NOTICES

OF THE

PROCEEDINGS

AT THE

MEETINGS OF THE MEMBERS

OF THE

Royal Institution of Great Britain,

WITH

ABSTRACTS OF THE DISCOURSES

DELIVERED AT

THE EVENING MEETINGS.

VOLUME XIII.

1890—1892.

LONDON:
PRINTED BY WILLIAM CLOWES AND SONS, LIMITED,
STAMFORD STREET AND CHARING CROSS.
1893.

Patron.

HER MOST GRACIOUS MAJESTY

QUEEN VICTORIA.

Vice-Patron and Honorary Member.

HIS ROYAL HIGHNESS

THE PRINCE OF WALES, K.G. F.R.S.

President—THE DUKE OF NORTHUMBERLAND, K.G. D.C.L. LL.D.
Treasurer—SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S.—*V.P.*
Honorary Secretary—SIR FREDERICK BRAMWELL, BART. D.C.L. LL.D. F.R.S. M. INST. C.E.—*V.P.*

MANAGERS. 1892–93.

Sir Frederick Abel, K.C.B. D.C.L. F.R.S.—*V.P.*
Captain W. de W. Abney, C.B. R.E. D.C.L. F.R.S.
George Berkley, Esq. M. Inst. C.E.
Shelford Bidwell, Esq. M.A. F.R.S.
Joseph Brown, Esq. C.B. Q.C.
Arthur Herbert Church, Esq. M.A. F.R.S.
Sir Andrew Clark, Bart. M.D. LL.D. F.R.S.
Sir Douglas Galton, K.C.B. D.C.L. LL.D. F.R.S.—*V.P.*
The Right Hon. Lord Halsbury, M.A. D.C.L. F.R.S.—*V.P.*
William Huggins, Esq. D.C.L. LL.D. F.R.S.—*V.P.*
David Edward Hughes, Esq. F.R.S.—*V.P.*
The Right Hon. Lord Kelvin, D.C.L. LL.D. Pres. R.S.—*V.P.*
Hugo Müller, Esq. Ph.D. F.R.S.
John Rae, M.D. LL.D. F.R.S.
William Chandler Roberts-Austen, Esq. C.B. F.R.S.

VISITORS. 1892–93.

Thomas Buzzard, M.D. F.R.C.P.
Michael Carteighe, Esq. F.C.S.
Andrew Ainslie Common, Esq. F.R.S. F.R.A.S.
James Farmer, Esq. J.P.
Robert Hannah, Esq.
George Herbert, Esq.
Donald William Charles Hood, M.D. F.R.C.P.
James Mansergh, Esq. M. Inst. C.E.
Lachlan Mackintosh Rate, Esq. M.A.
John Callander Ross, Esq.
Arthur William Rücker, Esq. M.A. F.R.S.
Sir David Salomons, Bart. M.A. F.R.A.S. F.C.S.
John Bell Sedgwick, Esq. J.P. F.R.G.S.
John Isaac Thornycroft, Esq. M. Inst. C.E.
Robert Wilson, Esq. M. Inst. C.E.

Professors.

Honorary Professor of Natural Philosophy—JOHN TYNDALL, Esq. D.C.L. LL.D. F.R.S. &c.
Professor of Natural Philosophy—The Right Hon. LORD RAYLEIGH, M.A. D.C.L. LL.D. F.R.S. &c.
Fullerian Professor of Chemistry—JAMES DEWAR, Esq. M.A. LL.D. F.R.S. &c.
Fullerian Professor of Physiology—VICTOR HORSLEY, Esq. F.R.S. B.S. F.R.C.S.

Honorary Librarian—Mr. Benjamin Vincent.
Keeper of the Library and Assistant Secretary—Mr. Henry Young.
Clerk of Accounts and Collector—Mr. Henry C. Hughes.
Assistants in the Laboratories—Mr. R. N. Lennox, Mr. J. W. Heath, and Mr. G. Gordon.
Assistant in the Library—Mr. Herbert C. Fyfe.

CONTENTS.

1890.

PLATES.

Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, January 24, 1890.

Sir Frederick Abel, C.B. D.C.L. F.R.S. Vice-President, in the Chair.

Professor Dewar, M.A. F.R.S. *M.R.I.*

The Scientific Work of Joule.

(Abstract.)

Prof. Dewar commenced by remarking that the Royal Institution had been so closely identified with the great workers in physical science that it was impossible to allow the work of Joule, whose researches had produced as marked a revolution in Physical Science as Darwin's in Biology, to pass without recognition in the present series of Friday Evening Discourses. Sir William Thomson, as Joule's friend and fellow-worker to the last, had been invited to undertake the duty, and had agreed to do so; but at the last moment had been compelled to decline by reason of important official duties in Scotland, and the task had consequently devolved upon him.

Having given a brief account of Joule's parentage, early life, and education, Prof. Dewar reviewed, as fully as time would permit, his scientific work, which extended over about forty years, and was represented by 115 original memoirs. The first period (1838 to 1843) was distinguished as that in which Joule educated himself in experimental methods, chiefly in connection with electricity and electro-magnetic engines. This work led him in 1840 to his first great discovery, the true law governing the relation between electric energy and thermal evolution, which enabled him later on to account for the whole distribution of the current, not only in the battery in which it is produced, but in conductors exterior to it. Joule was thus led to take up the study of electrolysis. Faraday had already made the discovery that electrolytic bodies could be split up into equivalent proportions by the passage of the same electric current; Joule saw that there would be great difficulty in finding out the distribution of the current energy, and accounting for the whole of it. After a laborious research he succeeded in showing that during electrolytic action there was an absorption of heat equivalent to the heat evolved during the original combination of the constituents of the compound body. The prosecution of his electrical researches rapidly brought Joule on the road to his great discovery of the Mechanical Equivalent of Heat, it being clear from a foot-note to a paper dated 18th February, 1843, that he already had well in hand the study of the strict relations between chemical, electrical, and mechanical effects.

In working out these laws, it was to be remarked that Joule—in common with most inventors and seekers after new scientific truths—chose perhaps the most difficult means that could have been selected;

and in looking back at his work in the light of present knowledge, it seemed simply astounding that he should have succeeded so completely as he did. The original coil used by Joule for the mechanical determination of heat (kindly lent for the occasion by Prof. Rücker) was shown, and the course of the experiment explained. The vast difficulties which Joule had to overcome in order to prove that there was a definite, permanent, and persistent relation between the amount of mechanical energy expended and the heat produced were commented on; the thermal effects being produced not directly but through the medium of an electric current varying in intensity, and calculations having to be made not only for these fluctuations, but for the effects of radiation, the movement of the air, and other indirect complications. The very small increment of heat to be measured obliged Joule to use thermometers of great delicacy, and these he had to devise and construct himself. One of the thermometers so used was exhibited.

Working in this way, Joule was able by the end of July, 1843, to state definitely that the amount of heat capable of increasing the temperature of a pound of water by 1° F. was equal to, and might be converted into, a mechanical force capable of raising 838 lbs. to the height of one foot. Soon afterwards he attained almost identical results by a more direct method—the friction of water passing through small tubes—which gave him 770 foot pounds per unit of heat.

It was impossible, said the lecturer, to thoroughly appreciate Joule's work without glancing at the early history of the subject; and when one did so it was amazing to find how near men of the stamp of Rumford, Davy, and Young had been to Joule's great discovery, and yet missed it. Count Rumford was the first to clearly define the relation between the constant production of heat and loss of movement by frictional motion. He proved that the amount of heat produced by friction was continuous, and apparently unlimited; but he did not think of measuring the relation between the mechanical energy expended and the amount of heat produced. Alluding to the results obtained from this apparatus, the lecturer said that Count Rumford might have shown that in his experiments the heat produced was proportional to the time of working, and so obtained a result capable of being expressed in horse-power. The value so deducted from Rumford's experiments is not far removed from Joule's first number.

The experiments commenced by Count Rumford were carried on by Davy, at that time working with Beddoes at Bristol; and led to one of the most remarkable essays on heat of that period, which disposed for ever of the theory of the separate existence of caloric. Taking two pieces of ice on a cold day, Davy mounted them so that they could be rotated against each other with frictional pressure, the effect being that the pieces of ice were melted, and the water so produced had a much higher specific heat than the original ice. To guard against the possibility of heat being conveyed to the frictional apparatus by the surrounding air, Davy made an experiment in vacuo, isolating the apparatus by means of ice; and found that under such conditions sufficient heat could be produced to melt wax placed in the receiver. The lecturer here showed an experiment illustrating the production of water by the friction of two pieces of ice in vacuo, under conditions of temperature much more severe than those of Davy's experiment.

Following Davy, Young devoted a great deal of attention to the subject, and by 1812 he and Davy had quite changed their opinions, and had adopted the view that heat and motion were convertible effects.

Having by July 1843 assured himself of the principle of his discovery, Joule now devoted himself to the elaboration of methods of working, modifying and repeating experiments in various ways, but always approaching nearer and nearer to exactness, as shown by the following Table of results:—

Joule's values of the Mechanical Equivalent of Heat.

		Kilogramme metres.
Magneto-electric currents	1843	460
Friction of water in tubes	,,	424·6
Diminution of heat produced in a battery current when the current produces work	,,	499
Compression of air	1845	443·8
Expansion of air	,,	437·8
Friction of water	,,	488·3
,, ,, ,,	1847	428·9
,, ,, ,,	1850	423·9
,, ,, mercury	,,	424·7
,, ,, iron	,,	425·2
Heat developed in Daniel's cell	,,	419·5
,, ,, in wire of known absolute resistance	1867	429·5
Friction of water in calorimeter	1878	423·9

Prof. Dewar here exhibited and explained the action of the original calorimeter used by Joule. It was seen to consist of a set of vanes which were made to revolve in water by the falling of known weights through a definite and known height, the heat produced being due (after making the necessary deduction for the friction due to the momentum of the weights) entirely to the friction of the fluid. It was found that whatever fluid was employed, the same definite results were obtained:—a production of heat in the liquid bearing a constant relation to the unit of mechanical energy expended. The extreme delicacy of Joule's apparatus, and the marvellous accuracy of his observations were shown by the fact that working with weights of 29 lbs. each, and repeating each observation 20 times, the total increase of temperature did not exceed half a degree Fahrenheit. In contrast to this the lecturer showed, by means of apparatus kindly lent by Prof. Ayrton, the method now employed for repeating Joule's work and arriving at substantially the same results by much simpler means

While continuing to work intermittently at his great discovery, Joule employed himself in the following years in elaborate investigations bearing upon the point of maximum density of water, specific gravity, and atomic volumes. An illustration of his method of determining maximum density was given by means of two large cylinders filled with water and connected by a narrow channel in which was placed a floating indicator. It was shown that the slightest variation in density of the water of either cylinder—variations far beyond the scope of the most delicate thermometer—set up currents which were immediately detected by the movement of the indicator, and that by this means it was quite possible to ascertain the exact temperature at which water attained its maximum density.

Joule's determinations of atomic volumes were marvellous at the time they were made, and were still interesting. Illustrations of his work in this direction were given by means of a solution of sugar, which was seen to occupy practically the same space as was occupied by an amount of water exactly equivalent to that combined in the carbohydrate. The carbon hypothetically combined with the water to form the sugar appearing to make no sensible difference to the volume; and in contrast to this was seen the enormous difference in volume brought about by dissolving two equal portions of soda carbonate, one portion being ordinary hydrated crystals and the other portion being anhydrous, in equal volumes of water.

Joule's last great research was carried out conjointly with Sir William Thomson, and occupied nearly ten years of laborious enquiry. Its chief object was to prove that in compressing a gas the amount of heat produced is equivalent to the work done, and independent of the mere fact of the approach of the particles. But Joule was desirous of amplifying the enquiry, and in fact the work might be divided into three sections: (1) the study of gases passing through narrow apertures; (2) the velocity attained by bodies passing through the air; and (3) the temperature ultimately attained by such moving bodies. With respect to 2 and 3, it was shown that a body rotating in the air at the rate of about 150 to 180 feet per second increased in temperature by nearly 1° F., and that this increase of temperature was definite for a given velocity, and independent of the size of the moving mass and the density of the gaseous medium. With regard to (1) the relation of gaseous pressure and volume to temperature, the researches of Regnault had already shown that the simple law of Marriotte and Boyle could not stand by itself; and Joule sought to modify it by the study of gases passing through very small tubes or porous bodies. The investigations were carried out at Manchester on a large scale, and were assisted by a Government grant. Steam engines were employed to maintain a current of gas at a constant temperature and pressure through long coils of pipe placed in water tanks. They proved that any difference of temperature in the gas brought about in its passage through the porous body must be due to work done by it, and that this difference of temperature varied for different gases, according to their constitution.

Working under the same conditions, hydrogen was shown to be reduced a small amount in temperature, air somewhat more (about 0·3°), and carbonic acid a much greater amount. A repetition of Joule and Thomson's experiment was shown by means of a 100 feet coil of lead pipe, compressed hydrogen, air, and carbonic acid gas being employed, and the original results verified in each case. The effect of this research was to enable Joule and Thomson to formulate a great improvement on the gaseous laws; for instead of the product of the volume and pressure being strictly proportional to the absolute temperature, as it had been hitherto believed to be, they found that a new term was involved, which is equivalent to a constant divided by the absolute temperature and multiplied by the volume.

In conclusion, Prof. Dewar read the following letter, which he had received from Sir Lyon Playfair in response to his request for some reminiscences of Joule:—

DEAR DEWAR, *20th Jan.*, 1890.

You ask for some of my memories of Joule from 1842 to 1845, when I was Professor of Chemistry at the Royal Institution in Manchester. The great Dalton died in the autumn of 1844, and had long been President of the Manchester Philosophical Society. He naturally gave impulse to the study of science in that town, where there was an active band of young workers in research.

Joule was, even then, foremost among these; and the names of Binney, Williamson, Schunck, Angus Smith, Young, and others show that the spirit of scientific inquiry was active. We were also stimulated by the fact that Baron Liebig and Bunsen came to pay me visits during that time; they were men to excite research.

Joule was a man of singular simplicity and earnestness. We used to meet at each other's houses at supper, to help the progress of our work by discussion. Joule was an earnest worker, and was then engaged on his experiments on the mechanical equivalent of heat. He took me to his small laboratory to show me his experiments, and I of course quickly recognised that my young friend the brewer was a great philosopher. We jointly worked upon questions of far less importance than his great central discovery, but he was equally interested. I was very anxious that he should devote his life to science, and persuaded him to become candidate for the Professorship of Natural Philosophy at St. Andrews. He was on the point of securing this, but his slight personal deformity was an objection in the eyes of one of the electors; and St. Andrews lost the glory of having one of the greatest discoverers of our age.

When Joule first sent an account of his experiments to the Royal Society, the paper was referred, among others, to Sir Charles Wheatstone, who was my intimate personal friend. Wheatstone was an eminently fair man and a good judge, but the discovery did not then recommend itself to his mind. For a whole Sunday afternoon we walked on Barnes Common, discussing the experiments and their consequences, if true, to science. But all my arguments were insufficient to convince my friend; and I fear that then the Royal Society did not appreciate and publish the researches. I write from memory only, for I know that, later, no society or institution honoured Joule more than the Royal Society and its members.

Not for one moment, however, did Joule hesitate in the accuracy of his experiments or his conclusions. He once suggested to me that we might take a trip together to the Falls of Niagara, not to look at its beauties, but to ascertain the difference of temperature of the water at the top and bottom of the fall. Of course the change of motion into heat was a necessary consequence of his views.

No more pleasant memory of my life remains than the fact that, side by side, at my lectures in the Royal Institution, used to sit the illustrious Dalton, with his beautiful face, so like that of Newton, and the keenly intelligent Joule. I can give no other explanation than the fact of organic chemistry being then a new science that two philosophers of such eminence should come to the lectures of a mere tyro in science. I used to look upon them as two types of the highest progress in science. Newton had introduced law, order, and number into the movements of masses of matter in the universe; Dalton introduced the same into the minute masses which we call atoms; and Joule, with a keen insight into the operations and correlation of forces, connected them together and showed their mutual equivalence.

I do not know whether these memories are of any use to you, but, such as they are, they are at your disposal for your lecture on the friend of my youth.

Yours sincerely,

Lyon Playfair.

WEEKLY EVENING MEETING,

Friday, January 31, 1890.

Sir Frederick Bramwell, Bart. D.C.L. F.R.S. Honorary Secretary and Vice-President, in the Chair.

Sir Frederick Abel, C.B. D.C.L. D.Sc. F.R.S. *V.P.R.I.*

Smokeless Explosives.

The production of smoke which attends the ignition or explosion of gunpowder is often a source of considerable inconvenience in connection with its application to naval or military purposes, its employment in mines, and its use by the sportsman, although occasions not unfrequently arise during naval and military operations when the shroud of smoke produced by musketry or artillery fire, has proved of important advantage to one or other, or to both, of the belligerents during different periods of an engagement.

Until within the last few years, however, but little, if any, thought appears to have been given to the possibility of dispensing with or greatly diminishing the production of smoke in the application of fire-arms, excepting in connection with sport. The inconvenience and disappointment often resulting from the obscuring effects of a neighbouring gun-discharge, or of the first shot from a double-barrel arm, led the sportsman to look hopefully to gun-cotton, directly after its first production in 1846, as a probable source of greater comfort and brighter prospects in the pursuit of his pastime and in his strivings for success.

A comparison between the chemical changes attending the burning, explosion, or metamorphosis of gun-cotton and of gunpowder, serves to explain the cause of the production of smoke in the latter case, and the reason of smokelessness in the case of gun-cotton. Whilst the products of explosion of the latter consist exclusively of gases, and of water which assumes the transparent form of highly-heated vapour at the moment of its production, the explosive substances classed as gunpowder, composed of mixtures of saltpetre, or another nitrate of a metal, with charred wood or other carbonised vegetable matter, and with variable quantities of sulphur, furnish products, of which very large proportions are not gaseous, even at high temperatures. Upon the ignition of such a mixture, these products are in part deposited in the form of a fused residue, which constitutes the fouling in a fire-arm, and are in part distributed, in an extremely fine state of division, through the gases and vapours developed by the explosion, thus producing smoke.

In the case of gunpowder of ordinary composition, the solid products amount to over fifty per cent. by weight of the total products of explosion, and the dense white smoke which it produces, consists in part of extremely finely divided potassium carbonate which is a component of the solid products, and, to a great extent, of potassium sulphate produced chiefly by the burning of one of the important solid products of explosion, potassium sulphide, when it is carried in a fine state of division into the air by the rush of gas.

With other explosives, which are also smoke-producing, the formation of the smoke is due to the fact that one or other of the products, although existing as vapour at the instant of its development, is immediately condensed to a cloud composed of minute liquid particles, or of vesicles, as in the case of mercury vapour liberated upon the explosion of mercuric fulminate, or of the aqueous vapour produced upon the ignition of a mixture of ammonium nitrate and charcoal, or ammonium nitrate and picric acid.

Until within the last half-dozen years, the varieties of gunpowder which have been applied to war purposes in this and other countries have exhibited comparatively few variations in chemical composition. The proportions of charcoal, saltpetre, and sulphur, employed in their production, exhibit slight differences in different countries, and these, as well as the character of the charcoal used, its sources and method of production, underwent but little modification for very many years. The same remark applies to the nature of the successive operations pursued in the manufacture of black powder for artillery purposes in this and other countries.

The replacement of smooth-bore guns by rifled artillery, which followed the Crimean war, and the increase in the size and power of guns consequent upon the application of armour to ships and forts, soon called for the pursuit of investigations having for their object the attainment of means for variously modifying the action of fired gunpowder, so as to render it suitable for the different calibres of guns, whose full power could not be effectively, or in some instances safely, developed by the use of the kind of gunpowder previously employed indiscriminately in artillery of all known calibres.

In order to control the violence of explosion of gunpowder, by modifying the rapidity of transmission of explosion from particle to particle, or through the mass of each individual particle, of which the charge of a gun is composed, the accomplishment of the desired results was, in the first instance, and indeed throughout practical investigations extending over many years, sought exclusively in modifications of the size and form of the individual masses composing a charge of powder, and of their density and hardness; it being considered that, as the proportions of saltpetre, charcoal and sulphur, generally employed in the production of gunpowder, very nearly correspond to those required for the development of the greatest chemical energy by those incorporated materials, it was advisable to seek for the attainment of the desired results by modifications of the physical and mechanical characters of gunpowder, rather than by any modification in the proportions and chemical characters of its ingredients.

The varieties of powder, which, as the outcome of careful practical and scientific researches in this direction, have been introduced into artillery-service from time to time, and some of which, at any rate, have proved fairly efficient, have been of two distinct types. The first of these, produced by breaking up more or less highly-pressed cakes of black powder into grains, pebbles, or boulders, of approximately uniform size and shape, the sharp edges and rough surfaces being afterwards removed by attrition (reeling and glazing), are simply a further development of one of the original forms of granulated or corned powder, represented by the old F. G., or small-arms, and L. G., or cannon-powder. Gunpowders of this class, ranging in size from about 1000 pieces to the ounce, to about six pieces to the pound, have been introduced into artillery-service, and certain of them, viz. R. L. G. (rifle large grain), which was the first step in advance upon the old cannon-powder (L. G.); pebble-powder (P), and large pebble or boulder-powder (P 2), are still employed more or less extensively in some guns of the present day.

The other type of powder has no representative among the more ancient varieties; it has its origin in the obviously sound theoretical view that uniformity in the results furnished by a particular powder, when employed under like conditions, demands not merely identity in regard to composition, but also identity in form, size, density, and structure of the individual masses composing the charge used in a gun. The practical realisation of this view should obviously be attained, or at any rate approached, by submitting equal quantities of one and the same mixture of ingredients, presented in the form of powder of uniform fineness and dryness, to a uniform pressure for a fixed period in moulds of uniform size, and under surrounding conditions as nearly as possible alike. The fulfilment of these conditions would, moreover, have to be supplemented by an equally uniform course of proceeding in the subsequent drying and other finishing processes to which the powder-masses would be submitted.

The only form of powder, introduced into our artillery-service for a brief period, in the production of which these conditions were adhered to as closely as possible, was a so-called pellet powder, which consisted of small cylinders, having semi-perforations with the object of increasing the total inflaming surface of the individual masses.

Practical experience with this powder, and with others prepared upon the same system, but with much less rigorous regard to uniformity in such details as state of division and condition of dryness of the powder before its compression into cylindrical or other forms, showed that uniformity in the ballistic properties of black powder could be as well and even more readily secured by the thorough blending or mixing together of batches presenting some variation in regard to density, hardness, or other features, as by aiming at an approach to absolute uniformity in the characters of each individual mass composing a charge.

At the time that our attention was first actively given to this subject of the modification of the ballistic properties of powder, it had already been to some extent dealt with in the United States by Rodman and Doremus, and the latter was the first to propose the application, as charges for guns, of powder-masses produced by the compression of coarsely grained powder into moulds of prismatic form. In Russia the first step was taken to utilise the results arrived at by Doremus, and to adopt a prismatic powder for use in guns of large calibre.

Side by side with the development and perfection of the manufacture of prismatic powder in Russia, Germany, and in this country, new experiments on the production of powder-masses suitable, by their comparatively gradual action, for employment in the very large charges required for the heavy artillery of the present day, by the powerful compression of mixtures of more or less finely broken up powder-cake into masses of greater size than those of the pebble, pellet, and prism powders, were actively pursued in Italy, and also by our own Government Committee on Explosives, and the outcome of very exhaustive practical investigations were the very efficient Fossano powder, or *poudre progressif*, of the Italians, and the boulder and large cylindrical powders known as P^2 and C^2, produced at Waltham Abbey, which scarcely vied, however, with the Italian powder in the uniformity of their ballistic properties.

Researches carried out by Captain Noble and the lecturer some years ago with a series of gunpowders differing considerably in composition from each other, indicated that advantages might be secured in the production of powders for heavy guns by so modifying the proportions of the constituents (e. g. by considerably increasing the proportion of charcoal and reducing the proportion of sulphur) as to give rise to the production of a much greater volume of gas, and at the same time to diminish the heat developed by the explosion.

These researches served, among other purposes, to throw considerable light upon the cause of the wearing or erosive action of powder-explosions upon the inner surface of the gun, which in time produces so serious a deterioration of the arm that the velocity of projection and accuracy of shooting suffer very greatly, an effect the extent of which increases in an increasing ratio to the size of the guns, in consequence, obviously, of the large increase in the weight of the charges fired.

Several causes undoubtedly combine to bring about the wearing away of the gun's bore, which is especially great where the products of explosion, while under the maximum pressure, can escape between the projectile and the bore. The great velocity with which the very highly heated gaseous and liquid (fused solid) products of explosion sweep over the heated surface of the metal gives rise to a displacement of the particles composing it, which increases as the surface becomes roughened by the first action upon the least compact portions of the metal, and thus opposes greater resistance; at the same time, the effect of the high temperature to which the surface is raised is to reduce its rigidity and power of resisting the force of the gaseous torrent, and lastly some amount of chemical action upon the metal, by certain of the highly heated non-gaseous products of explosion, contributes towards an increase in the erosive effects. A series of careful experiments made by Captain Noble with powders of different composition, and with other explosives, afforded decisive evidence that the explosive agent which furnished the largest proportion of gaseous products, and the explosion of which was attended by the development of the smallest amount of heat, exerted least erosive action.

It is probable that important changes in the composition of powders manufactured by us for our heavy guns would have resulted from those researches, but in the meantime, two eminent German gunpowder manufacturers had occupied themselves independently, and simultaneously, with the important practical question of producing some more suitable powder for heavy guns than the various new forms of ordinary black powder, the rate of burning of which, especially when confined in a close chamber, was, after all, reduced only in a moderate degree by the increase in the size of the masses, and by such increase in their density as it was practicable to attain. The German experimenters directed their attention not merely to an alteration of the proportions of the powder ingredients, but also to a modification in the character of charcoal employed, and the success attending their labours in these directions led to the practically simultaneous production, by Mr. Heidemann at the Westphalian Powder Works, and Mr. Düttenhofer at the Rottweil Works near Hamburg, of a prismatic powder of cocoa-brown colour, consisting of saltpetre in somewhat higher proportion, of sulphur in much lower proportion, than in normal black powder, and of very slightly burned charcoal, similar in composition to the charcoal (*charbon roux*) which Violette, a French chemist, first produced in 1847 by the action of superheated steam upon wood or other vegetable matter, and which he proposed for employment in the manufacture of sporting powder. These brown prismatic powders (or "cocoa-powders," as they were termed from their colour), are distinguished from black powder not only by their appearance, but also by their very slow combustion in open air, by their comparatively gradual and long-sustained action when used in guns, and by the simple character of their products of explosion as compared with those of black powder. As the oxidising ingredient, saltpetre, is contained, in brown or cocoa powder, in larger proportion relatively to the oxidisable components sulphur and charcoal than in black powder, these become fully oxidised, while the products of explosion of the latter contain, on the other hand, larger proportions of unoxidised material, or of only partially oxidised products. Moreover, there is produced upon the explosion of brown powder a relatively very large amount of water-vapour, not merely because the finished powder contains a larger proportion of water than black powder, but also because the very slightly charred wood or straw used in the brown powder is much richer in hydrogen than black charcoal, and therefore furnishes by its oxidation a considerable amount of water. The total volume of gas furnished by the brown powder (at 0° C. and 760 mm. barometer) is only about 200 volumes per kilogramme of powder, against 278 volumes furnished by a normal sample of black powder, but the amount of water-vapour furnished upon its explosion is about three times that produced from black powder, and this would make the volume of gas and vapour developed by the two powders about equal if the heat of its explosion were the same in the two cases; the actual temperature produced by the explosion of brown powder is, however, somewhat the higher of the two.

Although the smoke produced upon firing a charge of brown powder from a gun appears at first but little different in denseness to that of black powder, it certainly disperses much more rapidly, a difference which is probably due to the speedy absorption, by solution, of the finely divided potassium salts by the large proportion of water-vapour distributed throughout the so-called smoke.

This class of powder was substituted with considerable advantage for black powder in guns of comparatively large calibre; nevertheless it became desirable to attain even slower or more gradual action in the case of the very large charges required for guns of the heaviest calibres, such as those which propel shot of about 2000 lbs. weight. Accordingly, the brown powder has been modified in regard to the proportions of its ingredients to suit these conditions, while, on the other hand, powder intermediate with respect to rapidity of action between black pebble powder and the brown powder, has been found more suitable than the former for use in guns of moderately large calibre.

The recent successful adaptation of machine guns and comparatively large quick-firing guns to naval service, more especially for the defence of ships against attack by torpedo boats, &c., has rendered the provision of a powder for use with them, which would produce comparatively little or no smoke, a matter of very considerable importance, inasmuch as the efficiency of such defence must be greatly diminished by the circumstance that, after a very brief use of the guns with black powder, the objects against which their fire is destined to operate, become more or less completely hidden from those directing them, by the dense veil of powder-smoke produced. Hence much attention has been directed during the last few years to the production of smokeless, or nearly smokeless powders for naval use in the above directions. At the same time, the views of many military authorities regarding the importance of dispensing with smoke in land engagements has also created a demand, the apparent urgency of which has been increased by various circumstances, for a smokeless powder suitable for field artillery and small arms.

The properties of ammonium nitrate, of which the products of decomposition by heat are, in addition to water-vapour, entirely gaseous, have rendered it a tempting material to work upon in the hands of those who have striven to produce a smokeless powder, but its deliquescent character has been the chief obstacle to its application as a component of an explosive agent susceptible of substitution for black powder for service purposes.

A German chemical engineer, F. Gäns, conceived that, by incorporating charcoal and saltpetre with a particular proportion of ammonium nitrate, he had produced an explosive material which did not partake of the hygroscopic character common to other ammonium-nitrate mixtures, and that, by its explosion, the potassium in the saltpetre formed a volatile combination with nitrogen and hydrogen, a *potassium amide*, so that, although containing nearly half its weight of potassium salt, it would furnish only volatile products. The views of Mr. Gäns regarding the changes which his so-called *amide powder* undergoes upon explosion were not borne out by existing chemical knowledge, while the powder compounded in accordance with his views proved to be by no means smokeless, and was certainly not non-hygroscopic. Mr. Heidemann has, however, been successful, by modifications of Gäns's prescription and by application of his own special experience in powder-manufacture, in producing an ammonium nitrate powder possessed of remarkable ballistic properties, furnishing comparatively little smoke, which speedily disperses, and exhibiting the hygroscopic characteristics of ammonium nitrate preparations in a decidedly less degree than any other hitherto prepared. The powder, while yielding a very much larger volume of gas and water-vapour than black or brown powder, is considerably slower than the latter; the charge required to produce equal ballistic results is less, while the chamber-pressure developed is lower, and the pressures along the chase of the gun are higher, than in the case of brown powder.

The ammonium nitrate powder contains, in its normal, dried condition, more water than even brown powder; it does not exhibit any great tendency to absorb moisture from an ordinarily dry or even a somewhat moist atmosphere, but if the amount of atmospheric moisture approaches saturation, it will rapidly absorb water, and when once the process begins it continues rapidly, the powder masses becoming speedily quite pasty. The charges for quick-firing guns are enclosed in metal cases, in which they are securely sealed up; the powder is therefore prevented from absorbing moisture from the external air, but it has been found that if the cartridges are kept for long periods in ships' magazines, in which, from their position relatively to the ship's boilers, the temperature is more or less elevated, sometimes for considerable periods, the expulsion of water from some portions of the powder-masses composing the hermetically sealed charge, and its consequent irregular distribution, may give rise to a want of uniformity in the action of the powder, and to the occasional development of high pressures. Although, therefore, this ammonium-nitrate powder may be regarded as the first successful advance towards the production of a comparatively smokeless artillery-powder, it is not uniformly well adapted to the requirements which it should fulfil in naval service.

Attention was first seriously directed to the subject of smokeless powder by the reports received about four years ago of remarkable results stated to have been obtained in France with such a powder for use with the magazine rifle (the Lebel) which was being adapted to military service. These Reports were speedily followed by others, descriptive of marvellous velocities obtained with small charges of this powder, or some modifications of it, from guns of very great length. As in the case of mélinite, the fabulously destructive effects of which were much vaunted at about the same time, the secret of the precise nature of the smokeless powder was so well preserved by the French authorities, that surmises could only be made on the subject even by those most conversant with these matters. It is now well known, however, that more than one smokeless explosive has succeeded the original powder, the perfection of which was reported to be beyond dispute, and that the material now adopted for use in the Lebel rifle bears, at any rate, great similarity to preparations which have been made the subject of patents in this country, and which are still experimental powders in other countries.

So far as smokelessness is concerned, no material can surpass *gun-cotton* pure and simple; but, even if its rate of combustion in a fire-arm could be controlled with certainty and uniformity, although only used in very small charges, such as are required for military rifles, its application as a safe and reliable propulsive agent for military and naval use is attended by so many difficulties, that the non-success of the numerous attempts, made in the first twenty-five years of its existence, to apply it in this direction, is not surprising.

Soon after its discovery by Schönbein and Böttger in 1846, endeavours were made to apply gun-cotton wool, rammed into cases, as a charge for small arms, but with disastrous results. Subsequently von Lenk, who made the first practical approach to the regulation of the explosive power of gun-cotton, produced small arm cartridges by superposing layers of gun-cotton threads, these being closely plaited round a core of wood. Von Lenk's system of regulating the rapidity of burning of gun-cotton, so as to suit it either for gradual or violent action, consists, in fact, in converting coarse or fine, loosely or lightly twisted, threads or rovings of finely carded cotton into the most explosive form of gun-cotton, and of arranging these threads or yarns in different ways so as to modify the mechanical condition i. e. the compactness and extent and distribution of enclosed air-spaces, of the mass of gun-cotton composed of them. Thus, small arm cartridges were composed, as already stated, of compact layers of tightly plaited, fine gun-cotton thread; cannon cartridges were made up of coarse, loose gun-cotton yarn wound very compactly upon a core; charges for shells consisted of very loose cylindrical hollow plaits (like lamp wicks) along which fire flashed almost instantaneously; and mining charges were made in the form of a very tightly twisted rope with a hollow core. While the two latter forms of gun-cotton always burned with almost instantaneous rapidity in open air, and with highly destructive effects if they were strongly confined, the tightly wound or plaited masses burned slowly in air, and would frequently exert their explosive force so gradually when confined in a firearm, as to produce good ballistic results without appreciably destructive effect upon the arm. Occasionally, however, in consequence of some slight unforeseen variation in the compactness of the material, or in the amount and disposition of the air-spaces in the mass, very violent action would be produced, showing that this system of regulating the explosive force of gun-cotton was quite unreliable.

Misled by the apparently promising nature of the earliest results which von Lenk obtained, the Austrian Government embarked, in 1862, upon a somewhat extensive application of von Lenk's gun-cotton to small arms, and provided several batteries of field guns for the use of this material. The abandonment of these measures for applying a smokeless explosive to military purposes soon followed upon the attainment of unsatisfactory results, and was hastened by the occurrence of a very destructive explosion at gun-cotton stores at Simmering, near Vienna, in 1862.

It was at about this time that the attention of the English Government, and through them of the lecturer, was directed to the subject of gun-cotton, the Austrian Government having communicated details regarding improvements in its manufacture accomplished by von Lenk, and results obtained in the extended experiments which had been carried out on its application to the various purposes above indicated, according to the system devised by that officer. One of the results of the lecturer's researches, subsequently carried on at Woolwich and Waltham Abbey, was his elaboration of the system of manufacture and employment of gun-cotton which has been in extensive use at the government works with little if any modification for over eighteen years, and has been copied from us by France, Germany, and other countries. By reducing the partially purified gun-cotton-fibre to pulp as in the ordinary process of making paper, completing its purification when in that condition, and afterwards converting the finely-divided explosive into highly compressed homogeneous masses of any desired form and size, very important improvements were effected in its stability, its uniformity of composition and action, and its adaptability to practical uses, a great advance being made in the exercise of control over the rapidity of combustion or explosion of the material.

No success had attended the experiments instituted in England with wound cannon cartridges of gun-cotton-threads made according to von Lenk's plan; on the other hand a number of results which at first sight appeared very promising, were obtained at Woolwich in 1867–8 with bronze field-guns and cartridges built up of compressed gun-cotton-masses arranged in different ways (with varied air-spaces, &c.) with the object of regulating the rapidity of explosion of the charge. But although the attainment of high velocities with comparatively small charges of the material, unaccompanied by any indications of injury to the gun, was frequent, it became evident that the fulfilment of the conditions essential to safety to the arm were exceedingly difficult to attain with certainty; they appeared indeed to be altogether beyond absolute control, even in so small a gun as the twelve-pounder. Military authorities not being, in those days, alive to the advantages which might accrue from the employment of an entirely smokeless explosive in artillery, the lecturer received no encouragement to persevere with experiments in this direction, and the same was the case with respect to the possible use of a smokeless explosive in military small arms, with which, however, far more promising results had at that time been obtained at Woolwich.

Abel's system of preparing gun-cotton was no sooner elaborated than its application to the production of smokeless cartridges for sporting purposes was achieved with considerable success by Messrs. Prentice of Stowmarket. The first gun-cotton cartridge, which found considerable favour with sportsmen, consisted of a roll of felt-like paper composed of gun-cotton and ordinary cotton, and produced from a mixture of the pulped materials. Afterwards a cylindrical pellet of slightly compressed gun-cotton pulp was used, the rapidity of explosion of which was retarded, while it was at the same time protected from absorption of moisture, by impregnation with a small proportion of india-rubber. Neither of these cartridges afforded promise of sufficient uniformity of action to fulfil military requirements, but after a series of experiments which the lecturer made with compressed gun-cotton arranged in various ways, very promising results were attained, especially with the Martini-Henry rifle and a charge of pellet-form, the rapidity of explosion of which was regulated by simple means.

A sporting powder which was nearly smokeless had, in the meantime, been produced by Colonel Schultze, of the Prussian Artillery, from wood cut up into very small cube-like fragments, converted into a mild form of nitro-cellulose after a preliminary purifying treatment, and impregnated with a small portion of an oxidising agent. Subsequently the manufacture of the Schultze powder was considerably modified; it was converted into the granular form and rendered considerably more uniform in character and less hygroscopic, and it then bore considerable resemblance to the E.C. powder, a granulated nitro-cotton powder, produced, in the first instance, at Stowmarket, and consisting of a less highly nitrated cotton than gun-cotton (trinitro-cellulose), incorporated in the pulped condition with a somewhat considerable proportion of the nitrates of potassium and barium, and converted into grains through the agency of a solvent and a binding material. Both of these powders produced some smoke when fired, though the amount was small in comparison with that from black powder. They did not compete with the latter in regard to accuracy of shooting, when used in arms of precision, but they are interesting as being the forerunners of a variety of so-called smokeless powders, of which gun-cotton or some form of nitro-cellulose is the basis, and of which those of Johnson and Borland, and of the Smokeless Powder Company, are the most prominent in this country.

In past years, both camphor and liquid solvents, such as acetic ether and acetone, for gun-cotton, and mixtures of ether and alcohol for nitro-cotton, have been applied to the hardening of the surfaces of compressed masses or granules of those materials, by von Förster and others, with a view to render them non-porous, and in the E.C. powder manufacture the latter solvent was thus applied to harden the powder-granules. In the Johnson-Borland powder, camphor is applied to the same purpose; in smokeless powders of French and German manufacture acetic ether and acetone have been used, and the solvent has been applied, not merely to harden the granules or tablets of the explosive, but to convert the latter into a homogeneous horn-like material.

Much mystery has surrounded the nature and origin of the first smokeless powder adopted, apparently with undue haste, by the French Government, for use with the Lebel magazine rifle. A few particles of the Vieille powder, or *Poudre* B, were seen by the lecturer about two years ago, and very small specimens appear to have fallen into the hands of the German Government about that time. They were in the form of small yellowish-brown tablets of about 0·07 inch to 0·1 inch square, of the thickness of stout notepaper, and had evidently been produced by cutting up thin sheets of the material. They appeared to contain, as an important ingredient, picric acid (the basis of "mélinite") a substance extensively used as a dye, and obtained by the action of nitric acid, at a low temperature, upon carbolic acid and cresylic acid, constituents of coal tar. Originally produced by the action of nitric acid upon indigo, and afterwards by similar treatment of Botany Bay gum, it was first known as carbazotic acid, and is one of the earliest of known explosives of organic origin. When sufficiently heated, or when set light to, it burns with a yellow smoky flame, and even very large quantities of it have been known to burn away somewhat fiercely, but without exploding. Under certain conditions, however, and especially if subjected to the action of a powerful detonator, it explodes with very great violence and highly destructive effects, as pointed out by Sprengel in 1873, and recent experiments at Woolwich have shown that it does this even, as in the case of gun-cotton, when it contains as much as 15 per cent. of water. It is no longer a secret that picric acid at any rate forms the basis of the much-vaunted and mysterious explosive for shells for which the French Government were said to have paid a very large sum of money, and the destructive effects of which have been described as nothing less then marvellous. M. Turpin patented, in 1875, the use of picric acid alone as an explosive for shells and for other engines of destruction, and whether or not his claims to be the inventor of mélinite are valid, there appears no doubt that his patent in France was the starting-point of the development and adoption of that explosive.

The attention thus directed in France to the properties of picric acid appears to have given rise to experiments resulting in its employment as an ingredient of the first smokeless powder (*Poudre B*) adopted for the French magazine rifle.

The idea of employing picric acid preparations as explosive agents for propulsive purposes originated with Designolle about twenty years ago, but no useful results attended the experiments with the particular mixtures proposed by him. It is certain that the recent adaptation of that substance in France was of a different character, and that, promising as were the results of the new smokeless powder, of which it appears to have formed an ingredient, and a counterpart of which was made the subject of experiments at Woolwich about three years ago, its deficiency in the all-essential quality of stability must have been, at any rate, one cause of its abandonment in favour of another form of smokeless powder, which there is reason to believe is of more simple character.

In Germany, the subject of smokeless powder for small arms and artillery was being steadily pursued in secret, while the sensational reports concerning *Poudre B* were spread about in France, and a small-arm powder giving excellent results in regard to ballistic properties and uniformity, was elaborated at the Rottweil powder-works, and appears to have been adopted into the German service for a time, but its first great promise of success seems to have failed of fulfilment through defects in stability.

Reference has already been made to the conversion of gun-cotton (trinitrocellulose), and to mixtures of it with less explosive forms of nitrated cotton (or cellulose of other description), by the action of solvents, into horn-like materials. These are in the first instance obtained in the form of gelatinous masses, which, prior to the complete evaporation, or removal in other ways, of the solvent, can be pressed or squirted into wires, rods, or tubes, or rolled or spread into sheets; when they have become hardened, they may be cut up into tablets, or into strips or pieces of size suitable for conversion into charges or cartridges. Numerous patents have been secured for the treatment of gun-cotton, nitro-cotton, or mixtures of these with other substances, by the methods indicated; but in this direction the German makers of the powder just now referred to seem to have secured priority. Experiments were made about a year and a half ago with powder produced in this way at Woolwich, and the Wetteren Powder Company in Belgium has also manufactured so-called paper powders, or horn-like preparations, of the same kind, which were brought forward as counterparts of the French small arm- and artillery-smokeless powder.

Mr. Alfred Nobel, to whom the mining world is so largely indebted for the invention of dynamite, and of other very efficient blasting agents of which nitro-glycerine is the basis, was the first to apply the latter explosive agent, in conjunction with one of the lower products of nitration of cellulose, to the production of a smokeless powder. This powder bears great resemblance to one of the most interesting of known violent explosive agents, also invented by Mr. Nobel, and called by him blasting gelatine, in consequence of its peculiar gelatinous character. When the nitro-cotton is impregnated and allowed to digest with nitro-glycerine, it loses its fibrous nature and becomes gelatinised while assimilating the nitro-glycerine, the two substances furnishing a product which has almost the character of a compound. By macerating the nitro-cotton with from 7 to 10 per cent. of nitro-glycerine, and maintaining the mixture warm, the whole soon becomes converted into a plastic material from which it is very difficult to separate a portion of either of its components. This preparation, and certain modifications of it, have acquired high importance as blasting agents more powerful than dynamite, and possessed of the valuable property that their prolonged immersion in water does not separate from them any appreciable proportion of nitro-glycerine.

In the earlier days of the attempted application of blasting gelatine to military uses, in Austria, when endeavours were there made to render the material less susceptible of accidental explosion on active service (as by the penetration of bullets or shell fragments into transport wagons containing supplies of the explosive), this result was achieved by Colonel Hess by incorporating with the components a small proportion of camphor, a substance which had then, for some time past, played an important part in the technical application of nitro-cotton to the production of the remarkable substitutes for ivory, horn, &c., known as Xylenite. By incorporating with nitro-glycerine a much larger proportion of nitro-cotton than used in the production of blasting gelatine, and by employing camphor as an agent for promoting the union of the two explosives, as well as, apparently, for deadening the violence, or reducing the rapidity of explosion of the product, Mr. Nobel has obtained a material of almost horn-like character, which can be pressed into pellets or rolled into sheets while in the plastic condition, and which compares favourably with the gun-cotton preparations of somewhat similar physical characters just referred to, as regards ballistic properties, stability and uniformity, besides being almost absolutely smokeless. The retention in its composition of some proportion of the volatile substance camphor, which may gradually be reduced in amount by evaporation, renders this explosive liable to undergo some modification in its ballistic properties in course of time; it is believed that this point has been dealt with by Mr. Nobel, and accounts from Italy speak favourably of the results of trials of his powder in small arms, while Mr. Krupp is reported to be carrying on experiments with it in guns of several calibres.

The Government Committee on Explosives, in endeavouring to remedy the above defect of Nobel's original powder, were led by their researches to the preparation of other varieties of nitro-glycerine-powder, which, when applied in the form of wires or rods, made up into sheaves or bundles, have given, in the service small-bore rifle, excellent ballistic results. The most promising of them, which fulfils, besides, the conditions of smokelessness and of stability, so far as can be guaranteed by the application of special tests of exposure to elevated temperatures, &c., is now being submitted to searching experiments with the view of so applying it in the arm as to overcome certain difficulties attending the employment, in a very small-bore rifle, of an explosive developing much greater energy than the black-powder charge, which therefore gives very considerably higher velocities even with much smaller charges, and consequently heats the arm much more. Thus, the service black-powder charge furnishes, with the small-bore rifle, an average (and variable) velocity of 1800 f.s., together with pressures ranging from 18 to 20 tons per square inch; on the other hand, with considerably less of the explosive referred to, there is no difficulty in securing a very uniform velocity of about 2200 f.s. with pressures not exceeding 17 tons, while velocities as high as 2500 f.s. are obtainable with pressures not greater than the maximum allowed with the black-powder charge.

It is obvious, from what has already been said respecting the causes of the erosive action of powder in guns, that comparatively considerable erosive effects would be expected to be produced by powders of high energy as compared with black powder. Moreover, the freedom of the products of explosion from any solid substances, and consequently the absence of any fouling or deposition of residue in the arm, causes the heated surfaces of the projectile and of the interior of the barrel to remain clean, and in a condition, therefore, very favourable to close adherence together. If to these circumstances be added the fact that the behaviour of the smokeless powder has to be adapted to suit an arm, a cartridge, and a projectile originally designed for use with black powder, it will be understood that the devising of an explosive which shall be practically smokeless, sufficiently stable, and susceptible of perfectly safe use in the arm under all service conditions, easy of manufacture and not too costly, is, after all, but a small part of the difficult problem of adapting a smokeless powder successfully to the new military rifle—a problem which, however, appears to be on the near approach to satisfactory solution.

The experience already acquired in guns ranging in calibre from 1·85 inches to 6 inches, with the smokeless powder devised for use in our service, has been very promising, and indicates that the difficulties attending its adaptation to guns designed for black powder are likely to prove considerably less than in the case of the small arm. But here again, the circumstances that much smaller charges are required to furnish the same ballistics as the service black-powder charges, and that the comparatively gradual and sustained action of the new powder gives rise to lower pressures in the chamber of the gun, and higher pressures along the chase, demonstrate that the full utilisation of the ballistic advantages, and the increase in the power of guns of a given calibre and weight, with the new form of powder, are only attainable by some modifications in the designs of the guns—such as a reduction in size of the charge-chamber, and some additions to the strength, and perhaps, in some cases, of the length, of the chase.

When, however, the smokeless powder has been adapted with success in all respects to artillery, from small machine guns to guns of comparatively heavy calibre, and when its ballistic advantages have been fully utilised in guns of suitable design, it will remain to be determined how far such a powder—undeniably of much more sensitive constitution than black powder, or any of its modifications—will withstand, unchanged and unharmed, the various vicissitudes of climate, and the service storage-conditions in ships and on land in all parts of the world,—a condition essential to its adaptability to naval and military use, and especially to the service of our Empire; and whether sufficient confidence can be placed in its stability for long periods under these extremely varied conditions to warrant the necessary freedom from apprehension of possible danger, emanating from within the material itself, to allow of its being substituted for black powder wherever its use may present advantages.

Possible it might be, that the storage, with perfect safety, of such a powder in ships, forts, or magazines might demand the adoption of precautionary measures which might place some comparatively narrow limits upon the extent of its practicable service applications; even then, however, an imperative need for the introduction of special arrangements to secure safety and immunity from deterioration may be of small importance as compared with the great advantages which the provision of a thoroughly efficient smokeless powder may secure to the possessor of it, especially in naval warfare.

That the opinions respecting the importance of such advantages are founded upon a sound basis, one can hardly doubt, after the views expressed by several of the highest military and naval authorities, although opinions as to their extent may differ very considerably even among such authorities.

The accounts furnished from time to time from official and private sources of the effects observed, at some considerable distance, by witnesses of practice with the smokeless powders successively adopted in France, have doubtless been regarded by military authorities as warranting the belief that the employment of such powders must effect a great revolution in the conduct of campaigns. Not only have the absence of smoke and flame been dwelt upon as important factors in such a revolution, but the recorders of the achievements of smokeless powder—whose descriptions have doubtless been to some extent influenced by the vivid pictures already presented to them of what they *should* anticipate—have even been led to make such explicit assertions as to the *noiselessness* of these powders, that high military authorities have actually been thereby misled to pourtray, by vivid word-painting, the contrast between the battles of the future and the past;—to imagine the terrific din caused by the discharge of several hundred field-guns and the roar of musketry in the great battles of the past, giving place to noise so slight that distant troops will no longer receive indications where their comrades are engaged, while sentries and advanced posts will no longer be able to warn the main body of the approach of an enemy by the discharge of their rifles, and, that battles might possibly be raging within a few miles of columns on the march without the fact becoming at once apparent to them.

It is somewhat difficult to conceive that, in these comparatively enlightened days—an acquaintance with the first principles of physical science having for many years past constituted a preliminary condition of admission to the training establishments of the future warrior—the physical impossibility of such fairy tales as appear to be considered necessary in France for the delusion of the ordinary public, would not at once have been obvious. Yet, even in professional publications in Germany, where we are led to expect that the judgment of experts would be comparatively unlikely to be led astray through lack of scientific knowledge, we have, during the earlier part of last year, read, in articles upon the influence of smokeless powder upon the art of war (based evidently upon the reports received from France), such passages as these:—"The art of war gains in no way as far as simplicity is concerned; on the contrary, it appears to us that the absence of so important a mechanical means of help as *noise* and smoke were to the commander, requires increased skill and circumspection in addition to the qualities demanded by a general. . . . ." "The course of a fight will certainly be mysterious, on account of the *relative stillness* with which it will be carried on."

