, notably a slight instability detected by accurate measurements of the length of test bars repeated over the years or else performed after a variety of heat treatments.

The character of these changes varies from one alloy to another and to describe it effectively it must be related to a particular alloy.

A bar of invar, for example, cooled in air from the forging temperature and kept at ambient temperature extends over the years at rather a fast rate to begin with, then at a progressively slower rate such that after twenty years very accurate measurements can only positively detect the change that occurs in one year.

If the cooling had been interrupted by a period at 100° C, the bar would have extended at an incomparably higher rate than at ordinary temperatures and, after about a hundred hours, the movement would almost have ceased to be perceptible. Generally speaking, the rapid initial rate is an exponential function of the temperature, such that each time the temperature increases by 20° C, the rate increases by a factor of about 7. This movement thus obeys the laws formerly established by Arrhenius.

From almost nothing at 100° C the movement re-ensues when the bar is brought to ordinary temperature but it is much less than in the absence of an intermediate stage. If after a few months or years have elapsed the bar is reheated to 100° C it can be seen to contract rapidly and revert to its length after the first heating.

Thus, at each temperature, the length of the bar tends towards a limit

value; between 0° and 100° C these values lie practically on a curve, the ordinates (negative) of which are proportional to the square of the temperature, starting from common zero. This rule continues beyond 100° C, then the rate of change decreases and when the alloy passes into the non-magnetic state, it undergoes only very slight changes.

The difference between the ultimate lengths of the bars at 0° and 100° C is of the order of 30 millionths for standard invar. The first extension, at 100° C, after the forging temperature, has about the same value. However, the magnitude of these changes is a steep function of the nickel content. Thus, when this content is increased from that corresponding to invar the magnitude of the changes diminishes rapidly and there is no change when the nickel content attains 42%; the change then reverses, passes through a negative minimum then continues to increase and ceases perceptibly when the nickel content reaches 70%.

A number of very significant results recently emerged from my study of ternary alloys in the region of instability.

I found, one after another, that the presence of manganese and of chromium reduces the instability while the presence of carbon aggravates it. By means of experiments in which, through the offices of the Aciéries d'Imphy, the carbon additions were made with the ultimate degree of accuracy, I found that the instability is proportional to the carbon and that consequently alloys from which all carbon had been removed would be perfectly stable.

However, ferric alloys cannot be altogether freed from carbon and it might be thought that invar will always retain a slight degree of instability.

The cause of this instability appeared to me to be probably in the transformations of cementite (Fe_3C), a compound which forms almost always when iron and carbon occur together and which, again, undergoes transformations with accompanying change in volume.

The answer to the instability problems will thus be to eliminate the cementite. The method is simple, consisting in introducing into the alloy a constituent having a strong affinity for carbon, e.g. chromium, tungsten or vanadium.

Reducing the carbon content to the minimum lowers the expansibility; the supplementary constituent raises it slightly. The matter is novel and the best conditions have not yet been established nor, more especially, achieved. But even now it has been possible to obtain alloy castings, whose expansibility remained that of a good quality invar and whose instability was reduced to one tenth of that of standard invar.

The slow changes in the elastic properties of the alloys, associated with those of their dimensions, have not been studied systematically. However, it may be taken as almost certain that they obey analogous laws in the sense that any increase in volume entails a reduction in the modulus. The ratio of the relative changes in these two properties, either in the transformation of the irreversible alloys, or else in just the expansion of normal metals and alloys, is of an order of magnitude between 20 and 30, i.e. an increase in length is accompanied by a relative decrease in modulus greater by a factor of twenty to thirty.

In the absence of a direct investigation it is possible with a certain degree of probability to guess the form of the laws governing the progressive or transient changes in elastic properties, and even the order of magnitude of their parameters.

Applications

The unusual properties of nickel steels afford an approach by entirely new methods to the generally simple solution of technical and scientific problems hitherto regarded as difficult or complex.

The elasticity or expansion anomalies, and even the magnetic transformations of these alloys, have led to certain applications, the enumeration of which will be sufficient to make them immediately evident.

The presence in a magnetic circuit of a component made of an alloy with approximately the composition at which magnetism disappears rapidly alters the properties of that circuit as a function of temperature. If, say, this component is the armature of a magnet, the mutual attraction will decrease with increasing temperature until it disappears altogether. This circuit can thus be applied as an automatic temperature limiter; for that purpose it is sufficient to load the armature with an appropriate mechanical component. With the alloy piece it is also possible to make a shunt across the magnetic field, the value of which decreases with increasing temperature such that the field proper tends to reinforce itself. Proceeding from this idea E. Meylan has equipped measuring instruments with temperature compensation for field variations.

Furthermore, along the expansibility curve there is a region close to 45% Ni in which the successive alloys have the expansibility of all the usual types of glass. These alloys could be used to make constant-grip optical mountings,

or else conductor wires for incandescent lamps. In the latter case the wire, from which the large quantity of occluded gas has first been removed by prolonged heating in vacuo, readily fuses to the glass. The *platinite* so formed has, for this purpose, almost completely supplanted platinum which is thus freed for other applications. The saving achieved in this way is approximately one hundred million crowns.

Elinvar, for its part, is suitable for making monofilar suspensions or, with a slight change in composition, for making tuning-forks, the period of which is independent of temperature. This information, which I announced in 1898, was confirmed in 1912 by Félix Robin who worked extensively on the study of nickel steel thermoelasticity.

This rapid survey clearly demonstrates the diversity of the problems which can be solved with the aid of nickel steels.

We shall now study in detail very refined applications, the method or scientific importance of which prompt a closer examination.

Measurement of length

General – To determine the limits to which nickel steels can be used in constructing instruments for the measurement of length, it is necessary to know those of their properties which will have to be involved in each specific case.

These alloys take a wonderful polish; provided the manganese or carbon addition is not too close to the minimum they are generally free from pitting. Perfectly clean traces can be made on them. They are slightly oxidizable and are able to withstand several hours' immersion in cold water. After a few years in a moist atmosphere the polished surfaces are unimpaired or else covered with a very fine film which can be removed simply by rubbing gently with a soft leather; on the other hand acid vapours attack nickel steels quite rapidly and their action must carefully be avoided.

I should mention, however, that the alloys suitable for all the length measuring applications of invar are not exactly the same. Lengths are measured by means of rulers, the marked surfaces of which are observed through a microscope, or else by means of freely suspended wires, as we shall shortly see. The absence of pitting is an essential quality of the metals used for marked standards; for wires a high elastic limit is required. In the first case the manganese content will be increased, in the second a slight amount of chromium will be introduced into the alloy. Carbon would be useful for both but

we have seen that it is the cause of instability; hence it must be used as little as possible.

Although on the one hand it is true that the finished wires must have a high elastic limit, their manufacture on the other hand requires high malleability. These properties appear contradictory to a certain degree. Nevertheless the invar type alloys achieve them in the sense that in the annealed state they are highly extensible but they harden rapidly when mechanically deformed so that if the initial length of an annealed wire has been doubled by drawing, it is capable of quite large elastic deformations.