In an amusing article, in imitation of the account of the Battle of Dorking, which appeared in the 'Deutsche Heeres Zeitung,' of April last, the consternation is described with which a battalion receives the information from a wounded fugitive from the outposts that the enemy's bullets have been playing havoc among them, without any visible or audible indications as to the quarter of attack. Later in the year, and especially since the manœuvres before the German and Austrian Emperors, when the employment of the new smokeless powder was the event of the day, the absurdity of the assertions as to the noiselessness of the new powders became a theme for strong observations in the German service papers; the assumed existence of a noiseless powder was ridiculed as a thing equally impossible with a recoil-less powder; the violence of the report, or explosion, produced upon the discharge of a firearm being in direct relation to the volume and tension of the gaseous matter projected into the surrounding air.

The circumstance that blank ammunition was alone used in the smokeless powder exhibition at the German manœuvres may have served to lend some support to the assertions as to *comparatively* little noise made by the powder—the report of blank cartridges being slight, on account of the small and lightly confined charges used. It is said that the sound of practice with blank ammunition at the German manœuvres, was scarcely recognised at a distance of 100 metres. In a recently published pamphlet on the results of employment of the latest German smokeless powder in the manœuvres, it is stated, on the other hand, that the difference between the violence of the report of the new powder and of black powder is scarcely perceptible; that it is sharper and more ringing, but not of such long duration. This description accords exactly with our own experience of the reports produced by different varieties of smokeless powder, and of the lecturer's earlier experience with gun-cotton charges fired from rifles and field guns. The noise produced by the latter was decidedly more ringing and distressing to the ear in close proximity to the gun, but also of decidedly less volume, than the report of a black-powder charge, when heard at a considerable distance from the gun.

As regards smokelessness: the present German service powder is not actually smokeless, but produces a thin, almost transparent, bluish cloud which is immediately dissipated. Independent rifle-firing was not rendered visible by the smoke produced at a distance of 300 metres, and at shorter ranges the smoke presented the appearance of a puff from a cigar. The most rapid salvo-firing during the operations near Spandau did not have the effect of obscuring those firing from distant observers.

That, in future warfare, if smokeless or nearly smokeless powders have maintained their position as safe and reliable propelling agents for small arms and field artillery, belligerents of both sides will be alike users of them, there can be no doubt. The consequent absence of the screening effect of smoke—which, on the one hand, removes an important protection and the means of making rapid advances or sudden changes of position in comparative safety, and, on the other hand, secures to both sides the power of ensuring to the fullest extent accuracy of shooting, and of making deadly attack by individual fire through the medium of cover, with comparative immunity from detection—can scarcely fail to change more or less radically many of the existing conditions under which engagements are fought.

As regards the naval service, it is especially and, at present at any rate, exclusively for the new machine- and quick-firing guns that a smokeless powder is wanted; for such service the advantages which would be secured by the provision of a reliable powder of this kind can scarcely be over-estimated, and their realisation within no distant period may, it is believed, be anticipated with confidence.

[F. A. A.]

GENERAL MONTHLY MEETING,

Monday, February 3, 1890.

Sir James Crichton Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

F. W. Fison, Esq. M.A. (Oxon.) F.C.S.
Dr. C. A. Martius,
The Right Hon. Earl Russell,
William Schooling, Esq. F.R.A.S.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned for the following Donation to the Fund for the Promotion of Experimental Research:—

Professor Dewar, 50*l*.

The decease of Sir William Withey Gull, Bart. M.D. D.C.L. F.R.S. *M.R.I.* on January 29th, was announced from the Chair.

The following Resolution passed by the Managers at their Meeting this day was read:—

Resolved:—That the Managers of the Royal Institution of Great Britain desire, at this, their first Meeting after the death of Sir William Withey Gull, to place on record in their minutes their sense of the great loss sustained by the death of one who, for nearly thirty years, had been a Member of the Institution; who had occupied the Chair of Fullerian Professor of Physiology, and who, quite recently, had been one of their colleagues on the Board of Management, and who had, on every occasion, shown the deep interest he took in the Institution, and in the welfare of all connected with it.

The Managers further desire to be permitted to offer to Lady Gull the expression of their most sincere sympathy and condolence with her in her bereavement.

Resolved:—That the Honorary Secretary do send to Lady Gull a copy of this Resolution.

The following resolution passed by the Managers at their Meeting this day was read:—

Resolved:—That the thanks of the Managers of the Institution be given to the National Telephone Company for the great assistance which they rendered to Professor Rücker in illustrating his Course of Christmas Lectures, by so kindly enabling him to show the operations of the Telephone as actually employed for exchange work.

Resolved:—That the Honorary Secretary do send to the Company a copy of this Resolution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Governor-General of India—Geological Survey of India: Records, Vol. XXII. Part 4. 4to. 1889.

The Lords of the Admiralty—Nautical Almanack, 1893. 8vo. 1889.

Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta: Rendiconti. 2° Semestre, Vol. V. Fasc. 5–10. 8vo. 1889.
Agricultural Society of England, Royal—Journal, Second Series, Vol. XXV. Part 2. 8vo. 1889.
Asiatic Society of Bengal—Journal, Vol. LVIII. Part I. No. 1; Part II. Nos. 1 and 2. 8vo. 1889.
Proceedings, 1889, Parts 1 to 6. 8vo.
Modern Vernacular Literature of Hindustan. By G. A. Grierson. 8vo. 1889.
Astronomical Society, Royal—Monthly Notices, Vol. L. Nos. 1, 2. 8vo. 1890.
Ateneo Veneto—Revista Mensile, 1888–9. 8vo.
Bankers, Institute of—Journal, Vol. X. Part 10; Vol. XI. Part 1. 8vo. 1889–90.
Batavia Observatory—Magnetical and Meteorological Observations, 1888, Vol. XI. 4to. 1889.
Rainfall in East Indian Archipelago, 1888. 8vo. 1889.
Birmingham Philosophical Society—Proceedings, Vol. VI. Part 2. 8vo. 1889.
British Architects, Royal Institute of—Proceedings, 1889–90, Nos. 4–7. 4to.
Canada, Geological and Natural History Survey of—Annual Reports, &c. 1887–8. 8vo. 1889.
Canadian Institute—Proceedings, 3rd Series, Vol. VII. Fas. 1. 8vo. 1889.
Chemical Industry, Society of—Journal, Vol. VIII. Nos. 11, 12. 8vo. 1890.
Chemical Society—Journal for Dec. 1889 and Jan. 1890. 8vo.
Civil Engineers' Institution—Proceedings, Vol. XCVIII. 8vo. 1889.
Corporation of City of London—Catalogue of the Guildhall Library. 8vo. 1889.
Cracovie, l'Académie des Sciences—Bulletin, 1889, Nos. 10–12. 8vo.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I. (the Editor)—Journal of the Royal Microscopical Society, 1889, Part 6. 8vo.
Dawson, G. M. Esq. D.Sc. F.G.S. (the Author)—Earlier Cretaceous Rocks of N.W. Canada. 8vo. 1889.
Ore Deposit of Treadwell Mine, Alaska. 8vo. 1889.
Glaciation of British Columbia. 8vo. 1889.
East India Association—Journal, Vol. XXII. No. 1. 8vo. 1889.
Editors—American Journal of Science for Dec. 1889 and Jan. 1890. 8vo.
Analyst for Dec. 1889 and Jan. 1890. 8vo.
Athenæum for Dec. 1889 and Jan. 1890. 4to.
Chemical News for Dec. 1889 and Jan. 1890. 4to.
Chemist and Druggist for Dec. 1889 and Jan. 1890. 8vo.
Electrical Engineer for Dec 1889 and Jan. 1890. fol.
Engineer for Dec. 1889 and Jan. 1890. fol.
Engineering for Dec. 1889 and Jan. 1890. fol.
Horological Journal for Dec. 1889 and Jan. 1890. 8vo.
Industries for Dec. 1889 and Jan. 1890. fol.
Iron for Dec. 1889 and Jan. 1890. 4to.
Ironmongery for Dec. 1889 and Jan. 1890.
Murray's Magazine for Dec. 1889 and Jan. 1890. 8vo.
Nature for Dec. 1889 and Jan. 1890. 4to.
Photographic News for Dec. 1889 and Jan. 1890. 8vo.
Revue Scientifique for Dec. 1889 and Jan. 1890. 4to.
Telegraphic Journal for Dec. 1889 and Jan. 1890. 8vo.
Zoophilist for Dec. 1889 and Jan. 1890. 4to.
Electrical Engineers, Institution of—Journal, Nos. 82, 83. 8vo. 1889.
Florence Biblioteca Nazionale Centrale—Bolletino, Nos. 94–97. 8vo. 1889.
Franklin Institute—Journal, Nos. 768, 769. 8vo. 1889.
Geographical Society, Royal—Proceedings, New Series, Vol. XI. No. 12; Vol. XII. Nos. 1, 2. 1889–90.
Geological Institute, Imperial, Vienna—Verhandlungen, 1889, Nos. 13–17. 8vo.
Glasgow Philosophical Society—Proceedings, Vol. XX. 8vo. 1889.
Iron and Steel Institute—Journal for 1889, Vol. II. 8vo. 1889.
John Hopkins University—University Circulars, Nos. 76, 77. 4to. 1889–90.
Junior Engineering Society—Address. 8vo. 1889.

Kew Observatory—Report, 1889. 8vo.
Latzina, M. F. (*the Compiler*)—Censo General de la Ciudad de Buenos Aires, Tome II. 8vo. 1889.
Linnean Society—Journal, Nos. 123, 172. 8vo. 1889–90.
Manchester Geological Society—Transactions, Vol. XX. Parts 11–13. 8vo. 1889.
Manchester Steam Users' Association—Boiler Explosions Act, 1882. Report, Nos. 284–350. 4to. 1889.
Mechanical Engineers' Institution—Proceedings, 1889, No. 3. 8vo.
Meteorological Office—Weekly Weather Reports, Nos. 48–52. 4to. 1889.
Quarterly Weather Report, 1880, Part 1. 4to. 1889.
Meteorological Society, Royal—Quarterly Journal, No. 72. 8vo. 1889.
Meteorological Record, No. 34. 8vo. 1889.
Middlesex Hospital—Report for 1888. 8vo. 1889.
Ministry of Public Works, Rome—Giornale del Genio Civile, Seria Quinta, Vol. III. Nos. 10, 11. And Disegni. fol. 1889.
New York Academy of Sciences—Transactions, Vol. VIII. Parts 5–8. 8vo. 1890.
North of England Institute of Mining and Mechanical Engineers—Transactions, Vol. XXXVIII. Part 4. 8vo. 1890.
Numismatic Society—Chronicle and Journal, 1889, Part 4. 8vo. 1889.
Odontological Society of Great Britain—Transactions, Vol. XXII. Nos. 2, 3. New Series. 8vo. 1889.
Pennsylvania Geological Survey—Annual Report, 1887. 8vo. 1889.
Dictionary of Fossils. Vol. I. A–M. 8vo. 1889.
Pharmaceutical Society of Great Britain—Journal, Dec. 1889 and Jan. 1890. 8vo.
Calendar, 1890. 8vo.
Photographic Society—Journal, Vol. XIV. Nos. 3, 4. 8vo. 1889.
Relfe Bros. Messrs. (*the Publishers*)—Modern Thought and Modern Thinkers. By J. F. Charles. 12mo. 1889.
Rio de Janeiro Observatory—Revista, Nos. 10–12. 8vo. 1889.
Royal Historical and Archæological Association of Ireland—Journal, Vol. IX. (4th Series), No. 80. 8vo. 1889.
Royal Society of Edinburgh—Proceedings, Vol. XV.; Vol. XVI. Parts 1 to 7. 8vo. 1889.
Royal Society of London—Proceedings, No. 284. 8vo. 1889.
Saxon Society of Sciences, Royal—Philologisch-historischen Classe:
Abhandlung. Band XI. No. 5. 8vo. 1889.
Berichte, 1889, Nos. 2, 3. 8vo. 1889.
Scottish Society of Arts, Royal—Transactions, Vol. XII. Part 3. 8vo. 1889.
Society of Architects—Proceedings, Vol. II. No. 4. 8vo. 1890.
Society of Arts—Journal for Dec. 1889 and Jan. 1890. 8vo.
Statistical Society—Journal, Vol. LII. Part 4. 8vo. 1889.
St. Pétersbourg Académie Impériales des Sciences—Mémoires, Tome XXXVII. No. 2. 4to 1889.
Bulletin, Tome XXXIII. No. 2. 4to. 1889.
Sweden Royal Academy of Sciences—Handlingar, Band XX. XXI. and Atlas 4to. 1882–87.
Bihang, Band IX.–XIII. 8vo. 1883–8.
Ofversigt, Band XLI.–XLV. 8vo. 1884–8.
Lefnadsteckningar, Band II. Heft 3. 8vo. 1885.
Forteckning (Table des Matieres), 1826–1883. 8vo. 1884.
United Service Institution, Royal—Journal, No. 151. 8vo. 1889.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1889: Heft 9–12. 4to.
Victoria Institute—Transactions, No. 91. 8vo. 1889.
Wright & Co. Messrs. J. (*the Publishers*)—Deformities of Children. By W. Pye. 8vo. 1890.

WEEKLY EVENING MEETING,

Friday, February 7, 1890.

JOHN RAE, M.D. LL.D. F.R.S. Vice-President, in the Chair.

HENRY B. WHEATLEY, ESQ. F.S.A.

The London Stage in Elizabeth's Reign.

As the words "stage" and "drama" are sometimes used synonymously, it is necessary to state at the outset that the subject of the discourse is the material stage which grew up in this reign, and not the Elizabethan drama. During the first eighteen years of Elizabeth's reign the growth of the drama was but gradual, and the appliances for the acting of plays were but little different from what they had been in the previous reigns, while in 1576 a great change occurred, and the first playhouse was erected in the fields to the west of the highway at Shoreditch. It was called the Theatre, and the name alone seems to make it certain that this was the first special building for the purpose; but mention must be made of two statements which seem to militate against this view. The Rev. William Harrison, an Elizabethan divine, wrote a description of England, which was published with Holinshed's 'Chronicles,' and a chronology of the world, which is still in MS. In the latter work, under date 1572, Harrison writes "Plaies are banished for a time out of London," on account of the plague; and, he adds, "would to God these common plaies were exiled for altogether as seminaries of impietie and their theatres pulled down. It is an evident token of a wicked time when plaiers waxe so riche that they can build such houses." It is possible that this was written after 1572, and after the Theatre was built; but there was evidently a certain looseness of writing respecting places where plays were acted as playhouses. Thus, in the 1631 issue of Howes's edition of Stow's 'Annales' we read that Whitefriars theatre of 1629 was "the 17th stage or common play house which hath been new made within the space of three score years within London and the suburbs." Now sixty years from 1629, takes us back to 1569; but in these seventeen playhouses are included inns, St. Paul's Singing-school, &c., which cannot be considered as distinct buildings for the performance of plays. A modern instance may be cited in the use of the dormitory of Westminster School for the Latin play; for the time being it would not be improper to style it a theatre or playhouse, although the dormitory soon loses all appearance of its late use. After considering the bearing of Harrison's and Howes's words, I think we must come to the conclusion that the general opinion as to the Theatre being the first building erected as a playhouse is correct. It is worth while to stop for a moment to ask what was the meaning attached to the word theatre when it was first introduced. To us it means a place of amusement specially devoted to the drama, but this was not, I think, the meaning which was conveyed to the populace of London in 1576; a theatre was probably understood as a place for the exhibition of spectacles. This opinion is corroborated by a passage in Barclay's 'Argenis' (lib. 4, cap. xiii.), where we read of "shoutes in a theatre at the fall of a sword bearer," and we know that fencing was commonly exhibited at these early playhouses. The Theatre had a short life of twenty-three years, and it never seems to have taken a very high standing. The Curtain, which was situated close by the Theatre, and was built in the year 1577, was a much more distinguished playhouse. Marlowe was an actor there, and Shakespeare was associated with it in his early career. The two theatres in Shoreditch remained alone for a few years. Plays were occasionally acted in Blackfriars and Whitefriars, but another theatre was not erected until the Rose was built on the Bankside. This playhouse is shown in Norden's Plan of London in 1593, which is the earliest representation of an English theatre known to exist.*

There can be no doubt that the evils connected with the theatres were very considerable, and the Lord Mayor and Aldermen threw every obstacle in the way of the players. First of all they would have no theatres built within the city walls. Some inn-yards where plays had been acted were within, but no new building was allowed. Then they threw obstacles in the way of those outside, and if the erection of a new building were sanctioned, an old one was usually at the same time condemned. This was the case with the Fortune which replaced the Curtain, as the Globe replaced the Theatre when the lease of the latter expired. The Lords of the Council took a rather different view of the situation. They approved of the closing of theatres during times of sickness; but in view of the Queen's very strong predilection for the stage, they did not allow the city authorities to go quite so fast as they wished. The very interesting volume printed by the Corporation of London, which gives an account of the contents of the *Remembrancia* contains note of several letters from the Lord Mayors, and the Lords of the Council on this matter. In November 1581, the Lords of the Council directed the Lord Mayor to allow plays to be acted, and give this reason—"in order to relieve the poor players, and to encourage

* A diagram was exhibited which showed the space within the city walls unoccupied by any theatre, and the relative positions of the theatres outside the walls. On the north of the river were the Theatre and the Curtain, the Red Bull at Clerkenwell, and the Fortune in Barbican. The Blackfriars theatre was opened in 1596, and was within the walls, but Blackfriars was outside the city jurisdiction. On the south side were the Swan at Paris Garden, the Globe, the Rose, and the Bear Garden (afterwards the Hope), all on the Bankside. At Newington Butts was another theatre, of which we know little or nothing.

their being in readiness with convenient matters for Her Highness's solace this next Christmas."

The Middlesex Justices were often troubled with complaints of disturbances at theatres, and the valuable volumes edited by Mr. Cordy Jeaffreson and published by the Middlesex County Record Society, contain some important notices of the early stage, more particularly a remarkable document respecting the Theatre.

We have representations of the outsides of several of the early theatres, and the reason why we have these is because they formed picturesque objects on the banks of the river, and it suited the artists who took views of London from the most attractive point, viz. the south side of the river, to show them in their positions. The Theatre and the Curtain, the Fortune and the Red Bull, the Blackfriars and the Newington theatres were not such prominent objects, and were not represented. None of the theatres were thought to be worthy of being drawn for their own particular interest.

Until 1888 we had no representation of a Shakespearian play-house, but in that year the world was enriched by the publication of a contemporary view of the inside of the Swan theatre, which had been found in a MS. at Utrecht. The late Dr. P. A. Tiele, University Librarian at Utrecht, found this curious drawing in his Library, with a short description in the hand-writing of Arend van Buchell, and purporting to be taken down or copied from the observations of John De Witt. This was published to the world in 1888, by Dr. Gaedertz, who added a careful commentary in which he showed that De Witt must have been in England in the year 1596, when the Swan was a new building.* In this same year, 1888, I had the honour of reading a paper on the subject before the New Shakspere Society, and certain difficulties which arose were found to be insoluble without esort to the MS. Dr. Furnivall, therefore, appealed for the loan of this, and in due course it was deposited for a time at the British Museum under the care of Dr. Garnett. The difficulties were then solved, and we are all greatly indebted to the authorities for this liberal instance of international courtesy.†

This drawing of the interior of the Swan theatre shows about a third of the round of the entire amphitheatre, and the movable stage which was used for the acting of plays and cleared away when the centre was required for bull-baiting, bear-baiting, and other sports. This stage stands upon legs and does not appear to be raised many feet above the arena. At the back is an erection with doors from which issued the actors, and above are the private boxes. This erection is inscribed "Mimorum Ædes." Over the uppermost gallery is a roof inscribed "Tectum." The stage and the arena are open to

* 'Zur Kenntnis der altenglischen Bühne, nebst andern Beiträgen zur Shakespeare-Literatur, von Karl Theodor Gaedertz.' Bremen, 1888.

† A reproduction of the original drawing will accompany the paper in the next part of the New Shakspere Society's Transactions.

the sky. At the top of the building is the little turret which is shown in the exterior of most of the Bankside theatres, and from it flies the flag with the sign of the house—the "Swan." The trumpeter who announced to the outside world that the performance was about to commence is shown on a slight platform. Round the building are the galleries alternately, three with seats and two for standing room, styled respectively "sedilia" and "porticus," the latter are represented as a species of colonnade, and probably access to the "sedilia" was obtained from the "porticus." The standing room in front of the stage is inscribed "arena," and to the left is a portion of space inscribed "orchestra"; near by are a few steps marked "ingressus," which gave access to the first tier of "sedilia." No other stairs are shown, but we obtain some insight into the mode of entering the galleries from a paper of agreement for the new building of the Bear Garden in 1613, which is printed in the third volume of the Variorum edition of Shakespeare (1821). From this it appears that the Swan was taken as a model for the new theatre, and from the agreement we learn that there were two staircases to lead to the galleries.

Gilbert Katherens, described as a carpenter, was to build the Bear Garden, "of suche large compasse, forme, wideness and height, as the plaie house called the Swan in the libertie of Paris Garden, in the saide parishe of St. Saviour's now is. And shall also builde two steare cases without and adjoining to the saide playe house, in suche convenient places as shal be most fitt and convenient for them to stande uppon, and of suche largnes and height as the steare cases of the saide playe house called the Swan now are or be."

It will be seen that this view throws great light upon the evolution of the English stage. We know that the form of the Bankside theatres was circular, but we do not know for certain whether the theatres on the north side of the river were also round. The words of De Witt which accompany the sketch would imply that they were alike, for he writes (in Latin):—

"There are in London four amphitheatres of beauty worth seeing. . . . Of these the two most excellent are those on the other side of the Thames towards the south, named after the signs that hang out, the Rose and the Swan. The two others are outside the town towards the north."

We are here in a realm of conjecture; but we have some few lines of guidance. Was the word theatre used in its strictly classical sense, as Milton writes ('Samson Agonistes')—

> "The building was a spacious theatre,
> Half round, on two main pillars vaulted high."

or was the building really an amphitheatre? *

---

* Hentzner, in the account of his visit to this country in 1598, describes the amphitheatre on the Bankside, used exclusively for the baiting of bulls and bears, as a *Theatre*. (*Itinerarium*, 1629, p. 196.)

The Italians had before this time erected theatres which were copied from the classical stage, and it might be imagined that James Burbage, when about to build a special house for theatrical entertainments, would have followed some such model, but there is no evidence whatsoever that he did so. Mr. Halliwell Phillipps believed (I do not know on what authority) that the Curtain, our second London theatre, was round; indeed, he believed it to be the "wooden O" of Henry V. (in opposition to the claims of the ever-memorable Globe).

The chief reasons for supposing that almost all the Elizabethan theatres were round, are (1) because the early theatres were not intended exclusively for dramatic entertainments, but were used for fencing, tumbling, bear-baiting &c., and the circular form is much more convenient for sports in an arena; (2) because it is highly probable that the Bankside buildings were copied from something that went before; (3) because this shape is frequently alluded to by the dramatists, and the word "Round" is used by them as the name of a theatre. Thus, in Brome's *City Wit* (printed in 1653), one of the characters, Sarpego, who delivers the prologue says—

> "Some in this *round* may have both seen't and heard
> Ere I, that bear its title, bore a beard."

The Fortune theatre, near St. Giles's, Cripplegate, according to the Indenture dated January 1599–1600, was built on the same plan as the Globe, which had just been erected, with the exception that the auditorium was square instead of round. This was found to be inconvenient, so that when the Fortune was rebuilt in 1622, it was made round.

The only other view of the interior of an early London theatre which we possess, is that of the Red Bull, in the reign of Charles II., in which we find the same expedient as to the stage, so that we may safely come to the conclusion that, whether square or round, the same system was adopted with regard to the plan of the stage.

This form had the advantage of being convenient for all kinds of entertainment. If it were general, it is clear that the influence of the classical stage upon the foundation of the modern stage was practically non-existent, and also there is sufficient evidence to allow us to set aside the popular notion that the modern theatre has grown out of the old inn-yard. I fail to see any solid ground or basis for this view, and the only point in its favour seems to be that the pit was frequently called the yard—and this can be otherwise explained. If we agree that the original form of the theatre was a round, with a movable stage in the centre, it follows that when the time came for the building to be devoted exclusively to dramatic entertainments, the stage would naturally be brought back to the portion of the round which had become useless by reason that any would-be spectators placed there could see nothing, and the modern theatre at once stands confessed as a circus flattened at one side—an evolution from the amphitheatre.

It will be seen that with a movable stage placed in the centre of an amphitheatre, effective scenery was practically an impossibility. At the Restoration, however, scenery came into general use, and one reason for this was that the stage having been completely crushed during the Commonwealth an entirely new era then commenced. The different kinds of dramatic entertainment, which had been hitherto kept distinct, were united by Davenant and others, and scenery which had been previously confined to masques was adopted for other plays. Women, who had long before acted in masques, now took their place upon the public stage. The history of the drama is continuous, but that of the stage is in two parts, divided by the period of the Commonwealth. The history of the modern stage does not go farther back than the period of the Restoration.

There are two other points connected with the early theatres which require some slight notice—these are size and outside appearance. With regard to the first, De Witt states that the Swan theatre would seat 3000 persons, which is a rather startling statement, as the ordinary capacity of these theatres was to hold about a thousand. Although the Swan was evidently a larger building than most of the other theatres, it is not easy to believe that its size was so much greater as these figures would necessitate. It is necessary however for us to enlarge our ideas as to the number of the sightseers. Although the population of London in Elizabeth's reign was small when compared with what it is now, it was very considerable for the period, and I think it will be found that the attendants at theatres formed a much larger percentage of the population than they do now. It is not necessary to enlarge upon this point here, but mention may be made of the large number of watermen who were employed upon the Thames, and were fully engaged in taking the sightseers from one side of the river to the other. When, in James I.'s reign, the theatres on the Bankside fell into decay and Blackfriars theatre and other playhouses on the northern side of the river were alone fashionable places of resort, the watermen suffered severely by reason of their loss of custom. To retrieve their position they made a most astonishing demand. In 1613 Taylor, the water poet, was chosen by the Company of Watermen to present their petition to the King. This petition set forth the watermen's services to Queen Elizabeth and the advantages to the State of favouring them. On this foundation they based their extravagant claim that the players might not have a playhouse in London or in Middlesex within four miles of the city on that side of the Thames. If the players were made to return to the Bankside the watermen expected a return of their former prosperity. The substance of Taylor's statement is, that the theatres were first chiefly to the north of London and the Thames; that they were afterwards transferred to the south, on the Bankside in Southwark, and then again removed to the north. During the time they were at Bankside the traffic on the river so greatly increased that the additional number of watermen with their families between Windsor and Gravesend amounted to something like 20,000 persons, and that when they were moved back again to the north they drew every day from 3000 to 4000 persons who used to go by water. Reckoning a waterman's family at five persons, the number of watermen between Windsor and Gravesend at the height of this traffic would be 8000. This statement as to numbers is very remarkable, and shows that the sightseers of London in Elizabeth's reign were a considerable body.*

It is worthy of notice that changes in the habits of the English took place very rapidly even in the time of Queen Elizabeth. When the theatres were first established the visitors went to them on horseback, later on they took boat to Southwark, and in the last years of the reign, when Blackfriars theatre was fashionable, coaches had become numerous.

As to the exterior, De Witt distinctly says that it was cased with flint, and this assertion has been doubted chiefly because Hentzner said that the theatres on the Bankside were all of wood. I don't think that we can reject the testimony of one who was apparently a careful observer, on the strength of a general statement such as that of Hentzner. [Enlarged representations of the Swan, the Bear Garden, and the Globe, taken from views of the Bankside, were shown in diagrams on the wall]. These views of the theatres on the Bankside are but small in the originals, and too much stress must not be laid upon their appearance, but I think a difference between the look of the Swan and the Bear Garden on the one side, and of the Globe on the other may be noted. We know that the first Globe theatre was made of wood, but the other two look as if they might have been cased with stone or built up with brick.

[H. B. W.]

---

* In order to have some basis of comparison, my friend Mr. Danby P. Fry drew out a theoretical section which makes the arrangement of the seats easier to understand. This drawing was enlarged in a diagram on the wall. He has calculated that the height of the building would be about 50 feet, and this number is arrived at thus:—The uppermost gallery of seats is taken as 8 feet in height and the other two as 10 feet, the two rows of porticus at 7 feet each, and the orchestra as 7 feet. To estimate the size of the round is more difficult; but supposing there to have been eleven rows of seats, that is, three rows in the uppermost gallery and four rows in each of the lower galleries, in order to seat 3000 persons, 273 must have been seated in each row, and this would necessitate a round of more than two-thirds the size of Drury Lane theatre. If we suppose De Witt to mean auditors generally, and not merely those seated, a much smaller circle would supply the need, because we could then count in all those standing in the porticus and the arena. If the building was arranged to hold 3000 persons when used as an amphitheatre, it would not probably accommodate more than 2000 when the stage was placed in its position, and a portion of the round was thereby made useless for spectators.

WEEKLY EVENING MEETING,

Friday, February 14, 1890.

William Crookes, Esq. F.R.S. Vice-President, in the Chair.

Professor J. A. Fleming, M.A. D.Sc. *M.R.I.*

Problems in the Physics of an Electric Lamp.

More than eighty years ago Sir Humphry Davy provided the terminal wires of his great battery of 2000 pairs of plates with rods of carbon, and, bringing their extremities in contact, obtained for the first time a brilliant display of the electric arc.* The years that have fled away since that time have seen all the marvellous developments of electro-magnetic engineering, have placed in our possession the electric glow-lamp, and brought the art of electrical illumination to a condition in which it progresses each year with giant strides. In addition to the importance attaching to their ever-increasing industrial use, there are many questions of purely scientific interest which present themselves to our minds when we proceed to examine the actions that take place when a carbon conductor is rendered incandescent in a high vacuum, or when an electric arc is formed between two carbon poles. It is to a very few of these physical problems that I desire to direct your attention to-night, but more especially to one which is particularly interesting from the bearing which it has on the general nature of electric discharge.

We know as a very familiar fact that if we attempt to raise the temperature of a carbon conductor enclosed in a vacuum beyond a certain limit, not far removed from the melting point of platinum, the carbon begins to volatilise with great rapidity. If an electric glow-lamp has passed through its carbon more than a certain strength of current, the glass bulb speedily becomes darkened by a deposit of this volatilised carbon condensed upon it; and experience shows us that we cannot raise the temperature of that carbon beyond a definite point without causing this waste of the conductor to become very rapid. In the highly rarefied atmosphere within the bulb of a glow-lamp, the carbon, when at its normal incandescence, must be con-

---

* Sir Humphry Davy laid a request before the managers of the Royal Institution on July 11th, 1808, that they would set on foot a subscription for the purchase of a large galvanic battery. The result of this suggestion was that a galvanic battery of 2000 pairs of copper and zinc plates were set up in the Royal Institution, and one of the earliest experiments performed with it was the production of the electric arc between carbon poles, on a large scale. It is probable, however, that Davy had produced the light on a small scale some six years before, and, according to Quetelet, Curtet observed the arc between carbon points in 1802. See Dr. Paris' 'Life of Sir H. Davy.'

sidered to be projecting off molecules of carbon in all directions, partly in virtue of purely thermal actions, but probably also in consequence of certain electrical effects to be presently discussed. This scattering of the material of the carbon conductor takes place with disadvantageous rapidity from an industrial point of view at and beyond a certain temperature,* but it exists as well at much lower temperatures than that which is found to determine the practical limit of durability. A curious appearance is found in many incandescent lamps which have been "over-run," which shows us that this projection of carbon molecules from the hot conductor is not, perhaps, best described by calling it a vaporisation of its substance, but that the surface molecules are shot off in straight lines, and that they reach the glass envelope without being hindered to any great extent by the molecules of the residual air.

If an electric current is passed through an otherwise uniform carbon conductor, which possesses at any one place a specific resistance higher than that of the remaining portion, the current, in accordance with a well-known law, there develops a higher temperature, and the molecular scattering at that spot may in consequence be greatly exaggerated. It may be that the detrition of the conductor at that locality will be so great as to cut it through after a very short time. When the carbon has the form of a simple horseshoe loop, and when this molecular scattering takes place from some point in the middle of one branch, the molecular projection makes itself evident by producing a "molecular shadow" of the other leg upon the interior of the glass. I will project upon the screen an image of the carbon horseshoe loop taken from an old glow-lamp, and you will be able to see that the filament has been cut through at one place. At that position some minute congenital defect caused the carbon to have a higher resistance, the temperature at that point when it was in use became excessive, and an intensified molecular scattering took place from that locality. On examining the glass bulb from which it was taken, we find that the glass has been everywhere darkened by a deposit of the scattered carbon except along one narrow line (*see* Fig. 1), and that line is in the plane of the carbon loop and on the side opposite to the point of rupture of the filament.†

I may illustrate to you by a very simple experiment the way in which that "shadow" has been formed. Here is a ∩-shaped rod: this shall represent the carbon conductor in the lamp; this sheet of cardboard placed behind it, the side of the glass receiver. I have affixed a little spray-producer to one side of the loop, and from that

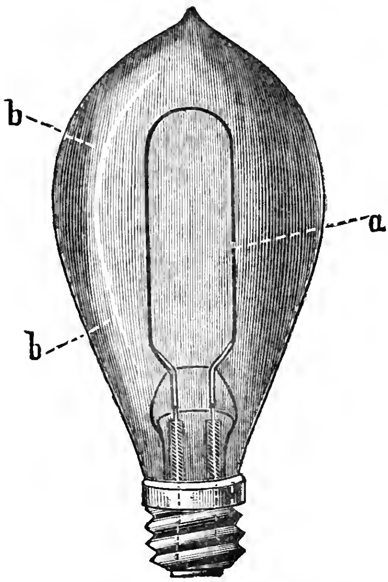
---

* When the rate of expenditure of energy in the carbon conductor is raised until it reaches a value of about 500 watts, or 360 foot-pounds per second per square inch of radiative surface, a limit of useful temperature has been reached for economical working, under the usual present conditions of steam-engine-driven dynamos and modern glow-lamps.

† The writer desires to express his indebtedness to the Editor of the 'Electrician' for the loan of the blocks illustrating this abstract.

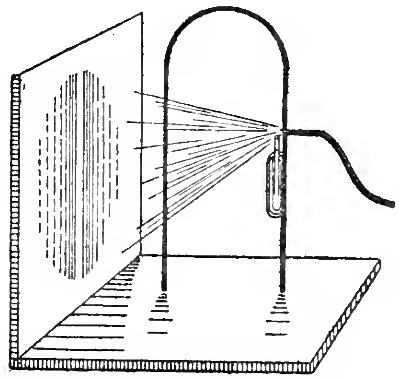
point blow out a spray of inky water. Consider the ink spray to represent the carbon atoms shot off from the overheated spot. We see that the cardboard is bespattered on all points except along one line where it is sheltered by the opposite side of the loop. We have thus produced a "spray shadow" on the board (Fig. 2). The

Fig. 1.



Glow-lamp, having the glass bulb blackened by deposit of carbon, showing the molecular scattering which has taken place from the point *a* on the filament, and the shadow or line of no deposit produced at *b*.

Fig. 2.

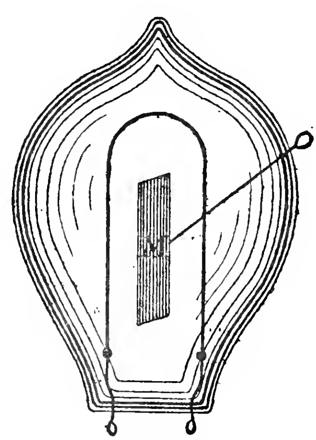


"Spray shadow" of a rod thrown on cardboard screen to illustrate formation of molecular shadow in glow lamps.

existence of these molecular shadows in incandescent lamps leads us therefore to recognise that the carbon atoms must be shot off in straight lines, or else obviously no such sharp shadow could thus be formed. This phenomenon confirms in a very beautiful manner the deductions of the Kinetic theory of gases. I may remind you that at the ordinary temperature and pressure the mean free path of a molecule of air is deduced to be about four one-millionths of an inch. This is the average distance which such a gaseous molecule moves over before meeting with a collision against a neighbour which changes the direction of its path. Let the air be rarefied, as in these bulbs, to something like a millionth of the ordinary atmospheric pressure, and the mean free path is increased to several inches. The space within the bulb—though from one point of view densely populated with molecules of residual air—is yet, as a fact, in such a condition of rarefaction that a carbon molecule projected from the conductor can move over a distance of three or four inches on an average without meeting with interference by collision with another molecule, and the facts revealed to us by these shadows show that this must be the case. I have also at hand some Edison lamps in which these "molecular shadows" are finely shown, but in these cases the deposit on the interior of the bulb is not carbon but copper, because the molecular scattering has here taken place by excessive temperature developed at the copper clamps by which the carbon filament is attached to the platinum wires. The theory, however, is the same. The deposit of copper shows a fine green colour by transmitted light in the thinner portions. One curious lamp also before me had by an accident an aluminium plate volatilised within the bulb. The glass receiver has in consequence been covered with a mirror-like deposit of aluminium, which on the thinner portions shows a fine blue colour by transmitted light, and a silvery lustre by reflected light. This lamp also shows a fine "molecular shadow."

These facts prepare us to accept the view that when a glow-lamp is in operation the highly rarefied residual air in the interior of the bulb is being traversed in all directions by multitudinous carbon atoms projected off from the incandescent carbon conductor. I now wish to pass in review before you some facts which indicate that these carbon atoms carry with them electric charges, and that they are charged, if at all, with *negative electricity*. I may preface all by saying that much of what I have to show you will be seen to be closely related to the phenomena studied by Mr. Crookes in his splendid and classical researches on radiant matter. Our starting-point for this purpose is a discovery made by Mr. Edison in 1884, and which received careful examination at the hands of Mr. Preece in the following year,* and by myself more recently. Here is the initial experiment. A glow-lamp having the usual horseshoe-shaped carbon (*see* Fig. 3) has a metal plate held on a platinum wire sealed through the glass bulb. This plate is so fixed that it stands up between the two sides of the carbon arch without touching either of them. We shall illuminate the lamp by a continuous current of electricity, and for brevity's sake speak of that half of the loop of carbon on the side by which the current enters it as the positive leg, and the other half of the loop as the negative leg. The diagram in Fig. 4 shows

FIG. 3.

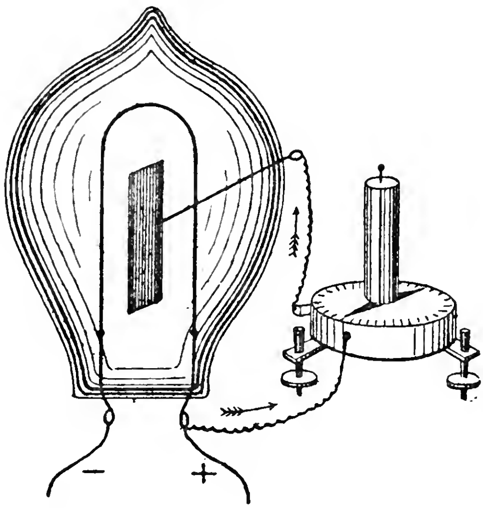


Glow-lamp having insulated metal middle plate M sealed into bulb to exhibit "Edison effect."

* Mr. Preece's interesting paper on this subject is published in the 'Proceedings' of the Royal Society for 1885, p. 219. See also 'The Electrician,' April 4th, 1885, p. 436.

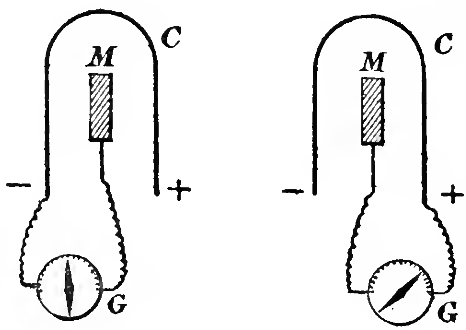
the position of the plate with respect to the carbon loop. There is a distance of half-an-inch, or in some cases many inches, between either leg of the carbon and this middle plate. Setting the lamp in action, I connect a sensitive galvanometer between the middle plate and the

FIG. 4.



Sensitive galvanometer connected between the middle plate and positive electrode of a glow-lamp, showing current flowing through it when the lamp is in action ("Edison effect").

FIG. 5.

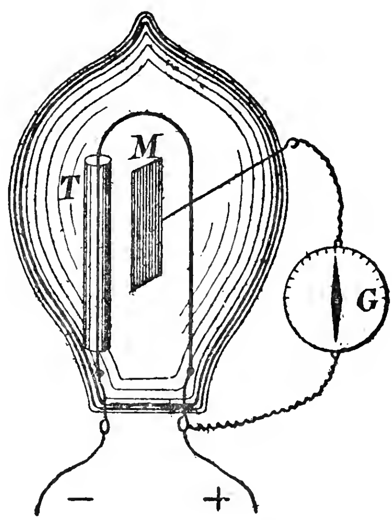


Mode of connection of galvanometer *G* to middle plate *M* and carbon horseshoe shaped conductor *C* in the experiment of the "Edison effect."

negative terminal of the lamp, and you see that there is no current passing through the instrument. If, however, I connect the terminals of my galvanometer to the middle plate and to the *positive electrode* of the lamp, we find a current of some milliamperes is passing through it. The diagrams in Fig. 5 show the mode of connection of the galvanometer in the two cases. This effect, which is often spoken of as the "Edison effect," clearly indicates that an insulated plate so placed in the vacuum of a lamp in action is brought down to the same potential or electrical state as the negative electrode of the carbon loop. On examining the direction of the current through the galvanometer we find that it is equivalent to a flow of negative electricity taking place through it *from* the middle plate *to* the positive electrode of the lamp. A consideration of this fact shows us that there must be some way by which negative electricity gets across the vacuous space from the negative leg of the carbon to the metal plate, whilst at the same time a negative charge cannot pass from the metal plate across to the positive leg. Before I pass away from this initial experiment, I should like to call your attention to a curious effect at the moment when the lamp is extinguished. Connecting the galvanometer as at first, between the middle plate and the negative electrode of the lamp, we notice that though made highly sensitive the galvano meter indicates no current flowing through it whilst the lamp is in action. Switching off the current from the lamp produces, as you see, a violent kick or deflection of the galvanometer, indicating a sudden rush of current through it.

In endeavouring to ascertain further facts about this effect one of the experiments which early suggested itself was one directed to determine the relative effects of different portions of the carbon conductor. Here is a lamp (*see* Fig. 6) in which one leg of the carbon horseshoe has been enclosed in a glass tube of the size of a quill, which shuts in one-half of the carbon. The bulb contains, as before, an insulated middle plate. If we pass the actuating current through this lamp in such a direction that the covered or sheathed leg is the *positive* leg, we find the effect existing as before. A galvanometer connected between the plate and positive terminal of the lamp yields a strong current, whilst if connected between the negative terminal and the middle plate there is no current at all. Let us, however, reverse the current through the lamp so that the shielded or enclosed leg is now the negative one, and the galvanometer is able to detect no current, whether connected in one way or the other. We establish, therefore, the conclusion that it is the negative leg of the carbon loop which is the active agent in the production of this "Edison effect," and that if it is enclosed in a tube of either glass or metal, no current is found flowing in a galvanometer connected between the positive terminal of the lamp and this middle collecting plate.

Fig. 6.

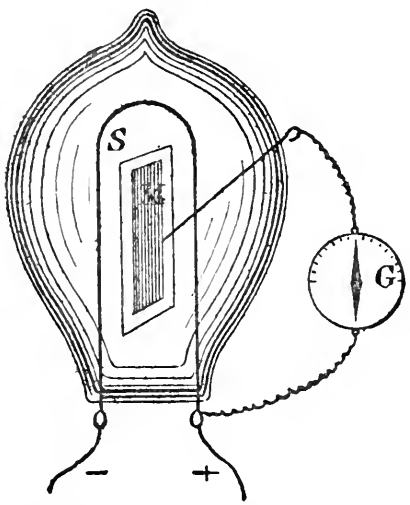


Glow-lamp having negative leg of carbon enclosed in glass tube *T*, the "Edison effect" thereby being annulled or greatly diminished.

Another experiment which confirms this view is as follows:—This lamp (*see* Fig. 7) has a middle plate, which is provided with a little mica flap or shutter on one side of it. When the lamp is held upright the mica shield falls over and covers one side of the plate, but when it is held in a horizontal position the mica shield falls away from the front of the plate and exposes it. Using this lamp as before we find that when the positive leg of the carbon loop is opposite to the shielded face of the plate, we get the "Edison effect" as before in any position of the lamp. Reversing the lamp current and making that same leg the *negative* one, we find that when the lamp is so held the metal plate is shielded by the interposition of the mica, and the galvanometer current is very much less than when the shield is shaken on one side and the plate exposed fully to the negative leg.

At this stage it will perhaps be most convenient to outline briefly the beginnings of a theory proposed to reconcile these facts, and leave you to judge how far the subsequent experiments confirm this hypothesis. The theory very briefly is as follows:—From all parts of the incandescent carbon loop, but chiefly from the negative leg, carbon molecules are being projected which carry with them, or are charged with, negative electricity. I will in a few moments make a suggestion to you which may point to a possible hypothesis on the manner in which the molecules acquire this negative charge. Supposing this, however, to be the case, and that the bulb is filled with these negatively-charged molecules, what would be the result of introducing into their midst a conductor such as this middle metal plate which is charged positively? Obviously, they would all be attracted to it and discharge against it. Suppose the positive charge of this conductor to be continually renewed, and the negatively-charged molecules continually supplied, which conditions can be obtained by connecting the middle plate to the positive electrode of the lamp, the obvious result will be to produce a current of electricity flowing through the wire or galvanometer, by means of which this middle plate is connected to the positive electrode of the lamp. If, however, the middle plate is connected to the negative electrode of the lamp, the negatively-charged molecules can give up no charge to it, and produce no current in the interpolated galvanometer. We see that on this assumption the effect must necessarily be diminished by any arrangement which prevents these negatively-charged molecules from being shot off the negative leg or from striking against the middle plate. Another obvious corollary from this theory is that the "Edison effect" should be annihilated if the metal collecting plate is placed at a distance from the negative leg much greater than the mean free path of the molecules.

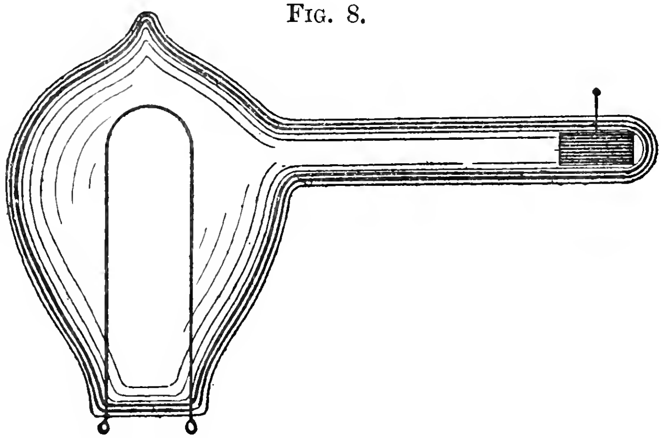
Fig. 7.



Glow-lamp having mica shield *S* interposable between middle plate *M* and negative leg of carbon, thereby diminishing the "Edison effect."

Here are some experiments which confirm this deduction. In this bulb (Fig. 8) the metal collecting plate, which is to be connected through the galvanometer with the positive terminal of the lamp, is placed at the end of a long tube opening out of and forming part of the bulb. We find the "Edison effect" is entirely absent, and that the galvanometer current is zero. We have, as it were, placed our target at such a distance that the longest range molecular bullets

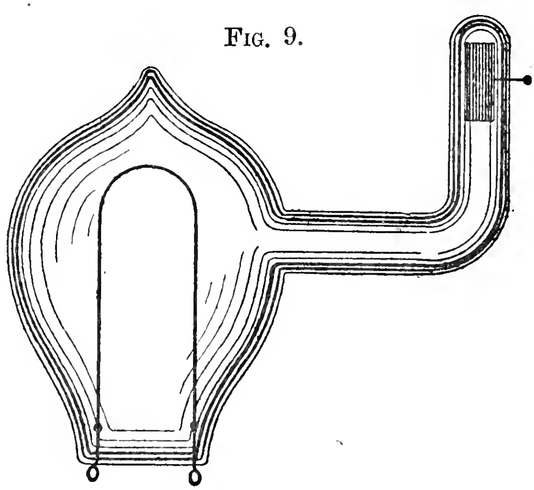
Fig. 8.



Collecting plate placed at end of a tube, 18 in. in length, opening out of the bulb.

cannot hit it, or, at least, but very very few of them do so. Here again is a lamp in which the plate is placed at the extremity of a tube opening out of the bulb, but bent at right angles (Fig. 9). We

Fig. 9.



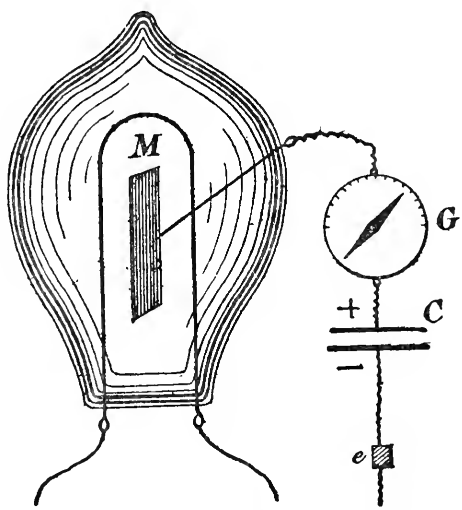
Collecting plate placed at end of an elbow tube opening out of the bulb.

find in this case, as first discovered by Mr. Preece, that there is no "Edison effect." Our molecular marksman cannot shoot round a corner. None of the negatively-charged molecules can reach the plate, although that plate is placed at a distance not greater than would suffice to produce the effect if the bend were straightened out. Following out our hypothesis into its consequences would lead us to conclude that the material of which the plate is made is without influence on the result, and this is found to be the case. Many of the foregoing facts were established by Mr. Preece as far back as 1885, and I have myself abundantly confirmed his results.

We should expect also to find that the larger we make our plate, and the nearer we bring it to the negative leg of the carbon, the greater will be the current produced in a circuit connecting this plate to the positive terminal of the lamp. I have before me a lamp with a large plate placed very near the negative leg of the carbon of a lamp, and we find that we can collect enough current from these molecular charges to work a telegraph relay and ring an electric bell. The current which is now working this relay is made up of the charges collected by the plate from the negatively-charged carbon molecules which are projected against it from the negative leg, across the highly perfect vacuum. I have tried experiments with lamps in which the collecting plate is placed in all kinds of positions, and has various forms, some of which are here, and are represented in the diagrams before you; but the result may all be summed up by saying that the greatest effects are produced when the collecting plate is as near as possible to the base of the negative end of the loops, and, as far as possible, encloses, without touching, the carbon conductor. Time will not permit me to make more than a passing reference to the fact that the magnitude of the current flowing through the galvanometer when connected between the middle plate and the positive terminal of the lamp often "jumps" from a low to a high value, or *vice versâ*, in a remarkable manner, and that this sudden change in the current can be produced by bringing strong magnets near the outside of the bulb.