The instability of nickel steels imposes, as has been stated, a limit to the field of applications for which one might initially be tempted to use them. However, providing that a measuring standard made of, say, invar can from time to time be checked against a perfectly stable standard, it may with almost complete reliability be used as an interpolation instrument; in all cases where its temperature is difficult to determine or cannot be determined with accuracy its value becomes paramount.

The detailed study of the laws governing the changes undergone by invar and the alloys close to it have in the past enabled the results to be freed from most of the effects of these changes. And, for the future, the prospect of making a perfectly stable invar removes most of the objections that might be raised to the use of low expansion alloys in very high precision measurements.

Geodetic base-lines – Every geodetic grid relies on one or more base-lines, each comprising a length marked out on the ground between fixed limits, the intervening distance being as large as permitted by the circumstances and generally of the order of about ten kilometres and measured with the maximum accuracy.

The determination of this distance, in the open air and under frequently ill-defined conditions of temperature, calls for careful precautions which geodesists have persistently striven to elaborate during the last century. During the first three-quarters of the century they tried mainly to improve accuracy largely regardless of the cost of the operations. Then a reaction set in; it was realized that there was more to be gained, as regards dividing the work between measuring angles and measuring lengths, by increasing the number of base-lines or extending them at the cost of a slight loss in measuring accuracy.

E. Jäderin, who assembled and codified the sparse methods, invented new

processes and developed a complete technique, was the chief proponent of this reaction which gave rise to a new conception of the organisation of geodetic grids.

In traditional geodesy the measuring instrument was the rigid rod (generally 4 metres long) in conjunction with microscopes sighted on the end-marks to fix their distance on the ground; then, after the rod had been moved a distance of its own length, the position of a third microscope was fixed by reference to the second, and so on. The method was accurate but costly. To avoid the action of radiation the instruments and the observers were accommodated in mobile huts. The instruments themselves were heavy and certain preparatory steps were required before they could be placed in position on the ground. The full team required for the measurements thus totalled about sixty men and, on days when everything went off smoothly, one hundred lengths, i.e. 400 metres, could be measured.

The measuring instrument proposed by Jäderin is a wire under tension at constant load and which is used to determine the relative distances of a series of lightweight marks that are readily transported and placed in position.

The length given to the wire by Jäderin is normally 24 metres, which reduces to one sixth the number of spans. The ground no longer needs preparation; with longer wires, if need be, ravines or rivers can be crossed (the longest wire used so far was 168 metres), all with 12 to 15 men; in short it is much more economical to use and its rate of measurement is quite different from that of the ruler and microscopes.

Temperature remained a very difficult point, of course.

Jäderin had seen, quite rightly, that the best procedure was to apply the principle adopted by Borda and Lavoisier, namely to measure each of the spans in succession by means of a steel wire and a brass wire, the difference between them giving at each moment the common temperature. However, the method developed before the discovery of invar was substantially less accurate than the ruler and microscope method, since the temperature remained unreliable; but the method as a whole was there in the rational form of a much better balance between the number of base-lines and the number of triangles.

Immediately he knew of the existence of invar, Jäderin, who was busy preparing for the measurement of base-lines for the Swedish-Russian Spitzbergen Expedition requested me to supply him with the necessary wire. I had already given the matter my attention and had had the Aciéries d'Imphy prepare wires suitable for base-line measurements, which wires had been

used in a number of preliminary experiments. The effect of drawing on the expansibility was already partly known and it was possible to supply the Expedition with wires that were practically non-expanding.

The success of the Spitzbergen measurements in 1899 was quite remarkable. A letter from Jäderin dated Treurenberg Bay, September 13, 1899 actually states the following:

« I am now pleased to inform you that our base-line measurements carried out this summer were completely successful. In both the outward and return directions we measured a base-line of 10,024 metres. So far we have made only a single provisional calculation which shows a discrepancy of merely 19 mm. We made no allowance for the expansion of the wires. »

And the letter closed with these words which, through their infinite melancholy, seem to me full of fine courage:

« The last of our boats leaves tomorrow for Stockholm and until June 1900 we shall be left completely separated from the world. Only once post has arrived for us; part of it seems to have gone astray. »

This message from the ends of the inhabitable world was for me the most powerful and the most valued encouragement. Benoît and I performed our own experiments with greater urgency and when, a year later, the Geodetic Association met in Paris, while the results submitted by the Swedish-Russian Expedition created a tremendous sensation, we were able to assert that a very high accuracy ought to be attained by an even greater perfection of the method, to which we were prepared to devote our utmost efforts. The Association passed a resolution to that effect and the committee vested with the permanent authority over the International Bureau finally entered this project on its programme.

A wall base-line which was built at once has been used since 1901 for studies which have been continuously in progress ever since.

The observations which we have made in about twenty years now number some hundreds of thousands. However, our labours have been amply rewarded since we have been able to define the conditions under which invar wires which have first been specially treated, then determined at the Bureau or in any standardization laboratory, can be transported in the form of a coil to the base-line area, then uncoiled and used without their characteristics altering.

By eliminating temperature-induced errors from measurements altogether, the use of invar wire appreciably raised the accuracy of the method. To take advantage of the method's potentialities Benoît and I were led to make

a partly new equipment which now ensures that in the measurement of each span – normally 24 metres – errors of the order of a hundred thousandth of the measured length will never be made. In accordance with the principle of the addition of errors, a length of 100 spans will thus not be subject to errors of the order of one millionth as a result of the observations.

A great many base-line measurements have been carried out in recent years using invar wire. The checks obtained either by the determinations performed in the outward and return directions or else by successive measurements with different wires have revealed that an accuracy of one millionth is quite common. Since the equipment is very easily installed it is now possible to use sites which would have been quite unsuitable with the old instruments comprising rigid rulers and microscopes. Moreover, in view of the speedy installation and the long spans coupled with the light weight and ready transportability of the equipment, it has been possible to reduce the number of personnel to about one fifth, and increase the speed tenfold; a well-trained team is capable of measuring up to 5 km in a day. Thus, the over-all saving in the measurement of base-lines is about 98%.

The increase in the number of base-lines and their greater length strongly reduce the effects of errors in the measurement of angles and the requirements which traditional geodesy was obliged to impose on them can be relaxed; the result is a second appreciable saving.

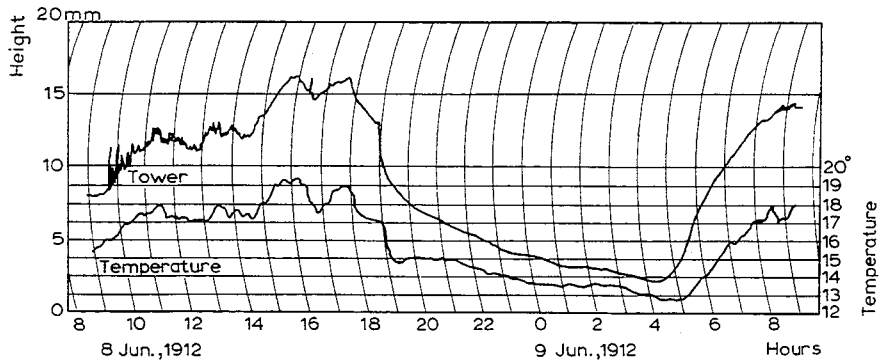
Non-expanding transmissions – From the point of view of the principle I shall confine myself almost entirely to the matter which has just been discussed by mentioning transmission over a fixed distance by means of an invar wire.