Let us now follow out into some other consequences this hypothesis that the interior of the bulb of a glow-lamp when in action is populated by flying crowds of carbon atoms all carrying a negative charge of electricity. Suppose we connect our middle collecting plate with some external reservoir of electric energy, such as a Leyden jar, or with a condenser equivalent in capacity to many hundreds of Leyden jars, and let the side of the condenser which is charged positively be first placed in connection through a galvanometer with the middle plate (see Fig. 10), whilst the negative side is placed in connection with the earth. Here is a condenser of two microfarads capacity so charged and connected. Note what happens when I complete the circuit and illuminate the lamp by passing the current through its filament. The condenser is at once discharged. If, however, we repeat the same experiment with the sole difference that the negatively charged side of the condenser is in connection with the middle plate then there is no discharge. The experimental results may be regarded from another point of view. In order that the condenser may be discharged as in the first case, it is essential that the negatively charged side of the condenser shall be in connection with some part of the circuit of the incandescent carbon loop. This experiment with the condenser discharged by the lamp may be then looked upon as an arrangement in which the plates of a charged condenser are connected respectively to an incandescent carbon loop and to a cool metal plate, both being enclosed in a highly vacuous space, and it appears that when the incandescent conductor is the negative electrode of this arrangement the discharge takes place, but not when the cooler metal plate is the negative electrode of the charged condenser. The negative charge of the condenser can be carried across the vacuous space from the hot carbon to the colder metal plate, but not in the reverse direction.

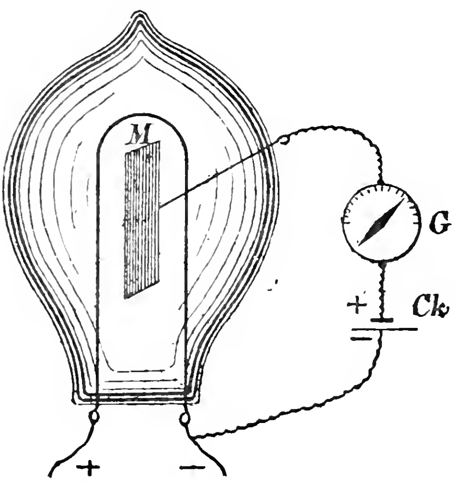
FIG. 10.



Charged condenser C discharged by middle plate M, when the positively charged side of condenser is in connection with the plate and other side to earth e.

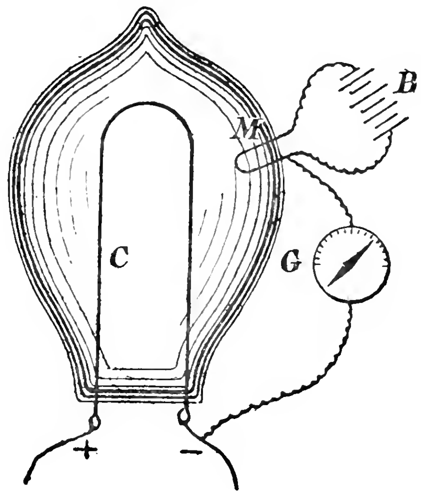
This experimental result led me to examine the condition of the vacuous space between the middle metal plate and the negative leg of the carbon loop in the case of the lamp employed in our first experiment. Let us return for a moment to that lamp. I join the galvanometer between the middle plate and the negative terminal of the lamp, and find, as before, no indication of a current. The metal plate and the negative terminal of the lamp are at the same electrical potential. In the circuit of the galvanometer we will insert a single galvanic cell having an electromotive force of rather over one volt. In the first place let that cell be so inserted that its negative pole is in connection with the middle plate, and its positive pole in connection through the galvanometer with the negative terminal of the lamp (*see* Fig. 11). Regarding the circuit of that cell alone, we find that it consists of the cell itself, the galvanometer wire, and that half-inch of highly vacuous space between the hot carbon conductor and the middle plate. In that circuit the cell cannot send any sensible current at all, as it is at the present moment connected up. But if we reverse the direction of the cell so that its positive pole is in connection with the middle plate, the galvanometer at once gives indications of a very sensible current. This highly vacuous space, lying between the middle metal plate on the one hand, and the incandescent carbon on the other, possesses a kind of unilateral conductivity, in that it will allow the current from a single galvanic cell to pass one way but not the other. It is a very old and familiar fact that in order to send a current from a battery through a highly rarefied gas by means of metal electrodes, the electromotive force of the battery must exceed a certain value. Here, however, we have indication that if the negative electrode by which that current seeks to enter the vacuous space is made incandescent the current will pass at a very much lower electromotive force than if the electrode is not so heated.

Fig. 11.



Current from Clark cell *Ck* being sent across vacuous space between negative leg of carbon and middle plate *M*. Positive pole of cell in connection with plate *M* through galvanometer *G*.

Fig. 12.

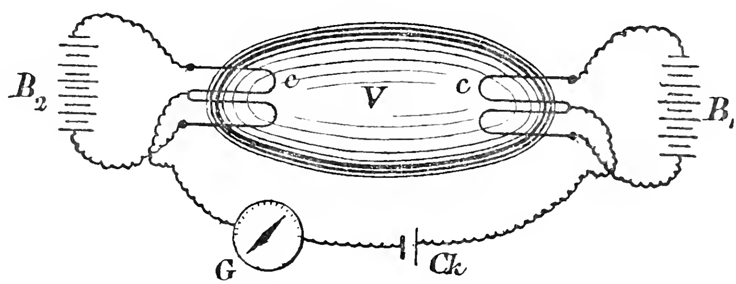


Experiment showing that when the "middle plate" is a carbon loop rendered incandescent by insulated battery *B*, a current of negative electricity flows from *M* to the positive leg of main carbon *C* across the vacuum.

A little consideration of the foregoing experiments led to the conclusion that in the original experiment, as devised by Mr. Edison, if we could by any means render the middle plate very hot, we should get a current flowing through a galvanometer when it is connected between the middle plate and the negative electrode of the carbon. This experiment can be tried in the manner now to be shown. Here is a bulb (Fig. 12) having in it two carbon loops; one of these is of ordinary size, and will be rendered incandescent by the current from the mains. The other loop is very small, and will be heated by a well-insulated secondary battery. This smaller incandescent loop shall be employed just as if it were a middle metal plate. It is, in fact, simply an incandescent middle conductor. On repeating the typical experiment with this arrangement, we find that the galvanometer indicates a current when connected between the middle loop and either the positive *or* the negative terminal of the main carbon. I have little doubt but that if we could render the platinum plate in our first-used lamp incandescent by concentrating on it from outside a powerful beam of radiant heat we should get the same result.

A similar set of results can be arrived at by experiments with a bulb constructed like an ordinary vacuum tube, and having small carbon loops at each end instead of the usual platinum or aluminium wires. Such a tube is now before you (*see* Fig. 13), and will not

FIG. 13.

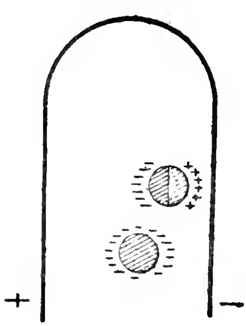


Vacuum tube having carbon loop electrodes, *c c*, at each end rendered incandescent by insulated batteries B_1 B_2, showing current from Clark cell, *Ck*, passing through the high vacuum when the electrodes are incandescent.

allow the current from a few cells of a secondary battery to pass through it when the carbon loops are cold. If, however, by means of well insulated secondary batteries we render both of the carbon loop electrodes highly incandescent, a single cell of a battery is sufficient to pass a very considerable current across that vacuous space provided the resistance of the rest of the circuit is not large. We may embrace the foregoing facts by saying that if the electrodes, but especially the negative electrode, which form the means of ingress and egress of a current into a vacuous space are capable of being rendered highly incandescent, and if at that high temperature they are made to differ in electrical potential by the application of a very small electromotive force, we may get under these circumstances a very sensible current through the rarefied gas. If the electrodes are cold a very much higher electromotive force will be necessary to begin the discharge or current through the space. These facts have been made the subject of elaborate investigation by Hittorf and Goldstein, and more recently by Elster and Geitel. It is to Hittorf that I believe we are indebted for the discovery of the fact that by heating the negative electrode we greatly reduce the apparent resistance of a vacuum.

Permit me now to pave the way by some other experiments for a little more detailed outline of the manner in which I shall venture to suggest these negative molecular charges are bestowed. This is really the important matter to examine. In seeking for some probable explanation of the manner in which these wandering molecules of carbon in the glow-lamp bulb obtain their negative charges, I fall back for assistance upon some facts discovered by the late Prof. Guthrie. He showed some years ago new experiments on the relative powers of incandescent bodies for retaining positive and negative charges. One of the facts he brought forward * was that a bright red-hot iron ball, well insulated, could be charged negatively, but could not retain for an instant a positive charge. He showed this fact in a way which it is very easy to repeat as a lecture experiment. Here is a gold-leaf electroscope, to which we will impart a positive charge of electricity, and project the image of its divergent leaves on the screen. A poker, the tip of which has been made brightly red-hot, is placed so that its incandescent end is about an inch from the knob of the electroscope. No discharge takes place. Discharging the electroscope with my finger, I give it a small charge of negative electricity, and replace the poker in the same position. The gold leaves instantly collapse. Bear in mind that the extremity of the poker, when brought in contiguity to the knob of the charged electroscope, becomes charged by induction with a charge of the opposite sign to that of the charge of the electroscope, and you will at once see that this experiment confirms Prof. Guthrie's statement, for the negatively-charged electroscope induces a positive charge on the incandescent iron, and this charge cannot be retained. If the induced charge on the poker is a negative charge, it is retained, and hence the positively-charged electroscope is not discharged, but the negatively-charged electroscope at once loses its charge. Pass in imagination from iron balls to carbon molecules. We may ask whether it is a legitimate assumption to suppose the same fact to hold good for them, and that a hot carbon molecule or small carbon mass just detached from an incandescent surface behaves in the same way and has a greater grip for negative than for positive charge? If this can possibly be assumed, we can complete our hypothesis as follows:—Consider a carbon molecule or small congerie of molecules just set free by the high temperature from the negative leg of the incandescent carbon horseshoe. This small carbon mass finds itself in the electrostatic field between the branches of the incandescent carbon conductor (*see* Fig. 14). It is acted upon inductively, and if it behaves like the hot iron ball in Prof. Guthrie's experiment it loses its positive charge. The molecule then being charged negatively is repelled along the lines of electric force against the positive leg. The forces moving it are electric forces, and the repetition of this action would cause a torrent of negatively-charged molecules to pour across from the negative to the positive side of the carbon horseshoe. If we place a metal plate in their path, which is in conducting con-

FIG. 14.



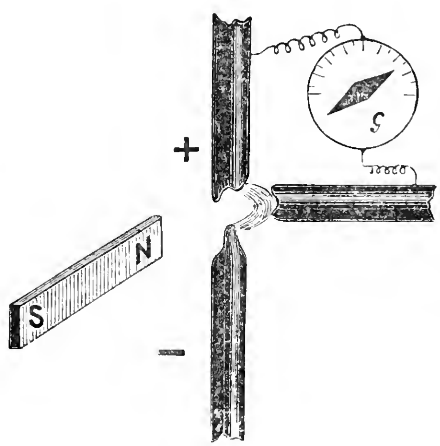
Rough diagram illustrating a theory of the manner in which projected carbon molecules may acquire a negative charge.

* "On a New Relation between Electricity and Heat," Phil. Mag. vol. xlv. p. 308. 1873.

nection with the positive electrode of the lamp carbon, the negatively-charged molecules will discharge themselves against it. A plate so placed may catch more or less of this stream of charged molecules which pour across between the heels of the carbon loop. There are many extraordinary facts, which as yet I have been able only imperfectly to explore, which relate to the sudden changes in the direction of the principal stream of these charged molecules, and to their guidance under the influence of magnetic forces. The above rough sketch of a theory must be taken for no more than it is worth, viz. as a working hypothesis to suggest further experiments.

These experiments with incandescence lamps have prepared the way for me to exhibit to you some curious facts with respect to the electric arc, and which are analogous to those which we have passed in review. If a good electric arc is formed in the usual way, and if a third insulated carbon held at right angles to the other two is placed so that its tip just dips into the arc (*see* Fig. 15), we can show a similar series of experiments. It is rather more under control if we cause the arc to be projected against the third carbon by means of a magnet. I have now formed on the screen an image of the carbon poles and the arc between them, in the usual way. Placing a magnet at the back of the arc, I cause the flame of the arc to be deflected laterally and to blow against a third insulated carbon held in it. There are three insulated wires attached respectively to the positive and to the negative carbons of the arc, and to the third or insulated carbon, the end of which dips into the flame of the arc projected by the magnet. On starting the arc this third carbon is instantly brought down to the same electrical potential as the negative carbon of the arc, and if I connect this galvanometer in between the negative carbon and the third or insulated carbon I get, as you see, no indication of a current. Let me, however, change the connections and insert the circuit of my galvanometer in between the positive carbon of the arc and the middle carbon, and we find evidence, by the violent impulse given to the galvanometer, that there is a strong current flowing through it. The direction of this current is equivalent to a flow of negative electricity from the middle carbon through the galvanometer to the positive carbon of the arc. We have here then the "Edison effect" repeated

Fig. 15.

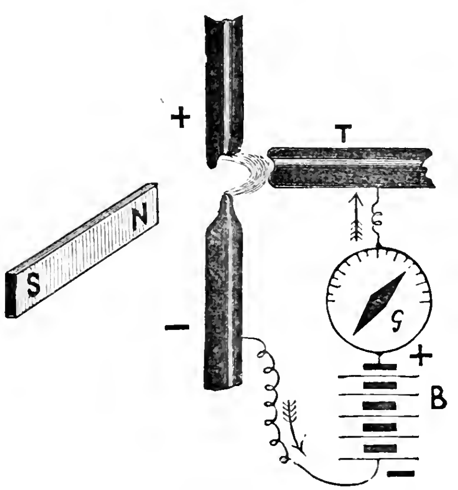


Electric arc projected by magnet against a third carbon, and showing a strong electric current flowing through a galvanometer, *G*, connected between the positive and this third carbon.

with the electric arc. So strong is the current flowing in a circuit connecting the middle carbon with the positive carbon that I can, as you see, ring an electric bell and light a small incandescent lamp when these electric-current detectors are placed in connection with the positive and middle carbons.

We also find that the flame-like projection of the arc between the negative carbon possesses a unilateral conductivity. I join this small secondary battery of fifteen cells in series with the galvanometer, and connect the two between the middle carbon and the negative carbon of the arc. Just as in the analogous experiment with the incandescent lamp, we find we can send negative electricity along the flame of the arc one way but not the other. The secondary battery causes the galvanometer to indicate a current flowing through it when its negative pole is in connection with the negative carbon of the arc (*see* Fig. 16), but not when its positive pole is in connection with the negative carbon. On examining the third or middle carbon after it has been employed in this way for some time, we find that its extremity is cratered out and converted into graphite, just as if it had been employed as the positive carbon in forming an electric arc. Time forbids me to indulge in any but the briefest remarks on these experiments; but one suggestion may be made, and that is that they seem to indicate that the chief movement of carbon molecules in the electric arc is *from* the negative *to* the positive carbon. The idea suggests itself that, after all, the cratering out of the positive carbon of the arc may be due to a sand-blast-like action of this torrent of negatively-charged molecules which are projected from the negative carbon. If we employ a soft iron rod as our lateral pole, we find that, after enduring for some time the projection of the arc against it, it is converted at the extremity into *steel*.

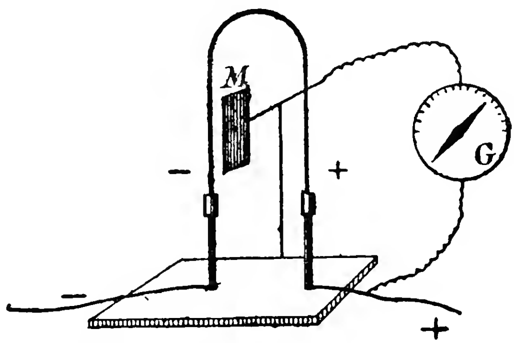
Fig. 16.



Galvanometer *G* and battery *B* inserted in series between negative carbon of electric arc and a third carbon to show unilateral conductivity of the arc between the negative and third carbons.

Into the fuller discussion as to the molecular actions going on in the arc, the source and nature of that which has been called the counter-electromotive force of the arc, and the causes contributing to produce unsteadiness and hissing in the arc, I fear that I shall not be able to enter, but will content myself with the exhibition of one last experiment, which will show you that a high vacuum, or, indeed, any vacuum, is not necessary for the production of the "Edison effect." Here is a carbon horseshoe-shaped conductor, not enclosed in any receiver (see Fig. 17). Close to the negative leg or branch, yet not touching it, we have adjusted a little metal plate. The sensitive galvanometer is connected between this metal plate and the base of the other or positive leg of this carbon arch. On sending a current through the carbon sufficient to bring it to bright incandescence, the galvanometer gives indications of a current flowing through it, and as long as the carbon endures, which is not, however, for many seconds, there is a current of electricity through it equivalent to a flow of negative electricity from the plate through the galvanometer to the positive electrode of the carbon. The interposition of a thin sheet of mica between the metal plate and the negative leg of the carbon loop entirely destroys the galvanometer current.*

Fig. 17.



"Edison effect" experiment shown with carbon in open air.

These experiments and brief expositions cover a very small portion of the ground which is properly included within the limits of my subject. Such fragments of it as we have been able to explore to-night will have made it clear that it is a region abounding in interesting facts and problems in molecular physics. The glow-lamp and the electric arc have revolutionised our methods of artificial lighting, but they present themselves also as subjects of scientific study, by no means yet exhausted of all that they have to teach.

[J. A. F.]

* This last experiment is due to my assistant, Mr. A. H. Bate.

WEEKLY EVENING MEETING,

Friday, February 21, 1890.

John Rae, M.D. LL.D. F.R.S. Vice-President, in the Chair.

Shelford Bidwell, Esq. M.A. LL.B. F.R.S. *M.R.I.*

Magnetic Phenomena.

The space around a magnet in which magnetic action is observed is called a field of magnetic force, or, more shortly, a magnetic field. Following Faraday's conception, we may specify a magnetic field by supposing it to be filled with a number of so-called "lines of force," the direction of the force (that along which a north pole is urged) being indicated by the direction of the lines, and its intensity by their concentration. In a uniform field of unit intensity, the lines of force are straight and parallel, and each line is exactly one centimetre distant from its nearest neighbour; so that, if a flat surface were held transversely to the direction of the lines, one line would pass through each square centimetre of the surface. In a weaker field the lines would be farther apart; in a stronger one they would be packed more closely together. The direction of the earth's magnetic force at any point in or near London is, roughly speaking, from south to north, at an inclination of 67° to the horizon; its intensity is approximately such that one line of force traverses every two square centimetres of a transverse plane surface, *i. e.* half a line, for each unit of area. The intensity of a unit field of magnetic force is therefore equal to about twice the total intensity of the magnetic field due to the earth.

It is a remarkable fact that iron, and in a less degree the two rarer metals nickel and cobalt, when placed in a magnetic field, possess the property of multiplying the number of lines that would naturally fill the space occupied by them. Thus, a long and thin iron rod placed lengthwise in the earth's magnetic field will not merely be traversed by half a line for each square centimetre of its section, as a glass or copper rod would be; the half line will (at least in the middle portion of the rod) be multiplied something like 600 times, raising the actual number of lines through the iron to about 300 per centimetre of section.

By means of electric currents it is easy to produce magnetic fields having a far higher intensity than that of the earth. Suppose, for example, we take a long brass tube, and wind around it a quantity of insulated copper wire, forming 16 convolutions in each centimetre of length; a current of 10 ampères circulating through such a coil would generate in the interior a magnetic field having an intensity of about 200 units. An iron rod placed inside this tube would be traversed by perhaps as many as 18,000 lines per centimetre. Although this is a very large number, it will be noticed that it is smaller, in proportion, than was obtained when the magnetic field of the earth alone was employed. In that case a field of half a line to the centimetre was found to induce 300 lines in the iron, the multiplying power being 600. But with an external field of 200 the multiplying power is only about 90, a very considerable falling off. It is usual to denote the number of lines per square centimetre in the magnetic field by the letter H, and those induced in the iron by B, while the multiplier is indicated by the Greek letter μ. We may therefore write:—

$$B = \mu H.$$

B is commonly spoken of as the "magnetic induction," and μ as the "permeability."

It used to be assumed that, except in strong fields, the permeability μ was practically a constant for the same specimen of metal. We have already seen that this is by no means the case, and how very far it is from being so is clearly shown by the following table, in the first and third columns of which are given corresponding values of H and μ for an average specimen of wrought iron.

TABLE I.—IRON.

H Field.	B ($= \mu \times$ H) Induction.	μ Permeability.
0·2	80	400
0·5	300	600
1	1,400	1,400
2	4,800	2,400
4	8,800	2,200
7	11,200	1,600
11	13,200	1,200
16	14,400	900
65 (Rowland)	16,500	255
200 (Bidwell)	18,000	90
585 "	20,000	34
24,500 (Ewing)	45,300	1·9

It will be remarked that, as the strength of the field increases from the smallest values, the permeability at first rises with enormous rapidity, attaining in a field of 2 or 3 units a maximum value of more than 2000; then it falls again, rapidly at first, and afterwards more slowly, until with a field of 65 lines to the centimetre the permeability is no more than 255. So far, the figures in the table (which are given in round numbers) are based upon experiments made by Prof. Rowland sixteen years ago. Plotting corresponding values of μ and B, Rowland constructed a curve, the form of which led him to the remarkable conclusion that the value of the magnetic induction B could not possibly exceed a certain definite limit, and that, in fact, no magnetic force, however great, could induce in iron more than about 18,000 lines per centimetre. This conclusion, which seemed to be in agreement with Weber's theory of magnetism, was generally accepted as correct. Unfortunately, however, Rowland's experiment did not go quite far enough. If he had been able to carry his magnetising force a little beyond 65 units, he would have seen that there was no such limit as he supposed. More recently, an induction of 18,000 has been actually obtained with a field of only 200, the permeability being 90. With the stronger field of 585, the induction was found to be 20,000; and quite lately, Professor Ewing, employing a field of 24,500, has obtained an induction of 45,300, the permeability being 1·9. Ewing concludes that there is no limit whatever to the degree to which magnetic induction may be raised; and there can be no doubt that he is right.

But while Ewing's experiments tend to show that the number of magnetic lines which can conceivably be made to run through a piece of iron is indefinitely great, they at the same time clearly indicate that the number of *additional* lines in excess of those contained in the field before the iron was placed there, has a very definite limit. This limit, for the piece of wrought iron which he used, appears to have been about 21,000, and it was practically reached with an external field of about 2000. For this sample of iron we may, therefore, say that in fields of 2000 and upwards,

$$\mathsf{B} = \mathsf{H} + 21{,}000.$$

Closely connected with the questions which have just been discussed, are the further questions:—What are the conditions affecting the lifting-power of an electro-magnet? and, What is the greatest lifting-power attainable?

One point of fundamental importance was settled experimentally by Joule many years ago. He found that the power of a uniform electro-magnet varies directly as the sectional area of the iron core, so that, for example, a magnet with a section of two square inches would, other things being equal, carry twice the weight that could be supported by one with a section of only one square inch. Joule also studied the effect of varying the strength of the current passing through the surrounding coil, and ascertained that while up to a certain point increase of current was accompanied by marked increase of lifting-power, yet when the current exceeded a more or less definite limit, further increase of it produced comparatively little effect. Reasoning upon his experiments, he formed the opinion—in which long afterwards Rowland concurred—that no current, however great, "could give an attractive power equal to 200 lbs. per square inch."

It has, however, since been shown that this statement is not quite true. In the course of some experiments made in 1886, with a semicircular electro-magnet and a semicircular armature of soft iron, a weight of more than 200 lbs. per square inch was easily carried, though the current was very far indeed from being infinite; it was, in fact, about 5 ampères.

If, as we have seen to be the case, there is no limit to the number of magnetic lines which can be induced in an iron bar, then, theoretically, there can be no limit to the lifting-power which an electro-magnet can be made to exhibit. Practically, however, a limit is imposed by the fact that we cannot command an unlimited current of electricity, nor would wires of any known material convey it even if we could. With sufficient current a little 3-inch electro-magnet might no doubt be made to lift a weight of a ton, but any attempt to pass such a current would result in the immediate fusion, or even vaporisation, of the wire-coils by the intense heat that would be generated.

The lifting power of an electro-magnet with an iron armature is proportional to the square of the total number of magnetic lines which run through the iron, inclusive of those due simply to the current in the coil. Ewing's experiments enable us to determine the greatest weight that a magnetised iron bar could support by itself, without any assistance from the surrounding coil. In the case of his specimen of iron it would be about 260 lbs. per square inch of section.

The permeability of an iron rod depends not only upon the intensity of the field in which the rod is placed, but also to some extent upon the physical condition of the iron, and is affected by such causes as mechanical stress or changes of temperature. If, for instance, we hang an iron wire vertically in a not very strong field, and stretch it a little by attaching a weight to its lower end, we shall find that the stretching causes a temporary increase in the longitudinal permeability of the wire. But if the experiment be repeated in a strong field, the effect will be reversed; the same load which before increased the permeability of the wire will be found to diminish it. In a field of a certain medium strength which can be determined by trial, the stretching will have no effect at all upon the permeability. This value of the field is called, after the first discoverer of the phenomenon, the "Villari Critical Point" for a certain load.

The permeability of a nickel wire appears to be always diminished by stretching, whatever the strength of the field or the magnitude of the load.

As the magnetic qualities of a rod of iron or other magnetisable metal are affected by a temporary strain or slight alteration of its form, so it has been found that the form of such a rod may be slightly altered by magnetising it. By the aid of very delicate apparatus it is possible to show that in a continually increasing field the length of an iron bar is at first increased, and afterwards diminished; that of a cobalt bar is at first diminished, and afterwards increased; while that of a nickel bar is always diminished. The following table shows the nature of the changes of length undergone by certain rods of iron, cobalt, and nickel, when magnetised.

Magnetising Force.	Elongations in ten-millionths of Original Length.		
	Iron.	Cobalt.	Nickel.
65	13	..	− 104
125	19*	− 10	− 167
237	7	− 31	− 218
293	0	− 37	− 233
343	− 6	− 44†	− 240
500	− 35	− 30	..
745	− 50	0	..
1120	− 65	45	..
1400	− 66	75	− 245

* Maximum increment. † Maximum decrement.

It was shown by Professor J. J. Thomson, a year or two ago, that the elongations and contractions of iron under magnetisation are intimately connected with the phenomenon which has been referred to as the Villari reversal. With a knowledge of the Villari effect, the elongation and subsequent contraction of an iron rod under magnetisation might have been predicted, and *vice versâ*. Now, since the elongations and contractions of cobalt are of the opposite character to those of iron, Professor Thomson's reasoning would lead us to expect a Villari effect in cobalt, which would also be of the opposite character. Quite recently, Mr. Chree, at Professor Thomson's suggestion, made some experiments to test the accuracy of this presumption, and found the Villari reversal which was anticipated. Again, the circumstance that nickel is always shortened by magnetisation, and never lengthened, indicates that there is no Villari reversal in that metal; and, in fact, though one has been looked for by Professor Ewing and others, it has never been found.

A few words in conclusion with regard to the effect of heat. Iron, when gradually made very hot, loses its magnetic susceptibility quite suddenly at a low red heat, and practically becomes a non-magnetisable metal. Pure nickel loses the greater part of its magnetic quality at a much lower temperature, perhaps about 300° C. Both metals again become magnetisable when cold. Dr. Hopkinson has lately discovered a very remarkable effect of heat upon the magnetic properties of an alloy of iron and nickel. If a bar or wire of this alloy be made red hot, and then allowed to cool, it is rendered permanently non-magnetic, although the metals of which it is composed are by themselves both strongly magnetic. But if this non-magnetic material be cooled to a temperature a little below the freezing point, and then again allowed to resume the ordinary temperature of the air, it will be found to have become almost as strongly magnetic as a piece of steel, and it will continue to be magnetic until it is once more made red hot. This is one of the most remarkable discoveries in magnetism that has been made for many years. It revives the question first suggested by Faraday—whether any metal whatever may not possibly be rendered magnetisable by exposure to a sufficiently low temperature.*

[S. B.]

---

* The discourse was illustrated by about twenty experiments.

WEEKLY EVENING MEETING,

Friday, February 28, 1890.

Colonel James A. Grant, C.B. C.S.I. F.R.S. Vice-President, in the Chair.

Professor C. Hubert H. Parry, Mus. Doc. M.A.

Evolution in Music.

As far as I can discover, not much has been said on the subject before us as yet; and as there is a great deal to be said, my only preliminary will be to remind you of one of Mr. Herbert Spencer's definitions of evolution, which happens to be most apt to our subject.

The formula in question is as follows:—Evolution is a "change from indefinite incoherent homogeneity to a definite coherent heterogeneity," accompanying the dissipation of motion and integration of matter; * which, for present purposes, I may expand into—a change from vague indefinite chaos to an aggregate of clearly-defined separate entities or organisms, each with functions well determined.

I shall endeavour to keep these formulas steadily in view, and to show how the various departments and phases of music, as we know it, have developed in consonance with them. My argument must necessarily take the form of a mere summary, as the strength of the case rests to a great extent on the uniformity of the principles of development; and I do not think that it will be of any real use to take an isolated department and discuss it in detail before the general aspect of the matter is clearly understood. I will begin then at once with the subject of scale-making. I presume that music began before the existence of scales, and that they were developed in the early attempts made by our savage ancestors to express their feelings in sounds. In fact, though the making of scales and the discussion of scales is now such a dreary and thankless matter, originally they were the product of emotion and imitation. In order to follow the process of development we must take the original material of music before scale-making began to be figuratively a chaos of possibilities, in which no points or relations were established. The process began when some savage expressed his feelings in some group of sounds, and insisted upon them clearly enough, and often enough, to make his fellow savages imitate him. The variety of relations of notes chosen by savages is sufficiently shown by records of varieties of existent savage music; ranging from the horrible grinding glide of the voice which certain cannibals use to express their feelings when

* 'First Principles,' xvi. § 138.

contemplating their yet living dinners, which resemble quarter tones, to the strange intervals, exceeding a tone, which occur in many highly-developed scales. The probability is that when we meet with a scale containing an eccentric interval, this eccentric interval is the original nucleus of the scale, to which other notes were added as the instinct and general intelligence of the savages improved. The sum of the process of scale-making amounts to this:—That first a simple nucleus of two notes was formed, and by very slow degrees other notes were added, till the whole range of sounds possible to the human voice was mapped out. This, obviously, is the first example of progress from the confused chaos of indefinite and unsystematised sounds to the heterogeneity of perfectly established scales. When the difficulties presented by the problem of contriving scales are realised (as they may be by any one who studies the question a little), it will be seen that the process must have been an enormously long one, taxing the musical instinct of man for probably thousands of years. As a matter of fact, scale-making, even in primary stages, was going on vigorously till not much over a century ago, and in some phases cannot be said to be by any means finished yet. Scales are always liable to alteration, whenever the instinct of composers leads them to divine an opportunity for expanding the material at their command for artistic purposes; and whenever the instinct of a number of musical beings ratifies the change as logical and artistically practical, it takes its place as an established fact.

The next step to merely dividing off the possible range of sounds into fixed relative positions, is to classify them into groups in which special notes have special functions. The music of the ancient world being all melodic, men's instincts impelled them to develop a scale system which gave them best opportunities for melodic variety. This naturally resulted in their having as many modes as possible; or, in other words, having as many varieties of relationship as they could devise between the key-note or final and the other notes of the scale. And they looked upon these various modes as having particular qualities of feeling—one mode being sad, another gay, another solemn, and so forth.

The Greek system was, no doubt, a highly-developed one for melodic purposes; but whatever its traditions were, they did not have much influence on our modern music, except through the actual distribution of the notes into modes. The Romans seem to have had no instinct for music. Their energies were occupied in organising the world as then known into a workable empire, and their leisure was occupied with kinds of amusements which have a tendency to destroy the taste for refined music. No two things seem more poisonous to musical art than spectacles of brutal violence which give people a taste for excessive excitement, and a luxurious life of frivolity into which enters a strong element of vulgar display. The decrepit condition of music in the early centuries of our era was as much owing to the neglect of the art by the Romans as to the falling to pieces of their empire. I should like to think that their neglect of the higher art of music was a concomitant of the corrupt condition of society which led to their downfall. At any rate, the state of music when we take it up in the Christian era, is but a ragged reminiscence of Greek traditions. Their scale system had been maintained to a certain extent in the use of the Christian Church, in some of those curiously vague and picturesque pieces of melody which go by the name of plain song, or *cantus planus*. One of the characteristics of these tunes is their strange indefiniteness, which is the chief cause of their picturesqueness; as our minds instinctively divine them to belong to an ancient and undeveloped age, and recall the poetical side of a primitive religion. This vagueness and homogeneousness only by degrees passed away, under a phase of musical development which belongs exclusively to modern times. Under the influence of the development of harmony the scale was classified into new groups, in which the relation of every note to every other in every scale, and the function of every one of them, became by degrees established. The development of harmony proceeded in exactly the same manner. The first experiment was the essentially homogeneous one of singing the same melody in two or three parts at different pitches simultaneously. The interval chosen always astonishes the modern mind, because it is so alien to our habits. But it is very easily accounted for. The musicians of the tenth and eleventh centuries chose the intervals of fourth and fifth, partly because it suited the relative distances of the voices from one another, such as tenor to bass and soprano to contralto; and also because the fifth and the fourth are the only intervals at which melodies can be sung without any marked contradiction occurring between the notes of the respective scales. If a third was taken, a leading note below the third would conflict with the second of the lower scale, and the second of its scale would conflict with the fourth of the lower, and so on; whereas the scales of the fourth and fifth only rarely conflict with the lower scale.

Combined with this is the fact that these mediævals' sense of harmony was slow in developing. At first they only regarded the fifths and fourths as consonant, and were very slow indeed in developing the appreciation of such intervals as thirds and sixths. The human mind had to be trained and educated up to it, much in the same way as moderns are educated up to Brahms and Wagner. From harmony in pure fifths, musicians passed slowly on by introducing ornamental notes, which was often done extemporaneously by singers, giving rise to what was called the "*contrapunctus a mente*" of later times. But the homogeneous condition of fifths and fourths was slow in passing to a greater variety, and composers were several centuries overcoming the elementary difficulties of part singing, to a large extent owing to the fact that their scale, which had been contrived for melodic effect, was not suited for the purposes of harmony.

The development of harmony for six hundred years, from A.D. 1000 to A.D. 1600, had underlying it a constant but very slow change in the structure of the scales; and the progress was made all the slower by the notion prevalent in men's minds that these scales were divinely-appointed institutions, and that tampering with them was like mending the ordinances of the Deity. Much of the necessary mending was done by profane secularists, who wrote dance tunes and secular songs; and the changes crept into serious music in defiance of papal restrictions and ecclesiastical reluctance, in obedience to the instinct which was as powerful in its slow steady action as any law of the physical world. The thin end of the wedge for altering the scale was inserted in the shape of certain arbitrary accidentals which were introduced to modify obvious crudities of harmony; and when people got accustomed to them they by degrees established themselves as part of the scales, and supplied the means of a new system of classifying and defining the relative importance and functions of notes in a scale.

The methods adopted by the mediæval composers for regulating a piece of music were distinctly homogeneous. The commonest was to take a familiar tune and give it to the tenors to sing, and to add other parts to it in such a way as to produce a harmonious and expressive whole.

Another common device was to take two familiar tunes and to twist and alter them about till it was endurable to sing them together; sometimes adding another part, which sang nonsense syllables, such as Balaam, Portare, or what not, to such notes as were available. I cannot say that the result is commonly pleasing, but they improved in the course of centuries, and the art in general got the more heterogeneous as they found out fresh methods of artistic procedure. In all of these, till the end of the sixteenth century, the same principles are discernible. The harmony is always arrived at by combining independent voice parts together, never by writing definite lump chords. It was not till after the great development of pure choral art had passed to its highest culmination, in the time of Palestrina and Marenzio, that men began to think of writing chords as chords. While this lengthy development was going on, they were unconsciously absorbing the impressions which the sounds of chords produced upon them; and no one ever produced more divinely pure sounds in the shape of choral chords than Palestrina and Marenzio; but they managed to contrive them by the marvellous skill with which they distributed their combined independent voice parts, and not by writing them deliberately as chords; and the reluctance of the human mind to come to close quarters with chords as such hindered them from discovering the relationship of chords to one another; and hence kept their art in a singularly indefinite state. All the choral music of the greatest period, as well as of earliest days, is singularly indefinite in design, owing to this lack of a sense of chord relationship, and to uncertainty and variableness in the aspects of the cadences.

It is true the free lances of art and the secularists had done something towards defining the plan of movements by cadences like ours, but matters did not come to a crisis till men began to alter their point of view. The change of point of view was ultimately brought about by one of the most deliberate and conscious revolutions ever attempted in art.

The beginning of our modern development of opera and oratorio, and all the modern instrumental forms of art, was the fruit of some speculations of a group of Italian enthusiasts at the end of the sixteenth century, who hoped to be able to revive the ancient manner of performing Greek dramas. They imagined that it could be achieved by making a musical imitation of the cadences of the voice in declamation, and adding the support of some simple instrumental accompaniment. The result was one of the most chaotic and formless specimens of art ever devised by the mind of man. Their instinct for systematic progressions of chords was totally undeveloped, as was their sense of key in our modern sense; and they therefore had no principle by which to arrive at any effect of design. Moreover, their radical idea almost precluded the possibility of musical design, as they thought nothing was needed but recitation of the poetry, and that the dramatic situation and the language would carry the attention along and sufficiently occupy the mind without need of musical form. Their experiments were all the more crude because the composers were practically amateurs, with no knowledge of the technique of their art; and though they had great zeal, it was by no means zeal according to knowledge, but often outran discretion. But it may be said, on the other hand, that absence of knowledge of the traditions of their art left them all the freer to experiment in the new country they had found, and the obviousness of their mistakes led the sooner to their being reformed.

The situation is precisely analogous to that of the earliest stages of scale-making, only in a different plane. The texture of the early oratorios, operas, and cantatas was almost homogeneous. The recitative winds helplessly along, page after page, in monotonous inconsequence, only occasionally varied by a fragment rather more expressive than the rest, and by short fragments of very empty instrumental music called "ritornellos," and equally pointless fragments of chorus. The way in which nuclei began to form was through composers perceiving what excellent opportunities for musical expression were offered by salient points of special dramatic or pathetic interest in the plays, and they soon saw that a point which was brought out strongly in this manner, laid special hold of the audience. When this was once discovered, it did not take them long to realise the effect which was produced by repeating such a passage; and though it took them half a century to find the most suitable manner to dispose of such a balance of phrases, it was within twelve years of the first operatic venture that Monteverde made a great effect by the simple process of giving a very expressive phrase to a singer in a specially interesting situation, following it by a phrase which is more or less contrasted with it, and then going back to the first phrase again; a process which contains in miniature the design of that aria form which afterwards became so universal that it pervaded all operatic literature, and became a perfect plague from its constant recurrence.

The opening of public opera houses in Venice in 1637, and the great success which attended the venture, and its rapid extension, gave composers great opportunities, and enabled them to make rapid progress in defining the contents of their works. The introductory instrumental summons to attention, which in Monteverde's hands was a noisy clatter of braying instruments, all on one chord, developed into the neat little overture of Alessandro Scarlatti, which was of momentous importance as the immediate origin of our modern symphony; and the texture of the opera itself progressed to a stage in which the arias obtained a distinct and permanent (though too prominent) form, and alternated throughout with recitatives and ritornellos, and an occasional chorus. Unfortunately, progress was stayed here for a long time, through the indolent carelessness of operatic audiences, who used the performances even then as fashionable opportunities for gathering and talking, and only cared to listen to the prominent singers; and even composers as great as Handel fell in with the apparently inevitable too complacently; and though great skill was evolved in giving variety to the respective arias, and in giving them a definite and contrasting dramatic character, the scheme was so monotonous that it has condemned the operatic works of all composers till the end of Handel's time to irremediable oblivion. This tame acquiescence in the bad taste of the public has been their curse, and ours too; for though even Alessandro Scarlatti's operas contain fine music, and Handel's hundreds of things which are really splendid, the desperate monotony of the design makes them utterly unendurable as wholes to an average audience; and even as historical studies, it would take the strongest and most obstinate patience to sit out one of them with attention.

The development of oratorio, up to a little before Handel's time, had followed much the same lines as opera. The admirable skill and judgment of Carissimi had at first used the opportunities which the oratorio form affords with a success which was full of hopeful auguries; and his work was followed up with great power by Stradella. They both gave the form a high degree of variety, by introducing large and broad choruses among their solos, and by devising great variety of plan even in their solo music. But the blight of the star system fell upon oratorio likewise in Italy, and for a time it degenerated into the same monotonous scheme of alternate arias and recitatives as the opera; and it was not till this form of art became the cherished favourite of much more earnest and patient nations, that the oratorio developed into the noble plan and the large and well-defined proportions which we find in the few great masterpieces of oratorio art from Handel and

Bach's time onwards. The later development of oratorio and opera depended to a great extent upon the progress of instrumental forms of art, which were slower in making a beginning, but developed more steadily, and under the influence of a greater spirit of earnestness; as in instrumental music the temptations to mere meretricious display are not so great and inevitable.

Composers were very slow in finding out what to do in instrumental music. They imitated the old choral forms, such as madrigals and canzonas, being led to the procedure by the similarity of the group of independent instruments to the group of independent voices, but they did not arrive at anything very enjoyable, except in one line, which was our modern type of fugue. This, in its highest form, is probably as much an instrumental product as a vocal one, though originally based on choral forms of art. The immediate origin of its peculiar traditions for enunciating the subject or musical idea, is based upon the obvious device of making the different voices sing the same phrase to the same words; which was systematised in early days, up to a certain point, by making the voices take the phrases in the parts of the scale which best suited their register. This resulted in a very effective balance of question and answer (or Dux and Comes) even in early times; but in the old polyphonic days, after the first statement of the initial phrase by each member of the group of voices, the movement tailed off into indefiniteness, and the initial phrases did not appear again. In course of time composers found out the effect of coherence which a frequent repetition of so salient a feature as the initial phrase gave to a whole movement, and began to repeat their subject over and over again.

The progress from such modified homogeneity to definite heterogeneity was arrived at under the influence of modern harmonic conditions and modes of thought, in which these alternations of the subject and the episodes were accompanied by contrasting changes of key—passing out of the original key into others, and drawing the recurrence of the subjects closer and closer as the original key was returned to, and firmly re-established at the conclusion. This form was one of the first to arrive at maturity, partly through the genius of the great organist Frescobaldi, and later, obviously, through Handel and Bach; and it has not been materially improved upon by after ages, though its wonderful elasticity always admits of its being presented in artistic aspects, and with fresh artistic objects. And though pedantry has run riot in it, it has not ceased to be inviting to some types of really poetical and musical composers.

The other kind of instrumental music, which was the ultimate basis and root of at least half of all modern instrumental music, was the aboriginal dance form. At the time when this type began to attract the attention of artistic composers, it had reached the not very advanced stage of a tune divided by a strong close into two halves, the first of which tended out from the principal key centre to a melodic or harmonic centre which was in apposition to it, and the second of which journeyed home again. In these the musical material was more or less homogeneous throughout. But the necessity for clear periods and clear grouping of rhythm had impelled composers to discover harmonic closes very early in dance tunes; and musical instinct, while frequently observing them, was impelled to discover the most suitable ways for distributing them. And this same musical instinct, working on little more than common-sense lines, evolved from this little dance type the remarkable design which serves for all the finest movements of our modern symphonies and sonatas.

As this is one of the most remarkable examples of the manner in which things progressed from homogeneity to heterogeneity, I think it worth while to enter into it in detail.

The process of the development of the design was as follows:— At first the style of the music through the whole of the movement was homogeneous; and the only strong points which stood out and defined form were the beginnings and closes of each half of the movement. The beginning of the second half matched the beginning of the first half, but began in the antithetical key. The end of the whole matched the end of the first half in musical material; but the end of the first half was in the key of apposition, and the end of the whole was of course in the principal key. When this is merely described in this manner it sounds like a perfectly symmetrical design. In fact, it was too obviously symmetrical, and covered too little ground. The contrasts were insufficient, and the quality of the music was too uniform; and in course of time composers and auditors alike found this out. The first step in advance was to give more weight to the closes of each half, by which process a strong contrast began to present itself between the beginning and end of each half, as well as between the halves; as the cadence portion by degrees developed into such distinctness that it took upon it the appearance of a new subject.

Simultaneously with this, the character of the music underwent a change, and instead of a uniform contrapuntal flow, became a well-knit succession of independent and often strongly-contrasted ideas. By this means it came to pass that the movement began with a subject in a principal key, and then moved out to a contrasted key to present a contrasting subject; and this group formed the first half of the movement. The second half began with a restatement of the principal subject in the key of apposition, and then wandered about through strange keys to give a sense of contrast, till it reached the key the movement started from; in which key the second subject was given, and the movement then ended. In course of time the defects of this type became apparent. The beginning of the second half did not present sufficient contrast to the design of the first half. More freedom was obviously required, and more weight on the principal key at the conclusion. To attain this, the principal subject was repeated again when the return to the original key was made near the end. Then it was found that the principal subject came in its concrete form too often; so its reappearance at the beginning of the second half was dispensed with, leaving the design exactly as it appears in the finest movements of Mozart, Haydn, Beethoven, and Brahms. The functions of these various divisions may be summarised as follows:—The first half establishes the principal key of the movement and its contrasting centre; everything being contrived with the purpose of marking their apposition, and therefore tending to regularity. The second half begins with such treatment as gives the strongest relief to the regularity of the first by breaking up the subjects into small portions and interlacing them irregularly, and by keeping up a constant shimmer of modulation; and finally the principal key of the movement is re-established firmly by presenting both subjects successively in that key. Into subordinate modifications of this structure, and the details of it, it is not possible to enter here. It must be sufficient to say that in the greatest works of Beethoven there is hardly a bar or a step of one note to another in all the complex structure which has not its intelligible place and function in the general scheme of the movement, and it is difficult to see how the differentiation of parts and the distribution of functions could be carried out more perfectly.

The complete design of symphonies and sonatas comprised other movements of less complex and less interesting structure than this, which were combined with it for the sake of contrast and balance. The first type of such grouping of movements was the attempt of early composers to produce an artistic effect by playing two or more dance tunes together, so that their contrasts might show off one another. They began with such simple contrasts as Pavans and Galiards, and progressed up to the relative complexity of the suites of Couperin, Bach, and Handel. But this stage of advance was only an arrival at a very modified degree of heterogeneity; for the movements were always in the same key, and almost always in the same form; that of the dance tune in two balanced halves. The symphonic or sonata group obtained a much higher degree of contrast, by putting the central movements into contrasted keys, and by strongly contrasting the forms of the movements themselves. A common type is that of four movements, of which the first is the highly developed form, comprising strong contrasts above described; the second an imitation of the operatic aria; the third a dance tune pure and simple; and the fourth a rondo, which is commonly a simple series of alternate contrasting dance measures.

We must now take a rapid survey of the evolution of modern orchestration. The greater part of the evolution has been carried on in the department of instrumental music, especially in the symphonies of the greatest composers. These are derived from the overtures which preceded the early operas, which were divided as early as Alessandro Scarlatti's time into three movements; the first solid and quick, the second slow, and the third quick and light. The practice of playing them apart from the operas began very early, as they were found very useful at the feasts and dinnerparties of magnates, who kept private orchestras to encourage conversation and temper the asperity of any glaring absence of it. These symphonies came very greatly into request in the next generation after Bach and Handel, and were supplied in cartloads apart from their usual connection with operas, but were still called overtures or sinfonias, both of which names had commonly been applied to them while they were attached to operas. These works were of very limited interest, and were evidently very roughly played. The instruments were lumped together crudely to make a noise, and very little variety was aimed at; while the functions of the instruments were not ascertained or their idiosyncrasies observed. When, under more favouring circumstances, development definitely began, the evolution took the same aspect as in other departments of art. A violin player named Stamitz, who was conductor at Mannheim, gave the development a push by endeavouring to obtain variety in nuances, and by using the different qualities of the instruments for purposes of frequent contrast. Mozart visited Mannheim when a young man, just before his second visit to Paris, and was evidently struck by the possibilities Stamitz's procedure seemed to promise; and he gave his higher abilities to the work of diversifying the effects of orchestral colour. Before this time the violas commonly played a great deal with the basses, the wind instruments with the strings of the same average pitch; while the horns, which were not tractable enough to follow so slavishly, were the earliest to attain some independence, but did not as yet do much more than fill up the harmonies and increase the mass of sound. The colours had in fact been mixed up in aimless confusion; and the various instruments, except when playing long solos, did not have much definite independence one from another. After this time the violas drew away from the basses, and found their own separate place in the group, as representing a special colour and a special individuality. In like manner the special individuality of the hautboys found its true place as a factor in the complicated nexus of tone-quality and instrumental idiosyncrasy. Other instruments were added, which supplied other qualities of tone, and the particular functions which each instrument was most fitted to perform were by degrees ascertained by innumerable experiments and by development of instinct; the natural tendency being, as time went on, for each several instrument to attain more and more independence, and for more and more respect to be paid to the various idiosyncrasies of each member of the family. The hautboy and the clarinet no longer struggled to play fiddle passages, nor the bassoon only to reinforce the bass and play passages which were better fitted for stringed instruments; even that distinguished survival from the primitive music of savages, the drum, was no longer condemned merely to add to the noise of forte passages, but was used with dramatic significance, and even at times used to express characteristic musical figures, or to play mysterious and hazy-sounding chords. By such processes, and by establishing a clear distinction between the three groups of stringed, wood wind, and brass instruments, orchestral music progressed from the homogeneity in which all the functions of the different instruments were jumbled up together, to that elaborate heterogeneity of Wagner; in which every instrument, from piccolo to double bassoon, has its own place in the scheme, and its own function to perform. And, indeed, one of the things which is looked upon as a test point in good writing for an orchestra is that no player shall waste his breath or his muscular efforts in vain; and a composer who now writes a part for an instrument which does not "tell" is not a full master of his craft. This high development of orchestration is indeed the furthest point of subtlety to which modern musical development has progressed; and it has grown with a surprising degree of development in the public for appreciating rapid varieties of tone effect, and a certain dangerous susceptibility to the exciting effects of colour, which always has a tendency to deaden the faculty for appreciating beauty of artistic design. And in this direction we already see possibilities of decadence; as many works which take great hold of musical natures show a decided falling-off from the perfect design of the great masters, towards that hazy indefiniteness and intangible vagueness of progression and structure which clearly portends relapse into homogeneity in one respect. But it must be said, by way of caution against too hastily taking a pessimistic view of the situation, that in artistic works of real value there is always an element which defies pure intellectual analysis: and it may be that the principles of form we so admire in the works of our greatest musicians are undergoing some subtle change to which we are not at present capable of giving a definition.