Its specialist applications are many. On railways, for example, there are a great many transmissions between a manoeuvre in a signal box and a movement received by a signal on the track. The expansion of the control wires disturbs the transmission: in the case of a rather severe temperature drop it may cause the signal to operate prematurely, and if the temperature increases rapidly, the expansion may completely neutralize the manoeuvre. Invar wire has afforded a means of overcoming these drawbacks and of guarding against the resultant dangers; however, its use has not been very widely adopted since, except for in special cases, the price of the alloy is considered prohibitive.

In contrast, no such limitation is involved in the acceptance testing of metal structures where a wire, fixed at one end to the structure itself, faithfully transmits its movement to a recording instrument.

The study, interesting in itself, of the vertical movements of the Eiffel Tower, plainly demonstrates the variety of applications of which this technique is capable.

Invar wire has greatly facilitated this operation. A wire of this type, fixed at its lower end to a peg in the ground, was attached at the other to a lever mounted on the second platform of the Tower and which actuated a recording instrument; a damping device integral with the lever compelled the latter to return slowly to the position assigned to it by the straight wire when it had momentarily been bent by a gust of wind. The experiment showed that, however violent the movement of the air, moments of calm occur from time to time which are sufficiently long to allow the lever to return to its rest position. Thus, its limit positions indicated the true movements of the



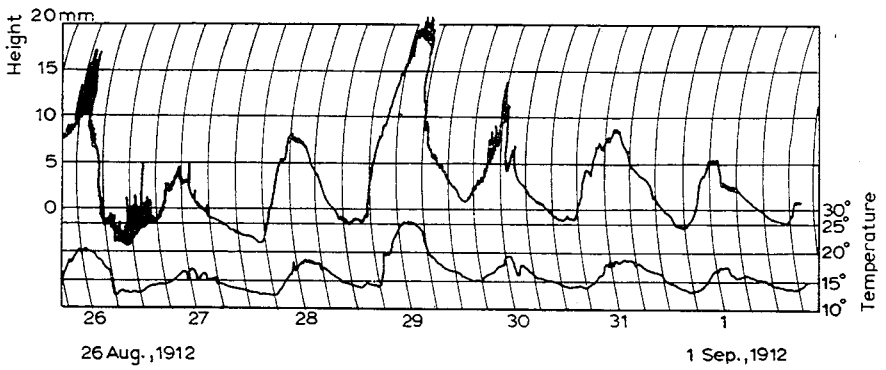


Fig. 8. Daily movements of the Eiffel Tower (a downpour at 19 hours caused a sudden temperature drop).

Fig. 9. Movements of the Tower under variable summer conditions (stormy week).

Tower, while a kind of tail, starting from the envelope, recorded the gusts of wind.

Figs. 8 and 9 show two examples of the movements observed. The first was obtained using a recording drum which rotated once per day, the other with one rotating once weekly. The lower curves are those of the thermograph.

When inspecting the two series of curves it is impossible not to be struck by their extraordinary similarity, which is such that each small inflection of one has its counterpart in the other. Thus the Eiffel Tower appears as a gigantic thermometer of high sensitivity, notwithstanding its enormous mass.

Measurement of time

Once time measuring instruments had become sufficiently accurate it became evident that their action was affected by temperature and attempts were made to guard against this by fitting them with devices known as compensators, the effectiveness of which had been demonstrated by a century and a half of experience, although they complicate the already complex mechanisms of clocks and watches.

Reference has been made earlier to the causes of the temperature-induced changes in the functioning of watches; they are very different from those specific to clocks, so that the two problems have to be treated separately.

The compensated pendulum in clocks – Of the various compensating devices invented to offset the influence of temperature on clock pendulums, the most widespread by far a few years ago was Graham's, which took advantage of the upward expansion of mercury in a vessel suspended from the pendulum rod to cancel out the effects of the rod's downward expansion.

Invar which has been treated in various ways is capable of all possible expansions between two limits close to zero and a bob of any desired metal will, when attached to the nut of the previously selected rod, correct the extensions of the rod by its upward expansion.

The advantages are many. Apart from the presence in the oscillating system of a liquid, Graham's pendulum has the drawback of being actually compensated only provided that the temperature is uniform from top to bottom of the clock case, a condition which is fully satisfied only in competently constructed installations.

It will be noticed that for this specific application, the instability of or-

dinary invar does not matter since the state of a clock must be checked from time to time, so that a slow change in its daily rate entails no error in the time measurement. After a few year's operations, this change will be reduced to two or three hundredths of a second per year.

The compensating spring of watches – A watch fitted with a steel balance spring and a brass balance loses, as we have seen, about 11 seconds per degree per day, almost the whole of this change being attributable to the variation in Young's modulus of the metal from which the spring is made. A correcting mechanism for watches is thus indicated.

In Ferdinand Berthoud's « marine clocks » this mechanism comprised a bimetallic plate which automatically modified the active length of the spring. In 1775 Arnold invented the compensating balance which was soon perfected by Earnshaw and which consists of a diametral arm, from the two extremities of which project semi-circular, bimetallic plates, steel inside, brass outside, which curve in towards the axis when the temperature rises, so reducing the moment of inertia of the assembly in the mean ratio of the change undergone by the elasticity modulus of the balance spring.

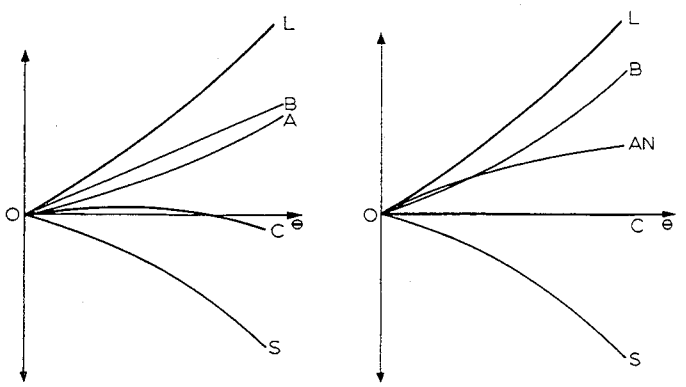
If the steel of the spring is replaced by a nickel steel of composition such that the maximum or minimum modulus occurs at ordinary temperatures, the watch, fitted with a monometallic balance, will function alike at two temperatures at either side of the ambient, e.g. 0° and 30° C, and between the two will gain or lose most at 15° C. The trend of the curve is such that the maximum gain (the loss occurs with alloys having a low elastic limit) will be 20 to 25 seconds per day compared to the running at extreme temperatures, this being twelve or fifteen times less than the discrepancies observed in the case of the steel balance spring. This progress has been considered so important by watchmakers that almost three million watches are equipped annually with the compensator spring.