The later evolution of the great forms of opera and oratorio does not demand very lengthy consideration. No branch of art affords so many examples of the non-survival of the imperfect as the opera. The stage offers so many opportunities of obtaining strong impressions by vapid means that the vast majority of people who write for it seem to get bewildered, and either deliberately appeal to the public by cheap claptrap, or lose their capacity of judging what is worthy of art and what is not; and the peculiar attitude of the operatic public has always been against thoroughness in any respect; and the result is that the composer who writes for popular success does nothing for art, and the composer who feels his art deeply gets no thanks or encouragement from the public. The opera of the type written by Handel, Hasse, John Christian Bach, Galuppi, and hundreds more, was once the joy of the world; but no branch of art is more utterly dead, or more incapable of revival on any terms whatever. Gluck's reforms came practically too soon, and beautiful as much of his work is, it is almost incapable of revival except in fragments. But he did give an impulse to the evolution of operatic art, and set men's instincts to work again to clear out dead matter and help the sluggish evolution to go on again. The stiff grouping of arias and recitatives was diversified by trios, quartettes, and such ensemble pieces; and by finales elaborately contrived of groups of various forms all well defined. The homogeneous character of the musical material grew into an infinite diversity of characteristic passages, each apposite to the character and situations in the play; and the art of orchestration growing parallel to its growth in instrumental music, afforded absolutely bewildering opportunities of effect in the hands of a competent composer. In Weber a very high standard of artistic perfection in all departments was arrived at; in Wagner, the utmost heterogeneity of which the art seems capable, both in respect of his orchestration, the definition of the several characters, the well-defined independent "leit motive," and the infinite variety of sentiment and expression; while the several functions of stage effect, dramatic interest, and musical expression are so well and clearly balanced in his best work that it is difficult to say at any given moment that any one is made subservient to the other.

The development of oratorio, not so cursed with over many facilities, has been on parallel lines to opera, and though it has not arrived at such a complexity of definite ingredients, cannot now be said to be in a chaotic or ill-developed condition.

It remains now only to point out the manner in which the art in general has progressed, like its constituents, from limited sameness to infinite well-defined variety. At the end of the sixteenth century there was nothing but choral music, and a little crude instrumental music, which was chiefly imitated from choral music. At the beginning of the seventeenth century opera and oratorio began to emerge from the nebulous state of the art, and went revolving off on their respective orbits. Instrumental music began to get independent status in the Suites and Toccatas, and so forth, and rapidly divided itself off into various well-defined groups. The orchestral symphony gained an independent definiteness on its part; the pianoforte sonata, like in form, but quite distinct in treatment, was defined by the growing skill by which the resources of the instrument were developed by composers and players. Chamber music for solo instruments grew up, with all its special artistic characteristics; then followed the new class of small lyrical compositions for the pianoforte, of which Chopin and Schumann are the happiest exponents, and their variety and well-defined independence in hundreds of examples is too familiar to need insisting upon. And so the art goes on, branching out and subdividing into infinity of part songs, solo songs of many calibres—noble, good, indifferent, and detestable—cantatas, symphonic poems, rhapsodies, concert overtures, comic operas, artistic studies, odes, and hundreds of other forms, each with their particular artistic idiosyncrasies and special adaptations, and all becoming by degrees more short lived, more journalistic, and more calculated for quick returns and rapid extinctions, to make way for fresh products, which will also serve their time and shortly make way for similarly short-lived journalistic productions, apt for their day in expressing the superficial tastes and moods of the people of their day, and no more.

The planets were evolved first, and are destined for a life of ages; the smaller things which are evolved after they have settled into their courses, are in great part short-lived and constantly changing their aspects. The greatest achievements of art come early in an art history, when there is room for composers or artists to move in unexhausted fields, and to create things which are great and broad and simple. When there are no new lands left to conquer or explore, men must make the best of their home gardens, and find something worth doing on a smaller and less permanent scale; and the best can be no more than that which expresses well and truly the best and truest things which lie in the emotions and mind of man. There still are martyrs who sacrifice their lives to ideals, and look for neither popularity nor pay; and it is still possible in music to write what the present generation on the whole will take no notice of, but the next will cherish. And if the average of what is heard is mere journalism, and the majority of what is produced is condemned by inexorable law to be ephemeral, the world still possesses the permanent great works of art; and those who have any instinot for what is noble and great in art can always learn to appreciate these products of great eras whose value is not impaired by the flight of time. And the store is so rich and abundant, that there is no fear of our musical hungers finding nothing to feed upon. If we set our minds to it early and late, and determine to appreciate and love the best, the fact that the laws of evolution make it apparently impossible for us to meet any more Homers, or Æschyluses, or Shakespeares, or any more Palestrinas, and Bachs, and Beethovens in the flesh, it is not so much to be regretted as long as we are in possession of their works.

[C. H. H. P.]

GENERAL MONTHLY MEETING,

Monday, March 3, 1890.

Sir James Crichton Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

Lesley Alexander, Esq.
Mrs. Shelford Bidwell,
Miss Florence Bramwell,
Sir John Coode, K.C.M.G.
Sir George Errington, Bart.
Ralph Heap, Esq. M.A. (Oxon.).
Miss Mary Lawrence,
William Ramsay, Esq. Ph.D. F.R.S.
Herbert C. Saunders, Esq. Q.C.
A. Percy Sinnett, Esq.
W. S. Squire, Esq.
John Strain, Esq.
Sir John Richard Somers Vine,
James Walker, Esq.
Mrs. James Watney,

were elected Members of the Royal Institution

The Special Thanks of the Members were returned to Warren de la Rue, Esq. *M.R.I.* for his valuable present of a Miniature Portrait of his father, the late Dr. Warren de la Rue, F.R.S. *M.R.I.*

The following letter from Lady Gull to the Honorary Secretary was read:—

"Dear Sir, "Torquay, *February 7th*, 1890.

"I beg to thank you for your kind letter of the 5th instant, enclosing the copy of a Resolution passed by the Managers of the Royal Institution expressive of their deep regret at the loss of my Husband, and of their intention to place on record on the Minutes of the Institution their sense of the same.

"May I ask you at the next Meeting to convey to the Managers my deep appreciation of their great kindness in this matter, as also of the expression of their sincere sympathy in my bereavement.

"I am, dear Sir,
" Yours faithfully,
"S. A. Gull."

The following Arrangements for the Lectures after Easter were announced:—

The Hon. George C. Brodrick, D.C.L. Warden of Merton College, Oxford —Three Lectures on The Place of Oxford University in English History; on Tuesdays, April 15, 22, 29.

Louis Fagan, Esq. Assistant Keeper of Prints and Drawings, British Museum—Three Lectures on The Art of Engraving: 1. Line Engraving; 2. Wood Engraving; 3. Mezzotint Engraving; on Tuesdays, May 6, 13, 20.

Andrew Lang, Esq.—Three Lectures on The Natural History of Society; on Tuesdays, May 27, June 3, 10.

C. V. Boys, Esq. A.R.S.M. F.R.S. *M.R.I.* Assistant Professor of Physics, Normal School of Science, South Kensington—Three Lectures on The Heat of the Moon and Stars (the Tyndall Lectures); on Thursdays, April 17, 24, May 1.

Professor Dewar, M.A. F.R.S. *M.R.I.* Fullerian Professor of Chemistry, R.I. Jacksonian Professor of Natural Experimental Philosophy, Cambridge—Six Lectures on Flame and Explosives; on Thursdays, May 8, 15, 22, 29, June 5, 12.

Captain W. de W. Abney, R.E. C.B. F.R.S. *M.R.I.*—Three Lectures on Colour and its Chemical Action; on Saturdays, April 19, 26, May 3.

Charles Waldstein, Esq. Litt.D. Ph.D.—Three Lectures on Excavating in Greece; on Saturdays, May 10, 17, 24.

The Rev. S. Baring-Gould, M.A.—Three Lectures on The Ballad Music of the West of England (with Musical Illustrations); on Saturdays, May 31, June 7, 14.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FOR

Academy of Natural Sciences, Philadelphia—Proceedings, 1888. Part 2. 8vo. 1889.

Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta: Rendiconti. 2° Semestre, Vol. V. Fasc. 11, 12. 8vo. 1890.

American Academy of Arts and Sciences—Proceedings, Vol. XV. Part 2. 8vo. 1889.

Astronomical Society, Royal—Monthly Notices. Vol. L. No. 3. 8vo. 1890.

Bankers, Institute of—Journal, Vol. XI. Part 2. 8vo. 1890.

British Architects, Royal Institute of—Proceedings, 1889–90, Nos. 8, 9. 4to.

British Museum (Natural History)—Catalogue of Fossil Reptilia and Amphibia, Part III. and Guide to the Mineral Galleries. 8vo. 1889.

Buckton, George B. Esq. F.R.S. M.R.I. (the Author)—Monograph of the British Cicadæ or Tettigiidæ, Part 1. 8vo. 1889.

Cambridge Observatory—Astronomical Observations, Vol. XXII. 1866–9. 4to. 1890.

Cassedy, W. S. Esq. (the Author)—Is the Copernican System of Astronomy True? 8vo. 1888.

Chemical Society—Journal for February, 1890. 8vo.

Cracovie, l'Académie des Sciences—Bulletin, 1890. No. 1. 8vo.

Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I.—Journal of the Royal Microscopical Society, Part 6a; 1890, Part 1. 8vo.

Editors—American Journal of Science for February, 1890. 8vo.

Analyst for February, 1890. 8vo.

Athenæum for February, 1890. 4to.

Chemical News for February, 1890. 4to.

Chemist and Druggist for February, 1890. 8vo.

Electrical Engineer for February, 1890. fol.

Engineer for February, 1890. fol.

Engineering for February, 1890. fol.

Horological Journal for February, 1890. 8vo.

Industries for February, 1890. fol.

Iron for February, 1890. 4to.

Ironmongery for February, 1890.

Murray's Magazine for February, 1890. 8vo.

Nature for February, 1890. 4to.

Photographic News for February, 1890. 8vo.

Revue Scientifique for February, 1890. 4to.

Telegraphic Journal for February, 1890. fol.

Zoophilist for February, 1890. 4to.

Florence Biblioteca Nazionale Centrale—Bolletino, No. 100. 8vo. 1890.
Geological Institute, Imperial, Vienna—Verhandlungen, 1889, No. 18; 1890, Nos. 1, 2. 8vo.
Geological Society—Quarterly Journal, No. 181. 8vo. 1889.
Georgofili, Reale Accademie—Atti, Quarta Seria, Vol. XII. No. 4. 8vo. 1889.
Harlem, Société Hollandaise des Sciences—Archives Néerlandaises, Tome XXIV. Liv. 1. 8vo. 1890.
Imperial Institute—School for Modern Oriental Studies, Inaugural Address, &c. 8vo. 1890.
Johns Hopkins University—University Circulars, No. 78. 4to. 1890.
Langley, S. P. Esq. (the Author)—Solar and Lunar Spectrum (National Academy of Sciences, Vol. IV.). 4to. 1889.
Liverpool Polytechnic Society—Journal, Vol. LII. 8vo. 1890.
Manchester Geological Society—Transactions, Vol. XX. Parts 14, 15. 8vo. 1890.
Medical and Chirurgical Society, Royal—Transactions, Vol. LXXII. 8vo. 1889.
Pharmaceutical Society of Great Britain—Journal, February, 1890. 8vo.
Photographic Society—Journal, Vol. XIV. No. 5. 8vo. 1890.
Rathbone, E. P. Esq. (the Editor)—The Witwatersrand Mining and Metallurgical Review, No. 1. 8vo. 1890.
Royal College of Surgeons, Edinburgh—Laboratory Reports, Vol. II. 8vo. 1890.
Royal Society of London—Proceedings, Nos. 285, 286. 8vo. 1890.
Selborne Society—Nature Notes, Vol. I. No. 2. 8vo. 1890.
Society of Arts—Journal for February, 1890. 8vo.
St. Bartholomew's Hospital—Report, Vol. XXV. 8vo. 1889.
St. Pétersbourg Académie Impériales des Sciences—Mémoires, Tome XXXVII. No. 3. 4to. 1890.
Bulletin, Tome XXXIII. No. 3. 4to. 1890.
United States Geological Survey—Monographs, Vols. XIII. and XIV. 4to. 1887–8.
Bulletins, Nos. 48–53. 8vo. 1888–9.
United States Department of Agriculture—North American Fauna, Parts 1–2. 8vo. 1889.
English Sparrow (Passer Domesticus) in North America. By W. B. Barrows. Bulletin 1. 8vo. 1889.
Wagner Free Institute of Science, Philadelphia—Transactions, Vol. II. 4to. 1889.
Wild, Dr. H.—Annalen der Physikalischen Central Observatorium, Theil I. 4to. 1889.
Repertorium fur Meteorologie, Band XII. 4to. 1889.
Wright & Co. Messrs. J. (the Publishers)—Medical Annual for 1890. 8vo. 1890.
Yale University—Transactions, Vol. I. Part 2. 4to. 1889.

WEEKLY EVENING MEETING,

Friday, March 7, 1890.

Sir James Crichton Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

Francis Gotch, Esq. Hon. M.A. Oxon. B.A. B.Sc.

Electrical Relations of the Brain and Spinal Cord.

(Abstract deferred.)

WEEKLY EVENING MEETING,

Friday, March 14, 1890.

William Crookes, Esq. F.R.S. Vice President, in the Chair.

Professor T. E. Thorpe, Ph.D. F.R.S. *M.R.I.*

The Glow of Phosphorus.

The word *phosphorus*—originally applied to any substance, solid or liquid, which had the property of shining in the dark—has gradually lost its generic sense, and is nowadays practically restricted, as a designation, to the waxlike inflammable substance which plays such an important part in the composition of an ordinary lucifer match. Phosphorus, indeed, is one of the most remarkable of the many remarkable substances known to the chemist. The curious method of its discovery; the universality of its distribution; its intimate connection with the phenomena of animal and vegetable life; its extraordinary physical properties and chemical activity; its abnormal molecular constitution; the Protean ease of its allotropic transformations—all combine to make up a history which abundantly justifies its old appellation of *phosphorus mirabilis.*

Godfrey Hankewitz, more than 150 years ago, wrote: "This phosphorus is a subject that occupies much the thoughts and fancies of some alchymists who work on microcosmical substances, and out of it they promise themselves golden mountains." Certainly no man of his time made more in the way of gold out of phosphorus than did Mr. Hankewitz, for, at his little shop in the Strand, he enjoyed for many years the monopoly of its sale, guarding his *Arcana* with all the jealousy of a modern manufacturer of the element.

Phosphorus, or, as it was then called, *noctiluca,* was first seen in this country in 1677. It was shown to Robert Boyle, who had already worked on phosphorescence in general, and who seems to have been specially struck with the remarkable peculiarity of a factitious body which could be made "to shine in the dark without having been before illumined by any lucid substance, and without being hot as to sense." In these respects the substance differed from all the *phosphori* hitherto known. The conditions which determine its glow were the subject of the earliest observations on phosphorus, and Boyle has left us a minute account of his work on this point. In the first place, he noticed that the substance was only luminous in presence of air. He accurately describes the nature of the light, and noticed that the water in which the phosphorus was partially immersed acquired a "strong and penetrant taste . . . . and relished a little like vitriol." "On evaporation it would not shoot into crystals . . . but coagulated into a substance like a gelly, or the whites of eggs, which would be easily melted by heat." On heating this "gelly" it gave off "flashes of fire and light" and had a "garlick smell." He also found that the *noctiluca* was soluble in certain oils, and he particularly mentions oil of cloves as a convenient means of showing the luminosity, as it is "rendered more acceptable to the standers-by by its grateful smell." "In Oyl of Mace it did not appear luminous nor in Oyl of Aniseeds." Boyle describes a number of experiments showing how small a quantity of the phosphorus is required to produce a luminous effect. "A grain of the *noctiluca* dissolved in Alcohol of Wine and shaken in Water; it render'd 400,000 times its weight luminous throughout. And at another Tryal I found that it impregnated 500,000 times its weight; which was more than one part of Cochineel could communicate its colour to." "And one thing further observable was that when it had been a long time exposed to the air it emitted strong and odorous exhalations distinct from the visible Fumes." The strong and odorous exhalations we now know to be ozone.

The earlier volumes of the 'Philosophical Transactions' contain several papers on the luminosity of phosphorus, and one by Dr. Frederick Slare is noteworthy as giving one of the earliest, if not actually the earliest, account of what is one of the most paradoxical phenomena connected with the luminosity of phosphorus, namely, its increase on rarefying the air. "It being now generally agreed that the fire and flame [of phosphorus] have their pabulum out of the air, I was willing to try this matter *in vacuo*. To effect this, I placed a considerable lump of this matter [phosphorus] under a glass, which I fixed to an engine for exhausting the air; then presently working the engine, I found it grow lighter [i.e. more luminous], though a charcoal that was well kindled would be quite extinguished at the first exhaustion; and upon the third or fourth draught, which very well exhausted the glass, it much increased its light, and continued so to shine with its increased light for a long time; on re-admitting the air, it returns again to its former dulness." This observation was repeated, and its result confirmed by Hawksbee in this country, and by Homberg in France, and seems subsequently to have led Berzelius and after him Marchand, to the conclusion that the luminosity of phosphorus was altogether independent of the air (i. e. the oxygen), but was solely due to the volatility of the body. Many facts, however, combine to show that the air (oxygen) is necessary to the phenomenon. Lampadius found that phosphorus would not glow in the Torricellian vacuum, and Lavoisier, in 1777, showed that it would not inflame under the same conditions; and the subsequent experiments of Schrötter, Meissner, and Müller are decisive on the point that the glow is the concomitant of a chemical process dependent upon the presence of oxygen. It is, however, remarkable that phosphorus will not glow in oxygen at the ordinary atmospheric pressure and temperature, but that if the oxygen be rarefied the glow at once begins, but ceases again the moment the oxygen is compressed. Indeed, phosphorus will not glow in compressed air, and the flame of feebly-burning phosphorus may be extinguished by suddenly increasing the pressure of the gas. Phosphorus, however, can be made to glow in oxygen at the ordinary pressure, or in compressed air, if the gases are gently warmed. In the case of oxygen the glow begins at 25°, and becomes very bright at 36°. In compressed air the temperature at which the glow is initiated depends upon the tension. If the oxygen is absolutely deprived of moisture, the phosphorus refuses to glow under any conditions. This fact, strange as it may seem, is not without analogy; the presence of traces of moisture appears to be necessary for the initiation or continuance of chemical combination in a number of instances.

It was observed by Boyle that a minute quantity of the vapour of a number of essential oils extinguished the glow of phosphorus. The late Professor Graham confirmed and extended these observations; he showed that relatively small quantities of olefiant gas, and of the vapours of ether, naphtha, and oil of turpentine entirely prevented the glow, and subsequent observers have found that many essential oils, such as those of peppermint and lemon, and the vapours of camphor and asafœtida, even when present in very small quantity, stop the absorption of oxygen and the slow combustion of phosphorus in air.

It has been established that whenever phosphorus glows in air, or in rarefied oxygen, ozone and hydrogen peroxide are formed, but it is not definitely known whether the formation of these substances is the cause or the effect of the chemical process of which the glow is the visible sign. That there is some intimate connection between the luminosity of the phosphorus and the production of these bodies is highly probable. Schönbein, as far back as 1848, sought to demonstrate that the glow depends on the presence of ozone. It is certainly true that many of the substances, such as the essential oils, which prevent the glow of phosphorus, also destroy ozone. At a low temperature phosphorus produces no ozone in contact with air, neither does it glow. It has been found, in fact, that with air ozone is produced in largest quantity at 25°, at which temperature phosphorus glows brightly. On the assumption that the oxidation of the phosphorus consists in the immediate formation of the highest oxide, the production of the ozone and the hydrogen peroxide has been represented by the following equations:—

$$\begin{aligned} P_2 + O_2 &= P_2O_5 + O \\ O + O_2 &= O_3 \\ O + H_2O &= H_2O_2. \end{aligned}$$

Both these reactions may, of course, go on simultaneously, as ozone and hydrogen peroxide are not mutually incompatible; the synthesis of hydrogen peroxide by the direct oxidation of water seems to occur in a number of processes. But such symbolic expressions can at most be only very partial representations of what actually occurs. It is highly probable that the combination which gives rise to the glow only occurs between the *vapour* of phosphorus and the oxygen. Phosphorus is sensibly volatile at ordinary temperatures, and by rarefying the atmosphere in which it is placed its volatilisation is increased, which serves to account for the increased glow when the pressure of the gas is diminished. When phosphorus is placed in an atmosphere of hydrogen, nitrogen, or carbonic acid, these gases, when brought into contact with oxygen, become luminous from the oxidation of the vapour of phosphorus diffused through them. The rapidity of volatilisation varies with the particular gas; it is greatest in the case of hydrogen, and least in that of carbonic acid. Indeed, a stream of hydrogen gas at ordinary temperatures carries away comparatively large quantities of phosphorus, which may be collected by appropriate solvents. No ozone and no glow are produced in oxygen gas at ordinary temperatures and pressures, but on warming the oxygen both the ozone and the glow are formed. On passing ozone into oxygen at temperatures at which phosphorus refuses to glow, the phosphorus at once becomes luminous, oxygen is absorbed and the characteristic cloud of oxide is produced, and the effect continues so long as the supply of ozone is maintained. A drop of ether at once extinguishes the glow.

The ether is in all probability converted into vinyl alcohol with simultaneous formation of hydrogen peroxide by the reaction indicated by Poleck and Thümmel

$$\left.\begin{matrix} CH_3CH_2 \\ CH_3CH_2 \end{matrix}\right> O + O_3 = \begin{matrix} CH_2CHOH \\ CH_2CHOH \end{matrix} + \left.\begin{matrix} OH \\ OH \end{matrix}\right\}$$

Formic, acetic, and oxalic acids are also formed by the action of ozonised oxygen on ether.

Phosphorus combines with oxygen in several proportions, and the study of the mode of formation and properties of these oxides is calculated to throw light upon the nature of the chemical process which attends the glow of phosphorus. Certain of these oxides have recently been the subject of study in the chemical laboratories of the Normal School of Science. When phosphorus is slowly burned in air, there is produced a considerable quantity of a volatile substance, having a characteristic garlic-like smell which solidifies, when cooled, in beautiful arborescent masses of white crystals. It melts at about 23°, and boils at 173°. In a sealed tube kept in the dark it may be preserved unchanged, but on exposure to light, and especially to bright sunshine, it rapidly becomes deep red. It slowly absorbs oxygen at the ordinary temperature and pressure, but from the mode in which the solid product of the reaction (P_2O_5) is deposited, it is evident that the union only takes place between the vapour of the oxide and the oxygen gas. Under diminished pressure the act of combination is attended with a glow which increases in brilliancy if ozone be present. On compressing the oxygen the glow ceases. No ozone is formed during the act of oxidation. The degree of rarefaction needed to initiate the glow depends upon the temperature of the oxide; the warmer the oxide the less is the diminution of pressure required. By gradually warming the oxide the luminosity steadily increases both in area and intensity, until at a certain temperature the mass ignites. The change from glow to actual flame is perfectly regular and gradual, and is unattended with any sudden increase in brilliancy. In this respect the process of oxidation is analogous to the slow and barely visible burning of fire-damp which is sometimes seen to occur in the Davy lamp, or to the slow combustion of ether and other vapours which has been specially studied by Dr. Perkin. Other instances of what may be called *degraded combustion* are known to chemists. Thrown into warm oxygen the substance bursts into flame at once, and burns brilliantly; and it also takes fire in contact with chlorine. Alcohol also ignites it, and when it is warmed with water or a solution of potash it evolves spontaneously inflammable phosphoretted hydrogen. In contact with cold water it suffers only a very gradual change, and many days may elapse before even a comparatively small quantity is dissolved. This substance has long been known; it was discovered, in fact, by the French chemist, Sage, but its true nature has only now been determined; its chemical formula is found to be P_4O_6; hence its composition is similar to that of its chemical analogue arsenious oxide.

The study of the properties of this remarkable substance enables us to gain a clearer insight into the nature of the chemical change attending the glow of phosphorus. When phosphorus is placed in oxygen, or in an atmosphere containing oxygen under such conditions that it volatilises, the phosphorus oxidises, partly into phosphoric oxide, and partly into phosphorous oxide; ozone is formed, possibly in the mode already indicated, and this reacts upon the residual phosphorus vapour and the phosphorous oxide with the production of the luminous effect to which the element owes its name. The glow itself is nothing but a slowly burning flame having an extremely low temperature, caused by the chemical union of oxygen with the vapours of phosphorus and phosphorous oxide. By suitable means this glow can be gradually augmented, until it passes by regular gradation into the active vigorous combustion which we ordinarily associate with flame. Many substances, in fact, may be caused to phosphoresce in a similar way. Arsenic, when gently heated, glows in oxygen, and sulphur may also be observed to become luminous in that gas at a temperature of about 200°.

[T. E. T.]

WEEKLY EVENING MEETING,

Friday, March 21, 1890.

Sir James Crichton Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

Professor G. F. Fitz Gerald, M.A. F.R.S.

Electromagnetic Radiation.

In order to discover whether actions are propagated in time or instantaneously, we may employ the principle of interference to measure the wave-length of a periodic disturbance, and determine whether it is finite or no. This is the principle employed by Hertz to prove experimentally Maxwell's theory as to the rate of propagation of electromagnetic waves. In order to confine the experiments within reasonable limits we require short waves, of a few metres' length at most. As the highest audible note gives waves of five or six miles long, and our eyes are sensitive only to unmanageably short waves, it is necessary to generate and observe waves whose frequency is intermediate between them, of some hundred million vibrations per second or so. For this purpose we may use a pair of conducting surfaces connected by a shorter or longer wire, in which is interposed a spark-gap of some few millimetres' length. When the conductors are charged by a coil or electrical machine to a sufficiently high difference of potential for a spark to be formed between them, they discharge in a series of oscillations, whose period for systems of similar shape is inversely proportional to the linear dimensions of the system so long as the surrounding medium is unaltered. When the surrounding non-conducting medium changes, the period depends on the electric and magnetic specific inductive capacities of this medium. Two such systems were shown. A large one, whose frequency was about 60 millions per second; and a small one, whose frequency was about 500 millions per second. The large one consisted of two flat plates, about 30 cm. square and 60 cm. apart, and arranged in the same way as is described by Prof. Hertz in Wiedemann's 'Annalen,' April 1888. The smaller vibrating system consisted of two short brass cylinders terminating in gilt brass balls of the same size, and arranged in the same way as the smaller system described by Prof. Hertz in Wiedemann's 'Annalen,' March 1889. This latter system was placed in the focal line of a cylindrical parabolic mirror of thin zinc plate, such as that described by Prof. Hertz in this paper.

These generators of electromagnetic oscillations may be called electric oscillators, as the electric charge oscillates from end to end. A circle of wire, or a coil in which an alternating current ran, or, if such a thing were attainable, a magnet alternating in polarity, might be called a magnetic oscillator. A ring magnet with a closed magnetic circuit is essentially an electric oscillator, while a ring of ring magnets would be essentially a magnetic oscillator again. The elementary theory of a magnetic oscillator can be derived from that of an electric oscillator by simply interchanging electric and magnetic force. Electricity and magnetism would be essentially interchangeable if such a thing existed as magnetic conduction. The only magnetic currents we know are magnetic displacement currents and convection currents, such as are used in unipolar and some other dynamos. It is in this difference that we must look for the difference between electricity and magnetism.

In order to observe the existence of these electromagnetic oscillations we can employ the principle of resonance to generate oscillations in a system whose free period of oscillation is the same. A magnetic receiver may be employed consisting of a single incomplete circle of wire broken by a very minute spark-gap, across which a spark leaps when the oscillations in the wire become sufficiently intense. In order that a large audience may observe the occurrence of sparks, the terminals of a galvanometer circuit were connected one with one side of the spark-gap and the other with a fine point which could be approached very close to the other side of the spark-gap. It was observed that when a spark occurred in the gap, a spark could also be arranged to occur into the galvanometer circuit, and with a delicate long-coil galvanometer (that used had 40,000 ohms resistance) a very marked deflection can be produced whenever a spark occurs. This arrangement we have only succeeded in working comparatively close to the generator, because the delicacy required in adjusting the two spark-gaps is so great. It can, however, be employed to show that the sparks produced in this magnetic resonant circuit are due to resonance by removing this receiver from the generator to such a distance that sparks only just occur, and then substituting for the single circuit a double circuit, which, except for resonance, should have a greater action than the single one, but which stops the sparking altogether. An electric receiver was also used which was identical with the generator, and had a corresponding, only much smaller, spark-gap between the two plates. When the plates are connected with the terminals of the galvanometer, upon the occurrence of each spark the galvanometer is deflected. It is not so easy to obtain sparks when the plates are connected with the galvanometer as when they are insulated, and it is this that has limited the use of this method of observation. By making the first metre or so of the wires to the galvanometer of extremely fine wire, so as to reduce their capacity, we have found that the difficulty of getting sparks is less than with thick wires. We have not observed any effect due to the thickness of the wires after a short distance from the receiver.

In the case of the small oscillator, a receiver exactly like the one described by Prof. Hertz in his second paper already quoted was placed in the focal line of a cylindrical parabolic mirror, and its receiving wires were connected with the wires leading to the galvanometer by some very fine brass wire. With the large sized generator and receiver, which were placed about 3 metres apart, it was shown that the sparking was stopped by placing a thin zinc sheet so as to reflect the radiations from a point close behind the receiver. By means of a long indiarubber tube hung from the ceiling it was shown how, when waves are propagated to a point whence they are reflected, the direct and reflected waves interfering produce a system of loops and nodes, with a node at the reflecting point. It was explained that these nodes, though places of zero displacement, were places of maximum rotation, and that the axis of rotation was at right angles to the direction of displacement. It was explained that an analogous state of affairs existed in the electromagnetic vibrations. If the electric force be taken as analogous to the displacement of the rope, the magnetic may be taken as analogous to its rotation, and the two are at right angles to one another. In the ether the electric node is a magnetic loop, and *vice versâ*. Though the two are separated in loops and nodes, they exist simultaneously in a simple wave propagation, just as in a rope when propagating waves in one direction the crest of maximum displacement is also that of maximum rotation. It was explained that by placing the reflector at a quarter of a wavelength from the receiver this would be at an electric loop, and have its sparking increased. It may thus be shown that there are a series of loops and nodes produced by reflection of these electromagnetic forces, like those produced in any other case of reflected wave-propagation. This was Hertz's fundamental experiment, by which he proved that electromagnetic actions are propagated in time, and by some approximate calculations he verified Maxwell's theory that the rate of propagation is the same as that of light. It follows that the luminiferous ether is experimentally shown to be the medium to which electric and magnetic actions are due, and that the electromagnetic waves we have been studying are really only very long light waves.

A rather interesting deduction from Maxwell's theory is that light incident on any body that absorbs or reflects it should press upon it and tend to move it away from the source of light. Illustrating this, an experiment was shown with an alternating current passing through an electro-magnet, in front of which a good conducting plate of silver was suspended. When the alternating current was turned on the silver was repelled. It was explained that as the silver could only be affected by what was going on in its own neighbourhood, and that if sufficiently powerful radiations from a distant source were falling on the silver, it would be acted on by alternating magnetic forces, this experiment was in effect an experiment on the repulsion of light, which was too small to have been yet observed, even in the case of concentrated sunshine. These slow vibrations are not stopped by a sheet of zinc, though much reduced by a magnetic sheet like tin-plate, though the rapid ones are quite stopped by either—thus showing that wave-propagation in a conductor is of the nature of a diffusion.

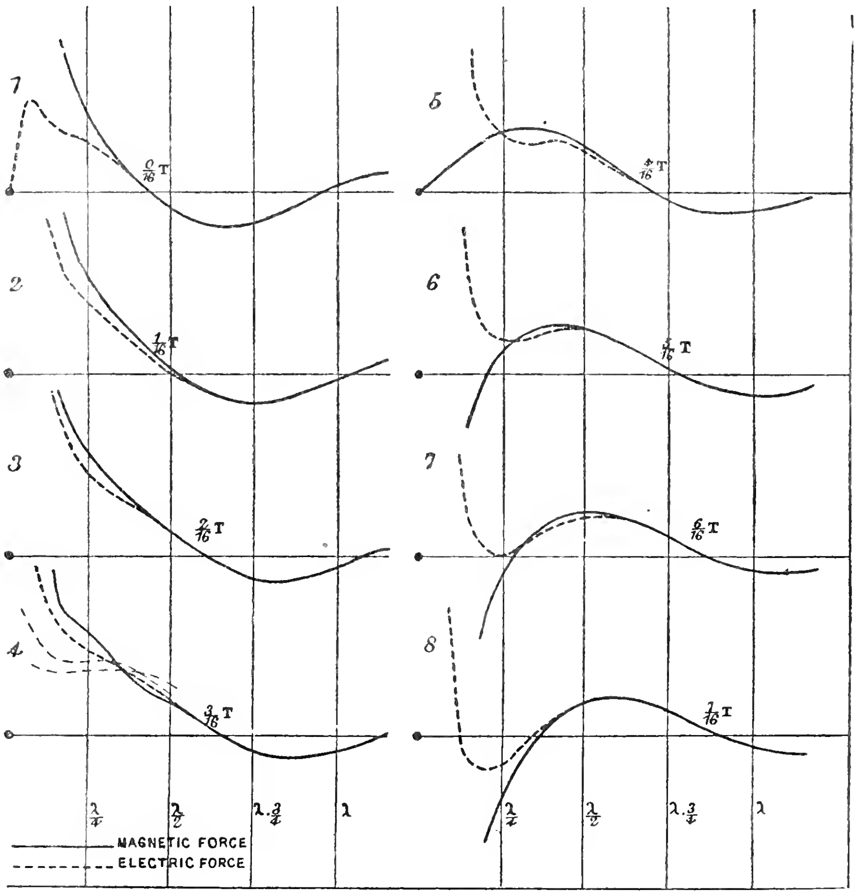
In all cases of diffusion where we consider the limits of the problem, terms involving the momentum of the parts of the body must be introduced. It appears from elementary theories of diffusion as if it were propagated instantaneously, but no action can be propagated from molecule to molecule, in air, for instance, faster than the molecules move, i. e. at a rate comparable with that of sound. In electromagnetic theory corresponding terms come in by introducing displacement currents in conductors, and it seems impossible but that some such terms should be introduced, as otherwise electromagnetic action would be propagated instantaneously in conductors. The propagation of light through electrolytes, and the too great transparency of gold leaf, point in the same direction.

The constitution of these waves was then considered, and it was explained that if magnetic forces are analogous to the rotation of the elements of a wave, then an ordinary solid cannot be analogous to the ether because the latter may have a constant magnetic force existing in it for any length of time, while an elastic solid cannot have continuous rotation of its elements in one direction existing within it. The most satisfactory model, with properties quite analogous to those of the ether is one consisting of wheels geared with elastic bands. The wheels can rotate continuously in one direction, and their rotation is the analogue of magnetic force. The elastic bands are stretched by a difference of rotation of the wheels, and introduce stresses quite analogous to electric forces. By making the elastic bands of lines of governor balls, the whole model may have only kinetic energy, and so represent a fundamental theory. Such a model can represent media differing in electric and magnetic inductive capacity. If the elasticity of the bands be less in one region than another, such a region represents a body of higher electric inductive capacity, and waves would be propagated more slowly in it. A region in which the masses of the wheels was large would be one of high magnetic inductive capacity. A region where the bands slipped would be a conducting region. Such a model, unlike most others proposed, illustrates both electric and magnetic forces and their inter-relations, and consequently light propagation.

In the neighbourhood of an electric generator the general distribution of the electric and magnetic forces is easily seen. The electric lines of force must lie in planes passing through the axis of the generator, while the lines of magnetic force lie in circles round this axis and perpendicular to the lines of electric force. It is thus evident that the wave is, at least originally, polarised. To show this, the small-sized oscillators with parabolic mirrors were used, and a light square frame, on which wires parallel to one direction were strung, was interposed between the mirrors. It was shown that such a system of wires was opaque to the radiation when the wires were parallel to the electric force, but was quite transparent when the frame was turned so that the wires were parallel to the magnetic force. It behaved just like a tourmaline to polarised light. It is of great interest to verify experimentally Maxwell's theory that the plane of polarisation of light is the plane of the magnetic force. This has been done by Mr. Trouton, who has shown that these radiations are not reflected at the polarising angle by the surface of a non-conductor, when the plane of the magnetic force in the incident vibration is perpendicular to the plane of incidence, but the radiations are reflected at all angles of incidence when the plane of the magnetic force coincides with the plane of incidence. Thus the long-standing dispute as to the direction of vibration of light in a polarised ray has been at last experimentally determined. The electric and magnetic forces are not simultaneous near the oscillator. The electric force is greatest when the electrification is greatest, and the magnetic force when the current is greatest, which occurs when the electrification is zero: thus the two, when near the oscillator, differ in phase by a quarter of a period. In the waves, as existing far from the oscillator, they are always in the same phase. It is interesting to see how one gains on the other. It may be worth observing again that though what follows deals with electric oscillators, the theory of magnetic oscillators is just the same, only that the distribution of magnetic and electric forces must be interchanged. Diagrams drawn from Hertz's figures published in Wiedemann's 'Annalen' for January 1889, and in 'Nature' for March 7th, 1889, and in the 'Philosophical Magazine' for March 1890, were thrown on the screen in succession, and it was pointed out how the electric wave, which might be likened to a diverging whirl ring, was generated, not at the oscillator, but at a point about a quarter of a wave length on each side of the oscillator, while it was explained that the magnetic force wave starts from the oscillator. It thus appears how one gains the quarter period on the other. The outflow of the waves was exhibited by causing the images to succeed one another rapidly by means of a zoetrope, in which all the light is used and the succession of images formed by having a separate lens for each picture and rotating the beam of light so as to illuminate the pictures in rapid succession.

As the direction of flow of energy in an electromagnetic field depends on the directions of electric and magnetic force, being reversed when either of these is reversed, it follows that in the neighbourhood of the oscillator the energy of the field alternates between the electric and magnetic forms, and that it is only the energy beyond about a quarter of the wave length from the oscillator which is wholly radiated away during each vibration. It follows that in ordinary electromagnetic alternating currents at from 100 to 200 alternations per second, it is only the energy which is some 3000 miles away which is lost. If an electromagnetic wave, having magnetic force comparable to that near an ordinary electro-magnet, were producible, the power of the radiation would be stupendous. If we consider the possible radiating power of an atom by calculating it upon the hypothesis that the atomic charge oscillates across the diameter of the atom, we find that it may be millions of millions of times as great as Prof. Wiedemann has found to be the radiating power of a sodium atom in a Bunsen burner, so that if there is reason to think that any greater oscillation might disintegrate the atom, it is evident that we are still a long way from doing so. It is to be observed that ordinary light waves are very much longer than the period of the vibration above referred to. Dr. Lodge has pointed out that quite large oscillators in comparison to molecules, namely, about the size of the rods and cones in the retina, are of the size to resound to light waves of the length we see, and so might be used to generate such waves. This seems to show that the electro-magnetic structure of an atom must be more complicated than a small sphere or other simple shape with an oscillating charge on it, for the period of vibration of a small system can be made long by making the system complex, e. g. a small Leyden jar of large capacity with a long wire wound many times round connecting its coats, could easily be constructed to produce electromagnetic waves whose length would bear the same proportion to the size of the jar as ordinary light waves do to an atom. The rate at which the energy of a Hertzian vibrator is transferred to the ether is so great that we should expect an atom to possess the great radiating power it has. This shows, on the other hand, how completely the vibrations of an atom must be forced by the vibrations of the ether in its neighbourhood, so that atoms, being close compared with a wave length, are, in any given small space, probably in similar phases of vibration. It is interesting to consider this in connection with the action of molecules in collision as to how far the forces between molecules after collision is the same as before. In the same connection the existence of intra-atomic electromagnetic oscillations is interesting in the theories of anomalous dispersion. An electromagnetic model of a prism with anomalous dispersion might be constructed out of pitch, through which conductors, each with the same rate of electromagnetic oscillation, were dispersed. In theories of dispersion a dissipation of energy is assumed, and it may be the radiation of the induced electromagnetic vibrations. These can evidently never be greater than the incident electromagnetic vibration, on account of this radiation of their own energy. In some theories a vibration of something much less than the whole molecule is assumed, and the possibility of intra-atomic electromagnetic oscillations would account for this. Some such assumption seems also required, in order to explain such secondary, if not tertiary, actions as the Hall effect and the rotation of the plane of polarisation of light, which are, apparently at least, secondary actions due to a reaction of the matter set in motion by the radiation on this radiation.

Some further diagrams were exhibited, plotted from Hertz's theory by Mr. Trouton, to whom much of the matter in this paper is due. They are here reproduced, and show eight simultaneous positions of the electric and magnetic waves during a semi-oscillation of an electric oscillator. The dotted line shows the electric force at various points, and the continuous line the magnetic force. In the first diagram the magnetic force is at its maximum near the origin, while the electric force there is zero. In the second the magnetic energy near the origin has partly turned into electric energy, and consequently electric force begins. The succeeding figures show how the magnetic force decreases near the origin, while the electric force grows and the waves already thrown off spread away. The change of magnetic force between Figures 4 and 5 is so rapid,



that a few dashed lines, showing interpolated positions, are introduced to show how it proceeds. It will be observed how a hollow comes in the line showing electric force, which gradually increases, and, crossing the line of zero force at about a quarter of a wave-length from the origin, is the source of the electric wave, which, starting with this odds, picks up and remains thenceforward coincident with the magnetic wave. From this origin of electric waves they spread out along with the magnetic waves and in towards the origin, to be reproduced again from this point on the next vibration. These electric and magnetic forces here shown as coincident are, of course, in space in directions at right angles to one another as already explained. The corresponding diagrams for a magnetic oscillator are got by interchanging the electric and magnetic forces.

A further experiment was shown to illustrate how waves of transverse vibration can be propagated along a straight hollow vortex in water. It was stated that what seemed a possible theory of ether and matter was that space was full of such infinite vortices in every direction, and that among them closed vortex rings represented matter threading its way through the ether. This hypothesis explains the differences in Nature as differences of motion. If it be true, ether, matter, gold, air, wood, brains are but different motions. Where alone we can know what motion in itself is, that is, in our own brains, we *know* nothing but thought. Can we resist the conclusion that all motion is thought? Not that contradiction in terms, unconscious thought, but living thought; that all Nature is the language of One in whom we live, and move, and have our being.

[G. F. F. G.]

WEEKLY EVENING MEETING,

Friday, March 28, 1890.

Sir Frederick Bramwell, Bart. D.C.L. F.R.S. Honorary Secretary and Vice-President, in the Chair.

The Right Hon. Lord Rayleigh, M.A. D.C.L. LL.D. F.R.S. *M.R.I.* Professor of Natural Philosophy, R.I.

Foam.

When I was turning over in my mind the subject for this evening, it occurred to me to take as the title of the lecture, "Froth." But I was told that a much more poetical title would be "Foam," as it would so easily lend itself to appropriate quotations. I am afraid, however, that I shall not be able to keep up the poetical aspect of the subject very long; for one of the things that I shall have most to insist upon is that foaming liquids are essentially impure, contaminated—in fact, dirty. Pure liquids will not foam. If I take a bottle of water and shake it up, I shall get no appreciable foam. If, again, I take pure alcohol, I get no foam. But if I take a mixture of water with 5 per cent. of alcohol there is a much greater tendency. Some of the liquids we are most familiar with as foaming, such as beer or ginger-beer, owe the conspicuousness of the property to the development of gas in the interior, enabling the foaming property to manifest itself; but of course the two things are quite distinct. Dr. Gladstone proved this many years ago by showing that beer from which all the carbonic acid had been extracted in vacuo still foamed on shaking up. I now take another not quite pure but strong liquid, acetic acid, and from it we shall get no more foam than we did from the alcohol or the water. The bubbles, as you see, break up instantaneously. But if I take a weaker acid, the ordinary acid of commerce, there is more, though still not much, tendency to foam. But with a liquid which for many purposes may be said to contain practically no acetic acid at all, seeing that it consists of water with but 1–1000th part of acid, the tendency is far stronger; and we get a very perceptible amount of foam. These tests with the alcohol and acetic acid are sufficient to illustrate the principle that the property of foaming depends on contamination. In pure ether we have a liquid from which the bubbles break even more quickly than from alcohol or water. They are gone in a moment. In some experiments I made at home I found that water containing a small proportion of ether foamed freely; but on attempting two or three days ago to repeat the experiment, I was surprised to find a result very different. I have here some water containing a very small fraction of ether, about 1–240th part. If I shake it up, it scarcely foams at all; but another mixture made in the same proportion from another sample shows more tendency to foam. This is rather curious, because both ethers were supposed to be of the same quality; but one had been in the laboratory longer than the other, and perhaps contained more greasy matter in solution.

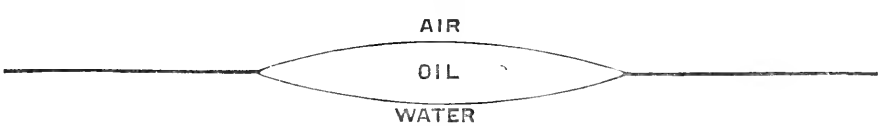
Another liquid which foams freely is water impregnated with camphor. Camphor dissolves sparingly; but a minute quantity of it quite alters the characteristics of water in this respect. Another substance, very minute quantities of which communicate the foaming property to water, is glue or gelatine. This liquid contains only 3 parts in 100,000 of gelatine, but it gives a froth entirely different from that of pure water. Not only are there more bubbles, but the duration of the larger bubbles is quite out of proportion to that of water-bubbles. This sample contains 5 parts in 100,000, nearly double as much; but even with but 1 part in 100,000, the foaming property is so evident as to suggest that it might in certain cases prove valuable for indicating the presence of minute quantities of impurities. I have been speaking hitherto of those things which foam slightly. They are not to be compared with, say, a solution of soap in water, which, as is well known to everybody, froths very vigorously. Another thing comparable to soap, but not so well known, is saponine. It may be prepared from horse chestnuts by simply cutting them in small slices and making an infusion with water. A small quantity of this infusion added to water makes it foam strongly. The quantity required to do this is even less than in the case of soap; so the test is more delicate. It is well known that rivers often foam freely. That is no doubt due to the effect of saponine or some analogous substance. Sea-water foams, but not, I believe, on account of the saline matter it contains; for I have found that even a strong solution of pure salt does not foam much. I believe it has been shown that the foaming of sea-water, often so conspicuous, is due to something extracted from seaweeds during the concussion which takes place under the action of breakers.

Now let us consider for a moment what is the meaning of foaming. A liquid foams when its films have a certain durability. Even in the case of pure water, alcohol, and ether, these films exist. If a bubble rises, it is covered for a moment by a thin film of the liquid. This leads us to consider the properties of liquid films in general. One of their most important and striking properties is their tendency to contract. Such surfaces may be regarded as being in the condition of a stretched membrane, as of india-rubber, only with this difference, that the tendency to contract never ceases. We may show that by blowing a small soap bubble, and then removing the mouth. The air is forced back again by the pressure exerted on the bubble by the tension of the liquid. This ancient experiment suffices to prove conclusively that liquid films exercise tension.

A prettier form of the same experiment is due to Van der Mensbrugghe, who illustrated liquid tension by means of a film in which he allowed to float a loop of fine silk, tied in a knot. As long as the interior of the loop, as well as the exterior, is occupied by the liquid film, it shows no tendency to take any particular shape: but if, by insertion of, say, a bit of blotting paper, the film within the loop be ruptured, then the tension of the exterior film is free to act, and the thread flies instantaneously into the form of a circle, in consequence of the tendency of the exterior surface to become as small as possible. The exterior part is now occupied by the soap film, and the interior is empty. Many other illustations of this property of liquids might be given, but time does not permit.

In the soap film, as in the films which constitute ordinary foam, each thin layer of liquid has two surfaces; each tends to contract; but in many cases we have only one such surface to consider, as when a drop of rain falls through the air. Again, suppose that we have three materials in contact with one another,—water, oil, and air. There are three kinds of surfaces separating the three materials, one separating water and oil, another oil and air, and a third surface separating the water from the air. These three surfaces all exert a tension, and the shape of the mass of oil depends upon the relative magnitudes of the tensions. As I have drawn it here (Fig. 1), it is implied that the tension of the water-air surface is less than the sum of the other two tensions—those of the water-oil surface and the air-oil surface; because the two latter acting obliquely balance the former. It is only under such conditions that the equilibrium of the three materials as there drawn in contact with one another is possible. If the tension of the surface separating water and air exceeded the sum of the other two, then the equilibrium as depicted would be impossible. The water-air tension, being greater, would assert its superiority by drawing out the edge of the lens, and the oil would tend to spread itself more and more over the surface.

FIG. 1.



And that is what really happens. Accurate measurements made by Quincke and others, show that the surface tension separating water and air, is really greater than the sum of the two others. So oil does tend to spread upon a surface of water and air. That this is the fact, we can prove by a simple experiment. At the feet of our chairman, our Honorary Secretary, is a large dish, containing water which at present is tolerably clean. In order to see what may happen to the surface of the water, it is dusted over with fine sulphur powder, and illuminated with the electric light. If I place on the surface a drop of water, no effect ensues; but if I take a little oil, or better still a drop of saponine, or of soap-water, and allow that to be deposited upon the middle of the surface, we shall see a great difference. The surface suddenly becomes dark, the whole of the dust being swept away to the boundary. That is the result of the spread of the film, due to the presence of the oil.

How then is it possible that we should get a lens-shaped mass of oil, as we often do, floating upon the surface of water? Seeing that the general tendency of oil is to spread over the surface of water, why does it not do so in this case? The answer is that it has already spread, and that this surface is not really a pure water surface at all, but one contaminated with oil. It is in fact only after such contamination that an equilibrium of this kind is possible. The volume of oil necessary to contaminate the surface of the water is very small, as we shall see presently; but I want to emphasise the point that, so far as we know, the equilibrium of the three surfaces in contact with one another is not possible under any other conditions. That is a fact not generally recognised. In many books you will find descriptions of three bodies in contact, and a statement of the law of the angles at which they meet; that the sides of a triangle, drawn parallel to the three intersecting surfaces must be in proportion to the three tensions. No such equilibrium, and no such triangle, is possible if the materials are pure; when it occurs, it can only be due to the contamination of one of the surfaces. These very thin films, which spread on water, and, with less freedom, on solids also, are of extreme tenuity; and their existence alongside of the lens, proves that the water prefers the thin film of oil to one of greater thickness. If the oil were spread out thickly, it would tend to gather itself back into drops, leaving over the surface of the water a film of less thickness than the molecular range.

One experiment by which we may illustrate some of these effects I owe to my colleague, Professor Dewar. It shows the variation in the surface tension of water, due to the presence on it of small quantities of ether. I hold in my hand masses of charcoal, which can be impregnated with ether. The greater part of the surface of the charcoal is covered with paraffin wax, and, in consequence, the ether which has already penetrated the charcoal can only escape from it again on one side. The result is that the water in the rear of this boat of charcoal will be more impregnated with ether than the part in front, so the mass of charcoal will enter into motion, and the motion will extend over a considerable interval of time. As long as the ether remains in sufficient quantity to contaminate the water in the rear, so long is there a tendency to movement of the mass. The water covered with the film of ether has less tension than the pure water in front, and the balance of tensions being upset, the mass is put in motion. If the nature of the case is such that the whole surface surrounding the solid body is contaminated, then there is no tendency to movement, the same balance in fact obtaining as if the water were pure.

Another body which we may use for this purpose is camphor. If we spread some camphor scrapings on a surface of pure water, they will, if the surface is quite clean, enter into vigorous movement, as you now see. This is because the dissolved camphor diminishes the surface tension of the water. But if I now contaminate the water with the least possible quantity of grease, the movements of the camphor will be stopped. I merely put my finger in, and you observe the effect. There is not much poetry about that! A very slight film, perfectly invisible by ordinary means, is sufficient so to contaminate the water that the effect of the dissolved camphor is no longer visible.