Correction of secondary error of chronometers by the integral balance – Arnold and Earnshaw's balance does not compensate entirely the action of temperature on the rate of watches. Ferdinand Berthoud noticed in 1775 a «secondary error» which Dent found in 1832 for the balance, and which consists in the fact that a watch equipped with a steel balance spring compensated by a steel-brass balance gains 2 to 3 seconds per day at 15° C, if it functions correctly at 0° C and 30° C.

Considerable efforts were made with a view to eliminating Dent's error.

We shall see how a rational application of a nickel steel afforded a very simple means to achieve that.

The reason for Dent's error lies in the following fact: the elasticity modulus of the balance spring varies with temperature in accordance with a pronounced curve OS (Fig. 10a). The action of the balance, for its part, is controlled by the difference in expansibility between the brass OL and the steel OA, expressed by functions in which the coefficients β are very approximately the same. Their difference OB will hence be linear and the algebraic sum of OS and OB, i.e. OC, will leave a quadratic remainder.



Figs. 10a, 10b. Diagrams showing the cause of the secondary error of chronometers and the principle of its correction.

Let us now replace OA by a curve OAN (Fig. 10b) representing the expansibility of a nickel steel with negative β. The difference with OL will be a curve OB which can be made symmetrical with OS and thus a zero algebraic sum is obtained at all its points.

In 1899 I made the design calculations for such a balance, the low expansion constituent of which was a steel containing 45% Ni, and in 1900 two Swiss chronometer makers, P. D. Nardin and P. Ditisheim, achieved complete temperature compensation by its aid.

From then on the new balance has replaced the old practically everywhere in precision chronometers. Rate logs thus became obsolete, not only owing to the elimination of the secondary error but because once chronometers no longer had this error it became important to attempt to correct the lesser defects. In fact, the rates of the best chronometers today are four to five times less erratic than twenty years ago.

The complete solution to the problem of compensation by the elinvar balance spring – For more than ten years I thought that the use of nickel steels had led to all the fundamental improvements in compensating mechanisms of which they were capable. Yet, in about 1912, a faint inkling occurred to me of a means to achieve perhaps the ultimate solution to the compensation problem. When studying ternary alloys I had just had a glimpse of the simultaneous reduction in the anomaly of expansibility and thermoelasticity.

A series of experiments was embarked on during the following year using two series of alloys with varying nickel contents and with 5% and 10% additions of chromium. The maximum thermoelastic coefficient in this second series was already very close to that of elinvar and only a very slight modification of the composition was needed to produce an alloy forming balance springs which in conjunction with monometallic balance wheels vouchsafed that the watches in which they were fitted ran almost the same at various temperatures and were almost free from the secondary error. The problem was hence very close to its solution. Then, after a pause, the experiments could be resumed; they recently resulted in the manufacture of balance springs which, in conjunction with a suitably chosen monometallic balance, constitute a regulating device almost entirely immune to temperature changes.

Such a simple solution as this to a long standing problem is regarded as revolutionary by the most competent chronometer makers. However, its very simplicity, if its retention is insisted upon, conceals a further slight defect.

The particular and immense advantage of the new combination is that of leading very close to perfection at the first attempt by the association of a balance spring having a low, constant thermoelastic coefficient with a balance of suitable expansibility. For industrial manufacture the balance metal will be matched as closely as possible to the requirements of the balance spring; the range of expansions between invar and brass affords ample scope for doing so. Nevertheless once the combination has been found it is not, at least in its immediate form, suitable for those progressive and meticulous small alterations which make the task of the regulator an art at once so difficult and so exciting.

To reduce the scope of this reservation to its correct proportions forthwith, I should state that at the present time it is very common for the combination of a balance spring and monometallic balance to be manufactured. the temperature-induced error of which is less than one hundredth of that which would occur were a steel spring used.

Currently the situation is that whereas the compensating spring used to be restricted to use in ordinary watches, it is now becoming established in the precision watch sector; at present it is still only barred from precision chronometry. However it has only just appeared and it would be presumptuous to think that it has reached the limit of its perfection at the first attempt.

The simplicity of the new compensating mechanism entails many advantages. Bimetallic plates invariably upset regulators slightly who see in them a possible cause of instability; deformations by centrifugal force necessitate ensuring that the large and small arcs are isochronous by slight alterations to the shape of the balance spring; lastly the rectangular shape and the presence of setting screws cause air to be entrained, so giving rise to a « barometric coefficient » whose value is sufficiently high for the changes in atmospheric pressure to constitute in certain circumstances the essential cause of rate variations in the present state of chronometry. On the other hand it is easy to taper the cross-section of a monometallic balance wheel, reducing as far as possible the entrainment of air. These various points enhance the relative advantages of the new balance spring and should justify relaxing the stipulations governing the ultimate limits of compensation.

Conclusion

Let us now glance back to the origins of the studies of which the results have just been reported. The point at issue was essentially to look for a metal having qualities which would enable it to be used to make standards of length that are inexpensive while satisfying metrological requirements. An initial solution had been found which required only slight refinement and it was during the search for this slight refinement that an unsuspected anomaly came to light which was a direct manifestation of the struggle between the various states of iron to which the association of nickel gives an unexpected shape, creating a class of alloys whose properties, unique so far, satisfy in a quite unhoped-for manner the wishes which metrologists would have formulated had it not been assumed that such properties could not exist in metals.

Then the problem broadened: from the measurement of length, which was the only aim of the original programme, the measurement of time arose of its own accord as a direct and to some extent obvious consequence, so true is it that in metrology everything is related and that a carefully executed measurement invariably entails generalizations.

The whole of the research programme required the constant collaboration of a factory and a laboratory. The great advantage of cooperation of this type is nowadays commonly appreciated and it would be superfluous to insist on it except to highlight a particular feature.

When in the spring of 1896, the Société de Commentry-Fourchambault & Decazeville undertook to assist a programme of research which, failing such assistance, would not have advanced beyond its initial stages, they did so without the directors imagining that they would be rewarded with anything but the satisfaction of having lent effective assistance to a task of scientific exploration. But the meticulous examination to which the alloys were subjected, and the increasingly stringent requirements imposed on the company as regards the compositions as well as the treatments, led, in the jointly explored field, to the creation of a kind of precision metallurgy, the result of which has been the manufacture of products considered impossible a few years ago.

That has been a new and unexpected outcome of the scope of precision measurements. Demanding unflagging and occasionally laborious effort of those who apply themselves to it, precision measurement research could not long be pursued without the guidance and illumination of an ideal of perfection. For the research worker himself, this ideal stimulated by a belief in the utility – even afar off – of every unselfish effort is his mainstay at all times and the counsellor of every initiative.

The highest approval which you have just conferred on my work highlights a task stretching back a quarter of a century and for that reason I feel obliged right here to express my deepest gratitude to the illustrious members of the Nobel Committee and of the Swedish Academy of Sciences, as well as to the memory of Alfred Nobel. From generation to generation my descendants will look with pride on the magnificent diploma and the Nobel medal, treasured testimony of your kindness and esteem. But also I wish to express my gratitude to you on behalf of the International Bureau which I have served for thirty-seven years and whose existence reflects at this moment the dramatic changes occurring in the world. Next I am grateful to you on behalf of metrology on which the conferment of a Nobel Prize casts a bright lustre well-fitted to commend it as a vocation. Lastly my gratitude is on behalf of the two countries with which my life is associated: Switzerland, of which I am a citizen and which I left equipped for life, and France, where I have known so many esteemed friendships.