I was very desirous to ascertain, if possible, the actual thickness of oil necessary to produce this effect, because all data relating to molecules are, in the present state of science, of great interest. From what I have already said, you may imagine that the quantity of oil required is very small, and that its determination may be difficult. In my experiments,* I used the surface of water contained in a large sponge bath three feet in diameter. By this extension of the surface, I was able to bring the quantity of oil required within the range of a sensitive balance. In Diagram 2, I have given a number of results

DIAGRAM 2.

A Sample of Oil somewhat Decolorised by Exposure.

Date.	Weight of Oil.	Calculated Thickness of Film in Micro-millimetres.	Effect upon Camphor Fragments.
	mg.		
Dec. 17 ..	0·40	0·81	No distinct effect.
Jan. 11 ..	0·52	1·06	Barely perceptible.
Jan. 14 ..	0·65	1·32	Not quite enough.
Dec. 20 ..	0·78	1·58	Nearly enough.
Jan. 11 ..	0·78	1·58	Just enough.
Dec. 17 ..	0·81	1·63	Just about enough.
Dec. 18 ..	0·83	1·68	Nearly enough.
Jan. 22 ..	0·84	1·70	About enough.
Dec. 18 ..	0·95	1·92	Just enough.
Dec. 17 ..	0·99	2·00	All movements very nearly stopped.
Dec. 20 ..	1·31	2·65	Fully enough.
		A Fresh Sample.	
Jan. 28 ..	0·63	1·28	Barely perceptible.
Jan. 28 ..	1·06	2·14	Just enough.

obtained at various dates, showing the quantity of oil required to produce the effects recorded in the fourth column. Knowing the weight of the oil deposit, and the area of the water surface upon which it was uniformly spread, it was easy to calculate the thickness of the film.

* Proc. Roy. Soc., March 1890.

It is seen that a film of oil about $1\frac{1}{2}$ millionth of a millimetre thick is able to produce this change. I know that large numbers are not readily appreciated, and I will therefore put the matter differently. The thickness of the oil film thus determined as sufficient to stop the motions of the camphor is one 400th of the wave length of yellow light. Another way of saying the same thing is that this thickness of oil bears to one inch the same ratio that one second of time bears to half a year.

When the movement of the camphor has been stopped by the addition of a minute quantity of oil, it is possible, by extending the water surface enclosed within the boundary, without increasing the quantity of oil, to revive the movements of the camphor; or, again, by contraction, to stop them. I can do this with the aid of a flexible boundary of thin sheet brass, and you see that the camphor recovers its activity, though a moment ago it was quite dead. It would be an interesting subject for investigation to determine what is the actual tension of an oily surface contaminated to an extent just sufficient to stop the camphor movements; but it is not an easy problem. Usually we determine surface tensions by the height to which the liquids will rise in very fine tubes. Here, however, that method is not available, because if we introduce a tube into such a surface, there is no proof that the contamination of the inner surface in the tube is the same as that prevailing outside. Another method, however, may be employed which is less open to the above objection, and that is to substitute for the very fine or capillary tube, a combination of two parallel plates open at their edges. We have here two such plates of glass, kept from absolutely closing by four pieces of thin metal inserted at the corners, the plates being held close against these distance-pieces by suitable clamps. If such a combination be inserted in water, the liquid will rise above the external level, and the amount of the rise is a measure of the surface tension of the water. You see now the image on the screen. A is the external water surface; B is the height of the liquid contained between the glass plates, so that the tension may be said to be measured by the distance AB. If a little oil be now deposited upon the surface, it will find its way between the plates. The fall which you now see shows that the surface tension has been diminished by the oil which has found its way in. A very minute quantity will give a great effect. When the height of the pure water was measured by 62, a small quantity of oil changed the 62 into 48, and subsequent large additions of oil could only lower it to 38. But after oil has done its worst, a further effect may be produced by the addition of soap. If Mr. Gordon now adds some soap, we shall find that there is a still further fall in the level, showing that the whole tension now in operation is not much more than one-third of what it was at first. This is an important point, because it is sometimes supposed that the effect of soap in diminishing the tension of water is due to merely the formation upon the surface of a layer of oil formed by decomposition of the soap. This experiment proves the contrary, because we find that soap can do so much more than oil. There is indeed, something more or less corresponding to the decomposition of the soap and the formation of a superficial layer of oil. But the decomposition takes place in a very peculiar manner, and under such conditions that there is a gradual transition from the soapy liquid in the interior to the oily layer at the top, and not, as when we float a layer of oil on water, two sudden transitions, first from water to oil, and secondly from oil to air. The difference is important, because, as I showed some years ago, capillary tension depends on the suddenness of change. If we suppose that the change from one liquid to another takes place by slow stages, though the final change may be as before, the capillary tension would absolutely disappear.

There is another very interesting class of phenomena due to oil films, which I hope to illustrate, though I am conscious of the difficulty of the task,—namely, the action of oil in preventing the formation of waves. From the earliest times we have records of the effect of oil in stilling waves, and all through the Middle Ages the effect was recognised, though connected with magic and fanciful explanations. Franklin, than whom, I suppose, no soberer inquirer ever existed, made the thing almost a hobby. His attention was called to it accidentally on board ship from noticing the effect on the waves caused by the greasy débris of a dinner. The captain assured him that it was due to the oil spread on the water, and for some time afterwards, Franklin used to carry oil about with him, so as never to miss a chance of trying an experiment. A pond is necessary to illustrate the phenomena properly, but we shall get an idea of it by means of this trough six feet long, containing water.* Along the surface of the water we shall make an artificial wind by means of a fan,† driven by an electro motor. In my first experiments I used wind from an organ bellows, which is not here available. Presently we shall get up a ripple, and then we will try the effect of a drop of oil put in to windward. I have now put on the drop, and you see a smooth place advancing along. As soon as the waves come up again, I will repeat the experiment. While the wind is driving the oil away, I may mention that this matter has been tested at Peterhead. Experiments were there made on a large scale to show the effect of oil in facilitating the entrance of ships into harbour in rough weather. Much advantage was gained. But here a distinction must be observed. It is not that the large swell of the ocean is damped down. That would be impossible. The action in the first instance is upon the comparatively small ripples. The large waves are not directly affectin by the oil; but it seems as if the power of the wind to excite and maintain them is due to the small ripples which form on their backs,

* The width is 8 inches, and the depth 4 inches. The sides are of glass; the bottom and ends of wood, painted white.

† For this fan and its fittings the Institution is indebted to the liberality of the Blackman Ventilating Company.

and give the wind, as it were, a better hold of them. It is only in that way that large waves can be affected. The immediate effect is on the small waves which conduce to that breaking of the large waves which from the sailor's point of view is the worst danger. It is the breaking waters which do the mischief, and these are quieted by the action of the oil.

I want to show also, though it can only be seen by those near, the return of the oil when the wind is stopped. The oil is at present driven to one end of the trough; * when the wind stops it will come back, because the oil film tends to spread itself uniformly over the surface. As it comes back, there will be an advancing wave of oil; and as we light the surface very obliquely by the electric lamp, there is visible on the bottom of the trough a white line, showing its progress.

Now, as to the explanation. The first attempt on the right lines was made by the Italian physicist, Marangoni. He drew attention to the importance of contamination upon the surface of the water, and to its tendency to spread itself uniformly, but for some reason which I cannot understand, he applied the explanation wrongly. More recently Reynolds and Aitken have applied the same considerations with better success. The state of the case seems to be this:—Let us consider small waves as propagated over the surface of clean water; as the waves advance, the surface of the water has to submit to periodic extensions and contractions. At the crest of a wave the surface is compressed, while at the trough it is extended. As long as the water is pure there is no force to oppose that, and the wave can be propagated without difficulty; but if the surface be contaminated, the contamination strongly resists the alternate stretching and contraction. It tends always, on the contrary, to spread itself uniformly; and the result is that the water refuses to lend itself to the motion which is required of it. The film of oil may be compared to an inextensible membrane floating on the surface of the water, and hampering its motion; and under these conditions it is not possible for the waves to be generated, unless the forces are very much greater than usual. That is the explanation of the effect of oil in preventing the formation of waves.

The all-important fact is that the surface has its properties changed, so that it refuses to submit to the necessary extensions and contractions. We may illustrate this very simply by dusting the surface of water with sulphur powder, only instead of dispersing the sulphur, as before, by the addition of a drop of oil, we will operate upon it by a gentle stream of wind projected downwards on the surface, and of course spreading out radially from the point of impact. If Mr. Gordon will blow gently on the surface in the middle of the dusty region, a space is cleared; † if he stops blowing, the dust comes back

* May 1890. Any moderate quantity of oil may be driven off to leeward; but if oleate of soda be applied, the quieting effect is permanent.

† This experiment is due to Mr. Aitken

again. The first result is not surprising, but why does the dusty surface come back? Such return is opposed to what we should expect from any kind of viscosity, and proves that there must be some force directly tending to produce that particular motion. It is the superior tension of the clean surface. No oil has been added here, but then no water surface is ever wholly free from contamination; there may be differences of degree, but contamination is always present to some extent. I now make the surface more dirty and greasy by contact of the finger, and the experiment no longer succeeds, because the jet of wind is not powerful enough to cleanse the place on which it impinges; the dirty surface refuses to go away, or if it goes in one direction it comes back in another.

I want now to bring to your notice certain properties of soap solutions, which, however, are not quite so novel as I thought when I first came upon them in my own inquiries.* If we measure by statical or slow methods the surface tension of soapy water, we find that it is very much less than that of clean water. We can prove this in a very direct manner by means of capillary tubes. Here, shown upon the screen, are two tubes of the same diameter, in which, therefore, if the liquids were the same, there would be the same elevation; one tube dips into clean water, and the other into soapy water, and the clean water rises much (nearly three times) higher than the soapy water.

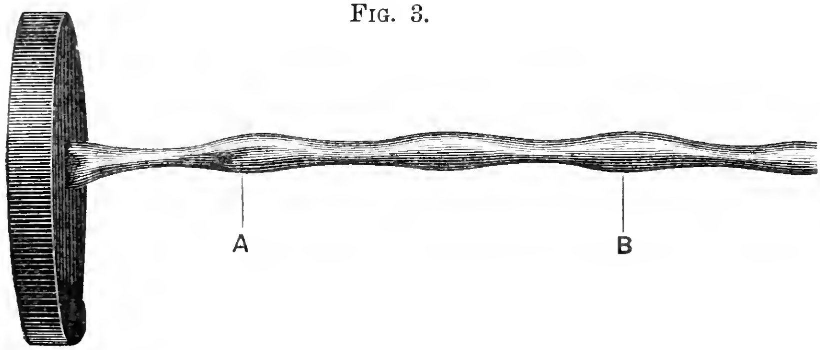
Although the tension of soapy water is so much less than that of pure water when measured in this way, I had some reason to suspect that the case might be quite different if we measured the tensions immediately after the formation of the surfaces. I was led to think so by pondering on Marangoni's view that the behaviour of foaming liquids was due to the formation of a pellicle upon their surfaces; for if the change of property is due to the formation of a pellicle, it is reasonable to suppose that it will take time, so that if we can make an observation before the surface is more than say $\frac{1}{100}$ of a second old, we may expect to get a different result. That may seem an impossible feat, but there is really no difficulty about it; all that is necessary is to observe a jet of the substance in question issuing from a fine orifice. If such a jet issues from a circular orifice it will be cylindrical at first, and afterwards resolve itself into drops. If, however, the orifice is not circular, but elongated or elliptical, the jet undergoes a remarkable transformation before losing its integrity. As it issues from the elliptical orifice, it is in vibration, and trying to recover the circular form; it does so, but afterwards the inertia tends to carry it over to the other side of equilibrium. The section oscillates between the ellipse in one direction and the ellipse in the perpendicular direction. The jet thus acquires a sort of chain-like appearance, and the period of the movement represented by the

---

* I here allude to the experiments of Dupré, and to the masterly theoretical discussion of liquid films by Professor Willard Gibbs.

distance between corresponding points A, B, Fig. 3, is a measure of the capillary tension to which these vibrations of the elliptical section about the circular form are due. A measure, then, of the wave-length of the recurrent pattern formed by the liquid gives us information as to the tension immediately after escape; and if we wish to compare the tensions of various liquids, all we have to do is to fill a vessel alternately with one liquid and another, and compare the wave-lengths in the various cases. The jet issues from a flask, to which is attached below a tubular prolongation; the aperture is made small in order that we may be able to deal with small quantities of liquid. You now see the jet upon the screen (Fig. 3), it issues from the

Fig. 3.

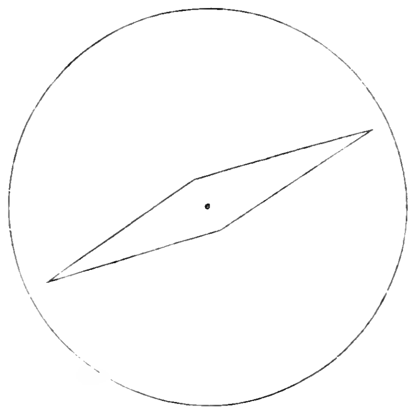


orifice; it oscillates, and we can get a comparative measure of the tension by observing the distance between corresponding points (A, B).

If we were now to take out the water, and substitute for it a moderately strong solution of soap or saponine, we should find but little difference, showing that in the first moments the tension of soapy water is not very different from that of pure water. It will be more interesting to exhibit a case in which a change occurs. I therefore introduce another liquid, water containing 10 per cent. of alcohol, and you see that the wave-length is different from before. So this method gives us a means of investigating the tensions of surfaces immediately after their formation. If we calculate by known methods how long the surface has been formed before it gets to the point B, at which the measurement is concluded, we shall find that it does not exceed $\frac{1}{100}$ of a second.

Another important property of contaminated surfaces is what Plateau and others have described as superficial viscosity. There are cases in which the surfaces of liquids—of distilled water, for example—seem to exhibit a special viscosity, quite distinct from the ordinary interior viscosity, which is the predominant factor in determining the rate of flow through long narrow tubes. Plateau's experiment was to immerse a magnetised compass needle in water; the needle turns, as usual, upon a point, and the water is contained in a cylindrical vessel, not much larger than the free rotation of the needle requires (Fig. 4). The observation relates to the time occupied by the needle in returning to its position of equilibrium in the meridian, after having been deflected into the east and west positions, and Plateau found that in the case of water more time was required when the needle was just afloat than when it was wholly immersed, whereas in the case of alcohol the time was greater in the interior. The longer time occupied when the needle is upon the surface of water is attributed by Plateau to an excessive superficial viscosity of that body.

FIG. 4.



Instead of a needle, I have here a ring of brass wire (Fig. 5), floating on the surface of the water. You see upon the screen the image of the ring, as well as the surface of the water, which has been made visible by sulphur. The ring is so hung from a silk fibre that it can turn upon itself, remaining all the while upon the surface of the water. Attached to it is a magnetic needle, for the purpose of giving it a definite set, and of rotating it as required by an external magnet. On this water, which is tolerably clean, when the ring is made to

FIG. 5.

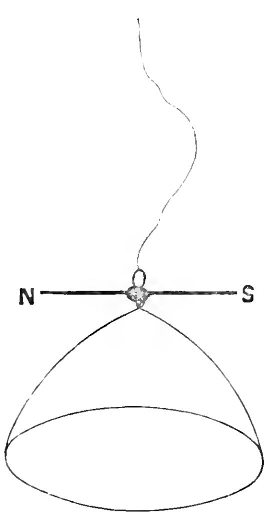
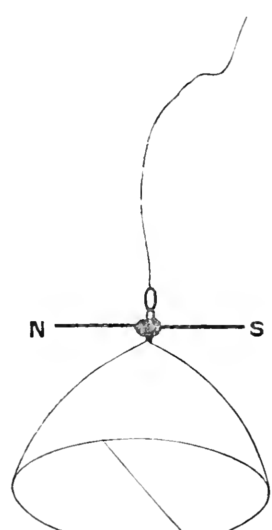


FIG. 6.



turn, it leaves the dust in the interior entirely behind. That shows that the water inside the ring offers no resistance to the shearing action brought into play. The part of the surface of water immediately in contact with the ring no doubt goes round; but the movement spreads to a very little distance. The same would be observed if we added soap. But if I add some saponine, we shall find a different result, and that the behaviour of the dust in the interior of the ring is materially altered. The saponine has stiffened the surface, so that the ring turns with more difficulty; and when it turns, it carries round the whole interior with it. The surface has now got a stiffness from which it was free before; but the point upon which I wish to fix your attention is that the surface of pure water does not behave in the same way. If, however, we substitute for the simple hoop another provided with a material diameter (Fig. 6), lying also in the surface of the water, then we shall find, as was found by Plateau in his experiment, that the water is carried round. In this case, it is no longer possible for the surface to be left behind, as it was with the simple hoop, unless it is willing to undergo local expansions and contractions of area. The difference of behaviour proves that what a water surface resists is not shearing, but expansions and contractions; in fact, it behaves just as a contaminated surface should do. On this supposition, it is easy to explain the effects observed by Plateau; but the question at once arises, can we believe that all water surfaces hitherto experimented upon are sensibly contaminated? and if yes, is there any means by which the contamination may be removed? I cannot in the time at my disposal discuss this question fully, but I may say that I have succeeded in purifying the surface of the water in Plateau's experiment, until it behaved like alcohol. It is therefore certain that Plateau's superficial viscosity is due to contamination, as was conjectured by Marangoni.

I must now return to the subject of foam, from which I may seem to have digressed, though I have not really done so. Why does surface contamination enable a film to exist with greater permanence than it otherwise could? Imagine a vertical soap film. Could the film continue to exist if the tension were equal at all its parts? It is evident that the film could not exist for more than a moment; for the interior part, like the others, is acted on by gravity, and, if no other forces are acting, it will fall 16 feet in a second. If the tension above be the same as below, nothing can prevent the fall. But observation proves that the central parts do not fall, and thus that the tension is not uniform, but greater in the upper parts than in the lower. A film composed of pure liquid can have but a very brief life. But if it is contaminated, there is then a possibility of a different tension at the top and at the bottom, because the tension depends on the degree of contamination. Supposing that at the first moment the film were uniformly contaminated, then the central parts would begin to drop. The first effect would be to concentrate the contamination on the parts underneath and diminish it above. The result of that would be an increase of tension on the upper parts. So the effect would be to call a force into play tending to check the motion, and it is only in virtue of such a force that a film can have durability. The main difference between a material that will foam and one that will not is in the liability of the surface to contamination from the interior.

I find my subject too long for my time, and must ask you to excuse the hasty explanations I have given at some parts. But I was anxious above all to show the principal experiments upon which are based the views that I have been led to entertain.

[RAYLEIGH.]

GENERAL MONTHLY MEETING,

Monday, April 7, 1890.

SIR JAMES CRICHTON BROWNE, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

Arthur Edward Ash, Esq.
Robert Dobbie, Esq.
William S. Hall, Esq.
Major Percy A. Macmahon, R.A.
Miss May Pollock,
Mrs. Joseph Shaw,
Major-General C. E. Webber, C.B.

were elected Members of the Royal Institution.

The following Alterations in the Bye Laws of the Royal Institution were passed:—

In CHAPTER I. (*Of the Government. And of the Enacting and Repealing of Bye Laws*).

In Art. 1, line 9, omit "*and fifteen Visitors, so chosen.*"

In Art. 1, line 10, omit "*either*" and "*or Visitor.*"

In Art. 1, line 11, insert "*neither shall the major part of the Visitors have served the office of Visitor during any part of the preceding year.*"

In CHAPTER II. (*Of the Election, Rights, and Privileges of the Members*).

Repeal Article 7.

In Art. 8, line 1, insert "*paid his admission fee of ten guineas, and who has.*"

In Art. 8, line 4, omit "*if his annual payment be five guineas, or the sum of ten guineas, if his annual payment be two guineas.*"

Repeal Article 9.

In Art. 10, line 1, for "*All Members of the Royal Institution elected after the fourth day of August*, 1823, *and all those Members elected previously to that time, who shall have given bond for the payment of the annual sum of five guineas, as long as they continue Members of the Institution, pursuant to the last Article*," substitute "*Members*."

In Art. 12, line 8, omit "*provided that nothing herein contained shall be construed to permit any Member, elected previously to the fourth day of August*, 1823, *who shall have given bond for the payment of the sum of 'four guineas' per annum, to withdraw unless he shall previously have executed the bond for the payment of five guineas annually, according to the provisions of Art.* 9 *of this Chapter*."

Repeal Article 15.

Repeal Article 17.

In Art. 21, line 3, omit "(*exclusive of the privilege reserved by Act of Parliament to those who were heretofore Proprietors*)."

In Chapter IV. (*Of the Election of Officers*).

In Art. 2, last line, insert "*On the filling up of such vacancy or vacancies the ballot shall remain open for only fifteen minutes*."

In Art. 4, line 2, for "*two o'clock P.M.*" substitute "*five o'clock P.M.*"

In Art. 4, line 5, for "*three o'clock P.M.*" substitute "*half-past five o'clock P.M.*"

In Chapter VI. (*Of the Duties of the Committee of Managers*).

In Art. 4, line 3, insert "*with the exception of the months of January, August, September, and October*."

In Art. 4, line 4, insert "*The Managers present at any Meeting shall have power should the business not be completed to adjourn to some date to be then agreed upon prior to the next monthly meeting*."

In Art. 6, line 3, insert (after "Member") "*of the Committee*."

In Art. 12, line 2, insert "*Library, the*."

In Art. 12, line 9, insert "*Books, Journals, and Periodicals*."

In Art. 15, line 3, omit "*Mineralogical Collection*."

In Art. 17, line 4, omit "*and those appointed by the Patrons of the Library*."

In Chapter VII. (*Of the Duties of the Committee of Visitors*).

In Art. 3, line 3, for "*one o'clock P.M.*" substitute "*five o'clock P.M.*"

In Art. 7, line 2, insert (after "Member") "*of that Committee*."

In Chapter VIII. (*Of the Duties of the Treasurer*).

In Art. 1, line 2, omit "*according to form (A) in the Appendix*."

In Art. 1, line 5, for "*such Manager or Visitor putting the letters Mr. or Vr. after his name*," substitute "*on such forms as shall be determined by the Managers from time to time. Any Manager or Visitor signing such receipt shall put the letters Mr. or Vr. after his name*."

In Chapter X. (*Of the General Meetings of the Members*).

In Art. 1, line 4, for "*two o'clock*" substitute "*five o'clock*."

In Art. 4, line 4, omit "*August*."

In Art. 8, line 7, omit "3. *Of any Propositions or Reports from the Scientific and Literary Committees, according to their dates.*"

In Art. 15, line 2, for "*only vote when the suffrages are equal*" substitute "*when the voices for and against the question proposed are equal have a second or casting vote.*"

In CHAPTER XIII. (*Of Life Subscribers and Annual Subscribers*).

For title substitute (OF LIFE SUBSCRIBERS AND OF SUBSCRIBERS TO COURSES OF LECTURES).

In Art. 1, line 1, omit "*shall.*"

In Art. 1, line 2, omit "*previously to the fourth day of August,* 1823, *according to the Bye Law then in force, and those persons who shall hereafter be admitted as Life Subscribers to the Royal Institution.*"

Repeal Article 2.

Repeal Article 3.

Repeal Article 4.

In Art. 5, line 5, for "*Persons desirous of attending such Course or Courses to be recommended by a Member of the Institution, according to the form (C) in the Appendix, to the Committee of Managers, who may issue the admission ticket,*" substitute "*Persons who attend such Course or Courses to conform to such regulations as may be made from time to time by the Managers.*"

In CHAPTER XIV. (*Of the Causes and Form of Ejection from the Institution*).

In Art. 1, line 9, insert "*or a subscriber thereto.*"

Repeal Article 2.

In Art. 4, lines 1 and 6, omit "*or Subscriber.*"

In Art. 4, line 3, omit "*or Subscribers.*"

In CHAPTER XVIII. (*Of the Weekly Meetings of the Members*).

In Art. 3, last line, insert "*and to listen to Discourses on such subjects.*"

In CHAPTER XX. (*Of the Library and Mineral Collection*).

Repeal the whole Chapter.

In CHAPTER XXI. (*Of the Laboratory and Apparatus*).

For title substitute (OF THE LIBRARY, THE LABORATORY, AND APPARATUS).

In Art. 1, line 1, insert after the first word "*Library, the.*"

In Art. 2, line 1, omit "*There shall be.*"

In Art. 2, line 2, insert "*shall be kept*" and "*Library, the.*"

Repeal the following Articles:—

Article 3.
Article 4.
Article 5.
Article 6.

Make all such alterations in the numbers of the Chapters and of the Articles as may be rendered necessary by the foregoing revision of the Bye Laws.

In APPENDIX omit forms (A. B. and C.).

The Special Thanks of the Members were returned for the following Donations to the Fund for the Promotion of Experimental Research:—

Ludwig Mond, Esq. .. .. £100.
Lachlan M. Rate, Esq. .. £50.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FOR

The Secretary of State for India—Report on Public Instruction in Bengal, 1888–9. fol. 1889.
The New Zealand Government—Statistics of the Colony of New Zealand, 1888. fol. 1890.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta: Rendiconti. 2° Semestre, Vol. V. Fasc. 13; 1° Semestre, Vol. VI. Fasc. 1, 2, 3. 8vo. 1890.
Antiquaries, Society of—Proceedings, Vol. XII. Part 4. 8vo. 1889.
Astronomical Society, Royal—Monthly Notices, Vol. L. No. 4. 8vo. 1890.
Basel Naturforschende Gesellschaft—Verhandlungen, 8th Theil, Heft 3. 8vo. 1890.
British Architects, Royal Institute of—Proceedings, 1889–90, Nos. 10, 11. 4to.
Cambridge Philosophical Society—Proceedings, Vol. VII. Part 1. 8vo. 1890.
Canada Meteorological Service—Annual Report, 1886. 8vo. 1890.
Canadian Institute—Annual Report, 1888–9. 8vo. 1889.
Chemical Society—Journal for March, 1890. 8vo.
Cracovie, l'Académie des Sciences—Bulletin, 1890, No. 2. 8vo.
Dax, Société de Borda—Bulletin, 4e Trimestre, 1889. 8vo.
Editors—American Journal of Science for March, 1890. 8vo.
Analyst for March, 1890. 8vo.
Athenæum for March, 1890. 4to.
Chemical News for March, 1890. 4to.
Chemist and Druggist for March, 1890. 8vo.
Electrical Engineer for March, 1890. fol.
Engineer for March, 1890. fol.
Engineering for March, 1890. fol.
Horological Journal for March, 1890. 8vo.
Industries for March, 1890. fol.
Iron for March, 1890. 4to.
Ironmongery for March, 1890.
Murray's Magazine for March, 1890. 8vo.
Nature for March, 1890. 4to.
Photographic News for March, 1890. 8vo.
Revue Scientifique for March, 1890. 4to.
Telegraphic Journal for March, 1890. fol.
Zoophilist for March, 1890. 4to.
Electrical Engineers, Institution of—Journal, No. 84. 8vo. 1890.
Index to Journal, Vols. I.–X. 1872–1882. 8vo. 1882.
Florence, Biblioteca Nazionale Centrale—Bollettino, Nos. 101, 102. 8vo. 1890.
Indice e Cataloghi, Vol. II. Fasc. 1. 8vo. 1890.
Franklin Institute—Journal, Nos. 770, 771. 8vo. 1890.
Freshfield, Edwin, Esq. LL.D. M.R.I. (the Editor)—Vestry Minute Books of the Parish of St. Bartholomew, 1567–1676. (Privately Printed.) 4to. 1890.
Geographical Society, Royal—Proceedings, New Series, Vol. XII. Nos. 3, 4. 8vo. 1890.
Jablonowski'sche Gesellschaft, Leipzig—Fürstliche Preisschrift, No. 27. 4to. 1889.
Johns Hopkins University—University Circulars, No. 79. 4to. 1890.
Kansas Academy of Sciences—Transactions, Vol. XI. 1887–88. 8vo. 1889.
Laboratory Club—Transactions, Vol. III. No. 4. 8vo. 1890.

Liverpool Literary and Philosophical Society—Proceedings, Vols. XLI.-XLIII. 8vo. 1886-89.
Manchester Geological Society—Transactions, Vol. XX. Parts 16, 17. 8vo. 1890.
Meteorological Office—Weekly Weather Reports, Nos. 1-13. 4to. 1890.
Report of Meteorological Council to R.S. 31 March, 1889. 8vo. 1890.
Meteorological Society, Royal—Quarterly Journal, No. 73. 8vo. 1889.
Ministry of Public Works, Rome—Giornale del Genio Civile, Serie Quinta, Vol. III. Nos. 12, 13; Vol. IV. No. 1. And Disegni. 8vo. 1890.
Odontological Society of Great Britain—Transactions, Vol. XXII. Nos. 4, 5. New Series. 8vo. 1889.
Pharmaceutical Society of Great Britain—Journal, March, 1890. 8vo.
Photographic Society—Journal, Vol. XIV. No. 6. 8vo. 1890.
Physical Society of London—Proceedings, Vol. X. Part 3. 8vo. 1890.
Preussische Akademie der Wissenschaften—Sitzungsberichte XXXIX.-LIII. 8vo. 1889.
Rathbone, E. P. Esq. (the Editor)—The Witwatersrand Mining and Metallurgical Review, No. 2. 8vo. 1890.
Richards, Admiral Sir S. H. K.C.B. F.R.S. &c. (the Conservator)—Report on the Navigation of the River Mersey, 1889. 8vo. 1890.
Richardson, B. W. (the Author)—The Asclepiad, Vol. VII. No. 25. 8vo. 1890.
Rio de Janeiro Observatory—Revista, No. 1. 8vo. 1890.
Royal Historical and Archæological Association of Ireland—Journal, Vol. IX. (4th Series), No. 81. 8vo. 1890.
Royal Irish Academy—Transactions, Vol. XXIX. Part 12. 4to. 1889.
Proceedings, Vol. I. Part 2. (3rd Series.) 8vo. 1889.
Royal Society of London—Proceedings, No. 287. 8vo. 1890.
Philosophical Transactions, Vol. CLXXX. 4to. 1890.
Royal Society of New South Wales—Journal and Proceedings, Vol. XXIII. Part 1. 8vo. 1889.
Catalogue of Scientific Books in Library, Part 1. 8vo. 1889.
Selborne Society—Nature Notes, Vol. I. No. 3. 8vo. 1890.
Society of Architects—Proceedings, Vol. II. Nos. 5-8. 8vo. 1890.
Society of Arts—Journal for March, 1890. 8vo.
Verein zur Beförderung des Gewerbfleises in Preussen—Verhandlungen, 1890: Heft 1, 2. 4to.
Wimshurst, James, Esq. M.R.I.—Electrical Influence Machines. By J. Gray. 8vo. 1890.

WEEKLY EVENING MEETING

Friday, April 18, 1890.

Sir Frederick Abel, C.B. D.C.L. F.R.S. Vice-President, in the Chair.

Sir Frederick Bramwell, Bart. D.C.L. F.R.S. Hon. Sec. R.I.

Welding by Electricity.

(Abstract deferred.)

WEEKLY EVENING MEETING,

Friday, April 25, 1890.

Colonel James A. Grant, C.B. C.S.I. F.R.S. Manager and Vice-President, in the Chair.

The Right Hon. Sir John Lubbock, Bart. M.P. D.C.L. LL.D. F.R.S. *M.R.I.*

The Shapes of Leaves and Cotyledons.

Attempts to explain the forms, colours, and other characteristics of animals and plants, though not new, were until recent years far from successful.

Our Teutonic forefathers had a pretty story which explained certain characteristics of several common plants. Balder, the God of Mirth and Merriment, was, characteristically enough, regarded as deficient in the possession of immortality. The other divinities, fearing to lose him, petitioned Thor to make him immortal, and the prayer was granted on condition that every animal and plant would swear not to injure him. To secure this object, Nanna, Balder's wife, descended upon the earth. Loki, the God of Envy, attended her, disguised as a crow (crows at that time were white), and settled on a little blue flower, hoping to cover it up so that she might overlook it. The flower, however, cried out "forget-me-not, forget-me-not" (and has ever since been known under that name). Loki then flew up into an oak and sat on a mistletoe. Here he was more successful. Nanna carried off the oath of the oak, but overlooked the mistletoe. She thought, however, and the divinities thought, that she had successfully accomplished her mission, and that Balder had received the gift of immortality.

One day, thinking Balder proof, they amused themselves by shooting at him, posting him against a holly. Loki tipped an arrow with a piece of mistletoe, against which Balder was not proof. This, unfortunately, pierced him to the heart, and he fell dead. Some drops of his blood dropped on the holly, which accounts for the redness of the berries; the mistletoe was so grieved that she has ever since borne fruit like tears; and the crow, whose form Loki had taken, and which till then had been white, was turned black.

This pretty myth accounts for several things, but is open to fatal objections. You will judge whether I am more fortunate. In the first place I need hardly observe that the forms of leaves are almost infinitely varied. To quote Ruskin's vivid words, they "take all kinds of strange shapes, as if to invite us to examine them. Star-shaped, heart-shaped, spear-shaped, arrow-shaped, fretted, fringed, cleft, furrowed, serrated, sinuated, in whorls, in tufts, in spires, in wreaths, endlessly expressive, deceptive, fantastic, never the same from foot-stalk to blossom, they seem perpetually to tempt our watchfulness, and take delight in outstripping our wonder."

Now, why is this marvellous variety, this inexhaustible treasury of beautiful forms? Does it result from some innate tendency of each species? Is it intentionally designed to delight the eye of man? Or have the form and size and texture some reference to the structure and organisation, the habits and requirements, of the whole plant?

The leaf, although so thin, is no mere membrane, but is built up of many layers of cells, and the interior communicates with the external air by millions of little mouths, called stomata, which are generally situated on the under side of the leaf. The structure of leaves varies as much as their forms.

It is, of course, principally in hot and dry countries that leaves require protection from too much evaporation.

The surface is in some cases protected by a covering of varnish, in others by saline or calcareous excretions. In others, again, the same object is attained by increased viscidity of the sap; in some, the leaves assume a vertical position or range themselves one under the other, thus presenting a smaller surface to the rays of the sun. In other cases the leaves become fleshy. Woolly hairs are also a common and effective mode of protection. The plants of deserts are very frequently covered with a thick felt of hair. Some species, again, which are smooth in the north, tend to become woolly in the south. Species of the cool spring again tend to be glabrous. The uses of hairs to plants are indeed very various. They serve, as just mentioned, to check too rapid evaporation. They form a protection for the stomata or breathing holes, and consequently, as these are mainly on the under side of leaves, we find that when one side of the leaf is covered with white felted hairs, as the white poplar, this is always the under side.

In other cases the use of hair is to throw off water. In some alpine and marsh plants this is important. If the breathing holes became clogged with moisture—with fog, for instance, or dew—they would be unable to fulfil their functions. The covering of hair, however, throws off the moisture, and thus keeps them dry. Thus these hairs form a protection both against too much drought, and too much moisture.

Another function of hairs, which cannot be omitted, is to serve as shades against too brilliant light and too much heat. Again, hairs serve as a protection against insects, and even against larger animals. The stinging hairs of the common nettle are a familiar example, and coarse woolly hairs are often distasteful to herbivorous quadrupeds.

Deciduous leaves especially characterise the comparatively cool and moist atmosphere of temperate regions. For different reasons evergreen leaves become more numerous in the Alps and in the tropics.

In the Alps it is necessary for plants to make the most of the short summer. Hence, perennial and evergreen species are more numerous in proportion than with us. Everybody must have noticed how our deciduous trees are broken if we have snow early in the season and when they are still in leaf.

Evergreen leaves, such as those of the comparatively tough and leathery oak and olive, are protected against animals by their texture, and often, as in the holly, by spines; they are better able to resist the heat and dryness of the south than the comparatively tender leaves of our deciduous trees, which would part too rapidly with their moisture. It is perhaps an advantage to evergreen leaves to be glossy, because it enables them better to throw off snow. Moreover, their stomata are often placed in pits, and protected with hair, which prevents too rapid evaporation. The texture and structure of leaves is indeed a wide and very interesting subject, but to-night I must confine myself to the shape.

It is impossible to classify plants by the form of the leaf, which often differs greatly in very nearly allied species. Thus, the common plantain of our lawns (*Plantago major*) has broad leaves, *P. lanceolata* narrow ones. The width or narrowness of leaves depends on various considerations. In herbaceous and stalkless plants, such as the plantain, prostrate leaves tend to be broad, those which are upright to be narrow. Thus, grasses, for instances, have more or less upright narrow leaves.

In other cases the width is determined by the distance between the buds, and in others again by the number of leaves in a whorl.

Cordate and Lobed Leaves.

Among broad leaves we may observe two distinct types, according as they are oval or palmate. Monocotyledonous plants, such as grasses, sedges, lilies, hyacinths, very generally have upright and narrow leaves. When they are wider, as, for instance, in the black bryony, this is mainly at the base, where, consequently, the veins are further apart, coming together again towards the apex. This we are tempted, therefore, to regard as the primitive type of a broad leaf.

There is, however, a totally different one, where the leaf is palmate, like a hand, widening towards the free end. Here the veins pursue a straight, diverging course; and as they not only serve to strengthen the leaf, but also to carry the nourishment, this is doubtless an advantage. Perhaps, however, the primary reason for this arrangement is found in the fact that these leaves are generally folded up, like a fan, while they are in the bud.

I have elsewhere dwelt on the case of the beech, and perhaps I may briefly refer to it again. The weight of leaves which a branch can carry will of course depend on its position and strength. The mode of growth of the beech and the hornbeam are very similar, but the twigs of the latter are slenderer, and the leaves smaller. If we cut off a beech branch below the sixth leaf we shall find that the superficial leaf area which it carries is about 18 square inches. But in our climate most leaves are glad of as much sunshine as they can secure, and are arranged with reference to it. The width of the beech leaves —about $1\frac{3}{4}$ inch—is regulated by the average distance between the buds. If the leaves were wider they would overlap. If they were narrower there would be a waste of space. The area on the one hand, and the width on the other, being thus determined, the length is fixed, because, to secure an area of 18 inches, the width being about $1\frac{3}{4}$ inch, the length must be about 2 inches. This, then, explains the form of the beech leaf.

Let us apply these considerations in other cases. I will take, for instance, the Spanish chestnut and the black poplar. In the Spanish chestnut the stem is much stronger than that of the beech. Consequently it can carry a greater leaf-surface. But the distance between the buds being about the same, the leaves cannot be much wider; hence they are much longer in proportion, and this gives them their peculiar sword-blade-like shape.

Again, if we look at a branch of black poplar, and compare it with one of white poplar, we are struck with two things: in the first place, the branch cannot be laid out on a sheet of paper so that the leaves shall not overlap; the leaves are too numerous and large. Secondly, in the white poplar the upper and under surfaces of the leaf are very different, the lower one being covered with a thick felt of hair, which gives it its white colour; in the black poplar, on the other hand, the two surfaces are nearly similar.

These two characteristics are correlated, for while in the white poplar the leaves are horizontal, in the black poplar, on the contrary, they hang vertically. Hence the two surfaces are under very similar conditions, and consequently present a similar structure; while for the same reason they hang free from one another.

Let us again look for a moment at the great group of Conifers. Why, for instance, do some have long leaves and some short ones? This, I believe, depends on the strength of the twigs and the number of years which the leaves last; long leaves dropping after one, two, or three years, while species with shorter ones retain them many years—the spruce fir, for instance, 8 or 10, *Abies Pinsapo* even as many as 18.

[Here Sir John dwelt on and explained the forms of several familiar leaves.]

Seedlings

I now come to the second part of my subject—the forms of cotyledons. Any one who has ever looked at a seedling plant must have been struck by the fact that the first leaves differ entirely from those which follow—not merely from the final form, but even from those which immediately follow. These first leaves are called cotyledons. The forms of many cotyledons have been carefully described, but no reason has been given for the forms assumed, nor any explanation offered why they should differ so much from the subsequent leaves. Klebs, indeed, in his interesting memoir on "Germination," characterises it as quite an enigma.

Mustard and cress were the delight and wonder of our childhood, but it never then occurred, to me at least, to ask why they were formed as they are. So they grew, and beyond that it did not occur to me, nor I think to most, that it was possible to inquire. I have, however, I think, suggested plausible reasons in many cases, some of which I will now submit for your consideration.

Cotyledons differ greatly in form.

Some are narrow, in illustration of which I may mention the fennel and ferula, in the stalk or ferule of which Prometheus is fabled to have brought down fire from heaven.

Some are broad, as in the beech and mustard. Moreover, some species have narrow cotyledons and broad leaves, while others have broad cotyledons and narrow leaves.

Some are emarginate, as in the mustard; lobed, as in the lime; bifid, as in Eschscholtzia; trifid, as in the cress; or with four long lobes, as in Pterocarya.

Some are unequal, as in the mustard; or unsymmetrical, as in the geranium.

Some are sessile, and some are stalked; some are large, some small.

Generally, they are green, leaf-like, and aërial, but sometimes they are thick and fleshy, as in the oak, nut, walnut, peas, beans, and many others, in which they never quit the seed at all.

Let us see, then, whether we can throw any light on these differences, and why they should be so unlike the true leaves.

If we cut open a seed, we find within it the future plant; sometimes, as in the larkspur, a very small oval body; sometimes, as in the ash or the castor-oil, a lovely little miniature plant, with a short stout root and two well-formed leaves, inclosing between them the rudiment of the future stem, the whole lying embedded in food-material or perisperm; while sometimes the embryo occupies the whole interior of the seed, the food-material being stored up, not round, but in, the seed-leaves or cotyledons themselves. Peas and beans, almonds, nuts, and walnuts, are familiar cases. In split peas, for instance,—who split the peas? If you look at them you will see that it is too regularly and beautifully done for human hands. In fact, the two halves are the two fleshy cotyledons: strictly speaking, they are not split, for they never were united.

Narrow Cotyledons.

Let us now begin with such species as have narrow cotyledons, and see if we can throw any light on this characteristic. The problem is simple enough in such cases as the Plane, where we have, on the one hand, narrow cotyledons, and, on the other hand, a long narrow seed containing a straight embryo. Again, in the Ash, the cotyledons lie parallel to the longer axis of the seed, which is narrow and elongated. Such cases are, however, comparatively few; and there are a large number of species in which the seeds are broad and even orbicular, while yet the cotyledons are narrow. In these it will generally be found that the cotyledons lie transversely to the seed.

The Sycamore has also narrow cotyledons, but the arrangement is very different. The fruit is winged, the seed an oblate spheroid and aperispermic—that is to say, the embryo, instead of lying embedded in food-material, occupies the whole cavity of the seed. Now, if we wished to pack a leaf into a cavity of this form, it would be convenient to choose one of a long, strap-like shape, and then roll it up into a sort of ball. This is, I believe, the reason why this form of cotyledon is most suitable in the case of the sycamore.

Broad Cotyledons.

I now pass to species with broad cotyledons. In the castor-oil plant (*Ricinus*), Euonymus, or the apple, for instance, the young plant lies the broad way of the seed, and the cotyledons conform to it. In the genus Coreopsis, *Coreopsis auriculata* has broad cotyledons, and *Coreopsis filifolia* has narrow ones—the first having broad, the second narrow seeds.

Emarginate Cotyledons.

In a great many species the cotyledons are emarginate—that is to say, they are more or less deeply notched at the end. This is due to a variety of causes. One of the simplest cases is that of the oak, where the two fleshy cotyledons fill the seed; and the walls of the seed being somewhat thickened at the end, and projecting slightly into the hollow of the seed, cause a corresponding depression in the cotyledons.

In such cases as the mustard, cabbage, and radish, the emargination is due to a very different cause. The seed is oblong, thick, and slightly narrower at one end than the other. There is no perisperm, so that the embryo occupies the whole seed, and as this is somewhat deep, the cotyledons, in order to occupy the whole space, are folded and arranged one over the other like two sheets of note-paper, the radicle being folded along the edge. To this folding the emargination is due. If a piece of paper be taken, folded on itself, cut into the form of the seed, and then unfolded, the reason for the form of the cotyledon becomes clear at once.

But it may be said that in the wallflower (*Cheiranthus*) the seed has a similar outline, and yet the cotyledons are not emarginate. The reason of this is that in the wallflower, the seed is more compressed than in the mustard and radish, and the cotyledons are not folded; so that the whole, not the half, of each cotyledon, corresponds to the form of the seed.

Lobed Cotyledons.

The great majority of cotyledons are entire, but some are more or less lobed. For instance, those of the mallow are broadly ovate, minutely emarginate, cordate at the base, and three-lobed or angled towards the apex, with three veins, each running into one of the lobes.

The embryo is green, curved, and occupies a great part of the seed. The cotyledons are applied face to face; then, as growth continues, the tip becomes curved and depressed into a median longitudinal furrow, the fold of the one lying in that of the other.

[Sir John then showed by diagrams and paper cuttings how the emargination arises, but it cannot be made clear without illustrations.]

The cotyledons of the Lime (*Tilia*) are very peculiar. They are deeply five-lobed, the central lobe being the longest; so that they are roughly shaped like a hand. The seed is an oblate spheroid, resembling an orange in form, and the embryo is embedded in semi-transparent albumen.

The embryo is at first straight; the radicle is stout and obtuse; the cotyledons ovate-obtuse, plano-convex, fleshy, pale green, and applied face to face. They grow, however, considerably, and when they meet the wall of the seed, they curve round it, following the general outline of the seed. If any one will take a common tea-cup and try to place in it a sheet of paper, the paper will, of course, be thrown into ridges. If these ridges be removed and so much left as will lie smoothly inside the cup, it will be found that the paper has been cut into lobes more or less resembling those of the cotyledons of the Lime. Or if, conversely, a piece of paper be cut into lobes resembling those of the cotyledons, it will be found that the paper will fit the concavity of the cup. The case is almost like that of our own hand, which can be opened and closed conveniently owing to the division of the five fingers.

Unequal Cotyledons.

In most cases the two cotyledons are equal, but there are several cases in which one of them is larger than the other. This had not escaped the attention of Darwin, who attributed the difference to the fact "of a store of nutriment being laid up in some other part, as in this hypocotyl, or one of the cotyledons." I confess that I do not quite see how this affords any explanation of the fact. The suggestion I have thrown out is that the difference is due to the relative position of the two cotyledons in the seed, which in some cases favours one of them at the expense of the other. Thus in the mustard they are unequal, and, as we have already seen, they are folded up, one inside the other. The outer one, therefore, has more space, and becomes larger. In many other Crucifers, though the cotyledons are not folded, they are what is called "incumbent"—that is to say, they are folded on the radicle, and the outer one has therefore more room than the other.

Unsymmetrical Cotyledons.

In other cases, as in the Geraniums, Laburnum, Lupines, &c., there is inequality, not between the two cotyledons, but between the two halves of each cotyledon. In the geraniums this is due to the manner in which the cotyledons are folded. In the cabbage and mustard we have seen that one cotyledon is folded inside the other; in the geranium they are convolute, one half of each being folded inside one half of the other, the two inner halves being the smaller, the two outer the larger ones.

In the laburnum, where the arrangement is very similar, the inequality in the two sides of the cotyledon is due to the inequality between the two sides of the seed.

Subterranean Cotyledons.

I have already observed that in some cases the cotyledons occupy the whole of the seed, which in more or less spherical seeds is effected, either by a process of folding and packing, or by the cotyledons becoming themselves more or less thickened, as in peas and beans, nuts and chestnuts. This is the reason why such seeds fall more or less readily into two halves, the radicle or plumule being so small in comparison as generally to escape notice, though, if a horse-chestnut is peeled, the radicle appears as a sort of tail.

In certain beans the cotyledons sometimes emerge from the seed, sometimes remain underground. In others, as also in the oak and horse-chestnut, they never leave the seed, or come above ground: they have lost the function of leaves, and become mere receptacles of nourishment.

Did it ever occur to you to think, when you have been eating walnuts, why their structure is so complex, and why the edible part is thrown into those complicated lobes and folds? The history is very interesting.

In the Walnut the cotyledons now never leave the seed, but in an allied genus, Pterocarya, they come above ground as usual, and are very peculiar in form, being deeply four-lobed. The reason of this is very curious. The fruit is originally much larger than the seed, but, as it approaches maturity, the hard woody tissue disintegrates at four places, leaving thus four hollow spaces. Into these spaces the seed sends four projections, and into these four projections each cotyledon sends a lobe. Hence the four lobes.

Now in the walnut a very similar process takes place, only the hollow spaces are much larger, so that, instead of a solid wall, with hollow spaces occupied by the seed, it gives the impression as if the seed was thrown into folds occupied by the wall of the fruit. To occupy these spaces fully, the cotyledons themselves were thrown into folds as we now see them. The fruit of Pterocarya is much smaller than that of the horse-chestnut, which doubtless was itself formerly not so large as it now is. As it increased, the cotyledons became fleshier and fleshier, and found it more and more difficult to make their exit from the seed, until at last they have given up any attempt to do so. Hence these curious folds, with which we are so familiar, are the efforts made by the originally leafy cotyledons to occupy the interior of the nut. If you separate them, you will easily find the little rootlet, and the plumule with from five to seven pairs of minute leaves.

But perhaps you will ask me why I have assumed that in these cases the cotyledons have conformed to the seeds? May it not be that the seed is determined, on the contrary, with reference to the cotyledons? The size, form, &c., of the seeds, however, evidently have relation to the habits, conditions, &c., of the parent plant.

Let me, in illustration, take one case. The cotyledons of the sycamore are long, narrow, and strap-like; those of the beech are short, very broad, and fan-like. Both species are aperispermic, the embryo occupying the whole interior of the seed.

Now, in the sycamore, the seed is more or less an oblate spheroid, and the long ribbon-like cotyledons, being rolled up into a ball, fit it closely, the inner cotyledon being often somewhat shorter than the other. On the other hand, the nuts of the beech are more or less triangular; an arrangement like that of the sycamore would therefore be utterly unsuitable, as it would necessarily leave great gaps. The cotyledons, however, are folded up like a fan, but with more complication, and in such a manner that they fit beautifully into the triangular nut.

Can we, however, carry the argument one stage further? Why should the seed of the sycamore be globular, and that of the beech triangular? Is it clear that the cotyledons are constituted so as to suit the seed? May it not be that it is the seed which is adapted to the cotyledons? In answer to this we must examine the fruit, and we shall find that in both cases the cavity of the fruit is approximately spherical. That of the sycamore, however, is comparatively small, say $\frac{1}{5}$ inch in diameter, and contains one seed, which exactly conforms to the cavity in which it lies. In the beech, on the contrary, the fruit is at least twice the size, and contains from two to four seeds, which consequently, in order to occupy the space, are compelled (to give a familiar illustration, like the segments of an orange) to take a more or less triangular form.

Thus, then, in these cases, starting with the form of the fruit, we see that it governs that of the seed, and that of the seed, again, determines that of the cotyledons. But though the cotyledons often follow the form of the seed, this is not invariably the case: other factors must also be taken into consideration; but when this is done, we can, I venture to think, throw much light on the varied forms which seedlings assume.

I have thus attempted to indicate some of the principles on which, as it seems to me, the shapes of leaves and seedlings depend, and to apply them in certain cases; but the study is only in its infancy: the number and variety of leaves is almost infinite, and the whole question offers, I venture to think, a very interesting field for observation and research—one, indeed, of the most fascinating in the whole of Natural History.

[J. L.]

ANNUAL MEETING,

Thursday, May 1, 1890.