Biography

Charles-Édouard Guillaume was born at Fleurier, in the Swiss-Jura, on February 15, 1861. His grandfather had left France for political reasons during the Revolution and established a watchmaking business in London. The business was carried on by his three sons but Charles' father, Édouard, eventually returned to settle in Fleurier.

Guillaume received his early education in Neuchâtel before going to the Zurich Polytechnic where he obtained his doctor's degree. He spent a short time as an officer in the artillery before entering the International Bureau of Weights and Measures, as an assistant, in 1883. He became Associate Director in 1902 and from 1915 until his retirement in 1936, he was Director of the Bureau. He remained as Honorary Director from 1936 until his death.

During his brief military career, Guillaume studied mechanics and ballistics but his earliest investigations at the bureau were with thermometry. He carried out important investigations on corrections to mercury-in-glass thermometers and he was responsible for the detailed calibration of thermometers used at the Bureau in the establishment of the thermal expansions of the standards of length. He was concerned in initial work on the International Metre and undertook a determination of the volume of one kilogram of water by the contact method.

A chance observation by Guillaume on the coefficient of expansion of nickel-iron alloys led to a systematic investigation of a whole series of alloys and the discovery of invar, an alloy with a very low coefficient of expansion; elinvar, for which the thermoelastic coefficient is practically zero, i.e. Young's modulus constant, over a considerable temperature range; together with other useful alloys. The applications of invar were quickly recognized and the material was used in rapid methods for the measurement of geodetic baselines. The alloy is widely used in instruments of precision, such as thermostats and pendulums of astronomic clocks. Guillaume's total compensating balance for high-grade watches and chronometers, which eliminates the secondary error, was perfected by an elinvar hair spring.

Guillaume's work is recorded in many papers published by the Bureau

and he has written, amongst other works, *Études thermométriques* (Studies on Thermometry, 1886), *Traité de thermométrie* (Treatise on Thermometry, 1889), *Unités et Étalons* (Units and Standards, 1894), *Les rayons X* (X-Rays, 1896), *Recherches sur le nickel et ses alliages* (Investigations on Nickel and its Alloys, 1898), *La vie de la matière* (The Life of Matter, 1899), *La Convention du Mètre et le Bureau international des Poids et Mesures* (Metrical Convention and the International Bureau of Weights and Measures, 1902), *Les applications des aciers au nickel* (Applications of Nickel-Steels, 1904), *Des états de la matière* (States of Matter, 1907), *Les récent progrès du système métrique* (Recent progress in the Metric System, 1907, 1913). His book *Initiation à la Mécanique* (Introduction to Mechanics) has been translated into several languages.

He was appointed Grand Officer of the Legion of Honour and received honorary Doctor of Science degrees from the Universities of Geneva, Neuchâtel and Paris. He was a President of the Société Française de Physique and a member, honorary member or corresponding member of more than a dozen of the leading scientific academies of Europe.

Charles-Édouard Guillaume married Mlle. A. M. Taufflieb in 1888. They had three children. He died on May 13, 1938.

Physics 1921

ALBERT EINSTEIN

«for his services to Theoretical Physics, and especially for his discovery of the law of the photoelectric effect»

Physics 1921

Presentation Speech by Professor S. Arrhenius, Chairman of the Nobel Committee for Physics of the Royal Swedish Academy of Sciences

Your Majesty, Your Royal Highnesses, Ladies and Gentlemen.

There is probably no physicist living today whose name has become so widely known as that of Albert Einstein. Most discussion centres on his theory of relativity. This pertains essentially to epistemology and has therefore been the subject of lively debate in philosophical circles. It will be no secret that the famous philosopher Bergson in Paris has challenged this theory, while other philosophers have acclaimed it wholeheartedly. The theory in question also has astrophysical implications which are being rigorously examined at the present time.

Throughout the first decade of this century the so-called Brownian movement stimulated the keenest interest. In 1905 Einstein founded a kinetic theory to account for this movement by means of which he derived the chief properties of suspensions, i.e. liquids with solid particles suspended in them. This theory, based on classical mechanics, helps to explain the behaviour of what are known as colloidal solutions, a behaviour which has been studied by Svedberg, Perrin, Zsigmondy and countless other scientists within the context of what has grown into a large branch of science, colloid chemistry.

A third group of studies, for which in particular Einstein has received the Nobel Prize, falls within the domain of the quantum theory founded by Planck in 1900. This theory asserts that radiant energy consists of individual particles, termed «quanta», approximately in the same way as matter is made up of particles, i.e. atoms. This remarkable theory, for which Planck received the Nobel Prize for Physics in 1918, suffered from a variety of drawbacks and about the middle of the first decade of this century it reached a kind of impasse. Then Einstein came forward with his work on specific heat and the photoelectric effect. This latter had been discovered by the famous physicist Hertz in 1887. He found that an electrical spark passing between two spheres does so more readily if its path is illuminated with the light from another electrical discharge. A more exhaustive study of this interesting phenomenon was carried out by Hallwachs who showed that under certain con-

ditions a negatively charged body, e.g. a metal plate, illuminated with light of a particular colour – ultraviolet has the strongest effect – loses its negative charge and ultimately assumes a positive charge. In 1899 Lenard demonstrated the cause to be the emission of electrons at a certain velocity from the negatively charged body. The most extraordinary aspect of this effect was that the electron emission velocity is independent of the intensity of the illuminating light, which is proportional only to the number of electrons, whereas the velocity increases with the frequency of the light. Lenard stressed that this phenomenon was not in good agreement with the then prevailing concepts.

An associated phenomenon is photo-luminescence, i.e. phosphorescence and fluorescence. When light impinges on a substance the latter will occasionally become luminous as a result of phosphorescence or fluorescence. Since the energy of the light quantum increases with the frequency, it will be obvious that a light quantum with a certain frequency can only give rise to the formation of a light quantum of lower or, at most, equal frequency. Otherwise energy would be created. The phosphorescent or fluorescent light hence has a lower frequency than the light inducing the photo-luminescence. This is Stokes' rule which was explained in this way by Einstein by means of the quantum theory.

Similarly, when a quantum of light falls on a metal plate it can at most yield the whole of its energy to an electron there. A part of this energy is consumed in carrying the electron out into the air, the remainder stays with the electron as kinetic energy. This applies to an electron in the surface layer of the metal. From this can be calculated the positive potential to which the metal can be charged by irradiation. Only if the quantum contains sufficient energy for the electron to perform the work of detaching itself from the metal does the electron move out into the air. Consequently, only light having a frequency greater than a certain limit is capable of inducing a photo-electric effect, however high the intensity of the irradiating light. If this limit is exceeded the effect is proportional to the light intensity at constant frequency. Similar behaviour occurs in the ionisation of gas molecules and the so-called ionisation potential may be calculated, provided that the frequency of the light capable of ionising the gas is known.