Sir James Crichton Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

The Annual Report of the Committee of Visitors for the year 1889, testifying to the continued prosperity and efficient management of the Institution, was read and adopted. The Real and Funded Property now amounts to above 82,000*l.* entirely derived from the Contributions and Donations of the Members.

Fifty-one new Members were elected in 1889.

Sixty-three Lectures and Nineteen Evening Discourses were delivered in 1889.

The Books and Pamphlets presented in 1889 amounted to about 283 volumes, making, with 539 volumes (including Periodicals bound) purchased by the Managers, a total of 822 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer, and the Honorary Secretary, to the Committees of Managers and Visitors, and to the Professors, for their valuable services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:

President—The Duke of Northumberland, K.G. D.C.L. LL.D.
Treasurer—Sir James Crichton Browne, M.D. LL.D. F.R.S.
Secretary—Sir Frederick Bramwell, Bart. D.C.L. F.R.S. M. Inst. C.E.

Managers.

Sir Frederick Abel, C.B. D.C.L. F.R.S.
Sir Benjamin Baker, K.C.M.G. M. Inst. C.E.
The Rt. Hon. A. J. Balfour, M.P. LL.D. F.R.S.
George Berkley, Esq. M. Inst. C.E.
William Crookes, Esq. F.R.S.
Warren W. de la Rue, Esq.
Edward Frankland, Esq. D.C.L. LL.D. F.R.S.
Charles Hawksley, Esq. M. Inst. C.E.
William Huggins, Esq. D.C.L. LL.D. F.R.S.
David Edward Hughes, Esq. F.R.S.
Alfred Bray Kempe, Esq. M.A. F.R.S.
The Right Hon. Earl Percy, F.S.A.
Edward Pollock, Esq.
William Chandler Roberts-Austen, Esq. F.R.S.
Basil Woodd Smith, Esq. F.R.A.S. F.S.A.

Visitors.

John Wolfe Barry, Esq. M. Inst. C.E.
Shelford Bidwell, Esq. M.A. F.R.S.
Alfred Carpmael, Esq.
Arthur Herbert Church, Esq. M.A. F.R.S.
Ernest H. Goold, Esq. F.Z.S.
George Herbert, Esq.
John Hopkinson, Esq. M.A. F.R.S. M. Inst. C.E.
John W. Miers, Esq.
Sir Thomas Pycroft, M.A. K.C.S.I.
Lachlan Mackintosh Rate, Esq. M.A.
Sir Owen Roberts, M.A. F.S.A.
Arthur William Rücker, Esq. M.A. F.R.S.
John Bell Sedgwick, Esq. J.P. F.R.G.S.
Joseph Wilson Swan, Esq.
Thomas Edward Thorp, Esq. Ph.D. F.R.S.

WEEKLY EVENING MEETING,

Friday, May 2, 1890.

William Crookes, Esq. F.R.S. Vice-President, in the Chair.

Walter H. Pollock, Esq. M.A.

Théophile Gautier.

[No Abstract.]

[The whole discourse is printed in the August number of "Longman's Magazine."]

GENERAL MONTHLY MEETING,

Monday, May 5, 1890.

The Duke of Northumberland, K.G. D.C.L. LL.D. President; and afterwards Sir James Crichton Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

The following Vice-Presidents for the ensuing year were announced:—

Sir Frederick Abel, C.B. D.C.L. F.R.S.
William Crookes, Esq. F.R.S.
Edward Frankland, Esq. D.C.L. LL.D. F.R.S.
William Huggins, Esq. D.C.L. LL.D. F.R.S.
The Right Hon. Earl Percy, F.S.A.
Basil Woodd Smith, Esq. F.R.A.S. F.S.A.
Sir James Crichton Browne, M.D. LL.D. F.R.S. Treasurer.
Sir Frederick Bramwell, Bart. D.C.L. F.R.S. Hon. Secretary.

Harry Baldwin, Esq. M.R.C.S.
A. R. Binnie, Esq. M. Inst. C.E.
Miss Frances Busk,
J. S. Jeans, Esq.
A. Kirkman Loyd, Esq.
Ronald A. Scott, Esq. F.R.G.S. F.Z.S.
Mrs. John I. Thornycroft,
John Jewell Vezey, Esq. F.R.M.S.
Laundy Walters, Esq.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to the Blackman Ventilating Company for their present of a Ventilating Fan, with fittings, and for their liberal offer to place the time of their engineers always at the service of the Institution.

John Tyndall, Esq. D.C.L. LL.D. F.R.S. was elected Honorary Professor of Natural Philosophy.

The Right Hon. Lord Rayleigh, M.A. D.C.L. LL.D. F.R.S. was elected Professor of Natural Philosophy.

The following Alterations in the Bye Laws of the Royal Institution were passed:—

In Chapter VII. (*Of the Duties of the Committee of Visitors*).

In Art. 3, line 3, omit, "*April*," "*and October*."

In Art. 3, line 3, insert after the word "*January*" the word "*and*."

The following was added to the Bye Laws of the Royal Institution as the Final Chapter:—

"*Notwithstanding anything hereinbefore contained, whenever any General Meeting, Managers' Meeting, or Visitors' Meeting would, under the foregoing provisions, be held on an Easter Monday, or on a Whitsun Monday, or on a Good Friday, or on any Bank Holiday, General Fast Day, or General Thanksgiving Day, it shall be lawful for the Committee of Managers to appoint some other and appropriate day for the holding of such meeting in place of the day hereinbefore appointed. Notice of such appointment shall be given, not less than twenty-one days prior to the holding of such meeting, to such persons as shall be entitled to be present at such meeting.*"

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FOR

Lords of the Admiralty—Greenwich Observations, 1887. 4to. 1889.
Appendices I. II. III. 4to. 1889.

Governor-General of India—Geological Survey of India: Palæontologia Indica, Series XIII. Vol. IV. Part 1. fol. 1889.
Records, Vol. XXIII. Part 1. 4to. 1890.

Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta: Rendiconti. 1° Semestre, Vol. VI. Fasc. 4. 8vo. 1890.

American Philosophical Society—Proceedings, No. 130, Vol. XXVI. 8vo. 1889.

Aristotelian Society—Proceedings, Vol. I. No. 3, Part 1. 8vo. 1890.

Astronomical Society, Royal—Monthly Notices, Vol. L. No. 5. 8vo. 1890.

Bankers, Institute of—Journal, Vol. XI. Part 4. 8vo. 1890.

Boston Society of Natural History—Proceedings, Vol. XXIV. Parts 1–2. 8vo. 1889.

British Architects, Royal Institute of—Proceedings, 1889–90, No. 13. 4to.

Brymner, Douglas, Esq. (the Archivist)—Report on Canadian Archives, 1889. 8vo. 1890.

Buckton, George B. Esq. F.R.S. M.R.I. (the Author)—Monograph of the British Cicadæ or Tettigiidæ, Part 2. 8vo. 1890.

Burdett-Coutts, A. L. B. Esq. M.P. M.R.I. (the Author)—The Pay System in Hospitals. 4to. 1888.

Chemical Industry, Society of—Journal, Vol. IX. No. 3. 4to. 1890.

Chemical Society—Journal for April, 1890. 8vo.

Civil Engineers Institution—Minutes of Proceedings, Vol. XCIX. 8vo. 1890.

Cracovie, l'Académie des Sciences—Bulletin, 1890, No. 3. 8vo.

Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I.—Journal of the Royal Microscopical Society, 1890, Part 2. 8vo.

Editors—American Journal of Science for April, 1890. 8vo.
Analyst for April, 1890. 8vo.
Athenæum for April, 1890. 4to.
Chemical News for April, 1890. 4to.
Chemist and Druggist for April, 1890. 8vo.
Electrical Engineer for April, 1890. fol.
Engineer for April, 1890. fol.
Engineering for April, 1890. fol.
Horological Journal for April, 1890. 8vo.
Industries for April, 1890. fol.
Iron for April, 1890. 4to.
Ironmongery for April, 1890.
Murray's Magazine for April, 1890. 8vo.
Nature for April, 1890. 4to.
Photographic News for April, 1890. 8vo.
Revue Scientifique for April, 1890. 4to.
Telegraphic Journal for April, 1890. fol.
Zoophilist for April, 1890. 4to.

Electrical Engineers, Institution of—Journal, No. 85. 8vo. 1890.
Florence, Biblioteca Nazionale Centrale—Bollettino, No. 103. 8vo. 1890.
Franklin Institute—Journal, No. 772. 8vo. 1890.
Geological Institute, Imperial, Vienna—Verhandlungen, 1890, Nos. 3–5. 8vo.
Hamilton, J. Lawrence, Esq. M.R.C.S. M.R.I. (the Author)—Report upon the Fish Supply of the Metropolis. 4to. 1890.
Horticultural Society, Royal—Journal, Vols. VII. (Part 2) VIII. IX. X. XI. (Parts 1–3) and XII. (Part 1). 8vo. 1886–90.
Langley, S. P. Esq. (the Author)—Temperature of the Moon, Part II. (National Academy of Sciences, Vol. IV.). 4to. 1889.
Linnean Society—Journal, Vol. XXVII. Nos. 174, 181. 8vo. 1890.
Madras Government Central Museum — Notes on the Pearl Fisheries. By E. Thurston. 8vo. 1890.
Mechanical Engineers—Proceedings, 1889, No. 4. 8vo. 1890.
Mensbrugghe, M. G. Van der (the Author)—Sur la Condensation de la Vapeur d'Eau. 8vo. 1890.
Meteorological Office—Weekly Weather Reports, Nos. 14–16. 4to. 1890.
Miller, W. J. C. Esq. (the Registrar)—Medical Register, 1890. 8vo.
Dentists' Register, 1890. 8vo.
Medical Students' Register, 1890. 8vo.

New York Academy of Sciences—Annals, Vol. XIV. No. 12. 8vo. 1889.
Nova Scotian Institute of Natural Science—Proceedings and Transactions, Vol. VII. Part 3. 8vo. 1889.
Pharmaceutical Society of Great Britain—Journal, April, 1890. 8vo.
Rathbone, E. P. Esq. (the Editor)—The Witwatersrand Mining and Metallurgical Review, No. 3. 8vo. 1890.
Rio de Janeiro Observatory—Revista, Nos. 2, 3. 8vo. 1890.
Rothschild, M. J. (the Editor)—L'Exposition Universelle. Par Henri de Parville. 12mo. 1890.
Seismological Society of Japan—Transactions, Vol. XIV. 8vo. 1889.
Selborne Society—Nature Notes, Vol. I. No. 3. 8vo. 1890.
Society of Architects—Proceedings, Vol. II. No. 9. 8vo. 1890.
Society of Arts—Journal for April, 1890. 8vo.
Statistical Society—Journal, Vol. LIII. Part 1. 8vo. 1890.
St. Pétersbourg, Académie Impériale des Sciences—Mémoires, Tome XXXVII. Nos. 4, 5. 4to. 1890.
Verein zur Beförderung des Gewerbfleises in Preussen—Verhandlungen, 1890, Heft 3. 4to.
Wild, Dr. H.—Annalen der Physikalischen Central Observatorium, Theil II. 4to. 1889.
Zoological Society of London—Transactions, Vol. XII. Part 10. 4to. 1890.
Proceedings, 1889. Part 4. 8vo.

WEEKLY EVENING MEETING,

Friday, May 9, 1890.

Sir James Crichton Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

R. Brudenell Carter, Esq. F.R.C.S.

Colour-Vision and Colour-Blindness.

It is a matter of familiar knowledge that the sense of vision is called into activity by the formation, on the retina or internal nervous expansion of the eye, of an inverted optical image of external objects—an image precisely analogous to that of the photographic camera. The retina lines the interior of the eyeball over somewhat more than its posterior hemisphere. It is a very delicate transparent membrane, about one-fifth of a millimetre in thickness at its thickest part, near the entrance of the optic nerve, and it gradually diminishes to less than half that thickness at its periphery. It is resolvable by the microscope into ten layers [*shown*], which are united together by a web of connective tissue, which also carries blood-vessels to minister to the maintenance of the structure. I need only refer to two of these layers; the anterior or fibre-layer, mainly composed of the fibres of the optic nerve, which spread out radially from their point of entrance in every direction, except where they curve around the central portion of the membrane; and the perceptive layer, which, as viewed from the interior of the eyeball, may be likened to an extremely fine mosaic, each individual piece of which is in communication with a nerve fibre, by which the impressions made upon it are conducted to the brain. The terminals of the perceptive layer are of two kinds, called respectively rods and cones; the former, as the name implies, being cylindrical in shape, and the latter conical. The bases of the cones are directed towards the interior of the eye, so as to receive the light; and it is probable that each cone may be regarded as a collecting apparatus, calculated to gather together the light which it receives, and to concentrate this light upon its deeper and more slender portion, or posterior limb, which is believed to be the portion of the whole structure which is really sensitive to luminous impressions. The distribution of the two elements differs greatly in different animals; and the differences point to corresponding differences in function. The cones are more sensitive than the rods, and minister to a higher acuteness of vision. In the human eye, there is a small central region in which the perceptive layer consists of cones only, a region which the fibres avoid by curving round it, and in which the other layers of the retina are much thinner than elsewhere, so as to leave a depression, and are stained of a lemon-yellow colour [*shown*]. In a zone immediately around this yellow spot each cone is surrounded by a single circle of rods [*shown*]; and, as we proceed outwards towards the periphery of the retina, the circle of rods around each cone becomes successively double, triple, quadruple, or even more numerous [*shown*]. The yellow spot receives the image of the object to which the eye is actually directed, while the images of surrounding objects fall upon zones which surround the yellow spot; and the result of this arrangement is that, generally speaking, the distinctness of vision diminishes in proportion to the distance of the image of the object from the retinal centre. The consequent effect has been well described by saying that what we see resembles a picture, the central part of which is exquisitely finished, while the parts around the centre are only roughly sketched in. We are conscious that these outer parts are there; but, if we desire to see them accurately, they must be made the objects of direct vision in their turn.

The indistinctness with which we see lateral objects is so completely neutralised by the quick mobility of the eyes, and by the manner in which they range almost unconsciously over the whole field of vision, that it seldom or never forces itself upon the attention. It may be conveniently displayed by means of an instrument called a perimeter [*shown*], which enables the observer to look steadily at a central spot, while a second spot or other object is moved along an arc, in any meridian, from the circumference of the field of view towards the centre, or *vice versâ*. Slight differences will be found between individuals; but, speaking generally, a capital letter one-third of an inch high, which is legible by direct vision at a distance of sixteen feet, and is recognisable as a dark object at 40° or 50° from the fixing point, will not become legible, at a distance of one foot, until it arrives within about 10°.

The image formed upon the retina is rendered visible by two different conditions—that is to say, by differences in the amount of light which enters into the formation of its different parts, and by differences in the quality of this light, that is, in its colour. The former conditions are fulfilled by an engraving, the latter by a painting. It is with the latter conditions only, and with the power of perceiving them, that we are concerned this evening.

Before such an audience as that which I have the honour to address, it is unnecessary to say more about colour than that it depends upon the power, possessed by the objects which we describe as coloured, to absorb and retain certain portions of white or other mixed light, and to reflect or transmit other portions. The resulting effect of colour is the impression produced upon the eye or upon the brain by the waves of light which are left after the process of selective absorption has been accomplished. Some substances absorb two of the three fundamental colours of the solar spectrum, others absorb one only, others absorb portions of one or more. Whatever remains is transmitted through the media of the eye; and, in the majority of the human race, suffices to excite the retina to a characteristic kind of activity. Few things are more curious than the multitude of different colour sensations which may be produced by the varying combinations of the three simple elements, red, green, and violet; but this is a part of the subject into which it would be impossible for me now to enter, and with which most of those who hear me must already be perfectly familiar.

Apart from the effects of colour as one of the chief sources of beauty in the world, it is manifest that the power of distinguishing it adds greatly to the acuteness of vision. Objects which differ from their surroundings by differences of colour are far more conspicuous than those which differ only by differences of light and shade. Flowers are much indebted to their brilliant colouring for the visits of the insects by which they are fertilised; and creatures which are the prey of others find their best protection in a resemblance to the colours of their environment. It is probably an universal truth that the organs of colour-perception are more highly specialised, and that the sense of colour is more developed, in all animals, in precise proportion to the general acuteness of vision of each.

From a variety of considerations, into which time will not allow me to enter, it has been concluded that the sense of colour is an endowment of the retinal cones, and that the rods are sensitive only to differences in the quantity of the incident light, without regard to its quality. Nocturnal mammals, such as mice, bats, and hedgehogs, have no cones; and cones are less developed in nocturnal birds than in diurnal ones. Certain limitations of the human colour-sense may almost be inferred from the anatomy of the retina. It is found, as that anatomy would lead us to suppose, that complete colour-sense exists only in the retinal centre, or in and immediately around the yellow spot region, and that it diminishes as we pass away from this centre towards the periphery. The precise facts are more difficult to ascertain than might be supposed; for, although it is easy to bring coloured objects from the circumference to the centre of the field of vision on the perimeter, it is by no means easy to be quite sure of the point at which the true colour of the advancing object can first be said to be distinctly seen. Much depends, moreover, on the size of this advancing object; because, the larger it is, the sooner will its image fall upon some of the more sparsely distributed cones of the peripheral portion of the retina. Testing the matter upon myself with coloured cards of the size of a man's visiting card, I find that I am conscious of red or blue at about 40° from the fixing point, but not of green until it comes within about 30°; while, if I take three spots, respectively of bright red, bright green, and bright blue, each half a centimetre in diameter, and separated from its neighbour on either side by an interval of half a centimetre—spots which would be visible as distinct and separate objects at eight metres—I cannot fairly and distinctly see all three colours until they come within 10° of the centre. Beyond 40°, albeit with slight differences between individuals, and on different meridians for the same individual, colours are only seen by the degree of their luminosity—that is, they appear as light spots if upon a dark ground, and as dark spots if upon a light ground. Speaking generally, therefore, it may be said that human vision is only tri-chromatic, or complete for the three fundamental colours of the solar spectrum, over a small central area, which certainly does not cover more than 30° of the field; that it is bi-chromatic, or limited to red and violet, over an annulus outside this central area; and that it is limited to light and shade from thence to the outermost limits of the field.

The nature and limitations of the colour-sense in man long ago suggested to Thomas Young that the retina might contain three sets of fibres, each set capable of responding to only one of the fundamental colours; or, in other words, that there are special nerve fibres for red, special nerve fibres for green, and special nerve fibres for violet. It has also been assumed that the differences between these fibres might essentially consist in the ability of each set to respond only to light-vibrations of a certain wave-length, much as a tuned string will only respond to a note with which it is in unison. In the human subject, so far as has yet been ascertained, no optical differences between the cones are discoverable; but the analogy of the ear, and the facts which have been supplied by comparative anatomy, combine to render Young's hypothesis exceedingly probable, and it is generally accepted, at least provisionally, as the only one which furnishes an explanation of the facts. It implies that elements of all three varieties are present in the central portion of the retina; that elements sensitive to green are absent from an annulus around the centre; and that the peripheral portions are destitute of any elements by which colour-sense can be called into activity.

According to the observation already made, that the highest degree of acuteness of vision is necessarily attended by a corresponding acuteness of colour-sense, we should naturally expect to find such a highly-developed colour-sense in birds, many of which appear, as regards visual power, to surpass all other creatures. I need not dwell upon the often-described acuteness of vision of vultures, or upon the vision of fishing birds; but may pass on to remark that the acuteness of their vision appears not only to be unquestionable, but also to be much more widely diffused over the retina than is the case with man. If we watch domestic poultry, or pigeons, feeding, we shall frequently see a bird, when busily picking up food immediately in front of its beak, suddenly make a lateral dart to some grain lying sideways to its line of sight, which would have been practically invisible to a human eye looking in the same direction as that of the fowl. When we examine the retina, the explanation both of the acuteness of vision and of its distribution becomes at once apparent. In birds, in some reptiles, and in fishes, not only are cones distributed over the retina much more abundantly and more evenly than in man, but the cones are provided with coloured globules, droplets of coloured oil, at their apices, through which the light entering them must pass before it can excite sensation, and which are practically impervious to any colour but their own. This lantern slide [*shown*] is taken from a drawing by Mr. Hulke, in a paper communicated by him to the Royal Society, and it exhibits the colour globules in the retinal cones of *Chelonia Mydas.* Each globule is so placed as to intervene between what is regarded as the collecting portion of the cone and what is regarded as its perceptive portion, in such a way that the latter can only receive colour which is capable of passing through the globule. The retinæ of many birds, especially of the finch, the pigeon, and the domestic fowl, have been carefully examined by Dr. Waelchli, who finds that near the centre green is the predominant colour of the cones, while among the green cones red and orange ones are somewhat sparingly interspersed, and are nearly always arranged alternately, a red cone between two orange ones and *vice versâ.* In a surrounding portion, called by Dr. Waelchli the red zone, the red and orange cones are arranged in chains, and are larger and more numerous than near the yellow spot; the green ones are of smaller size, and fill up the interspaces. Near the periphery the cones are scattered, the three colours about equally numerous and of equal size, while a few colourless cones are also seen [*shown*]. Dr. Waelchli examined the optical properties of the coloured cones by means of the micro-spectroscope, and found, as the colours would lead us to suppose, that they transmitted only the corresponding portions of the spectrum; and it would almost seem, excepting for the few colourless cones at the peripheral part of the retina, that the birds examined must have been unable to see blue, the whole of which would be absorbed by their colour globules. It would be necessary to be thoroughly acquainted with their food in order to understand any advantage which the birds in question may derive from the predominance of green, red, and orange globules over others; but it is impossible to consider the structure thus described without coming to the conclusion that the birds in which it exists must have a very acute sense of the colours corresponding to the globules with which they are so abundantly provided, and that this colour-sense, instead of being localised in the centre, as in the human eye, must be diffused over a very large portion of the retina. Dr. Waelchli points out that the coloration of the yellow spot in man must, to a certain extent, exclude blue from the central and most sensitive portion of his retina.

It is hardly necessary to mention how completely the high differentiation of the cones in the creatures referred to tends to support the hypothesis of Young, that a similar differentiation, although not equally manifest, exists also in man. If this be so, we must conclude that the region of the yellow spot contains cones, some of which are capable of being called into activity by red, others by green, and others by violet; that a surrounding annulus contains no cones sensitive to green, but such as are sensitive to red or to violet only; and that, beyond and around this latter region, such cones as may exist are not sensitive to any colour, but, like the rods, only to differences in the amount of light. When cones of only one kind are called into activity, the sensation produced is named red, green, or violet; and, when all three varieties are stimulated in about an equal degree, the sensation produced is called white. In the same way, the innumerable intermediate colour-sensations of which the normal eye is susceptible must be ascribed to stimulation of the three varieties of cones in unequal degrees.

The conditions of colour-sense, which, in the human race, or at least in civilised man, exist normally in outer zones of the retina, are found in a few individuals to exist also in the centre. There are persons in whom the region of the yellow spot is absolutely insensitive to colour, and recognises only differences in the amount or quantity of light. To such persons, the term "colour-blind" ought perhaps in strictness to be limited; but the individuals in question are so rare that they are hardly entitled to a monopoly of an appellation which is conveniently applied also to others. The totally colour-blind would see a coloured picture as if it were an engraving, or a drawing in black and white, and would perceive differences between its parts only in the degree in which they differed in brightness.

A more common condition is the existence, in the centre of the retina, of a kind of vision like that which normally exists in the zone next surrounding it—that is, a blindness to green. Persons who are blind to green appear to see violet and yellow much as these are seen by the normal-sighted; and they can see red, but they cannot distinguish it from green. Others, and this form is more common than the preceding, are blind to red; and a very small number of persons are blind to violet. Such blindness to one of the fundamental colours may be either complete or incomplete—that is to say, the power of the colour in question to excite its proper sensation may be either absent or feeble. In some cases, the defect is so moderate in degree as to be adequately described by the phrase "defective colour-sense."

The experiments of Helmholtz upon colour led him to supplement the original hypothesis of Young by the supposition that the special nerve elements excited by any one colour are also excited in some degree by each of the other two, but that they respond by the sensation appropriate to themselves, and not by that appropriate to the colour by which they are thus feebly excited. This, which is often called the Young-Helmholtz hypothesis, assumes that the pure red of the spectrum, while it mainly stimulates the fibres sensitive to red, stimulates in a less degree those which are sensitive to green, and in a still less degree those which are sensitive to violet, the resulting sensation being red. Pure green stimulates strongly the green-perceptive fibres, and stimulates slightly both the red-perceptive and the violet-perceptive—resulting sensation, green. Pure violet stimulates strongly the violet-perceptive fibres, less strongly the green-perceptive, least strongly the red-perceptive—resulting sensation, violet. When all three sets of fibres are stimulated at once, the resulting sensation is white; and when a normal eye is directed to the spectrum, the region of greatest luminosity is in the middle of the yellow; because, while here both the green-perceptive and the red-perceptive fibres are stimulated in a high degree, the violet-perceptive are also stimulated in some degree.

According to this view of the case, the person who is red-blind, or in whom the red-perceptive fibres are wanting or paralysed, has only two fundamental colours in the spectrum instead of three. Spectral red, nevertheless, is not invisible to him, because it feebly excites his green-perceptive fibres, and hence appears as a saturated green of feeble luminosity; saturated, because it scarcely at all excites the violet-perceptive fibres. The brightest part of the spectrum, instead of being in the yellow, is in the blue-green, because here both sets of sensitive fibres are stimulated. In the case of the green-blind, in whom the fibres perceptive of green are supposed to be wanting or paralysed, the only stimulation produced by spectral green is that of the red-perceptive and of the violet-perceptive fibres; and, where these are equally stimulated, we obtain the white of the green-blind, which, to ordinary eyes, is a sort of rose-colour, a mixture of red and violet [*shown*]. In like manner, the white of the red-blind is a mixture of green and violet [*shown*]; and, if we consider the facts, we shall see that spectral red, which somewhat feebly stimulates the green-perceptive fibres of the normal eye, and spectral green, which somewhat feebly stimulates the red-perceptive fibres of the normal, and also of the green-blind eye, must appear to the green-blind to be one and the same colour, differing only in luminosity, and that in an opposite sense to the perception of the red-blind. In other words, red and green are undistinguishable from each other, as colours, alike to the red-blind and to the green-blind; but to the former the red, and to the latter the green, appears, as compared with the other, to be of feeble luminosity. In either case, the two are only lighter and darker shades of the same colour. The conditions of violet-blindness are analogous, but the defect itself is very rare; and, as it is of small industrial importance, it has attracted but a small degree of attention.

Very extensive investigations, conducted during the last few years both in Europe and in America, have shown that those which may be called the common forms of colour-blindness, the blindness to red and to green, exist in about 4 per cent. of the male population, and in perhaps one per thousand of females; while among the rest there are slight differences of colour-sense, partly due to differences of habit and training, but of little or no practical importance. One such difference, to which Lord Rayleigh was the first to direct attention, has reference to yellow. The pure yellow of the spectrum may, as is generally known, be precisely matched by a mixture of spectral red with spectral green; but the proportions in which the mixture should be made differ within certain limits for different people. The differences must, I think, depend upon differences in the pigmentation of the yellow spot, rather than upon any defect in the nervous apparatus of the colour-sense. There is a very ingenious instrument, invented by Mr. Lovibond, and called by him the "tintometer," which allows the colour of any object to be accurately matched by combinations of coloured glass, and to be expressed in terms of the combination. In using this instrument, we not only find slight differences in the combinations required by different people, but also in the combinations required by the two eyes of the same person. Here, again, I think the differences must be due either to differences in the pigmentation of the yellow spot, or possibly also to differences in the colour of the internal lenses of the several eyes, the lens, as is well known, being usually somewhat yellow after middle age. The differences are plainly manifest in comparing persons all of whom possess tri-chromatic vision, and are not sufficient in degree to be of any practical importance.

The effect of the pigmentation of the yellow spot in modifying colour may be rendered visible by an ingenious experiment, for which we are indebted to Sir George Stokes. If the beam of the electric lamp be suffered to pass through a cell containing a solution of chloride of chromium [*shown*], and then to fall upon a white screen, the green colour of the solution will be more absorbed by the yellow spot than by the surrounding portions of the retina, and the result will be the appearance of a faint roseate cloud floating in the centre of the field. It would seem, from descriptions, that the pigmentation of the yellow spot is more pronounced, and hence that the cloud is more conspicuous, in some individuals than in others.

Taking the ordinary case of a red-blind or of a green-blind person, it is interesting to speculate upon the appearance which the world must present to them. Being insensible to one of the fundamental colours of the spectrum, they must lose, roughly speaking, one-third of the luminosity of nature; unless, as is possible, the deficiency is made good to them by increased acuteness of perception to the colours which they see. Whether they see white as we see it, or as we see the mixtures of red and violet, or of green and violet, which they make to match with it, we can only conjecture, on account of the inadequacy of language to convey any accurate idea of sensation. We have all heard of the blind man who concluded, from the attempts made to describe scarlet to him, that it was like the sound of a trumpet. If we take a heap of coloured wools, and look at them, first through a glass of peacock-blue, by which the red rays are filtered out [*shown*], and next through a purple glass, by which a large proportion of the green will be filtered out [*shown*], we may presume that, under the first condition, the wools will appear much as they would do to the red-blind; and under the second, much as they would do to the green-blind. It will be observed that the appearances differ in the two conditions, but that, in both, red and green are practically undistinguishable from each other, and appear as the same colour, but of different luminosity.

Prior to reflection, and still more, prior to experience, we should be apt to conjecture that the existence of colour-blindness in any individual could not remain concealed, either from himself or from those around him; but such a conjecture would be directly at variance with the truth. Just as it was reserved for Mariotte, in the reign of Charles II., to discover that there is, in the field of vision of every eye, a lacuna, or blind spot, corresponding with the entrance of the optic nerve, so it was reserved for a still later generation to discover the existence of so common a defect as colour-blindness. The first recorded case was described to Dr. Priestley by Mr. Huddart, in 1777, and was that of a man named Harris, a shoemaker at Maryport, in Cumberland, who had also a colour-blind brother, a mariner. Soon afterwards, the case of Dalton, the chemist, was fully described, and led to the discovery of other examples of a similar kind. The condition was still, however, looked upon as a very exceptional one; insomuch that the name of "Daltonism" was proposed for it, and is still generally used in France as a synonym for colour-blindness. Such use is objectionable, not only because it is undesirable thus to perpetuate the memory of the physical infirmity of an eminent philosopher, but also because Dalton was a red-blind, so that the name could only be correctly applied to his particular form of defect.

Colour-blindness often escapes detection on account of the use of colour-names by the colour-blind in the same manner as that in which they hear them used by other people. Children learn from the talk of those around them that it is proper to describe grass as green, and bricks or cherries as red; and they follow this usage, although the difference may appear to them so slight that their interpretation of either colour-name may be simply as a lighter or darker shade of the other. When they make mistakes, they are laughed at, and thought careless, or to be merely using colour-names incorrectly; and a common result is that they rather avoid such names, and shrink from committing themselves to statements about colour. Dr. Joy Jefferies gives an interesting description of the almost unconscious devices practised by the colour-blind in this way. He says:—

"The colour-blind, who are quick-witted enough to discover early that something is wrong with their vision by the smiles of their listeners when they mention this or that object by colour, are equally quick-witted in avoiding so doing. They have found that there are names of certain attributes they cannot comprehend, and hence must let alone. They learn, also, what we forget, that so many objects of every-day life always have the same colour, as red tiles or bricks, and the colour-names of these they use with freedom; whilst they often, even unconsciously, are cautious not to name the colour of a new object till they have heard it applied, after which it is a mere matter of memory stimulated by a consciousness of defect. I have often recalled to the colour-blind their own acts and words, and surprised them by an exposure of the mental jugglery they employed to escape detection, and of which they were almost unaware, so much had it become matter of habit. Another important point is, that as violet-blindness is very rare, the vast majority of defective eyes are red or green blind. These persons see violet and yellow as the normal-eyed, and they naturally apply these colour-names correctly. When, therefore, they fail in red or green, a casual observer attributes it to simple carelessness—hence a very ready avoidance of detection. It does not seem possible that any one who sees so much correctly, and whose ideas of colour so correspond with our own, cannot be equally correct throughout, if they will but take the pains to notice and learn."

When the colour-blind are placed in positions which compel them to select colours for themselves or others, or when, as sometimes happens, they are not sensitive with regard to their defect, but rather find amusement in the astonishment which it produces among the colour-seeing, the results which occasionally follow are apt to be curious. They have often been rendered still more curious, by having been the unconscious work of members of the Society of Friends. Colour-blindness is a structural peculiarity, constituting what may be called a variety of the human race; and, like other varieties, it is liable to be handed down to posterity. Hence, if the variety occurs in a person belonging to a community which is small by comparison with the nation, and among whose members there is frequent intermarriage, it has an increased probability of being reproduced; and thus, while many of the best known of the early examples of colour-blindness, including that of Dalton himself, were furnished by the Society of Friends, the examinations of large numbers of scholars and others, conducted during the last few years, have shown that, in this country, colour-blindness is more common among Jews than among the general population. The Jews have no peculiarities of costume; but the spectacle, which has more than once been witnessed, of a venerable Quaker who had clothed himself in bright green or in vivid scarlet, could scarcely fail to excite the derision of the unreflecting. Time does not allow me to relate the many errors of the colour-blind which have been recorded; but there is an instance of a clerk in a Government office, whose duty it was to tick certain entries, in relation to their subject-matter, with ink of one or of another colour, and whose accuracy was dependent upon the order in which his ink-bottles were ranged in front of him. This order having been accidentally disturbed, great confusion was produced by his mistakes, and it was a long time before these were satisfactorily accounted for. An official of the Prussian Post Office, again, who was accustomed to sell stamps of different values and colours, was frequently wrong in his cash, his errors being as often against himself as in his favour, so as to exclude any suspicion of dishonesty. His seeming carelessness was at last explained by the discovery of his colour-blindness, and he was relieved of a duty which it was impossible for him to discharge without falling into error.

The colour-mistakes of former years were, however, of little moment when compared with those now liable to be committed by engine drivers and mariners. The avoidance of collisions at sea and on railways depends largely on the power promptly to recognise the colours of signals; and the colours most available for signalling purposes are red and green, or precisely those between which the sufferers from the two most common forms of colour-blindness are unable with any certainty to discriminate. About thirteen years ago there was a serious railway accident in Sweden, and, in the investigation subsequent to this accident, there were some remarkable discrepancies in the evidence given with regard to the colour of the signals which had been displayed. Prof. Holmgren, of the University of Upsala, had his attention called to this discrepancy, and he found, on further examination, that the witness whose assertions about the signals differed from those of other people was actually colour-blind. From this incident arose Prof. Holmgren's great interest in the subject, and he did not rest until he had obtained the enactment of a law under which no one can be taken into the employment of a Swedish railway until his colour-vision has been tested, and has been found to be sufficient for the duties he will be called upon to perform. The example thus set by Sweden has been followed, more or less, by other countries, and especially, thanks to the untiring labours of Dr. Joy Jeffries, of Boston, by several of the United States; while at the same time much evidence has been collected to show the connection between railway and marine accidents and the defect.

It has been found, by very extensive and carefully conducted examinations of large bodies of men—soldiers, policemen, the workers in great industrial establishments, and so forth—as well as of children in many schools, that colour-blindness exists in a noticeable degree, as I have already said, in about four per cent. of the male industrial population in civilised countries, and in about one per thousand of females. Among the males of the more highly educated classes, taking Eton boys as an example, the colour-blind are only between two and three per cent., and perhaps nearer to two than to three. Whether a similar difference exists between females of different classes we have no statistics to establish. The condition of colour-blindness is absolutely incurable, absolutely incapable of modification by training or exercise, in the case of the individual; although the comparative immunity of the female sex justifies the suggestion that this may possibly be due to training throughout successive generations, on account of the more habitual occupation of the female eyes about colour in relation to costume. However this may be, in the individual, as I have said, the defect is unalterable; and if the difference between red and green is uncertain at eight years of age it will be equally uncertain at eighty. Hence the existence of colour-blindness, among those who have to control the movements of ships or of railway trains, constitutes a real danger to the public; and it is highly important that the colour-blind, in their own interests as well as in those of others, should be excluded from employments the duties of which they are unfit to discharge.

The attempts hitherto made in this country to exclude the colour-blind from railway and marine employment have not been by any means successful. As far as the merchant navy is concerned, so-called examinations have been conducted by the Board of Trade, with results which can only be described as ludicrous. Candidates have been "plucked" in colour at one examination, and permitted to pass at a subsequent one; as if correct colour-vision were something which could be acquired. Such candidates were either improperly rejected on the first occasion, or improperly accepted on the second. On English railways there has been no uniformity in the methods of testing, except, in so far as I am acquainted with them, that they have been almost uniformly misleading, calculated to lead to the imputation of colour-blindness where it did not exist, and to leave it undiscovered where it did. In these circumstances, it is not surprising that great discontent should have arisen among railway men in relation to the subject; and this discontent has led, indirectly, to the appointment of a committee by the Royal Society, with the sanction of the Board of Trade, for the purpose of investigating the whole question as completely as may be possible.

It is perhaps worth while, before proceeding to describe the manner in which the colour-sense of large bodies of men should be tested for industrial purposes, to say something as to the amount of danger which colour-blindness produces. A locomotive, as we all know, is under the charge of two men—the driver and the fireman. In a staff of one thousand of each, allotted to one thousand locomotives, we should expect, in the absence of any efficient method of examination, to find forty colour-blind drivers and forty colour-blind firemen. The chances would be one in twenty-five that either the driver or the fireman on any particular engine would be colour-blind; they would be one in 625 that both would be colour-blind. These figures appear to show a greater risk of accident than we find realised in actual working, and it is manifest that there are compensations to be taken into account. In the first place, the term "colour-blind" is itself in some degree misleading; for it must be remembered that the signals to which the colour-blind person is said to be "blind" are not invisible to him. To the red-blind, the red light is a less luminous green; to the green-blind, the green light is a less luminous red. The danger arises because the apparent differences are not sufficiently characteristic to lead to certain and prompt identification in all states of illumination and of atmosphere. It must be admitted, therefore, that a colour-blind driver may be at work for a long time without mistakes; and it is probable, knowing as he must that the differences between different signal lights appear to him to be only trivial, that he will exercise extreme caution Then it must be remembered that lights never appear to an engine-driver in unexpected places. Before being intrusted with a train, he is taken over the line, and is shown the precise position of every light. If a light did not appear where it was due, he would naturally ask his fireman to aid in the look-out. It must be also remembered that to overrun a danger signal does not of necessity imply a collision. A driver may overrun the signal, and after doing so may see a train or other obstruction on the line, and may stop in time to avoid an accident. In such a case he would probably be reported and fined for overrunning the signal; and, if the same thing occurred again, he would be dismissed for his assumed carelessness, probably with no suspicion of his defect. Colour-blind firemen are unquestionably thus driven out of the service by the complaints of their drivers; and none but railway officials know how many cases of overrunning signals, followed by disputes as to what the signals actually were, occur in the course of a year's work. I have never heard of an instance in this country in which, after a railway accident, the colour-vision of the driver concerned, or of his fireman, has been tested by an expert, on the part either of the Board of Trade or of the Company; but a fireman in the United States has recently recovered heavy damages from the Company for the loss of one of his legs in a collision which was proved to have been occasioned by the colour-blindness of the driver. Looking at the whole question, I feel that the danger on railways is a real one, but that it is minimised by the several considerations to which I have referred, and that it is much smaller than the frequency of the defect might lead us to think likely.

At sea, the danger is much more formidable. The lights appear at all sorts of times and places, and there may be only one responsible person on the look-out. Mr. Bickerton, of Liverpool, has lately published accounts of three cases in which the colour-blindness of officers of the mercantile marine, all of whom had passed the Board of Trade examination, was accidentally discovered by the captains being on deck when the officers in question gave wrong orders consequent upon mistaking the light shown by an approaching vessel. The loss of the *Ville du Havre* was almost certainly due to colour-blindness; and a very fatal collision in American waters, some years ago, between the *Isaac Bell* and the *Lumberman*, was traced, long after the event, to the colour-blindness of a pilot, who had been unjustly accused of being drunk at the time of the occurrence. In how many instances colour-blindness has been the unsuspected cause of wrecks and other calamities at sea, it is impossible to do more than conjecture.

It is necessary, then, alike in the public interest and in the interest of the colour-blind, who have doubtless often suffered in the misfortunes which their defects have produced, to detect them in time to prevent them from entering into the marine and railway services; and the next question is, how this detection should be accomplished. We have to distinguish the colour-blind from the colour-sighted; but we must be careful not to confound colour-blindness with the much more common condition of colour-ignorance.

It would surprise many people, more especially many ladies, to discover the extent to which sheer ignorance of colour prevails among boys and men of the labouring classes. Many, who can see colours perfectly, and who would never be in the least danger of mistaking a railway signal, are quite unable to name colours or to describe them; and they are sometimes unable to perceive, for want of education of a faculty which they notwithstanding possess, anything like fine shades of difference. Mr. Gladstone once published a paper on the scanty and uncertain colour-nomenclature of the Homeric poems; and he might have found very similar examples among his own contemporaries and in his own country. I have lately heard a description of a pattern card of coloured silks, issued by a Lyons manufacturer, which contains samples of two thousand different colours, each with its more or less appropriate name. There is here a larger colour-vocabulary than the entire vocabulary, for the expression of all his knowledge and of all his ideas, which is possessed by an average engine-driver or fireman; and, just as most of us would be ignorant of the names of the immense majority of the colours displayed on that card, so hundreds of men and boys among the labouring classes, especially in large towns, where the opportunities of education by the colours of flowers and insects are very limited, are ignorant of the names of colours which persons of ordinary cultivation mention constantly in their daily talk, and expect their children to pick up and to understand unconsciously. It is among people thus ignorant that the officials of the Board of Trade, and of railways, have been most successful in finding their supposed colour-blind persons; and these persons, who would never have been pronounced colour-blind by an expert, have been able, as soon as they have paid a little attention to the observation and naming of colour, to pass an official examination triumphantly. The sense of colour presents many analogies to that of hearing. Some people can hear a higher or a lower note than others, the difference depending upon structure, and being incapable of alteration. No one who cannot hear a note of a certain pitch can ever be trained to do so; but, within the original auditory limits of each individual, the sense of hearing may be greatly improved by cultivation. In like manner, a person who is blind to red or green must remain so; but one whose colour-sense is merely undeveloped by want of cultivation may have its acuteness for fine differences very considerably increased.

In order to test colour-vision for railway and marine purposes, the first suggestion which would occur to many people would be to employ as objects the flags and signal lanterns which are used in actual working. I have heard apparently sensible people use, with reference to such a procedure, the phrase upon which Faraday was wont to pour ridicule, and to say that the fitness of the suggested method "stands to reason." To be effectual, such a test must be applied in different states of atmosphere, with coloured glasses of various tints, with various degrees of illumination, and with the objects at various distances; so that much time would be required in order to exhaust all the conditions under which railway signals may present themselves. This being done, the examinee must be either right or wrong each time. He has always an even chance of being right; and it would be an insoluble problem to discover how many correct answers might be due to accident, or how many incorrect ones might be attributed to nervousness or to confusion of names.

We must remember that what is required is to detect a colour-blind person against his will; and to ascertain, not whether he describes a given signal rightly or wrongly on a particular occasion, but whether he can safely be trusted to distinguish correctly between signals on all occasions. We want, in short, to ascertain the state of his colour-vision generally; and hence to infer his fitness or unfitness to discharge the duties of a particular occupation.

For the accomplishment of this object, we do not in the least want to know what the examinee calls colours, but only how he sees them, what colours appear to him to be alike and what appear to be unlike; and the only way of attaining this knowledge with certainty is to cause him to make matches between coloured objects, to put those together which appear to him to be essentially the same, and to separate those which appear to him to be essentially different. This principle of testing was first laid down by Seebeck, who required from examinees a complete arrangement of a large number of coloured objects; but it has been greatly simplified and improved by Prof. Holmgren, who pointed out that such a complete arrangement was superfluous, and that the only thing necessary was to cause the examinee to make matches to certain test colours, and, for this purpose, to select from materials which contained not only such matches, but also the colours which the colour-blind were liable to confuse with them.

After many trials, Holmgren finally selected skeins of Berlin wool as the material best suited for this purpose; and his set of wools comprises about 150 skeins [*shown*]. The advantages of his method over every other are that the wool is very cheap, very portable, and always to be obtained in every conceivable colour and shade. The skeins are not lustrous, so that light reflected from the surfaces does not interfere with the accuracy of the observation; and they are very easily picked up and manipulated, much more easily than coloured paper or coloured glass. The person to be tested is placed before a table in good daylight, the table is covered by a white cloth, and the skeins are thrown upon it in a loosely arranged heap. The examiner then selects a skein of pale green much diluted with white, and throws it down by itself to the left of the heap [*shown*]. The examinee is directed to look at this pattern skein and at the heap, and to pick out from the latter, and to place beside the pattern, as many skeins as he can find which are of the same colour. He is not to be particular about lighter or darker shades, and is not to compare narrowly, or to rummage much amongst the heap, but to select by his eyes, and to use his hands chiefly to change the position of the selected material.

In such circumstances, a person with normal colour-sight will select the greens rapidly and without hesitation, will select nothing else, and will select with a certain readiness and confidence easily recognised by an experienced examiner, and which may even be carried to the extent of neglecting the minute accuracy which a person who distrusts his own colour-sight will frequently endeavour to display. Some normal-sighted people will complete their selection by taking greens which incline to yellow, and greens which incline to blue, while others will reject both; but this is a difference depending sometimes upon imperfect colour education, sometimes upon the interpretation placed upon the directions of the examiner; for the person who so selects sees the green element in both the yellow-greens and the blue-greens, and is not colour-blind. The completely colour-blind, whether to red or to green, will proceed with almost as much speed and confidence as the colour-sighted; and will rapidly pick out a number of drabs, fawns, stone-colours, pinks, or yellows. Between the foregoing classes we meet with a few people who declare the imperfection of their colour-sense by the extreme care with which they select, by their slowness, by their hesitation, and by their desire to compare this or that skein with the pattern more narrowly than the conditions of the trial should permit. They may or may not ultimately add one or more of the confusion colours to the green, but they have a manifest tendency to do so, and a general uncertainty in their choice. One of the great advantages of Holmgren's method over every other is the way in which the examiner is able to judge, not only by the final choice of matches, but also by the manner in which the choice is made, by the action of the hands, and by the gestures and general deportment of the examinee.

When confusion colours have been selected, or when an unnatural slowness and hesitation have been shown in selecting, the examinee must be regarded as either completely or incompletely colour-blind. In order to determine which, and also to which colour he is defective, he is subjected to the second test. For this, the wool is mixed again, and the pattern this time is a skein of light purple—that is, of a mixture of red and violet much diluted with white [*shown*]. To match this, the colour-blind always selects deeper colours. If he puts only deeper purples, he is incompletely colour-blind. If he takes blue or violet, either with or without purple, he is completely red-blind. If he takes green or grey, or one alone, with or without purple, he is completely green-blind. If he takes red or orange, with or without purple, he is violet-blind. If there be any doubt, the examinee may be subjected to a third test, which is not necessary for the satisfaction of an expert, but which sometimes strengthens the proof in the eyes of a bystander. The pattern for this third test is a skein of bright red, to be used in the same way as the green and the purple [*shown*]. The red-blind selects for this dark greens and browns which are much darker than the pattern; while the green-blind selects greens and browns which are lighter than the pattern.

The method of examination thus described is, I believe, absolutely trustworthy. It requires no apparatus beyond the bundle of skeins of wool, no arrangements beyond a room with a good window and a table with a white cloth. In examining large numbers of men, they may be admitted into the room fifty or so at a time, may all receive their instructions together, and may then make their selections one by one, all not yet examined watching the actions of those who come up in their turn, and thus learning how to proceed. The time required for large numbers averages about a minute a person. I have heard and read of instances of colour-blind people who had passed the wool test satisfactory, and had afterwards been detected by other methods; but I confess that I do not believe in them. I do not believe that in such cases the wool test was applied properly, or in accordance with Holmgren's very precise instructions; and I know that it is often applied in a way which can lead to nothing but erroneous results. Railway foremen, for example, receive out of store a small collection of coloured wools selected on no principle, and they use it by pulling out a single thread, and by asking the examinee, "What colour do you call that?" Men of greater scientific pretensions than railway foremen have not always selected their pattern colours accurately, and have allowed those whom they examined, and passed, to make narrow comparisons between the skeins in all sorts of lights, in a way which should of itself have afforded sufficient evidence of defect.

Although, however, the expert may be fully satisfied by the wool test that the examinee is not capable of distinguishing with certainty between red and green flags or lights in all the circumstances in which they can be displayed, it may still remain for him to satisfy the employer who is not an expert, the railway manager, or the shipowner, and to convince him that the colour-blind person is unfit for certain kinds of employment. It may be equally necessary to convince other workmen that the examinee has been fairly and rightly dealt with. Both these objects may be easily attained, by the use of slight modifications of the lights which are employed. Lanterns for this special purpose were contrived, some years ago, by Holmgren himself, and by the late Prof. Donders, of Utrecht, and what are substantially their contrivances have been brought forward within the last few months as novelties, by gentlemen in this country who have re-invented them. The principle of all is the same—namely, that light of varying intensity may be displayed through apertures of varying magnitude, and through coloured glasses of varying tint, so as to imitate the appearances of signal lamps at different distances, and under different conditions of illumination, of weather, and of atmosphere. To the colour-blind, the difference between a red light and a green one is not a difference of colour, but of luminosity; the colour to which he is blind appearing the less luminous of the two. He may therefore be correct in his guess as to which of the two is exhibited on any given occasion, and he is by no means certain to mistake one for the other when they are exhibited in immediate succession. His liability to error is chiefly conspicuous when he sees one light only, and when the conditions which govern its luminosity depart in any degree from those to which he is most accustomed. With the lanterns of which I have spoken, it is always possible to deceive a colour-blind person by altering the luminosity of a light without altering its colour. This may be done by diminishing the light behind the glass, by increasing the thickness of the red or green glass, or by placing a piece of neutral tint, more or less dark, in front of either [*shown*]. The most incredulous employer may be convinced, by expedients of this kind, that the colour-blind are not to be relied upon for the safe control of ships or of locomotives. With regard to the whole question, there are many points of great interest, both physical and physiological, which are still more or less uncertain; but the practical elements have, I think, been well-nigh exhausted, and the means of securing safety are fully in the hands of those who choose to master and to employ them. The lanterns, in their various forms, are useful for the purpose of thoroughly exposing the colour-blind, and for bringing home the character of their incapacity to unskilled spectators; but they are both cumbrous and superfluous for the detection of the defect, which may be accomplished with far greater ease, and with equal certainty, by the wool test alone.

I have already mentioned that the examinations which have been conducted in the United States, thanks to the indefatigable labours of Dr. Joy Jeffries, have led to the discovery of an enormous and previously quite unsuspected amount of colour-ignorance, a condition which is frequently mistaken for colour-blindness by the methods of examination which are in favour with railway companies and with the Board of Trade; and this colour-ignorance has been justly regarded as a blot on the American system of national education. It has therefore, in some of the States, led to the adoption of systematic colour-teaching in the schools; and, for this purpose, Dr. Joy Jeffries has introduced this wall-chart and coloured cards [*shown*]. The children are taught, in the first instance, to match the colours in the chart with those of the cards distributed to them; and, when they are tolerably expert at matching, they are further taught the names of the colours. It must, nevertheless, always be remembered that a knowledge of names does not necessarily imply a knowledge of the things designated; and that colour-vision stands in no definite relation to colour-nomenclature. Even this system of teaching may leave a colour-blind pupil undetected.

[R. B. C.]

WEEKLY EVENING MEETING,

Friday, May 16, 1890.

EDWARD FRANKLAND, Esq. D.C.L. LL.D. F.R.S. Vice-President, in the Chair.

PROFESSOR RAPHAEL MELDOLA, F.R.S. *M.R.I.*

The Photographic Image.

THE history of a discovery which has been developed to such a remarkable degree of perfection as photography has naturally been a fruitful source of discussion among those who interest themselves in tracing the progress of science. It is only my presence in this lecture theatre, in which the first public discourse on photography was given by Thomas Wedgwood at the beginning of the century, that justifies my treading once again a path which has already been so thoroughly well beaten. If any further justification for trespassing upon the ground of the historian is needed, it will be found in the circumstance that in the autumn of last year there was held a celebration of what was generally regarded as the jubilee of the discovery. This celebration was considered by many to have reference to the public disclosure of the Daguerreotype process, made through the mouth of Arago to the French Academy of Sciences on August 10, 1839. There is no doubt that the introduction of this process marked a distinct epoch in the history of the art, and gave a great impetus to its subsequent development. But, while giving full recognition to the value of the discovery of Daguerre, we must not allow the work of his predecessors and contemporaries in the same field to sink into oblivion. After the lapse of half a century we are in a better position to consider fairly the influence of the work of different investigators upon modern photographic processes.

I have not the least desire on the present occasion to raise the ghosts of dead controversies. In fact, the history of the discovery of photography is one of those subjects which can be dealt with in various ways, according to the meaning assigned to the term. There is ample scope for the display of what Mr. Herbert Spencer calls the "bias of patriotism." If the word "photography" be interpreted literally as writing or inscribing by light, without any reference to the subsequent permanence of the inscription, then the person who first intentionally caused a design to be imprinted by light upon a photo-sensitive compound must be regarded as the first photographer. According to Dr. Eder, of Vienna, we must place this experiment to the credit of Johann Heinrich Schulze, the son of a German tailor, who was born in the Duchy of Madgeburg, in Prussia, in 1687, and who died in 1744, after a life of extraordinary activity as a linguist, theologian, physician, and philosopher. In the year 1727, when experimenting on the subject of phosphorescence, Schulze observed that by pouring nitric acid, in which some silver had previously been dissolved, on to chalk, the undissolved earthy residue had acquired the property of darkening on exposure to light. This effect was shown to be due to light, and not to heat. By pasting words cut out in paper on the side of the bottle containing his precipitate, Schulze obtained copies of the letters on the silvered chalk. The German philosopher certainly produced what might be called a temporary photogram. Whatever value is attached to this observation in the development of modern photography, it must be conceded that a considerable advance was made by spreading the sensitive compound over a surface instead of using it in mass. It is hardly necessary to remind you here that such an advance was made by Wedgwood and Davy in 1802.* The impressions produced by these last experimenters were, unfortunately, of no more permanence than those obtained by Schulze three-quarters of a century before them.

It will, perhaps, be safer for the historian of this art to restrict the term photograph to such impressions as are possessed of permanence: I do not, of course, mean absolute permanence, but ordinary durability in the common-sense acceptation of the term. From this point of view the first real photographs, i.e. permanent impressions of the camera picture, were obtained on bitumen films by Joseph Nicéphore Niepce, of Châlons-sur-Saône, who, after about twenty years' work at the subject, had perfected his discovery by 1826. Then came the days of silver salts again, when Daguerre, who commenced work in 1824, entered into a partnership with Niepce in 1829, which was brought to a termination by the death of the latter in 1833. The partnership was renewed between Daguerre and Niepce de St. Victor, nephew of the elder Niepce. The method of fixing the camera picture on a film of silver iodide on a silvered copper plate—the process justly associated with the name of Daguerre, was ripe for disclosure by 1838, and was actually made known in 1839.

The impartial historian of photography who examines critically into the evidence will find that, quite independently of the French pioneers, experiments on the use of silver salts had been going on in this country, and photographs, in the true sense, had been produced almost simultaneously with the announcement of the Daguerreotype process, by two Englishmen whose names are as household words in the ranks of science—I refer to William Henry Fox Talbot and Sir John Herschel. Fox Talbot commenced experimenting with silver salts on paper in 1834, and the following year he succeeded in imprinting the camera picture on paper coated with the chloride. In January 1839 some of his "photogenic drawings"—the first "silver prints" ever obtained—were exhibited in this Institution by Michael

* "An Account of a Method of Copying Paintings upon Glass, and of making Profiles by the Agency of Light upon Nitrate of Silver. Invented by T. Wedgwood, Esq. With Observations by H. Davy." 'Journ. R. I.' 1802, p. 170.

Faraday. In the same month he communicated his first paper on a photographic process to the Royal Society, and in the following month he read a second paper before the same society, giving the method of preparing the sensitive paper and of fixing the prints. The outcome of this work was the "Calotype" or Talbotype process, which was sufficiently perfected for portraiture by 1840, and which was fully described in a paper communicated to the Royal Society in 1841. The following year Fox Talbot received the Rumford Medal for his "discoveries and improvements in photography." *

Herschel's process consisted in coating a glass plate with silver chloride by subsidence. The details of the method, from Herschel's own notes, have been published by his son, Prof. Alexander Herschel.† By this means the old 40-foot telescope at Slough was photographed in 1839. By the kindness of Prof. Herschel, and with the sanction of the Science and Art Department, Herschel's original photographs have been sent here for your inspection. The process of coating a plate by allowing a precipitate to settle on it in a uniform film is, however, impracticable, and was not further developed by its illustrious discoverer. We must credit him, however, as being the first to use glass as a substratum. Herschel further discovered the important fact that while the chloride was very insensitive alone, its sensitiveness was greatly increased by washing it with a solution of silver nitrate. It is to Herschel, also, that we are indebted for the use of sodium thiosulphate as a fixing agent, as well as for many other discoveries in connection with photography, which are common matters of history.

Admitting the impracticability of the method of subsidence for producing a sensitive film, it is interesting to trace the subsequent development of the processes inaugurated about the year 1839. The first of photographic methods—the bitumen process of Niepce—survives at the present time, and is the basis of some of the most important of modern photo-mechanical printing processes. [Specimens illustrating photo-etching from Messrs. Waterlow and Sons exhibited.] The Daguerreotype process is now obsolete. As it left the hands of its inventor it was unsuited for portraiture, on account of the long exposure required. It is evident, moreover, that a picture on an opaque metallic plate is incapable of reproduction by printing through, so that in this respect the Talbotype possessed distinct advantages. This is one of the most important points in Fox Talbot's contributions to photography. He was the first to produce a transparent paper negative from which any number of positives could be obtained by printing through. The silver print of modern times is the lineal descendant of the Talbotype print. After forty

* For these and other details relating to Fox Talbot's work, necessarily excluded for want of time, I am indebted to his son, Mr. C. H. Talbot, of Lacock Abbey.

† 'Photog. Journ. and Trans. Photog. Soc.,' June 15, 1872.

years' use of glass as a substratum, we are going back to Fox Talbot's plan, and using thin flexible films—not exactly of paper, but of an allied substance, celluloid. [Specimens of Talbotypes, lent by Mr. Crookes, exhibited, with celluloid negatives by the Eastman Company.]

If I interpret this fragment of history correctly, the founders of modern photography are the three men whose labours have been briefly sketched. The jubilee of last autumn marked a culminating point in the work of Niepce and Daguerre, and of Fox Talbot. The names of these three pioneers must go down to posterity as co-equal in the annals of scientific discovery. [Portraits by Mr. H. M. Elder shown.] The lecture theatre of the Royal Institution offers such tempting opportunities to the chronicler of the history of this wonderful art that I must close this treatment of the subject by reminding myself that in selecting the present topic I had in view a statement of the case of modern photography from its scientific side only. There is hardly any invention associated with the present century which has rendered more splendid services in every department of science. The physicist and chemist, the astronomer and geographer, the physiologist, pathologist, and anthropologist will all bear witness to the value of photography. The very first scientific application of Wedgwood's process was made here by the illustrious Thomas Young, when he impressed Newton's rings on paper moistened with silver nitrate, as described in his Bakerian Lecture to the Royal Society on November 24, 1803. Prof. Dewar has just placed in my hands the identical slide, with the Newton rings still visible, which he believes Young to have used in this classic experiment. [Shown.]

Our modern photographic processes depend upon chemical changes wrought by light on films of certain sensitive compounds. Bitumen, under this influence, becomes insoluble in hydrocarbon oils, as in the heliographic process of the elder Niepce. Gelatine mixed with potassium dichromate becomes insoluble in water on exposure to light, a property utilised in the photo-etching process introduced in 1852 by Fox Talbot, some of whose original etchings have been placed at my disposal by Mr. Crookes. [Shown.] Chromatized gelatine now plays a most important part in the autotype and many photo-mechanical processes. The salts of iron in the ferric condition undergo reduction to the ferrous state under the influence of light in contact with oxidizable organic compounds. The use of these iron salts is another of Sir John Herschel's contributions to photography (1842), the modern "blue print" and the beautiful platinotype being dependent on the photo-reducibility of these compounds. [Cyanotype print developed with ferricyanide.]

Of all the substances known to chemistry at the present time, the salts of silver are by far the most important in photography, on account of the extraordinary degree of sensitiveness to which they can be raised. The photographic image, with which it is my privilege to deal on this occasion, is that invisible impression produced by the action of light on a film of a silver haloid. Many methods of producing such films have been in practical use since the foundation of the art in 1839. All these depend on the double decomposition between a soluble chloride, bromide, or iodide, and silver nitrate, resulting in the formation of the silver haloid in a vehicle of some kind, such as albumen (Niepce de St. Victor, 1848), or collodion on glass, as made practicable by Scott Archer in 1851. For twenty years this collodion process was in universal use; its history and details of manipulation, its development into a dry plate process by Colonel Russell in 1861, and into an emulsion process by Bolton and Sayce in 1864, are facts familiar to every one.

The photographic film of the present time is a gelatino-haloid (generally bromide) emulsion. If a solution of silver nitrate is added to a solution of potassium bromide and the mixture well shaken, the silver bromide coagulates, and rapidly subsides to the bottom of the liquid as a dense curdy precipitate. [Shown.] If instead of water we use a viscid medium, such as gelatine solution, the bromide does not settle down, but forms an emulsion, which becomes quite homogeneous on agitation. [Shown.] This operation, omitting all details of ripening, washing, &c., as well known to practical photographers, is the basis of all the recent photographic methods of obtaining negatives in the camera. The use of this invaluable vehicle, gelatine, was practically introduced by R. L. Maddox in 1871, previous experiments in the same direction having been made by Gaudin (1853–61). Such a gelatino-bromide emulsion can be spread uniformly over any substratum—glass, paper, gelatine, or celluloid—and when dry, gives a highly sensitive film.

The fundamental problem which fifty years' experience with silver haloid films has left in the hands of chemists is that of the nature of the chemical change which occurs when a ray of light falls on such a silver salt. Long before the days of photography—far back in the sixteenth century—Fabricius, the alchemist, noticed that native horn silver became coloured when brought from the mine and exposed. The fact presented itself to Robert Boyle in the seventeenth century, and to Beccarius, of Turin, in the eighteenth century. The change of colour undergone by the chloride was first shown to be associated with chemical decomposition in 1777, by Scheele, who proved that chlorine was given off when this salt darkened under water. I can show you this in a form which admits of its being seen by all. [Potassium iodide and starch paper were placed in a glass cell with silver chloride, and the arrangement exposed to the electric light till the paper had become blue.] The gas which is given off under these circumstances is either the free halogen or an oxide or acid of the halogen, according to the quantity of moisture present and the intensity of the light. I have found that the bromide affects the iodide and starch paper in the same way, but silver iodide does not give off any gas which colours the test paper. All the silver haloids become coloured on exposure to light, the change being most marked in the chloride, less in the bromide, and least of all in the iodide. The latter must be associated with some halogen absorbent to render the change visible. [Strips of paper coated with the pure haloids, the lower halves brushed over with silver nitrate solution, were exposed.] The different degrees of coloration in the three cases must not be considered as a measure of the relative sensitiveness: it simply means that the products of photo-chemical change in the three haloids are inherently possessed of different depths of colour.

From the fact that halogen in some form is given off, it follows that we are concerned with photo-chemical decomposition, and not with a physical change only. All the evidence is in favour of this view. Halogen absorbents, such as silver nitrate on the lower halves of the papers in the last experiment, organic matter, such as the gelatine in an emulsion, and reducing agents generally, all accelerate the change of colour. Oxidizing and halogenizing agents, such as mercuric chloride, potassium dichromate, &c., all retard the colour change. [Silver chloride paper, painted with stripes of solutions of sodium sulphite, mercuric chloride, and potassium dichromate, was exposed.] It is impossible to account for the action of these chemical agents except on the view of chemical decomposition. The ray of light falling upon a silver haloid must be regarded as doing chemical work; the vibratory energy is partly spent in doing the work of chemical separation, and the light passes through a film of such haloid partly robbed of its power of doing similar work upon a second film. It is difficult to demonstrate this satisfactorily in the lecture-room, on account of the opacity of the silver haloids, but the work of Sir John Herschel, J. W. Draper, and others, has put it beyond doubt that there is a relationship of this kind between absorption and decomposition. It is well known, also, that the more refrangible rays are the most active in promoting the decomposition in the case of the silver haloids. This was first proved for the chloride by Scheele, and is now known to be true for the other haloids. It would be presumption on my part, in the presence of Captain Abney, to enlarge upon the effects of the different spectral colours on these haloids, as this is a subject upon which he can speak with the authority of an investigator. It only remains to add that the old idea of a special "actinic" force at the more refrangible end of the spectrum has long been abandoned. It is only because the silver haloids absorb these particular rays that the blue end of the spectrum is most active in promoting their decomposition. Many other instances of photo-chemical decomposition are known in which the less refrangible rays are the most active, and it is possible to modify the silver haloids themselves so as to make them sensitive for the red end of the spectrum.

The chemical nature of the coloured products of photo-chemical decomposition is still enshrouded in mystery. Beyond the fact that they contain less halogen than the normal salt, we are not much in advance of the knowledge bequeathed to us by Scheele in the last century. The problem has been attacked by chemists again and again, but its solution presents extraordinary difficulties. These products are never formed—even under the most favourable conditions of division and with prolonged periods of exposure—in quantities beyond what the chemists would call "a mere trace." Their existence appears to be determined by the great excess of unaltered haloid with which they are combined. Were I to give free rein to the imagination, I might set up the hypothesis that the element silver is really a compound body invariably containing a minute percentage of some other element, which resembles the compound which we now call silver in all its chemical reactions, but alone is sensitive to light. I offer this suggestion for the consideration of the speculative chemist.* For the coloured product as a whole, i.e. the product of photo-decomposition with its combined unchanged haloid, Carey Lea has proposed the convenient term "photosalt." It will avoid circumlocution if we adopt this name. The photosalts have been thought at various times to contain metallic silver, allotropic silver, a subhaloid, such as argentous chloride, &c., or an oxyhaloid. The free metal theory is disposed of by the fact that silver chloride darkens under nitric acid of sufficient strength to dissolve the metal freely. The acid certainly retards the formation of the photosalt, but does not prevent it altogether. When once formed the photo-chloride is but slowly attacked by boiling dilute nitric acid, and from the dry photosalt mercury extracts no silver. The assumption of the existence of an allotropic form of silver insoluble in nitric acid cannot be seriously maintained. The sub-haloid theory of the product may be true, but it has not yet been established with that precision which the chemist has a right to demand. We must have analyses giving not only the percentage of halogen, but also the percentage of silver, in order that it may be ascertained whether the photosalt contains anything besides metal and halogen. The same may be said of the oxyhaloid theory: it may be true, but it has not been demonstrated.

The oxyhaloid theory was first suggested by Robert Hunt † for the chloride; it was taken up by Sahler, and has recently been revived by Dr. W. R. Hodgkinson. It has been thought that this theory is disposed of by the fact that the chloride darkens under liquids, such as hydrocarbons, which are free from oxygen. I have been repeating some of these experiments with various liquids, using every possible precaution to exclude oxygen and moisture; dry silver chloride heated to incipient fusion has been sealed up in tubes in dry benzene,

* I have gone so far as to test this idea experimentally in a preliminary way, the result being, as might have been anticipated, negative. Silver chloride, well darkened by long exposure, was extracted with a hot saturated solution of potassium chloride, and the dissolved portion, after precipitation by water, compared with the ordinary chloride by exposure to light. Not the slightest difference was observable either in the rate of coloration or in the colours of the products. Perhaps it may be thought worth while to repeat the experiment, using a method analogous to the "method of fractionation" of Crookes.

† 'Researches on Light,' 2nd ed. 1854, p. 80.

petroleum, and carbon tetrachloride and exposed since March. [Tubes shown.] In all cases the chloride has darkened. The salt darkens, moreover, in a Crookesian vacuum.* By these experiments the oxychloride theory may be scotched, but it is not yet killed; the question now presents itself, whether the composition of the photosalt may not vary according to the medium in which it is generated. Analogy sanctions the supposition that when the haloid darkens under water or other oxygen-containing liquid, or even in contact with moist or dry air, that an oxychloride may be formed, and enter into the composition of the photosalt. The analogy is supplied by the corresponding salt of copper, viz. cuprous chloride, which darkens rapidly on exposure. [Design printed on flat cell filled with cuprous chloride by exposure to electric light.] Wöhler conjectured that the darkened product was an oxychloride, and this view receives a certain amount of indirect support from these tubes [shown], in which dry cuprous chloride has been sealed up in benzene and carbon tetrachloride since March; and although exposed in a southern window during the whole of that time, the salt is as white as when first prepared. Some cuprous chloride sealed up in water, and exposed for the same time, is now almost black. [Shown.]

When silver is precipitated by reduction in a finely divided state in the presence of the haloid, and the product treated with acids, the excess of silver is removed and coloured products are left which are somewhat analogous to the photosalts proper. These coloured haloids are also termed by Carey Lea photosalts because they present many analogies with the coloured products of photo-chemical change. Whether they are identical in composition it is not yet possible to decide, as we have no complete analyses. The first observations in this direction were published more than thirty years ago in a report by a British Association Committee,† in which the red and chocolate-coloured chlorides are distinctly described. Carey Lea has since contributed largely to our knowledge of these coloured haloids, and has at least made it appear highly probable that they are related to

* Some dry silver chloride which Mr. Crookes has been good enough to seal up for me in a high vacuum, darkens on exposure quite as rapidly as the dry salt in air. It soon regains its original colour when kept in the dark. It behaves, in fact, just as the chloride is known to behave when sealed up in chlorine, although its colour is of course much more intense after exposure than is the case with the chloride in chlorine. The tube in which the chloride had been sealed up in benzene, gave off a considerable quantity of hydrogen chloride on breaking the point in June.

† These results were arrived at in three ways. In one case hydrogen was passed through silver citrate suspended in hot water, and the product extracted with citric acid. "The result of treating the residue with chlorhydric acid, and then dissolving the silver by dilute nitric acid, was a rose-tinted chloride of silver." In another experiment the dry citrate was heated in a stream of hydrogen at 212° F., and the product, which was partly soluble in water, gave a brown residue, which furnished "a very pale red body on being transformed by chlorhydric and nitric acids." In another experiment silver arsenite was formed, this being treated with caustic soda, and the black precipitate then treated successively with

the products formed by the action of light. [Red photo-chloride and purple photobromide and iodide shown.]

The photographic image is impressed on a modern film in an inappreciable fraction of a second, whereas the photosalt requires an appreciable time for its production. The image is invisible simply because of the extremely minute quantity of haloid decomposed. In the present state of knowledge it cannot be asserted that the material composing this image is identical in composition with the photosalt, for we know the composition of neither the one nor the other. But they are analogous in so far as they are both the result of photochemical decomposition, and there is great probability that they are closely related, if not identical, chemically. It may turn out that there are various kinds of invisible images, according to the vehicle or halogen absorbent—in other words, according to the sensitiser with which the silver haloid is associated. The invisible image is revealed by the action of the developer, into the function of which I do not propose to enter. It will suffice to say that the final result of the developing solution is to magnify the deposit of photosalt by accumulating metallic silver thereon by accretion or reduction. Owing to the circumstance that the image is impressed with such remarkable rapidity, and that it is invisible when formed, it has been maintained, and is still held by many, that the first action of light on the film is molecular or physical, and not chemical. The arguments in favour of the chemical theory appear to me to be tolerably conclusive, and I will venture to submit a few of them.

The action of reagents upon the photographic film is quite similar to the action of the same reagents upon the silver haloids when exposed to the point of visible coloration. Reducing agents and halogen absorbents increase the sensitiveness of the film: oxidising and halogenising agents destroy its sensitiveness. It is difficult to see on the physical theory why it should not be possible to impress an image on a film, say of pure silver bromide, as readily as on a film of the same haloid embedded in gelatine. Every one knows that this cannot be done. I have myself been surprised at the extreme insensitiveness of films of pure bromide prepared by exposing films of silver deposited on glass to the action of bromine vapour. On the chemical theory we know that gelatine is a splendid sensitiser—i. e. bromine absorbent. There is another proof which has been in our hands for nearly thirty years, but I do not think it has been viewed in this light before. It has been shown by Carey Lea, Eder, and especially by Abney—who has investigated the matter most

chlorhydric and nitric acids: "Silver is dissolved, and there is left a substance . . . . [of] a rich chocolate or maroon, &c." This on analysis was found to contain 24 per cent. of chlorine, the normal chloride requiring 24·74 and the subchloride 14·08 per cent. The committee which conducted these experiments consisted of Messrs. Maskelyne, Hadow, Hardwich, and Llewelyn. 'B. A. Rep.,' 1859, p. 103.

thoroughly—that a shearing stress applied mechanically to a sensitive film leaves an impression which can be developed in just the same way as though it had been produced by the action of light. [Pressure marks on Eastman bromide paper developed by ferrous oxalate.] Now that result cannot be produced on a surface of the pure haloid; some halogen absorbent, such as gelatine, must be associated with the haloid. We are concerned here with a chemical change of that class so ably investigated by Prof. Spring, of Liège, who has shown that by mere mechanical pressure it is possible to bring about chemical reaction between mixtures of finely divided solids.* Then again, mild reducing agents, too feeble to reduce the silver haloids directly to the metallic state, such as alkaline hypophosphites, glucose or lactose, and alkali, &c., form invisible images which can be developed in precisely the same way as the photographic image. All this looks like chemical change, and not physical modification pure and simple.

I have in this discourse stoically resisted the tempting opportunities for pictorial display which the subject affords. My aim has been to summarise the position in which we find ourselves with respect to the invisible image after fifty years' practice of the art. This image is, I venture to think, the property of the chemist, and by him must the scientific foundation of photography be laid. We may not be able to give the formula of the photosalt, but if the solution of the problem has hitherto eluded our grasp it is because of the intrinsic difficulties of the investigation. The photographic image brings us face to face—not with an ordinary, but with an extraordinary class of chemical changes due entirely to the peculiar character of the silver salts. The material composing the image is not of that definite nature with which modern chemical methods are in the habit of dealing. The stability of the photosalt is determined by some kind of combination between the sub-haloid or oxy-haloid, or whatever it may be, and the excess of unaltered haloid which enters into its composition. The formation of the coloured product presents certain analogies with the formation of a saturated solution; the product of photo-chemical decomposition is formed under the influence of light up to a certain percentage of the whole photosalt, beyond which it cannot be increased—in other words the silver haloid is saturated by a very minute percentage of its own product of photo-decomposition. The photosalt belongs to a domain of chemistry—a no-man's land—peopled by so-called "molecular compounds," into which the pure chemist ventures but timidly. But these compounds are more and more urging their claims for consideration, and sooner or later they will have to be reckoned with, even if

---

* The connection between the two phenomena was suggested during a course of lectures delivered by me two years ago ('Chemistry of Photography,' p. 191). I have since learnt that the same conclusion had been arrived at independently, by Mr. C. H. Bothamley, of the Yorkshire College, Leeds.

they lack that definiteness which the modern chemist regards as the essential criterion of chemical individuality. The investigation may lead to the recognition of a new order of chemical attraction, or of the old chemical attraction in a different degree. The chemist who discourses here upon this subject at the end of the half-century of photography into which we have now entered, will no doubt know more about this aspect of chemical affinity; and if I may invoke the spirit of prophecy in concluding, I should say that a study of the photographic film with its invisible image will have contributed materially to its advancement.

[R. M.]

WEEKLY EVENING MEETING,

Friday, May 23, 1890.

The Right Hon. Earl Percy, F.S.A. Vice-President, in the Chair.

Alfred C. Haddon, Esq. M.A. M.R.I.A.

PROFESSOR OF ZOOLOGY IN THE ROYAL COLLEGE OF SCIENCE, DUBLIN.

Manners and Customs of the Torres Straits Islanders.

It is not my intention this evening to attempt a special study of any particular institution or series of customs, nor even to discuss the ethnological affinities of the natives inhabiting the islands of Torres Straits.

The comparative study of institutions and customs has led to brilliant suggestions, and has especially thrown light upon obscure facts in our own culture, and given a new significance to observances which, because they are of every-day occurrence, are passed by without comment. This field of inquiry is one which has only recently been systematically tilled, but it promises a rich harvest of unexpected results.

The detailed study of a single tribe or natural assemblage of people has great interest, as it puts one in touch with such varied subjects as the physical, mental, and moral characters of the people; and the tracing out of their affinities requires wide study and careful comparisons. A patient research of this kind always opens up questions of wider import than the initial inquiry.

Neither of these methods will occupy us to-night, as I wish to present before you as vivid a conception as I can of some of the manners and customs of a people small in number but rich in interest. We will consider, therefore, neither a composite image of savages in general, nor of rude customs, but the particular habits of a disappearing people, who, thirty years ago, were naked, unknown savages, who to-day are British subjects, and who in a very few years will have lost the last remnants of their individuality, and possibly ere long will practically cease to exist—at all events as a distinct people. The dissolving views which I shall exhibit this evening are a fit emblem of the facts which they illustrate.

My anthropological inquiries in Torres Straits may not inaptly be compared with the methods of the palæontologist, especially in his study of the more recent fossils. Amongst such fossils we find some representatives of existing forms, others slightly different from those we are accustomed to, others again which are quite dissimilar, and often of these only disconnected fragments may remain, and it takes great patience and careful piecing together to restore the latter into any semblance of their former selves; nor should surprise be felt if mistakes are occasionally made in the attempt.

A similar experience occurs to those who study an isolated people which is rapidly becoming modified and is dying out at the same time. Some facts collected from legend and myth precisely resemble the present habits of the natives; others have only lately fallen into desuetude. Lastly, some customs are so dissimilar from anything in our own country, that it is difficult to thoroughly understand them under favourable circumstances; but when these customs are no longer practised, and but imperfectly remembered, when they have to be described through the unsatisfactory medium of Jargon English, and when one bears in mind the great difference in the mental conceptions of narrator and listener, what wonder is there that disconnected narratives are recorded, or that errors creep in?

Happy is that traveller who has the opportunity of studying existing habits. It was my lot to recover recently lost or fast dying-out customs; our archæologists grapple with the problems of the past; it is the object of all to assist towards a complete History of Man.

Torres Straits, as you are aware, separate New Guinea, the largest island in the world, from Australia, the smallest continent. Although the Straits are eighty miles wide in their narrowest part, yet, owing to the presence of islands and of numerous and often extensive coral reefs, there is only one channel suitable for ocean-going steamers, and that averages a mile in width, and in places is much less.

The islands in Torres Straits may be divided into three geological groups by the lines of longitude 142° 48′ E. and 143° 30′ E.

The islands to the west are composed of old igneous rocks, and are surrounded by fringing reefs. These islands may in fact be regarded as disconnected portions of Northern Queensland. They are fertile, but there is no particularly luxuriant vegetation; doubtless irrigation and cultivation would greatly improve their productiveness.

The central group of islands is composed of low coral islets formed by wind and wave action; the soil is poor, and supports only a scrubby vegetation. Coco-palms grow on some of the islands, and there are occasional mangrove swamps.

The eastern islands, Uga, Erub, and the Murray Islands, are of volcanic origin, and are also fringed with coral reefs. In these the soil is rich and vegetation luxuriant, Uga and a great part of Mer being simply large gardens of coco-palms, bananas, and yams.

It is interesting to find that the inhabitants of the volcanic islands form one tribe, which I term the Eastern Tribe; the Western Tribe occupying all the remaining islands. The customs of the two tribes are different and their languages distinct, so much so that there are only a few words in common, and these are mainly trade words. Four subdivisions of the Western Tribe can be distinguished, the members of each of which inhabit certain intermarrying groups of islands.

Independently of the above-mentioned subdivisions, the islanders were divided into clans, each clan having some animal for its *augŭd* or "totem." For example, in the Western Tribe there were the dugong, turtle, dog, cassowary, snake, shark-clans, and so forth. There was supposed to be some relation between the clans and their respective *augŭd*, "all same [i.e. similar to] family," as it was expressed to me. A dog-man, for instance, was credited with understanding the habits and feelings of dogs, or a cassowary-man prided himself on having thin shanks like a cassowary, which would enable him to run quickly through the grass. With the exception of the first two clans, no one was allowed to kill or eat the totem of his own clan; if he did, his other clansmen would probably kill him for sacrilege. On a dugong expedition, no dugong-man might keep the first dugong he captured, but he might partake of the rest; the same restriction applied also to the turtle and the turtle-clan. If only one dugong or turtle was caught on the first day, the dugong- or turtle-man had to relinquish it; supposing only one was caught on the succeeding day, the account was, so to speak, "carried forward," and there was no *sabi* ("tabu") on it. The dugong and turtle were too important articles of food for the clan members to be entirely deprived from partaking of their *augŭd*.

The women, or at all events some of them, used to have a representation of their *augŭd* cut on the small of the back. I made inquiries on this point on most of the islands in the Straits, but could only find four old women who had them; these I sketched, and two of them I also photographed.

[Various photographs illustrating the appearance of the natives were then thrown on the screen.]

I have alluded to the fact that different customs characterise the Eastern and Western Tribes; as an example of this I may mention that in the latter tribe the girls proposed marriage to the men, while in the Eastern Tribe the more usual course was adopted.

It might be some time before a lad had an offer; but should he be a fine dancer, with goodly calves, and dance with sprightliness and energy at the festive dances, he would not lack admirers.

Should there still be a reticence on the part of his female acquaintances, the young man might win the heart of a girl by robbing a man of his head. Our adventurous youth could join in some foray; it mattered not to him what was the equity of the quarrel, or whether there was any enmity at all between his people and the attacked. So long as he killed some one—man, woman, or child—and brought the head back, it was not of much consequence to him whose head it was. Possibly a man killed would redound to his greater glory, but any skull was better than none, and its possession was recognised as an order of merit. How much more distinction would a man gain when he could boast of a whole trophy of skulls!

The girl's heart being won by prowess, dancing skill, or fine appearance, she would plait a string armlet, *tiapururu;* this she intrusted to a mutual friend, preferably the chosen one's sister. On the first suitable opportunity, the sister said to her brother, "Brother, I have some good news for you. A woman likes you." On hearing her name, and after some conversation, if he was willing to go on with the affair, he told his sister to ask the girl to keep some appointment with him in the bush.

When the message was delivered, the enamoured damsel informed her parents that she was going into the bush to get some wood or food, or made some such excuse.

In due course the couple met, sat down and talked, the proposal being made with perfect decorum.

The following conversation is given in the actual words used by my informant, Maino, the chief of Tud.

Opening the conversation, the man said, "You like me proper?"

"Yes," she replied, "I like you proper with my heart inside. Eye along my heart see you—you my man."

Unwilling to rashly give himself away, he asked, "How you like me?"

"I like your fine leg—you got fine body—your skin good—I like you altogether," replied the girl.

After matters had proceeded satisfactorily, the girl, anxious to clench the matter, asked when they were to be married. The man said, "To-morrow, if you like."

They both went home and told their respective relatives. Then the girl's people fought the man's folk, "For girl more big [i.e. of more consequence] than boy;" but the fighting was not of a serious character, it being part of the programme of a marriage.

"Swapping" sisters was the usual method of getting a wife. If a man had no sisters he might remain unmarried, unless he was rich enough to pay for a wife with a shell armlet (*waiwi*), or a canoe, or something of equal value. If a youth was "hard up," an uncle might take compassion on him and give one of his own daughters in exchange for a wife for his nephew.

This exchange of girls—a sister for a sister, or female cousin for another man's sister—was an economical method of getting a wife, as one was a set off against the other. The usual feasting occurred, but the presents were dispensed with, or at all events the purchase money was saved, and probably there would be no fighting.

When a young man of the Eastern Tribe arrived at an understanding with a girl, he put his *gelar* ("law" i.e. "tabu") on her, and made arrangements to fetch her away. She kept awake on the appointed night, listening for the preconcerted signal, and they quietly stole away to his parents' house, and the next morning he sent a messenger to say where the girl was. The girl's friends armed themselves with bows and arrows, shark's teeth fastened on to sticks, and other weapons, and proceeded to the other village, but the fight was not a serious affair. On the same day the girl would be painted red by her future mother-in-law, and clothed with a large number of leaf petticoats; and numerous ornaments would be suspended on her back, these making a clanking sound whenever the girl moved. For some months she remained in the house, and under the constant supervision of her future mother-in-law, the young man residing elsewhere. After say three months, negociations would commence between the two families, and the girl's relations would come to *taaugwat* (or scrape hands), and presents would be exchanged, and some alteration made in the decking of the girl. After a further probation period of a few months, some friend, in the secret, would engage the young man in conversation, and the bride would steal up behind him with some food she had previously cooked, and, while still behind his back, would thrust it by his side. He, looking round, exclaimed, "Why, that's my woman!" and then hung down his head in shame. Being informed that all was duly performed according to old usage, the couple ate food together, this being the ratification of the contract.

It appears that in the Eastern Tribe marriage was regarded as a state of "tabu," the man isolating one woman as his exclusive property, for he had powers of life and death over his wife. For several reasons I suspect that the Eastern Tribe has arrived at a slightly higher stage in the evolution of the family than the Western, as the man has a more independent position, and does not live more or less with his wife's people after marriage, as is the custom among the Western Tribe. In both tribes a wife had to be paid for; a canoe, dugong-harpoon, shell-armlet, or articles of equal exchange value, being the usual price.

Manhood is with us a gradual development of youth; with nearly all savages it is a state of privilege, the full advantages of which can be gained only by the observance of special ceremonies.

The growth of hair on the face warned the father that his boy was growing up, and he consulted with other fathers who had sons of about the same age.

"Good thing," he might have remarked; "boy no stop along woman now: he got hair, time we make him man now;" and arrangements would be duly made.

The following information, respecting the former initiation ceremonies, was gained at Tud (usually known as Warrior Island), the natives of which island were probably the most warlike of all the Western Islanders:—

The lads were handed over to their uncles, or to some old man, by their fathers, who then ceased to have any intercourse with them. They were conducted to the *Taiiokwod*, or open space sacred to the men, where no woman or child ever ventured, and which henceforth had for them many deep-rooted associations. The uncles washed the youths with water and then rubbed charcoal into the skin; this being daily repeated till the probation period was over. The lads were covered with mats doubled up like a tent with closed ends, and there they sat the livelong day in groups, without moving, playing, or even speaking. Four large mats stretched across the *Taiiokwod*, the mats belonging respectively to the *Sam* (cassowary), *Umai* (dog), *Kodal* (crocodile), and *Baidam* (shark) clans. For each mat there was a fireplace, the fire being tended by the young men of their respective clans. The old men sat on their appropriate mats, in the centre were the drums, and the dance-masks were placed along one side. Opposite the centre was a small mat, on which sat the chief of the island; for, contrary to the general custom of the tribe, this island had a recognised chief, the result, probably, of their belligerent habits. By the side of where the chief used to sit, a large ovoid stone was pointed out to me; it had a dire significance, for long ago four boys, tired of the irksomeness of the discipline, broke bounds, and meeting their mothers in the bush, asked for food. They were recaptured and were all killed by the old men with that stone, which was then placed in its present position, as a warning to other youths. The boys of the cassowary and dog clans sat at the end beyond the shark-fireplace, and the crocodile and shark boys were placed at the opposite end of the clearing.

Their instructors watched the lads, and communicated to them the traditions of the tribe, rules of conduct were laid down, information in all branches of native lore taught, and thus, generation after generation, the things of the fathers were transmitted to the sons.

The following are some of the rules which I was informed were imparted to the youths by the "old men":—

"You no steal."

"If you see food belong another man, you no take it, or you dead."

"You no take thing belong another man without leave; if you see a fish-spear and take it, s'pose you break it and you no got spear, how you pay man?"

"S'pose you see a dugong-harpoon in a canoe and take it, and man he no savvy, then you lose it or break it, how you pay him? You no got dugong-harpoon."

"You no play with boy and girl now; you a man now, and no boy."

"You no play with small play-canoe, or with toy-spear; that all finish now."

"You no like girl first; if you do, girl laugh at you and call you a woman." [That is, the young man must not propose marriage to a girl, but must wait for her to ask first.]

"You no marry the sister of your mate, or by and by you will be ashamed; mates all same as brothers." [But "mates" may marry two sisters.]

"You no marry your cousin; she all same as sister."

"If any one asks for food, or water, or anything, you give something; if you have a little, you give a little; if you have plenty, give half.

"Look after your mother and father; never mind if you and your wife go without."

"Don't speak bad word to mother."

"Give half of all your fish to your parents; don't be mean."

"Father and mother all along same as food in belly; when they die you feel hungry and empty."

"Mind your uncles, too, and cousins."

"If woman walk along, you no follow; by and by man look, he call you bad name."

"If a canoe is going to another place, you go in canoe; no stop behind to steal woman."

"If your brother is going out to fight, you help him; don't let him go first, but go together."

Who will say, after this, that the Torres Straits Islanders were degraded savages?

At length the month of isolation expired, and for the last time the uncle washed the lad; he then rubbed him with scented leaves, and polished him up with oil. Then he was decorated with armlets and leglets, breast-ornaments, and possibly a belt, his ears ornamented, and a shell-skewer passed through his nose; bright-coloured leaves would be inserted in his armlets, and his hair rolled into the approved string-like ringlets. So they "make him flash—flash like hell—that boy."

The afternoon of the eventful day was occupied in this congenial task, and at nightfall all the lads who were being initiated were marshalled by their uncles behind a large mat, which was held vertically. In this wise they marched to the village until they arrived at an open space where a mat was spread on the ground before a semicircle of friends and relatives. When the approaching party reached this mat the lads seated themselves upon it, and then the screening mat was lowered. Suddenly, for the first time for a month, the fathers and female relatives saw the boys, and great were the crying and shouting and exclamations of delight at the brave show. With tears the mothers cried out, "My boy! my boy!" and they and other elderly female relatives rushed up to them and fondled and caressed them, and the mothers surreptitiously put dainty morsels by their boys.

Sitting with legs crossed under them and down-turned faces, the boys neither moved nor exhibited the least emotion, for now they were men.

Less precise is my information respecting the corresponding rites of the Eastern Tribe. So far as I could gather, there were in Mer, the largest of the Murray Islands, two important ceremonies, which we may term the initiation and the recognition ceremonies. For the first the lads were assembled near a sacred round house, or *pelak*, in which the awe-inspiring masks were kept. The ceremony was conducted by three *zogole*, or sacred men, and their *tămileb*, or attendants. The latter arranged themselves in a double row, from the *pelak* to the place where the boys were assembled, and, holding long sticks, performed certain movements. Slowly the dread apparition advanced; the chief *zogole* came first, wearing a huge mask with human features and a beard of jaw-bones; the second *zogole* steadied this mask with a rope; the third *zogole* wore a long mask, shaped like a shark. Then for the first time the names of these masks were revealed to the lads —BOMAI and MALU. These were the sacred names which it was not lawful to communicate to the outsider, death to both being the penalty. Their collective name of *Agud* was, however, known to all.

I can only allude to the customary food-offering presented to the *zogole*, and the course of instruction instilled into the youths, one item of which was the narration of the legend of Malu, and must pass on to the recognition ceremony. This function took place in the afternoon on the sand beach outside the village of Las. A great concourse of people was assembled—men, women, and children—the newly initiated lads occupying the front row.

First four men of the dog-clan played about in pairs. (I may here parenthetically remark that it took me a fortnight's work to glean what little information I have respecting these two ceremonies. On one occasion I induced a number of men to rehearse some of the dances for me on the actual spot where they were originally performed, in order that I might gain a clear comprehension of them. One of my photographic "studies" I now throw on the screen.) The dog-men were followed by pigeon-men, who danced and beat their chests; later, whirling along the strand, came a body of dancers, circling from left to right as they advanced, an outer ring with sticks, an inner ring brandishing stone clubs, and possibly some drum-players in the centre. Lastly, the three *zogole* appeared, completely covered with white feathers, and each carrying five wands. Although seen by the women, their identity was supposed to be unknown.

This was the final function, and was followed by the ever-recurring feast. Thenceforth the lads took standing as men.

Strangely enough, at neither Tud nor Mer could I discover that the bull-roarer was employed at these ceremonies. The wide-spread use and sacred character of this simple instrument has been emphasised by Mr. Lang in one of his charming essays. Knowing its universal distribution in Australia, I was not surprised to find that in Muralug, or Prince of Wales' Island, which lies close to Cape York, its use was associated with the initiation of the lads. It was only by speaking in a low voice to the chief of the island and his son Georgie, whose photograph you have already seen, and by assuming more knowledge than I actually possessed, that I could induce them to admit of its being employed. Cautiously looking round to see that no one was near, its name, *wanĕs*, was whispered to me. After much persuasion, a model of one was made for me, on the express understanding that I should not show it to any woman on the island;

and I did not. It is now in the British Museum. All that I could gather was that it was whirled in the bush and then shown to the lads. Death was the penalty to both if a man exhibited it to a woman, or to any one who had not been initiated.

Great was my surprise when, shortly afterwards, I saw the Saibai boys who were staying at the mission station on Mer, playing with bull-roarers identical with the one with which I had been so secretly entrusted. The most sacred emblem in one island was a toy in another. In case some of you may not be acquainted with this most interesting implement, I have brought one of these bull-roarers.

From these important initiation ceremonies we may pass to others which had a less sacred significance. All the native ceremonies were associated with processions, or with movements of a less regular character, the performers of which were invariably specially dressed for the occasion—usually there was a special costume for a particular rite, one distinguishing feature of which was the wearing of masks or head-dresses. It is convenient to describe these functions as dances; and a series can be traced extending from the most sacred initiation and funeral dances on the one hand, through the seasonal dances to the war and ordinary festive dances on the other.

Profanation of the initiation or of the funeral ceremonies was punished with immediate death. In some instances, at all events, dance-masks could only be worn at the appropriate festival; even the casual putting on of one was supposed to cause slow but certain death. It was my good fortune to witness a seasonal dance at Thursday Island. This was anticipatory of the fishing season during the north-west monsoon.

The men were clothed with a petticoat made of the shredded sprouting leaves of the coco-palm, and adorned with various armlets and leglets; but the striking part of the costume was the mask, of which the lower portion represented a conventional crocodile's head, surmounted by a human face; above this was a representation of a saw-fish, some five feet in length, and overtopping all was a long red triangular erection decked with feathers. The ceremony was called the *Waiitutu kap*, or "Saw-fish dance." The actual dance consisted of two men at a time coming out from behind a screen and going through their simple evolutions to the monotonous accompaniment of the drum and a lugubrious chant.

More varied was the costume of the secular dance. All their bravery was donned. The effective head-dress of egret's feathers, or the cassowary coronet, framed the face, a shell skewer pierced the nose, breast ornaments, coco-palm leaf petticoats, armlets, leglets, ornaments or implements carried in the hand, all went to make up a picture of savage finery. Here, too, the women were occasionally allowed to participate, though of course both sexes never danced together. When women were allowed to be present at the more important dances, they were merely spectators.

The large canoes of the Torres Straits Islander of former times must have been very imposing objects when painted with red, white, and black, and decorated with white shells, black feathers, and flying streamers; and not less so when actively paddled by a noisy, gesticulating, naked crew, adorned with cassowary coronets, shell ornaments, and other native finery; or swiftly sailing, scudding before the wind with mat sails erect.

The body of a canoe is a simple dug-out, on to the sides of which gunwale boards are lashed. There is a central platform supported on a double outrigger. The thwart poles of the outriggers are usually six feet apart, and extend to some ten feet beyond the stem of the canoe; a doubly-pointed float is attached to the ends of the thwart poles on each side. Receptacles are built into each side of the platform for the storage of bows and arrows, fishing gear, water-bottles, and other belongings.

The sails are two in number, and are oblong erections of matting placed in the bows, some twelve feet in height, and each about five feet wide. The mats are skewered on to two long bamboos, which support the sails along their length; a bamboo stay also serves to keep the sail upright.

The longest canoe I measured was nearly sixty-eight feet in length. A stone lashed on to a rope is kept in the bow for an anchor. When sailing, a man stands in the stern holding the steering board.

The canoes are made at the mouth of the Fly River, in New Guinea, and are fitted with but a single outrigger, as theirs is only river navigation. I was informed that it was at Saibai that the canoes were refitted, this time with two outriggers, and an attempt at decoration was made, but the latter having a purely commercial significance was rather scant. The ultimate purchasers ornamented their canoes according to their fancy, as they usually prided themselves on having fine canoes.

I was much puzzled when I first went to Torres Straits by occasionally seeing a canoe with a single outrigger. I afterwards found it belonged to a native of Ware (one of the New Hebrides) residing at Mabuiag, and that he had re-outrigged a native canoe according to the fashion of his own people. When I was staying at Mabuiag some natives of that island were fitting up a canoe in imitation of this one. Here a foreign custom is being copied; how far it will spread among the Western Tribe it is impossible to say; but, strangely enough, the Eastern Tribe has entirely adopted an introduced fashion, and I did not see a solitary canoe with a double outrigger. It would be tedious to enter into a comparison between these various canoes. In the Eastern Islands the platform baskets are absent, and Europern sails are in universal use—mainsail, foresail, and jib. Among the Western Tribe, European sails have not yet quite supplanted the original mat sails. Throughout the Straits the canoes are not decorated in the old style. It was in Mabuiag alone that I found two canoes which were more or less decorated. Utilitarian ideas are now too widely spread for the æsthetic faculty to be indulged in.

I have dwelt at some length on this subject, as it is important to record all transitions. As an example of how rapidly and completely some changes occasionally come about, I may mention that at Mer, one of the Eastern Islands, some, at all events, of the young men did not appear to know that there had been a change in the rig of their canoes.

But, after all, the most interesting feature in connection with the canoes is the method by which they are purchased. I have previously mentioned that they were made on the mainland of New Guinea on the banks of the Fly River. Supposing a native of Muralug (Prince of Wales' Island, the island which is nearest to Cape York), wants a canoe. He sends word, say, to a relation of his in Moa, for the inhabitants of these two islands often intermarry. The latter sends a message to the next island of Badu. A Badu man passes on the word to Mabuiag (these two also were intermarrying islands); the Mabuiag native informs a friend in Saibai, who in turn delivers the message at Mowat, on the mainland of New Guinea, or Daudai, as the islanders call it, thence the word passes along the coast till it reaches the canoe makers. As soon as the canoe is ready it retraverses the route of the order, being handed on from place to place, and island to island, until it at length reaches its destination. Should, however, there be a new canoe for sale on any of the intermediate stations, this might be sold, and thus obviate the tedious delay of waiting for one to be made to order. Another trade route is through Nagir and Tud to Mowat. The Murray Islanders send to Erub, and the natives of the latter island trade directly with Parem and the mouth of the Fly River. The most remarkable feature in these transactions is that payment is usually extended over three years; in fact, that canoes are purchased on the three years' hire system. This method of purchase, though but recently adopted by ourselves, has for an unknown period been practised by the naked islanders. The mere fact of its existence demonstrates a high level of commercial morality, for if the debts were often repudiated, the whole system would long ago have collapsed.

This commercial morality corroborates to a considerable extent the ethical standard said to be imparted to the youths during initiation. Nor would I like to say that they acted less up to their standard than we up to ours; I doubt whether we would be much the gainers by a comparison. In making this statement it must be distinctly understood that I am only comparing their lives with their own ideals, and not judging them by the ethical standards of other races. It is true they were treacherous, often murdered strangers, and were head-hunters; that their ideas of sexual morality differed from ours, but these "crimes" were not prohibited by public conscience, and there was therefore no wrong in their committing them.

Our higher civilisation has swept over these poor people like a flood, and denuded them of more than their barbarous customs; the old morality has largely gone too.*

[A. C. H.]

---

* Further information as to customs and legends of the Torres Straits Islanders will be found in 'The Journal of the Anthropological Institute,' vol. xix. 1890, and in 'Folk-lore,' vol. i. 1890.

WEEKLY EVENING MEETING,

Friday, May 30, 1890.

WILLIAM HUGGINS, Esq. D.C.L. LL.D. F.R.S., Vice-President, in the Chair.

A. A. COMMON, ESQ. F.R.S. Treas. R.A.S. *M.R.I.*

Astronomical Telescopes.

BEFORE speaking of the enormous instruments of the present day, with their various forms and complicated machinery, it will be well to give some little time to a consideration of the principles involved in the construction of the telescope, the manner in which it assists the eye to perceive distant objects, and in a brief and general way to the construction and action of the eye as far it affects the use of the telescope, all as a help to consider in which way we may hope to still further increase our sense of vision.

I will ask you to bear with me when I mention some things that are very well known, but which if brought to mind may render the subject much more easy. Within pretty narrow limits the principles involved in the construction of the telescope are the same whatever form it ultimately assumes. I will take as an illustration the telescope before me, which has served for the finder to a large astronomical telescope, and of which it is really a model. On examination we find that it has, in common with all refracting telescopes, a large lens at one end and several smaller ones at the other; the number of these small lenses varies according to the purpose for which we use the telescope. Taking out this large lens we find that it is made of two pieces of glass; but as this has been done for a purpose to be presently explained which does not affect the principle, we will disregard this, and consider it only as a simple convex lens, to the more important properties of which I wish first of all particularly to draw your attention, leaving the construction of telescopes to be dealt with later on.

Stated shortly, such a lens has the power of refracting or bending the rays of light that fall upon it: while they are passing through the lens the course they take is altered; if we allow the light from a star to fall upon the lens, the whole of the parallel rays coming from the star on to the front surface are brought by this bending action to a point at some constant distance behind, and can be seen as a point of light by placing there a flat screen of any kind that will intercept the light. For all distant objects the distance at which the crossing of the rays takes place is the same. It depends entirely on the substance of the lens and the curvature we give to the surfaces, and not at all upon the aperture or width of the lens. The brightness only of the picture of the star, depends upon the size of the lens, as that determines the amount of light it gathers together. If, instead of one star we have three or four stars together, we will find that this lens will deal with the light from each star just as it did with the light of the first one, and just in proportion to the angular distance they are apart in the sky, so will the pictures we see of them be apart on our screen. So if we let the light from the moon fall on our lens, all the light from the various parts of the moon's surface will act like the separate stars, and produce a picture of the whole moon (in the photographic camera the lens produces in this manner a picture of objects in front of it, and this picture we see on the ground glass). When we attempt to get pictures of near objects that do not send rays of light that are parallel, we find that as the rays of light from them do not fall on the lens at the same angle to the axis, the picture is formed further away from the lens. The nearer the object whose picture we wish to throw upon the screen is to the lens, the further the screen must be moved. If we try this experiment we shall find, when we have the object at the same distance as the screen, the picture is then of the same size as the object, and the distance of the screen from the lens is twice that which we have found as the focal length; on bringing the object still nearer the lens, we find we must move the screen further and further away, until when the object is at the focus the picture is formed at an infinite distance away, or, what is more to our purpose, the rays of light from an object at the focus of a convex lens after passing through the lens are parallel, exactly as we have seen such parallel rays falling on the glass come to a focus, so that our diagram answers equally well whatever the direction of the rays; and this holds good in other cases where we take the effect of reflection as well as refraction.