Einstein's law of the photo-electrical effect has been extremely rigorously tested by the American Millikan and his pupils and passed the test brilliantly. Owing to these studies by Einstein the quantum theory has been perfected to a high degree and an extensive literature grew up in this field whereby

the extraordinary value of this theory was proved. Einstein's law has become the basis of quantitative photo-chemistry in the same way as Faraday's law is the basis of electro-chemistry.*

* Being too remote from Sweden, Professor Einstein could not attend the ceremony.

ALBERT EINSTEIN

Fundamental ideas and problems of the theory of relativity

*Lecture delivered to the Nordic Assembly of Naturalists at Gothenburg**
July 11, 1923

If we consider that part of the theory of relativity which may nowadays in a sense be regarded as bona fide scientific knowledge, we note two aspects which have a major bearing on this theory. The whole development of the theory turns on the question of whether there are physically preferred states of motion in Nature (physical relativity problem). Also, concepts and distinctions are only admissible to the extent that observable facts can be assigned to them without ambiguity (stipulation that concepts and distinctions should have meaning). This postulate, pertaining to epistemology, proves to be of fundamental importance.

These two aspects become clear when applied to a special case, e.g. to classical mechanics. Firstly we see that at any point filled with matter there exists a preferred state of motion, namely that of the substance at the point considered. Our problem starts however with the question whether physically preferred states of motion exist in reference to *extensive* regions. From the viewpoint of classical mechanics the answer is in the affirmative; the physically preferred states of motion from the viewpoint of mechanics are those of the inertial frames.

This assertion, in common with the basis of the whole of mechanics as it generally used to be described before the relativity theory, far from meets the above « stipulation of meaning ». Motion can only be conceived as the relative motion of bodies. In mechanics, motion relative to the system of coordinates is implied when merely motion is referred to. Nevertheless this interpretation does not comply with the « stipulation of meaning » if the coordinate system is considered as something purely imaginary. If we turn our attention to experimental physics we see that there the coordinate system is invariably represented by a « practically rigid » body. Furthermore it is assumed that such rigid bodies can be positioned in rest relative to one another

* The Lecture was not delivered on the occasion of the Nobel Prize award, and did not, therefore, concern the discovery of the photoelectric effect.

in common with the bodies of Euclidian geometry. Insofar as we may think of the rigid measuring body as existing as an object which can be experienced, the «system of coordinates» concept as well as the concept of the motion of matter relative thereto can be accepted in the sense of the «stipulation of meaning». At the same time Euclidian geometry, by this conception, has been adapted to the requirements of the physics of the «stipulation of meaning». The question whether Euclidian geometry is valid becomes physically significant; its validity is assumed in classical physics and also later in the special theory of relativity.

In classical mechanics the inertial frame and time are best defined together by a suitable formulation of the law of inertia: It is possible to fix the time and assign a state of motion to the system of coordinates (inertial frame) such that, with reference to the latter, force-free material points undergo no acceleration; furthermore it is assumed that this time can be measured without disagreement by identical clocks (systems which run down periodically) in any arbitrary state of motion. There are then an infinite number of inertial frames which are in uniform translational motion relative to each other, and hence there is also an infinite number of mutually equivalent, physically preferred states of motion. Time is absolute, i.e. independent of the choice of the particular inertial frame; it is defined by more characteristics than logically necessary, although – as implied by mechanics – this should not lead to contradictions with experience. Note in passing that the logical weakness of this exposition from the point of view of the stipulation of meaning is the lack of an experimental criterion for whether a material point is force-free or not; therefore the concept of the inertial frame remains rather problematical. This deficiency leads to the general theory of relativity. We shall not consider it for the moment.

The concept of the rigid body (and that of the clock) has a key bearing on the foregoing consideration of the fundamentals of mechanics, a bearing which there is some justification for challenging. The rigid body is only approximately achieved in Nature, not even with desired approximation; this concept does not therefore strictly satisfy the «stipulation of meaning». It is also logically unjustifiable to base all physical consideration on the rigid or solid body and then finally reconstruct that body atomically by means of elementary physical laws which in turn have been determined by means of the rigid measuring body. I am mentioning these deficiencies of method because in the same sense they are also a feature of the relativity theory in the schematic exposition which I am advocating here. Certainly it would be

logically more correct to begin with the whole of the laws and to apply the «stipulation of meaning» to this whole first, i.e. to put the unambiguous relation to the world of experience last instead of already fulfilling it in an imperfect form for an artificially isolated part, namely the space-time metric. We are not, however, sufficiently advanced in our knowledge of Nature's elementary laws to adopt this more perfect method without going out of our depth. At the close of our considerations we shall see that in the most recent studies there is an attempt, based on ideas by Levi-Civita, Weyl, and Eddington, to implement that logically purer method.

It more clearly follows from the above what is implied by «preferred states of motion». They are preferred as regards the laws of Nature. States of motion are preferred when, relative to the formulation of the laws of Nature, coordinate systems within them are distinguished in that with respect to them those laws assume a form preferred by simplicity. According to classical mechanics the states of motion of the inertial frames in this sense are physically preferred. Classical mechanics permits a distinction to be made between (absolutely) unaccelerated and accelerated motions; it also claims that velocities have only a relative existence (dependent on the selection of the inertial frame), while accelerations and rotations have an absolute existence (independent of the selection of the inertial frame). This state of affairs can be expressed thus: According to classical mechanics «velocity relativity» exists, but not «acceleration relativity». After these preliminary considerations we can pass to the actual topic of our contemplations, the relativity theory, by characterizing its development so far in terms of principles.

The special theory of relativity is an adaptation of physical principles to Maxwell-Lorentz electrodynamics. From earlier physics it takes the assumption that Euclidian geometry is valid for the laws governing the position of rigid bodies, the inertial frame and the law of inertia. The postulate of equivalence of inertial frames for the formulation of the laws of Nature is assumed to be valid for the whole of physics (special relativity principle). From Maxwell-Lorentz electrodynamics it takes the postulate of invariance of the velocity of light in a vacuum (light principle).

To harmonize the relativity principle with the light principle, the assumption that an absolute time (agreeing for all inertial frames) exists, had to be abandoned. Thus the hypothesis is abandoned that arbitrarily moved and suitably set identical clocks function in such a way that the times shown by two of them, which meet, agree. A specific time is assigned to each inertial frame; the state of motion and the time of the inertial frame are defined, in

accordance with the stipulation of meaning, by the requirement that the light principle should apply to it. The existence of the inertial frame thus defined and the validity of the law of inertia with respect to it are assumed. The time for each inertial frame is measured by identical clocks that are stationary relative to the frame.

The laws of transformation for space coordinates and time for the transition from one inertial frame to another, the Lorentz transformations as they are termed, are unequivocally established by these definitions and the hypotheses concealed in the assumption that they are free from contradiction. Their immediate physical significance lies in the effect of the motion relative to the used inertial frame on the form of rigid bodies (Lorentz contraction) and on the rate of the clocks. According to the special relativity principle the laws of Nature must be covariant relative to Lorentz transformations; the theory thus provides a criterion for general laws of Nature. It leads in particular to a modification of the Newtonian point motion law in which the velocity of light in a vacuum is considered the limiting velocity, and it also leads to the realization that energy and inertial mass are of like nature.

The special relativity theory resulted in appreciable advances. It reconciled mechanics and electrodynamics. It reduced the number of logically independent hypotheses regarding the latter. It enforced the need for a clarification of the fundamental concepts in epistemological terms. It united the momentum and energy principle, and demonstrated the like nature of mass and energy. Yet it was not entirely satisfactory – quite apart from the quantum problems, which all theory so far has been incapable of really solving. In common with classical mechanics the special relativity theory favours certain states of motion – namely those of the inertial frames – to all other states of motion. This was actually more difficult to tolerate than the preference for a single state of motion as in the case of the theory of light with a stationary ether, for this imagined a real reason for the preference, i.e. the light ether. A theory which from the outset prefers no state of motion should appear more satisfactory. Moreover the previously mentioned vagueness in the definition of the inertial frame or in the formulation of the law of inertia raises doubts which obtain their decisive importance, owing to the empirical principle for the equality of the inertial and heavy mass, in the light of the following consideration.

Let K be an inertial frame without a gravitational field, K′ a system of coordinates accelerated uniformly relative to K. The behaviour of material points relative to K′ is the the same as if K′ were an inertial frame in respect

of which a homogeneous gravitational field exists. On the basis of the empirically known properties of the gravitational field, the definition of the inertial frame thus proves to be weak. The conclusion is obvious that any arbitrarily moved frame of reference is equivalent to any other for the formulation of the laws of Nature, that there are thus no physically preferred states of motion at all in respect of regions of finite extension (general relativity principle).

The implementation of this concept necessitates an even more profound modification of the geometric-kinematical principles than the special relativity theory. The Lorentz contraction, which is derived from the latter, leads to the conclusion that with regard to a system K′ arbitrarily moved relative to a (gravity field free) inertial frame K, the laws of Euclidian geometry governing the position of rigid (at rest relative to K′) bodies do not apply. Consequently the Cartesian system of coordinates also loses its significance in terms of the stipulation of meaning. Analogous reasoning applies to time: with reference to K′ the time can no longer meaningfully be defined by the indication on identical clocks at rest relative to K′, nor by the law governing the propagation of light. Generalizing, we arrive at the conclusion that gravitational field and metric are only different manifestations of the same physical field.

We arrive at the formal description of this field by the following consideration. For each infinitesimal point-environment in an arbitrary gravitational field a local frame of coordinates can be given for such a state of motion that relative to this local frame no gravitational field exists (local inertial frame). In terms of this inertial frame we may regard the results of the special relativity theory as correct to a first approximation for this infinitesimally small region. There are an infinite number of such local inertial frames at any space-time point; they are associated by Lorentz transformations. These latter are characterised in that they leave invariant the «distance» ds of two infinitely adjacent point events – defined by the equation:

$$ds^2 = c^2dt^2 - dx^2 - dy^2 - dz^2$$

which distance can be measured by means of scales and clocks. For, x, y, z, represent coordinates and time measured with reference to a local inertial frame.

To describe space-time regions of finite extent arbitrary point coordinates in four dimensions are required which serve no other purpose than to pro-

vide an unambiguous designation of the space-time points by four numbers each, x_1, x_2, x_3 and x_4, which takes account of the continuity of this four-dimensional manifold (Gaussian coordinates). The mathematical expression of the general relativity principle is then, that the systems of equations expressing the general laws of Nature are equal for all such systems of coordinates.

Since the coordinate differentials of the local inertial frame are expressed linearly by the differentials dx_ν of a Gaussian system of coordinates, when the latter is used, for the distance ds of two events an expression of the form

$$ds^2 = \Sigma g_{\mu\nu}\, dx_\mu dx_\nu \quad (g_{\mu\nu} = g_{\nu\mu})$$

is obtained. The $g_{\mu\nu}$, which are continuous functions of x_ν, determine the metric in the four-dimensional manifold where ds is defined as an (absolute) parameter measurable by means of rigid scales and clocks. These same parameters $g_{\mu\nu}$ however also describe with reference to the Gaussian system of coordinates the gravitational field which we have previously found to be identical with the physical cause of the metric. The case as to the validity of the special relativity theory for finite regions is characterised in that when the system of coordinates is suitably chosen, the values of $g_{\mu\nu}$ for finite regions are independent of x_ν.

In accordance with the general theory of relativity the law of point motion in the pure gravitational field is expressed by the equation for the geodetic line. Actually the geodetic line is the simplest mathematically which in the special case of constant $g_{\mu\nu}$ becomes rectilinear. Here therefore we are confronted with the transfer of Galileo's law of inertia to the general theory of relativity.

In mathematical terms the search for the field equations amounts to ascertaining the simplest generally covariant differential equations to which the gravitational potentials $g_{\mu\nu}$ can be subjected. By definition these equations should not contain higher derivatives of $g_{\mu\nu}$ with respect to x_ν than the second, and these only linearly, which condition reveals these equations to be a logical transfer of the Poisson field equation of the Newtonian theory of gravity to the general theory of relativity.

The considerations mentioned led to the theory of gravity which yields the Newtonian theory as a first approximation and furthermore it yields the motion of the perihelion of Mercury, the deflection of light by the sun, and the red shift of spectral lines in agreement with experience.*

* As regards the red shift, the agreement with experience is not yet completely assured, however.

To complete the basis of the general theory of relativity, the electromagnetic field must still be introduced into it which, according to our present conviction, is also the material from which we must build up the elementary structures of matter. The Maxwellian field equations can readily be adopted into the general theory of relativity. This is a completely unambiguous adoption provided it is assumed that the equations contain no differential quotients of $g_{\mu\nu}$ higher than the first, and that in the customary Maxwellian form they apply in the local inertial frame. It is also easily possible to supplement the gravitational field equations by electromagnetic terms in a manner specified by the Maxwellian equations so that they contain the gravitational effect of the electromagnetic field.

These field equations have not provided a theory of matter. To incorporate the field generating effect of ponderable masses in the theory, matter had therefore (as in classical physics) to be introduced into the theory in an approximate, phenomenological representation.

And that exhausts the direct consequences of the relativity principle. I shall turn to those problems which are related to the development which I have traced. Already Newton recognized that the law of inertia is unsatisfactory in a context so far unmentioned in this exposition, namely that it gives no real cause for the special physical position of the states of motion of the inertial frames relative to all other states of motion. It makes the observable material bodies responsible for the gravitational behaviour of a material point, yet indicates no material cause for the inertial behaviour of the material point but devises the cause for it (absolute space or inertial ether). This is not logically inadmissible although it is unsatisfactory. For this reason E. Mach demanded a modification of the law of inertia in the sense that the inertia should be interpreted as an acceleration resistance of the bodies against *one another* and not against « space ». This interpretation governs the expectation that accelerated bodies have concordant accelerating action in the same sense on other bodies (acceleration induction).

This interpretation is even more plausible according to general relativity which eliminates the distinction between inertial and gravitational effects. It amounts to stipulating that, apart from the arbitrariness governed by the free choice of coordinates, the $g_{\mu\nu}$-field shall be completely determined by the matter. Mach's stipulation is favoured in general relativity by the circumstance that acceleration induction in accordance with the gravitational field equations really exists, although of such slight intensity that direct detection by mechanical experiments is out of the question.

Mach's stipulation can be accounted for in the general theory of relativity by regarding the world in spatial terms as finite and self-contained. This hypothesis also makes it possible to assume the mean density of matter in the world as *finite*, whereas in a spatially infinite (quasi-Euclidian) world it should disappear. It cannot, however, be concealed that to satisfy Mach's postulate in the manner referred to a term with no experimental basis whatsoever must be introduced into the field equations, which term logically is in no way determined by the other terms in the equations. For this reason this solution of the « cosmological problem » will not be completely satisfactory for the time being.

A second problem which at present is the subject of lively interest is the identity between the gravitational field and the electromagnetic field. The mind striving after unification of the theory cannot be satisfied that two fields should exist which, by their nature, are quite independent. A mathematically unified field theory is sought in which the gravitational field and the electromagnetic field are interpreted only as different components or manifestations of the same uniform field, the field equations where possible no longer consisting of logically mutually independent summands.

The gravitational theory, considered in terms of mathematical formalism, i.e. Riemannian geometry, should be generalized so that it includes the laws of the electromagnetic field. Unfortunately we are unable here to base ourselves on empirical facts as when deriving the gravitational theory (equality of the inertial and heavy mass), but we are restricted to the criterion of mathematical simplicity which is not free from arbitrariness. The attempt which at present appears the most successful is that, based on the ideas of Levi-Civita, Weyl and Eddington, to replace Riemannian metric geometry by the more general theory of affine correlation.

The characteristic assumption of Riemannian geometry is the attribution to two infinitely adjacent points of a « distance » ds, the square of which is a homogeneous second order function of the coordinate differentials. It follows from this that (apart from certain conditions of reality) Euclidian geometry is valid in any infinitely small region. Hence to every line element (or vector) at a point P is assigned a parallel and equal line element (or vector) through any given infinitesimally adjacent point P′ (affine correlation). Riemannian metric determines an affine correlation. Conversely, however, when an affine correlation (law of infinitesimal parallel displacement) is mathematically given, generally no Riemannian metric determination exists from which it can be derived.

The most important concept of Riemannian geometry, « space curvature », on which the gravitational equations are also based, is based exclusively on the « affine correlation ». If one is given in a continuum, without first proceeding from a metric, it constitutes a generalization of Riemannian geometry but which still retains the most important derived parameters. By seeking the simplest differential equations which can be obeyed by an affine correlation there is reason to hope that a generalization of the gravitation equations will be found which includes the laws of the electromagnetic field. This hope has in fact been fulfilled although I do not know whether the formal connection so derived can really be regarded as an enrichment of physics as long as it does not yield any new physical connections. In particular a field theory can, to my mind, only be satisfactory when it permits the elementary electrical bodies to be represented as solutions free from singularities.

Moreover it should not be forgotten that a theory relating to the elementary electrical structures is inseparable from the quantum theory problems. So far also relativity theory has proved ineffectual in relation to this most profound physical problem of the present time. Should the form of the general equations some day, by the solution of the quantum problem, undergo a change however profound, even if there is a complete change in the parameters by means of which we represent the elementary process, the relativity principle will not be relinquished and the laws previously derived therefrom will at least retain their significance as limiting laws.

Biography

Albert Einstein was born at Ulm, in Württemberg, Germany, on March 14, 1879. Six weeks later the family moved to Munich and he began his schooling there at the Luitpold Gymnasium. Later, they moved to Italy and Albert continued his education at Aarau, Switzerland and in 1896 he entered the Swiss Federal Polytechnic School in Zurich to be trained as a teacher in physics and mathematics. In 1901, the year he gained his diploma, he acquired Swiss citizenship and, as he was unable to find a teaching post, he accepted a position as technical assistant in the Swiss Patent Office. In 1905 he obtained his doctor's degree.

During his stay at the Patent Office, and in his spare time, he produced much of his remarkable work and in 1908 he was appointed Privatdozent in Berne. In 1909 he became Professor Extraordinary at Zurich, in 1911 Professor of Theoretical Physics at Prague, returning to Zurich in the following year to fill a similar post. In 1914 he was appointed Director of the Kaiser Wilhelm Physical Institute and Professor in the University of Berlin. He became a German citizen in 1914 and remained in Berlin until 1933 when he renounced his citizenship for political reasons and emigrated to America to take the position of Professor of Theoretical Physics at Princeton. He became a United States citizen in 1940 and retired from his post in 1945.

After World War II, Einstein was a leading figure in the World Government Movement, he was offered the Presidency of the State of Israel, which he declined, and he collaborated with Dr. Chaim Weizmann in establishing the Hebrew University of Jerusalem.

Einstein always appeared to have a clear view of the problems of physics and the determination to solve them. He had a strategy of his own and was able to visualize the main stages on the way to his goal. He regarded his major achievements as mere stepping-stones for the next advance.

At the start of his scientific work, Einstein realized the inadequacies of Newtonian mechanics and his special theory of relativity stemmed from an attempt to reconcile the laws of mechanics with the laws of the electromagnetic field. He dealt with classical problems of statistical mechanics and

problems in which they were merged with quantum theory: this led to an explanation of the Brownian movement of molecules. He investigated the thermal properties of light with a low radiation density and his observations laid the foundation of the photon theory of light.

In his early days in Berlin, Einstein postulated that the correct interpretation of the special theory of relativity must also furnish a theory of gravitation and in 1916 he published his paper on the general theory of relativity. During this time he also contributed to the problems of the theory of radiation and statistical mechanics.

In the 1920's, Einstein embarked on the construction of unified field theories, although he continued to work on the probabilistic interpretation of quantum theory, and he persevered with this work in America. He contributed to statistical mechanics by his development of the quantum theory of a monatomic gas and he has also accomplished valuable work in connection with atomic transition probabilities and relativistic cosmology.

After his retirement he continued to work towards the unification of the basic concepts of physics, taking the opposite approach, geometrisation, to the majority of physicists.

Einstein's researches are, of course, well chronicled and his more important works include *Special Theory of Relativity* (1905), *Relativity* (English translations, 1920 and 1950), *General Theory of Relativity* (1916), *Investigations on Theory of Brownian Movement* (1926), and *The Evolution of Physics* (1938). Among his non-scientific works, *About Zionism* (1930), *Why War?* (1933), *My Philosophy* (1934), and *Out of My Later Years* (1950) are perhaps the most important.

Albert Einstein received honorary doctorate degrees in science, medicine and philosophy from many European and American universities. During the 1920's he lectured in Europe, America and the Far East and he was awarded Fellowships or Memberships of all the leading scientific academies throughout the world. He gained numerous awards in recognition of his work, including the Copley Medal of the Royal Society of London in 1925, and the Franklin Medal of the Franklin Institute in 1935.

Einstein's gifts inevitably resulted in his dwelling much in intellectual solitude and, for relaxation, music played an important part in his life. He married Mileva Maritsch in 1901 and they had two sons; their marriage was dissolved and in 1917 he married his cousin, Elsa Einstein, who died in 1936. He died on April 18, 1955 at Princeton, New Jersey.

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