We can also produce pictures by means of bright concave surfaces acting by reflection on the light falling upon them. Such a mirror or concave reflecting surface as I have here will behave exactly as the lens, excepting, of course, that it will form the picture in front instead of behind. The bending of the rays in the case of the convex lens is convergent, or towards the axis, for all parallel rays; if we use the reverse form of lens—that is, one thicker at the edge than in the middle—we find the reverse effect on the parallel rays; they will now be divergent, or bend away from the axis; and so with reflecting surfaces if we make the concavity of our mirror less and less, till it ceases and we have a plane, we shall get no effect on the parallel rays of light except a change of direction after reflection. If we go beyond this and make the surface convex we shall then have practically the same effect on the reflected rays as that given to the refracted ray by the concave glass lens.

As regards the size of the picture produced by lenses or mirrors of different focal length, the picture is larger just as the focal length is greater, and the angular dimension is converted into a linear one on the screen in due proportion. Now, as we shall assume that the eye sees all things best at the distance of about nine inches, we may say that the picture taken with a lens of this focal length gives at once the proper and most natural representation we can possibly have of anything at which we can look. Such a picture of a landscape, if placed before the eye at the distance of nine inches, would exactly cover the real landscape point for point all over. A picture taken with a lens of shorter focal length, say four inches, will give a picture as true in all the details as the larger one, but if this picture is looked at, at nine inches distance, it is not a true representation of what we see; in order to make it so, we must look at it with a lens or magnifier. With a larger picture one can look at this at the proper distance, which always is the focal distance of the lens with which it was obtained, when we will see everything in the natural angular position that we have in the first case.

But if, instead of looking at this larger picture, which we may consider taken with a lens of say ninety inches focal length, at a distance of ninety inches, we look at it at a distance of nine inches, we have practically destroyed it as a picture by reducing the distance at which we are viewing it, and we have converted it into what is for that particular landscape a telescopic picture; we see it, not from the point at which it was taken, but just as if we were at one-tenth of the distance from the particular part that we examine. A telescope with a magnifying power of ten, would enable us to see the landscape just as we see it in the photograph, when we examine it in the way I have mentioned.

Having thus seen how a lens or mirror acts, we will turn our attention to the eye. Here we find an optical combination of lenses that act together in the same way as the single convex lens of which we have been speaking. We will call this combination the lens of the eye. It produces a picture of distant objects which in the normal eye falls exactly in focus upon the retina. We are conscious that we do see clearly at all distances beyond about nine inches.

At less than this distance objects becomes more and more indistinct as they are brought nearer to the eye. From what we have seen of the action of the lens in producing pictures of near and distant objects, we know that some movement of the screen must be made in order to get such pictures sharply focussed, a state of things necessary to perfect vision. We might therefore suppose that the eye did so operate by increasing when necessary the distance between the lens and retina, but we know that the same effect is produced in another way; in fact, the only other way. The eye by a marvellous provision of nature, secures the distinctness of the picture on the retina of all objects beyond a distance of about 9 inches, by slightly but sufficiently varying the curvature of one of the lenses; by an effort of will, we can make the accommodating power of the eye slightly greater, and so see things clearly a little nearer; but at about the distance of 9 inches, the normal eye is unconscious of any effort in thus accommodating itself to different distances. The picture produced by the lens of the eye, whose focal length we will assume to be six-tenths of an inch, falls on the retina, which we will assume further to be formed of a great number of separate sensible points, which, as it were, pick up the picture where it falls on these points, and through the nervous organisation, produce the sense of vision. Possibly when these points are affected by light, there may be some connective action, either produced by some slight spherical aberration of the lens or otherwise; but I do not wish to go any further in this matter than is necessary to elucidate my subject. What I am concerned with now is the extent to which the sensibility of the retina extends. Experiment tells us that it extends to the perception of two separate points of light whose angular distance apart is one minute of arc, or in other words, at the distance we can see best, two points whose distance apart is about 1/400 of an inch.

This marvellous power can be better appreciated when we remember that the actual linear distance apart of two such points on the retina is just a little more than 1/6000 of an inch.

In dealing with the shape of small objects the difference between a circle, square, and triangle, can be detected when the linear size of their images on the retina is about 1/2000 of an inch. It may be therefore fairly taken that these separate sensible points of the retina are somewhere about 1/12,000 part of an inch apart from each other. Wonderfully minute as must this structure be, we must remember, as we have already shown, that the actual size of the image it deals with is also extremely small. This minuteness becomes apparent when we consider what occurs when we look at some well-known object, such as the full moon. Taking the angular diameter of the moon as 30 minutes of arc, and the focal length of the eye at six-tenths of an inch, we find the linear diameter of the picture of the full moon on the retina is about 1/200 of an inch, and assuming that our number of the points in the retina is correct, it follows that the moon is subject to the scrutiny of 2800 of these points, each capable of dealing with the portion of the picture that falls upon it.

That is to say, the picture, as the retina deals with it, is made up to this number of separate parts, and is incapable of further division just as if it were a mosaic. I think this is really the case, and as such a supposition permits us to explain not only what occurs when we assist the eye by means of a telescope, but also what occurs when we use the telescope for photographing celestial objects, we will follow it up.

In the case of the eye we suppose the image of the moon to be made up through the agency of these 2800 points, each one capable of noting a variation in the light falling upon it. In order to make this rather important point plainer, I have had a diagrammatic drawing made on this plan. Taking a circle to represent the full moon I have divided it into this number of spaces, and into each space

I have put a black dot, large or small, according to the intensity of the light falling on that part of the image as determined by looking at a photograph of the moon. You will see by the picture of this moon the effect produced. It represents to those who are at a sufficient distance the moon much as it is really seen in the sky.

We can now with a lens of the same focal length as the eye obtain a picture of the full moon exactly of the size of the actual picture on the retina, and if we take a proper photographic process we can get particles of silver approximately of the same sizes as the dots we have used in making our diagram of the moon; the grouping is not exactly the same, but we may take it as precisely so for our purpose. I have not any photographs of the full moon of this size, but I have some here of the moon about five, seven and eight days old, which give a good idea of what I mean by the arrangements of the particles of silver being like our diagram.

It is now quite apparent that if we can by any means increase the size of the picture of the moon on the retina or make it larger on the photographic plate, we shall be able to employ more of our points in the retina of the eye or of our particles of silver in the photographic film, and so be able to see more clearly just in proportion as we increase the size of the picture in relation to the size of the separate parts that make it.

Now the telescope enables us to do this for the eye, and a lens of longer focal length will give us a larger photographic picture.

Let us assume that by means of the telescope we have increased the power of the eye one hundred times. The picture of the moon on the retina would now be one-half inch diameter, and instead of employing 2800 points to determine its shape, and the various markings upon it, we should be employing 28,000,000 of these points; and similarly with the photograph, by increasing the size of our lens we shall obtain a picture made up of this enormous number of particles of silver. But we can go further in the magnification of the picture on the retina—we can also use a still longer focus photographic lens.

A power of magnification of one thousand is quite possible under favourable circumstances; this means that the picture of one two-hundredth of an inch would be now of five inches in diameter, so we must deal with only a portion of it. Let us take a circle of one-tenth of this, equalling one-hundredth of our original picture, which in the eye, unaided by the telescope, would have a diameter of one two-thousandth of an inch, or an area of less than one five-millionth of a square inch. This means that with this magnification, we have increased the power so enormously that we are now employing for the photographic picture two thousand eight hundred million particles of silver, and in the eye the same degree of increase in the number of points of the retina employed in scrutinising the picture piece by piece as successive portions are brought into the central part.

Photography enables me to show that the result I have given of the wonderful effect of increasing the optical power is perfectly correct as far as it is concerned. We will deal with a part only of the moon, representing, as I have just said, about one-tenth of its diameter, or one-hundredth of its visible surface. Two such portions of the moon are marked, as you see, on the diagram. I have selected these portions as I am able to show you them just as taken on a large scale by photography so that you can make the comparison in the most certain manner; but let us first analyse our diagrammatic moon—let us magnify it about ten times, and see what it looks like.

I now show you a picture of this part of the diagram, inclosing the portions I wish to speak about, magnified ten times, so that you can see that about twenty-eight of our points, and by supposition twenty-eight of our particles of silver on the photographic plate, make up the picture. You will see that these dots vary in size; the difference is due to the amount of light falling within what we may call the sphere of action of each point, and should represent it exactly. The result can hardly be called a picture, as it conveys no impression of continuity of form to the mind. We have got down to the structure or separate parts, and to the limit of the powers of the eye and the photographic plate, of course on the assumption we have made as to the size of the points in the one case and the particles of silver in the other. I will now show you the same parts of the moon as represented by the circles on our diagram exactly as delineated by photography. You now see a beautiful picture giving mountains, valleys, craters, peaks, and plains, and all that makes up a picture of lunar scenery. We have thus seen how the power of the eye is increased by the enlargement of the picture on the retina by the telescope, and also how, by increasing the size of the photograph, we also get more and more detail in the picture.

We know we cannot alter the number of those separate points on the retina which determine the limit of our powers of vision in one direction, but we may be able to increase enormously the number of particles of silver in our photographic picture by processes that will give finer deposits, and so, in conjunction with more perfect and larger photographic lenses, we may reasonably look for a great improvement in our sense of vision—it may be even beyond that given by the telescope alone; although it always will be something in favour of the telescope that the magnification obtained in the eye is about fifteen times greater than that obtained by photography when the image on the retina is pitted against the photograph of the same size, unless we use a lens to magnify the photograph of the same focal length as the eye, in which case it is equal. But we *may* go much further in our magnification of the photographic image. In other ways there is great promise when we consider the difference in the action of the eye and the chemical action in the sensitive film under the action of light. As I pointed out in the discourse I gave about four years ago in this theatre, the eye cannot perceive objects that are not sufficiently illuminated, though this same amount of illumination will, by its cumulative effect, make a photographic picture, so that there are ways in which the photographic method of seeing celestial bodies can be possibly made superior to the direct method of looking with a telescope.

With some celestial objects this has been already done: stars too faint to be seen have been photographed, and nebulæ that cannot be seen have also been photographed; but much more than this is possible: we may be able to obtain photographs of the surface of the moon similar to those I have shown, but on a very much larger scale, and we may obtain pictures of the planets that will far surpass the pictures we would see by the telescope alone.

I have mentioned that the distance at which the normal eye can best see things is about nine inches, as that gives the greatest angular size to the object while retaining a sharp picture on the retina; but, as many of us know, eyes differ in this power: two of the common infirmities of the eyes are long or short-sightedness, due to the pictures being formed behind the retina, in the first case, and in front of it in the other. Towards the end of the thirteenth century it was found that convex lenses would cure the first infirmity, and, soon afterwards, that concave lenses would cure the second, as can be easily seen from what I have said about the action of these lenses; so that during the fifteenth and sixteenth centuries the materials for the making of a telescope existed; in fact, in the sixteenth century, Porta invented the camera obscura, which is in one sense a telescope. It seems very strange that the properties of a convex and concave lens when properly arranged were not known much earlier than 1608. Most probably, if we may judge from the references made by some earlier writers, this knowledge existed, but was not properly appreciated by them. Undoubtedly, after the first telescopes were made in Holland in 1608, the value of this unique instrument was fully appreciated, and the news spread rapidly, for we find that in the next year "Galileo had been appointed lecturer at Padua for life, on account of a perspective like the one which was sent from Flanders to Cardinal Borghese." As far as can be ascertained, Galileo heard of the telescope as an instrument by which distant objects appeared nearer and larger, and that he, with this knowledge only, reinvented it. The Galilean telescope is practically, though not theoretically, the simplest form. It is made of a convex lens in combination with a concave lens to intercept the cone of rays before they come to a focus, and render them parallel so that they can be utilised by the eye. It presents objects as they appear, and the picture has less colour in this form than in the other where a convex eye-glass is used. It is used as one form of opera-glass at the present time. Made of one piece of glass in the shape of a cone, the base of which is ground convex, and the apex slightly truncated and ground concave, it becomes a single-lens telescope that can be looked upon just as an enlargement of the outer lens of the eye.

Galileo was undoubtedly the first to make an astronomical discovery with the telescope: his name is, and always will be, associated with the telescope on this account alone.

Very soon after the introduction of the Galilean telescope, the difficulties that arise from the coloured image produced by a single lens turned attention to the possibility of making a telescope by using the reflecting surface of a concave mirror instead of a lens. Newton, who had imperfectly investigated the decomposition of light produced by its refraction through a prism, was of opinion that the reflecting principle gave the greatest possibilities of increase of power. He invented, and was the first to make, a reflecting telescope on the system that is in use to the present day; thus the two forms of telescope—the refracting and reflecting—came into use within about 60 years of each other. It will be perhaps most convenient in briefly running through the history of the telescope, that I should give what was done in each century.

Commencing, then, with the first application of the telescope to the investigation of the heavenly bodies by Galileo in 1609, we find that the largest telescope he could make gave only a magnifying power of about 30.

The first improvement made in the telescope, as left by Galileo, was due to a suggestion—by some attributed to Kepler, but certainly used by Gascoigne—to replace the concave eye-lens that Galileo used by a convex one. Simple as this change looks, it makes an important, indeed vital improvement. The telescope could now be used, by placing a system of lines or a scale in the common focus of the two lenses, to measure the size of the image produced by the large lens; the axis or line of collimation could be found, and so the telescope could be used on graduated instruments to measure the angular distance of various objects; in fact, we have now in every essential principle the true astronomical telescope. It is useless as an ordinary telescope, as it inverts the objects looked at, while the Galilean retains them in their natural position. The addition, however, of another lens or pair of lenses reinverts the image, and we then have the ordinary telescope. It was soon found that the single lens surrounds all bright objects with a fringe of colour, always of a width of about one-fiftieth of the diameter of the object-glass, as we must now call the large lens; and as this width of fringe was the same whatever the focal length of the object-glass, the advantage of increasing this focal length and so getting a larger image without increasing the size of the coloured fringe became apparent, and the telescope therefore was made longer and longer, till a length of over one hundred feet was reached; in fact, they were made so long that they could not be used. A picture of one of these is shown, from which it can be easily imagined the difficulties of using it must have been very great, yet some most important measurements have been made with these long telescopes. Beyond the suggestions of Gregory and Cassegrain for improvements in the reflecting telescope, little was done with this instrument.

During the eighteenth century immense advances were made in both kinds of telescopes. With the invention of the achromatic telescope by Hall and Dollond, the long-focus telescopes disappeared.

Newton had turned to the reflecting telescopes believing from his investigations that the dispersion and refraction were constant for all substances; this was found not to be so, and hence a means was possible to render the coloured fringe that surrounds bright objects when a single lens is used less prominent, by using two kinds of glass for the lens, one giving more refraction with somewhat similar dispersion, so that while the dispersion of one lens is almost corrected or neutralised by the other, there is still a refraction that enables the combination to be used as a lens giving an image almost free from colour.

In 1733, Hall had made telescopes having double object-glasses on this plan, but never published the fact. Dollond who had worked independently at the subject, came to the conclusion that the thing could be done, and succeeded in doing it; the invention of the achromatic telescope is with justice, therefore, connected with his name.

Although this invention was a most important one, full advantage could not be taken of it owing to the difficulty of getting disks of glass large enough to make into the compound object-glass, disks of about four inches being the largest diameter it was possible to obtain. With the reflecting telescope, unhampered as it always has been by any except mechanical difficulties, advance was possible, and astronomers turned to it as the only means of getting larger instruments. Many most excellent instruments were made on the Newtonian plan. The plan proposed by Gregory was largely used, as in this instrument objects are seen in their natural position, so that the telescope could be employed for ordinary purposes.

Many were also made on the plan proposed by Cassegrain. The diagrams on the wall enable you to at once see the essential points of these different forms of reflectors.

About 1776 Herschel commenced his astronomical work; beginning with reflecting telescopes of six or seven inches, he ultimately succeeded in making one of four feet aperture. With these instruments, as everyone knows, most brilliant discoveries were effected, and the first real survey of the heavens made.

Herschel's larger telescopes were mounted by swinging them in a surrounding framed scaffolding that could itself be rotated. The smaller ones were mostly mounted on the plan of the one now before us, which the Council of the Royal Astronomical Society have kindly allowed me to bring here. The plan nearly always used by Sir William Herschel was the Newtonian, though for the larger instruments he used the plan proposed years before by Le Maire, but better known as the Herschelian, when the observer looks directly at the large mirror, which is slightly tilted, so that his body does not hinder the light reaching the telescope. In all cases the substance used for the mirrors was what is called speculum metal.

During the present century the aperture of the refracting telescope has increased enormously; the manufacture of the glass disks has been brought to a high state of perfection, particularly in France, where more attention is given to this manufacture than in any other country. Early in the century the great difficulty was in making the disk of flint glass. M. Guinand, a Swiss, beginning in 1784, succeeded in 1805 in getting disks of glass larger and finer than had been made before, and refractors grew larger and larger as the glass was made. In 1823 we have the Dorpat glass of 9·6 inches, the first large equatorial mounted with clock-work; in 1837 the 12-inch Munich glass; in 1839 the 15-inch at Harvard, and in 1847 another at Pulkowa; in 1863 Cooke finished the 25-inch refractor which Mr. Newall gave, shortly before his death last year, to the Cambridge University.

This telescope the University has accepted, and it is about to be removed to the Observatory at Cambridge, where it will be in charge of the Director, Dr. Adams. In accordance with the expressed wish of the late Mr. Newall, it will be devoted to a study of stellar and astronomical physics. There is every prospect that this will be properly done, as Mr. Frank Newall, one of the sons of the late Mr. Newall, has offered his personal services for five years in carrying on this work. Succeeding this we have the 26-inch telescope at Washington, the 26-inch at the University of Virginia, the 30-inch at Pulkowa, and the 36-inch lately erected at Mount Hamilton, California—all these latter by Alvan Clark and his sons. By Sir Howard Grubb we have many telescopes, including the 28-inch at Vienna. Most of these telescopes have been produced during the last twenty years, as well as quite a host of others of smaller sizes, including nearly a score of telescopes of about 13 inches diameter by various makers, to be employed in the construction of the photographic chart of the heavens, which it has been decided to do by international co-operation.

The first of these photographic instruments was made by the Brothers Henry, of the Paris Observatory, who have also made many very fine object glasses and specula, and more important than all, have shown that plane mirrors of perfect flatness can be made of almost any size; the success of M. Lœwy's new telescope, the equatorial *coudé*, is entirely due to the marvellous perfection of the plane mirrors made by them.

The reflecting telescope has quite kept pace with its elder brother.

Lassell in 1820 began the grinding of mirrors, he like Sir William Herschel working through various sizes, finally completing one of 4 feet aperture, which was mounted equatorially Lord Rosse also took up this work in 1840; he made two 3-foot specula, and in 1845 finished what yet remains the largest telescope, one of 6 feet aperture. All these were of speculum metal, and all on the Newtonian form. In 1870, Grubb completed for the Melbourne Observatory a telescope of 4 feet aperture, on the Cassegrain plan, the only large example. The mirror of this is of speculum metal. In 1856 it was proposed by Steinheil, and in 1857 by Foucault, to use glass as the material for the concave mirror, covering the surface with a fine deposit of metallic silver in the manner that had then just been perfected. In 1858 Draper in America, completed one on this plan of 15 inches aperture, soon after making another of 28 inches. In France several large ones have been made, including one of 4 feet at the Paris Observatory: in England this form of telescope is largely used, and mirrors up to 5 feet in diameter have been made and mounted equatorially.

Optically the astronomical telescope, particularly the refractor, has arrived at a splendid state of excellence; the purity of the glass disks and the perfection of the surfaces is proved at once by the performances of the various large telescopes. No limit has yet been set to the increase of size by the impossibility of getting disks of glass or working them, nor is it probable that the limit will be set by either of these considerations. We must rather look for our limiting conditions to the immense cost of mounting large glasses, and the absorption of light by the glass of which the lenses are made, coming injuriously into play to reduce the light-gathering power, though it will be probably a long time before this latter evil will be much felt.

With the reflecting telescope the greater attention given to the working and testing of the optical surface has enabled the concave mirror to be made with a certainty that the earlier workers never dreamed of. The examination of the surface can be made optically at the centre of curvature of the mirror in the manner that was used by Hadley in the beginning of the last century, and revived some years ago by Foucault who brought this method of testing specula to a high degree of perfection; in fact, with the addition of certain methods of measuring the longitudinal aberrations we have now a means of readily testing mirrors with a degree of accuracy that far exceeds the skill of the worker. It enables every change that is made in the surface during the progress of the figuring, as the parabolisation of the surface is called, to be watched and recorded, and the exact departure of any part from the theoretical form measured and corrected; mirrors can be made of very much greater ratio of aperture to focal length. I have one here where the focal length is only $2\frac{1}{4}$ times the aperture: such a mirror in the days of speculum metal mirrors with the methods then in use would have necessarily had a focal length of about 20 feet. The difference in curvature between the centre and edge of this mirror is so great that it can be easily measured by an ordinary spherometer, amounting as it does with one of 6 inches diameter to 3/10,000 of an inch, an amount sufficient to make the focus of the outer portion about 1 inch longer than the inner when it is tested at the centre of curvature. The diagram on the wall, copied roughly from one of the records, I keep of the progress of the work on a mirror during the figuring, shows how this system of measurements enables one to follow closely the whole operation.

The use of silver on glass as the reflecting surface is as important an improvement in the astronomical telescope as the invention of the achromatic telescope. It gives a permanency to a good figure once obtained that did not exist with the mirrors of speculum metal. To restore the surface of silver to the glass speculum is only a small matter now. How readily this is done may be seen by the practical illustration of the method I will give. I have here two liquids—one a solution of the oxide of silver, and another a reducing agent, the chief material in solution being sugar. I pour the two together in this vessel, the surface of which has been cleaned and kept wet by distilled water, which I shall partly empty, leaving the rest to mix with the two solutions; you will see in the course of about 5 minutes the silver begin to form, eventually covering the whole surface with a brilliant coating that can be polished on the outer surface as bright as that you will see through the glass.

Reflecting telescopes have advantages over the refracting telescopes in many ways, but in some respects they are not so good. They give images that are absolutely achromatic, while the other form always has some uncorrected colour. They can be made shorter, and as the light-grasping power is not reduced by the absorption of the glass of which the lenses are made, it is in direct proportion to the surface or area of the mirror. They have not had in many cases the same care bestowed upon either their manufacture or upon their mounting as has been given in nearly every case to the refracting telescope. Speaking generally, the mounting of the reflecting telescope has nearly always been of a very imperfect kind—a matter of great consequence, for upon the mounting of the astronomical telescope so much depends. To direct the tube to any object is not difficult, but to keep it steadily moving so that the object remains on the field of view requires that the tube should be carried by an equatorial mounting of an efficient character. The first essential of such a mounting is an axis parallel to the axis of rotation of the earth. The tube, being supported on this, will follow any celestial object, such as a star, by simply turning the polar axis in a contrary motion to that of the earth, and at the same rate as the earth rotates on its axis. If we make the telescope to swing in a plane parallel to the polar axis, we can then direct the telescope to any part of the sky, and we have the complete equatorial movement. There are several ways in which this is practically done: we can have a long open-work polar axis supported at top and bottom, and swing the telescope in this, or we can have short strong axes. As examples of the first, I will show you pictures of the mountings designed for Cambridge and Greenwich Observatories some forty years ago by Sir G. Airy, lately and for so long our eminent Astronomer-Royal; and as examples of the other form, amongst others, the large telescope lately erected at Nice, and also the larger one at Mount Hamilton, California, now under the direction of Prof. Holden.

The plan of bringing all the various handles and wheels that control the movement of the telescope and the various accessories down to the eye end, so as to be within reach of the observer, is carried to the highest possible degree of perfection here, as we can see by an inspection of the picture of the eye end of this telescope. The observer with the reflecting telescope is, with moderate-size instruments, never very far from the floor, but in the case of the Lick telescope he might have to ascend some thirty feet for objects low down in the sky. Thanks to the ingenuity of Sir Howard Grubb, to whom the idea is due, the whole of the floor of the Observatory is made to rise and fall by hydraulic machinery at the will of the observer—a charming but expensive way of solving the difficulty, as far as safety goes, but not meeting the constant need of a change in position as the telescope swings round in keeping up with the motion of the object to which it is directed. The great length of the tube of large refractors is well seen in this picture of the Lick telescope: it suggests flexure as the change is made in the direction in which it points, and the consequent change of stress in the different parts of the tube.

The mounting of the reflector has been treated, if not so successfully, with more variety than in the case of the refractor as we shall see from the pictures I will show you, especially where the Newtonian form is used. The 4-foot reflector at Melbourne is mounted on the German plan, in a similar way to a refractor, and an almost identical plan has been followed by the makers of the 4-foot at the Paris Observatory. Lassell, who was the first to mount a large reflector equatorially, used a mounting that may be called the forked mounting, the polar axis being forked at its upper end, and the tube of the telescope swinging between the forks; a very excellent plan, dispensing with all counterpoising. Wishing to obtain certain conditions that I thought and think now favourable to the performance of the reflector, I devised a mounting where the whole tube was supported at one end on a bent arm; a 3-foot mirror was mounted on this plan in 1879, and worked admirably. The Newtonian form demands the presence of the observer near the high end of the telescope, and the trouble of getting him there and keeping him safely close to the eye-piece is very great. As we see from the various photographs, several means have been employed to do this, none of them quite satisfactory.

All the refracting telescopes of note in the world are covered by domes that effectually protect them from the weather; these domes are in some cases comparable in cost with the instruments they cover. It is not surprising, therefore, that efforts have been made to devise a means of getting rid of this costly dome and the long movable tube.

It was suggested many years ago that a combination of plane mirrors could be used to direct light from any object into a fixed telescope. This idea in a modified form has often been used for special work, one plane mirror being used as we see in the picture on the screen to throw a beam of light into a telescope fixed horizontally; for certain kinds of work this does admirably, but the range is restricted as can be easily seen, and the object rotates in the field of view as the earth goes round. The next step would be to place the telescope pointing parallel to the axis of the earth and send the beam of light into it from the mirror, which could now be carried by the tube so that by simply rotating the tube on its own axis the object would be kept in the field of view. Sir Howard Grubb makes a small telescope on this plan, and some years ago proposed a somewhat similar plan. A sketch of this plan I will show you. You will see, however, that here again the range is restricted, and to use the telescope, means would be required to constantly vary the inclination of the small mirror at one-half the rate of inclination of the short tube carrying the object-glass.

By the use of two plane mirrors, however, the solution of the problem of a rotating telescope tube placed as a polar axis is solved. By having such a telescope with a plane mirror at an angle of 45° to the axis of the telescope in front of the object-glass, we can, by simply rotating the telescope, see every object lying on the equator; and by adding another similar plane mirror at an angle of 45° to the axis of the telescope, *as bent out at right angles by the first plane mirror.* and giving the mirror a rotation perpendicular to this axis, we obtain the same power of pointing the telescope as we have in the equatorial. The idea of doing this was published many years ago, but it was left to the skill and perseverance of M. Lœwy, of the Paris Observatory, to put it into practical use. He devised, and had made, a telescope on this principle, of $10\frac{1}{2}$ inches aperture, which was completed in 1882. It has proved itself an unqualified success, and many other larger ones are now being made in Paris, including one of 23 inches aperture, now nearly completed, for the Paris Observatory.

A lantern copy of a drawing of this latter telescope will be thrown on the screen, in order that you may see what manifest advantages exist in this form of telescope. There is but one objection that can be urged—that is, the possible damage to the definition by the plane mirrors; but this seems, from what I have seen of the wonderful perfection of the plane mirrors made by the Brothers Henry, to be an unreasonable one—at any rate not an insurmountable one. In every other respect, except perhaps a slight loss of light, this form of telescope is so manifestly superior to the ordinary form that it must supersede it in time, not only for general work, but for such work as photography and spectroscopy.

Note on a Method of Silvering Glass Mirrors.

Solutions.—Make up 10 per cent. solutions of pure recrystallised nitrate of silver, pure caustic potash, and loaf sugar. To the sugar solution add ½ per cent. of pure nitric acid and 10 per cent. of alcohol. The sugar solution is very much improved by keeping, its action being more rapid and the film cleaner when the sugar solution has been made for a long time. Make up also a weak solution, say 1 per cent. of nitrate of silver and a 10 per cent. solution of ammonia. (90 per cent. distilled water, 10 per cent. ammonia, ·880 specific gravity). *Distilled water* must be used for all the solutions.

Cleaning the Mirror.—Thoroughly clean the mirror. To do this pour on a strong solution of caustic potash, rub well with cotton wool, rinse with ordinary water, wash again with absolute alcohol, and rinse; finally pour on strong nitric acid, and rub with a piece of cotton wool, inserted in the open end of a test tube. Rinse again thoroughly with ordinary water, and then place the mirror face downwards in distilled water in a dish sufficiently large to leave two inches margin round the edge of the mirror, and to keep the face of the mirror one inch from the bottom of the dish. The liquid should stand half an inch above the face of the mirror which should not be completely submerged, and care should be taken to exclude all air-bubbles.

For Silvering a 12-inch Mirror.—Take 400 c.c. of the nitrate of silver solution and add strong ammonia until the brown precipitate first formed is nearly dissolved, then use the diluted ammonia until the solution is just clear. Then add 200 c.c. of the caustic potash solution. A brown precipitate is again formed, which must be dissolved in ammonia exactly as before, the ammonia being added until the liquid is just clear. Now add the 1 per cent. solution of silver nitrate until the liquid becomes a light brown colour, about equal in density of colour to sherry. This colour is important, and can only be properly obtained by adding the weak solution. Dilute the liquids to 1500 c.c. with distilled water.

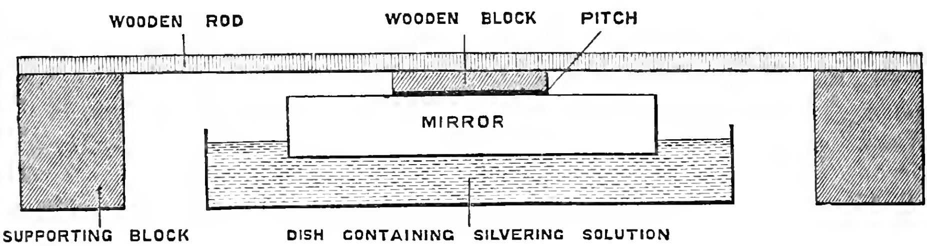
All being ready add 200 c.c. of the sugar solution to 500 c.c. of water. Then lift the mirror out of the dish, taking care to keep its face downwards during the time it is out of the water, pour the washing water away, add the sugar solution to the silver potash solution, taking care they are thoroughly mixed, and pour them into the dish. Place the mirror face downwards in this solution, taking care to exclude all air-bubbles.

The liquid will turn light brown, dark brown, and finally black. In four or five minutes, often sooner, a thin film of silver will commence to form on the mirror, and this will thicken until in about twenty minutes the whole liquid has acquired a yellowish brown colour, with a thin film of metallic silver floating on the surface.

Lift the mirror out, thoroughly wash with distilled water, and stand the mirror on its edge, or rest it in an inclined position until it is dry; if time can be allowed, the silvered mirror may be left to soak in distilled water over night. Leave it to dry until next day, then the slight yellowish "bloom" can be polished off by rubbing softly with a pad of chamois leather and cotton wool. Carefully polish afterwards with a little dry well-washed rouge on the leather pad. The film should be opaque and brilliant, and with careful handling will be very little changed with long use.

Dishes.—Use porcelain, glass, or earthenware dishes whenever possible; but, if these are not available, a zinc dish, coated inside with paraffin or best beeswax.

For small mirrors (up to 12 inches) the easiest method of supporting them during silvering is to attach them to a wooden rod by pitch, and arrange the dish thus



Temperature and Time.—Half an hour is the usual time taken in silvering, but this is shortened by using warmer liquids. About 65° F. is best for silvering. In colder weather longer time must be allowed for the film to be deposited. In very hot weather a smaller quantity of sugar can be used, say 150 c.c. For a 12-inch mirror it is a safe rule to allow four times the time required to get the first indications all over the mirror as the total time for the mirror to be in the bath.

In cases when it is necessary to silver face upwards, a band may be put round the mirror, and the solutions poured on. It is necessary in this case to *leave out the potash solution*, and allow a longer time for the silver to deposit; as much as two hours being sometimes necessary.

If a very thick film is required, two silvering baths can be used, the mirror being left in the first for 15 minutes, then lifted out, rinsed with distilled water, and at once immersed in the second bath, which should be ready in a second dish. The film must not be allowed to dry during the operation of changing from one bath to the other.

GENERAL MONTHLY MEETING,

Monday, June 2, 1890.

Sir James Crichton Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

C. J. Cullingworth, M.D. F.R.C.P.
Jonathan Hutchinson, Esq. F.R.S. F.R.C.S.
Rudolph Messel, Esq. Ph.D. F.C.S.
Henry Charles Mylne, Esq.
Dan Rylands, Esq.

were elected Members of the Royal Institution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz:—

FOR

Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta: Rendiconti. 1^{o} Semestre, Vol. VI. Fasc. 5, 6. 8vo. 1890.
Agricultural Society of England, Royal—Journal, Third Series, Vol. I. Part 1. 8vo. 1890.
Asiatic Society of Bengal—Journal, Vol. LVIII. Part I. No. 2; Part II. Nos. 3 4, and Sup. 8vo. 1889–90.
Proceedings, 1889, Parts 7–10. 8vo.
Astronomical Society, Royal—Monthly Notices, Vol. L. No. 6. 8vo. 1890.
Babbage, Henry P. Esq.—Babbage's Occulting Telegraph. 8vo. 1890.
Bankers, Institute of—Journal, Vol. XI. Part 5. 8vo. 1890.
Brazil Republic—Boletins Mensaes do Observatorio Meteorologico, Vols. I.–III. 8vo. 1886–1888.
British Architects, Royal Institute of—Proceedings, 1889–90, Nos. 14, 15. 4to.
British Association for Advancement of Science—Report of Meeting at Newcastle-on-Tyne, 1889. 8vo. 1890.
Chemical Industry, Society of—Journal, Vol. IX. No. 4. 4to. 1890.
Chemical Society—Journal for May, 1890. 8vo.
Cracovie, l'Academie des Sciences—Bulletin, 1890, No. 4. 8vo.
Dax: Société de Borda—Bulletin, 1890, 1^{e} Trimestre. 8vo.
Editors—American Journal of Science for May, 1890. 8vo.
Analyst for May, 1890. 8vo.
Athenæum for May, 1890. 4to.
Chemical News for May, 1890. 4to.
Chemist and Druggist for May, 1890. 8vo.
Electrical Engineer for May, 1890. fol.
Engineer for May, 1890. fol.
Engineering for May, 1890. fol.
Horological Journal for May, 1890. 8vo.
Industries for May, 1890. fol.
Iron for May, 1890. 4to.
Ironmongery for May, 1890.
Murray's Magazine for May, 1890. 8vo.
Nature for May, 1890. 4to.
Photographic News for May, 1890. 8vo.
Revue Scientifique for May, 1890. 4to.
Telegraphic Journal for May, 1890. fol.
Zoophilist for May, 1890. 4to.
Electrical Engineers, Institution of—Journal, No. 86. 8vo. 1890.
Florence Biblioteca Nazionale Centrale—Bolletino, Nos. 104–106. 8vo. 1890.

Franklin Institute—Journal, No. 773. 8vo. 1890.
Geneva, Société de Physique et d'Histoire Naturelle—Mémoires, Tome XXX. Partie 2. 4to. 1889–90.
Geographical Society, Royal—Proceedings, New Series, Vol. XII. No. 5. 8vo. 1890.
Geological Institute, Imperial, Vienna—Abhandlungen, Band XIII. Heft 1; Band XV. Heft 1. fol. 1890.
Jahrbuch, Band XXXVIII. Heft 4; Band XXXIX. Heft 3, 4. 8vo. 1890.
Verhandlungen, 1889, Nos. 13–17. 8vo.
Geological Society—Quarterly Journal, Vol. XLVI. Part 2, No. 182. 8vo. 1890
Georgofili, Reale Accademia—Atti, Vol. XIII. Disp. 1. 8vo. 1890.
Johns Hopkins University—University Circulars, No. 80. 4to. 1890.
Laboratory Club—Transactions, Vol. III. No. 5. 8vo. 1890.
Linnean Society—Journal, Vol. XXVII. No. 182. 8vo. 1890.
Meteorological Office—Weekly Weather Reports, Nos. 17–21. 4to. 1890.
Meteorological Observations at Foreign and Colonial Stations, 1852–1886. 4to. 1890.
Meteorological Society, Royal—Quarterly Journal, No. 74. 8vo. 1889.
Meteorological Record, No. 35. 8vo. 1889.
National Life-boat Institution—Annual Report for 1890. 8vo. 1890.
North of England Institute of Mining and Mechanical Engineers—Transactions, Vol. XXXVIII. Part 5. 8vo. 1890.
Odontological Society of Great Britain—Transactions, Vol. XXII. No. 6. New Series. 8vo. 1890.
Paris Universal Exhibition, 1889—British Section, Report of the Council. 12mo. 1890.
Pharmaceutical Society of Great Britain—Journal, May, 1890. 8vo.
Photographic Society—Journal, Vol. XIV. Nos. 7, 8. 8vo. 1890.
Rathbone, E. P. Esq. (the Editor)—The Witwatersrand Mining and Metallurgical Review, No. 4. 8vo. 1890.
Rio de Janeiro Observatory—Revista, No. 4. 8vo. 1890.
Royal Irish Academy—Transactions, Vol. XXXIX. Part 13. 4to. 1890.
"Cunningham Memoirs," No. 5. 4to. 1890.
Royal Society of Antiquaries of Ireland—Journal, Vol. I. No. 1. 5th Series. 8vo. 1890.
Royal Society of London—Proceedings, No. 288. 8vo. 1890.
Saxon Society of Sciences, Royal—Philologisch-historischen Classe:
Abhandlungen, Band XI. No. 6. 8vo. 1890.
Berichte, 1889, No. 4. 8vo. 1890.
Mathematische-Physischen Classe:
Abhandlungen. Band XV. Nos. 7–9. 4to. 1889.
Berichte, 1889, Nos. 2–4. 8vo. 1890.
Register-Berichte, 1846–1885; Abhandlungen, Band I.–XII. 8vo. 1889.
Selborne Society—Nature Notes, Vol. I. No. 5. 8vo. 1890.
Simpson, James, Esq. (the Author)—The Scottish Press and the Gipsies. 8vo. 1890.
Smithsonian Institution—Bureau of Ethnology, Fifth and Sixth Reports. 4to. 1887–1888. Various Papers. 8vo. 1887–1889.
Society of Architects—Proceedings, Vol. II. No. 10. 8vo. 1890.
Society of Arts—Journal for May, 1890. 8vo.
Teyler Museum—Archives, Série II. Vol. III. Fas. 4. 4to. 1890.
Catalogue de la Bibliothèque, Vol. II. Livraison 1–3. 4to. 1890.
United Service Institution, Royal—Journal, No. 152. 8vo. 1890.
United States Geological Survey—Seventh Annual Report, 1885–86. 4to. 1890.
University of London—Calendar, 1890–91. 8vo.
Vereins zur Beforderüng des Gewerbfleises in Preussen—Verhandlungen, 1890: Heft 4. 4to.
Victoria Institute—Transactions, No. 92. 8vo. 1890.
Yorkshire Archæological and Topographical Association—Journal, Parts 41, 42. 8vo. 1890.

WEEKLY EVENING MEETING,

Friday, June 6, 1890.

Basil Woodd Smith, Esq. F.R.A.S. F.S.A. Vice-President, in the Chair.

Professor W. Boyd Dawkins, M.A. F.R.S.

The Search for Coal in the South of England.

1. Introductory—2. The conditions under which the coal-measures were formed —3. The break up of the Carboniferous alluvia into isolated coal-basins—4. Godwin-Austen's conclusions—5. The conclusions of Prestwich and the Coal Commission—6. The range of the coal-measures under the Newer Rocks of Somerset—7. Coal-measures in Oxfordshire—8. The district of London—9. The Weald of Sussex—10. The coal-fields of Northern France, Belgium, and Westphalia—11. The discovery of a coal-field at Dover—12. General conclusions.

1. The bare facts of the recent discovery of coal-measures at Shakespeare Cliff, near Dover, have been published in the press, and the full account cannot be written till the completion of the inquiry which is now going on. It is, however, not unfitting that the bearing of the discovery on the general question of the existence of workable coal-fields in Southern England should be discussed within these walls, not merely on account of its general interest, but because it naturally follows the paper read by Mr. Godwin-Austen before the Royal Institution, in 1858, "On the Probability of Coal beneath the South-eastern parts of England." In 1855 he had placed before the Geological Society of London the possibility of the existence of coal in South-eastern England at a workable depth. In the two years which had elapsed, "the possibility" had grown in his mind into the "probability," and in the thirty-two years which have passed between the date of the paper before this Institution and the present time, "the probability" has been converted into a certainty by the recent discovery at Dover. In this communication, the lines of the inquiry laid down by Godwin-Austen will be strictly followed. We must first examine the conditions under which the coal-measures were accumulated.

2. The seams of coal are proved, by the surface-soil traversed by roots and rootlets, to which in some cases the trunks are still attached, to have been formed *in situ* by the growth and decay of innumerable generations of Plants (*Lepidodendra*, *Sigillaria*, *Calamites*), Pines, (*Trigonocarpa*, *Dadoxylon*, *Sternbergia*), allied to *Salisburia*, and a vast undergrowth of Ferns, all of which contributed to form a peat-like morass. Each seam represents an accumulation on a land-surface, just as the sandstones and shales above it point to a period

of depression during which sandbanks and mudbanks were deposited by water. The fact also that the coal-seams in a given sinking are parallel, or nearly parallel, implies that they were formed on horizontal tracts of alluvium, while the marine and fresh-water shells in the associated sandstones and shales prove that they were near the level of the sea, or within reach of a mighty river. This tract of forest-clad marsh-lands, as Godwin-Austen and Prestwich have pointed out, occupied the greater part of the British Isles, from the Highlands of Scotland southwards as far as Brittany, and eastwards far away into the valley of the Rhine, and westwards over the greater part of Ireland. It swept round the hills of South Scotland and the Lake district and the region of Cornwall. It occupied a delta like that of the Mississippi, in which the forest-growths were from time to time depressed beneath the water-line, until the whole thickness of the coal-measures (7200 feet thick in Lancashire, 7600 in South Wales, and 8400 in Somersetshire) was built up. After each depression the forest spread again over the sand and mud of the submerged parts, and another peat-layer of vegetable matter was slowly accumulated above that buried beneath the sand and mud. The great extent of this delta implies the existence of a large river draining a large continent, of which the Highlands of Scotland and the Scandinavian peninsula formed parts, and which I have described before the Royal Institution under the name of Archaia.

3. At the close of the Carboniferous age, this vast tract of alluvium was thrown into a series of folds by earth-movements. These have left their mark in the south of England and the adjacent parts of France, in the anticline of the English Channel, the syncline of Devonshire, the anticline of the Mendip Hills and of the lower Severn, and the syncline of the South Wales coal-fields. These great east and west folds have been traced from the south of Ireland on the west, through 35 degrees of latitude, through North France and Belgium, as far as the region of Westphalia. Next, the upper portions of the folds were attacked by the subaerial and marine agents of denudation over the whole of the Carboniferous area, leaving the lower parts to form the existing coal-fields which lie scattered over the surface of the British Isles, and are isolated from each other by exposures of older rocks; and a broad east and west ridge was carved out of the folded and broken Carboniferous and older rocks, extending from the anticline of the Mendip Hills eastward through Artois into Germany, and constituting the ridge or axis of Artois of Godwin-Austen.

The next stage in the history of the folded Carboniferous and older rocks is marked by the deposition of the Permian and Secondary rocks on their eroded and waterworn edges, by which they were partially concealed or wholly buried, and these newer strata thin off as they approach the ridge of Artois. This barrier, also, of folded Carboniferous and older rocks sank gradually beneath the sea in the Triassic, Liassic, Oolitic, and Cretaceous ages, and against it the strata of the first three named ages thin off, while in France and

Belgium the Cretaceous deposits rest immediately upon the waterworn older rocks.

From these general considerations it is clear that the coal-measures which formerly extended over nearly the whole of Southern England can now only be met with in isolated basins under the newer rocks, and that these are thinnest along the line of the above-mentioned barrier.

4. The exposed coal-fields in Britain, and on the Continent also, Godwin-Austen pointed out, along this line, are of the same mineral character, and the pre-Carboniferous rocks are the same. This ridge or barrier also, where it is concealed by the newer rocks, is marked by the arch-like fold (anticlinal) of the chalk of Wiltshire, and by the line of the North Downs in Surrey and Kent. Godwin-Austen finally concluded that there are coal-fields beneath the Oolitic and Cretaceous rocks in the South of England, and that they are near enough to the surface along the line of the ridge to be capable of being worked. He mentioned the Thames valley and the Weald of Kent and Sussex as possible places where they might be discovered.

These strikingly original views gradually made their way, and in the next eleven years became part of the general body of geological theory. They were, however, not accepted by Sir Roderick Murchison, the then head of the Geological Survey, who maintained to the last that there were no valuable coal-fields in Southern England.

5. The next important step in the direction of their verification was that taken by the Coal Commission of 1866–67, by whom Mr. Godwin-Austen was examined at length, and the results of the inquiry embodied in the Report by Mr. Prestwich. In the Report Mr. Godwin-Austen's views are accepted, and fortified by a vast number of details relating both to the coal-fields of Somersetshire and of France and Belgium. Mr. Prestwich also calls special attention to the physical identity of the coals of these two regions, and to the fact that the Carboniferous and older rocks in both are similarly disturbed. He concludes, further, that the coal-fields which now lie buried beneath the newer rocks are probably equal in value and in extent to those which are exposed in Somerset and South Wales on the west, and in Belgium and France on the east.

We will now proceed to test these theoretical conclusions by the light of recent observations.

6. The coal-fields of Somerset and Gloucester were proved by the labours of Prof. Prestwich and the Coal Commission of 1866–67 to be small fractions of the great coal basin which lies buried beneath the Triassic, Liassic, and Oolitic rocks, from the Mendip Hills northwards past Bristol to Wickwar. On the west also three small isolated coal-basins occur—those of Nailsea and Portishead, which are partially, and that of Aust, which is wholly, concealed by the newer rocks. The coal-measures are folded and broken, and traversed by great "overthrust" faults, which at Kingswood give the same series of coals twice over in the sinkings of one colliery. Their southern

boundary is the line of the Mendip Hills. They also probably occur at a depth which remains to be proved, still further to the south, in the valley of the Axe and the district of Glastonbury, the most southern boundary being the mountain limestone of Cannington, near Bridgwater (see map). The great Somerset and Gloucester field may extend to the east under the newer rocks, between Freshford and Beckington, in the district south of Bath.

The value of the evidence of the coal-fields of the West of England on the general question consists in the fact that they may be taken as fair samples of those which lie concealed along the line of the buried ridge through South-eastern England in the direction of France, Belgium, and Germany.

7. One of these concealed coal-fields has been struck in a deep boring at Burford, near Witney, in Oxfordshire, at a depth of 1184 feet, under the following rocks:—

Oolites	148 feet.
Lias	598 ,,
Rhoetic	10 ,,
Triassic rocks	428 ,,

The sandstones and shales of the coal-measures were penetrated to a depth of 225 feet.*

These coal-measure rocks form, as suggested by Hull, one of the same series of coal-basins as those of South Wales and the Forest of Dean, and probably mark the line of the continuation of the South Wales syncline in the direction of Harwich, where Carboniferous shale has been struck at a depth of 1052 feet from the surface.

This boring proves not merely the presence of coal-measures at a workable depth in Oxfordshire, but also the important fact that the Triassic rocks, which are of great thickness further north, have dwindled down to an unimportant thickness in their range southwards and eastwards. Further, that south, in the London area, these rocks are wholly absent; and farther to the east, at Harwich, the Liassic and Oolitic strata and Lower Greensand are absent, and the Gault rests on the eroded Lower Carboniferous rocks, inclined at a high angle.

8. The water-worn surface of the folded rocks, which are older than the Carboniferous, has been repeatedly struck in deep borings for water in the neighbourhood of London, at depths ranging from 839 feet at Ware to 1239 feet at Richmond. They consist of Silurian strata in the north at Ware, and of Old Red Sandstone or Devonian rocks in the other localities. From their high angle of dip, as in the case of similar rocks underlying the coal-fields of Somerset and Northern France and Belgium, it may be inferred that coal-fields lie in the synclinal folds in the neighbouring areas.

From the fact of the Silurian rocks being in the north, while all

* De Rance, Manch. Geol. Soc., 26th March, 1878.

the rest of the borings to the south terminate in the Devonian or Old Red rocks, it may be inferred that the chalk of the North Downs probably conceals the coal-measures. It must also be noted that there are no Wealden rocks in the London area, and no Lower Greensands, and that the Lower Oolites at their thickest are only 87 feet. The secondary rocks, which are of great thickness in the midland and northern counties, thin off as they pass southwards towards London, against the ridge of older rocks, as both Austen and Prestwich have pointed out.

It is therefore in the area south of London, rather than in that immediately to the north, that the coal-measures are to be looked for at a workable depth beneath the surface, and underneath the chalk of the North Downs. It must, however, be noted that the line of the South Wales syncline through Burford passes to the north of Ware, and that there may be coal-measures in the northern parts of Essex and of Hertfordshire at a workable depth.

9. The Report of the Coal Commission was published in 1871, and in the following year the Sub-Wealden Exploration Committee was organised by Mr. Henry Willett, to test the question of the existence of the Carboniferous and pre-Carboniferous rocks in the Wealden area by an experimental boring. The site chosen was Netherfield, about three miles south of Battle, in Sussex, where the lowest rocks of the Wealden formation constitute the bottom of the valley. The rocks penetrated were as follows:—

Section of Netherfield.

Purbeck strata	200 feet.
Portland strata	57 ,,
Kimmeridge clay	1073 ,,
Corallian strata	515 ,,
Oxford clay	60 ,,
	1905 ,,

This boring showed that the coal-measures and older rocks are, in that region, more than 1900 feet from the surface of the ground. We may also infer, from the fact of the bottom of the bore-hole being in the Oxford clay, and from the known thickness of the Bath Oolitic strata in the nearest places, that it lies buried beneath considerably more than 2000 feet of newer rocks. With this valuable, though negative result, the Sub-Wealden Exploration came to an end. It was a purely scientific inquiry, paid for by subscription, and largely supported by those who had no pecuniary interest in the result.

The experience of the boring at Netherfield showed that the search for the coal-measures and older rocks of Godwin-Austen's ridge would have to be carried out at some spot further to the north, in the direction of the North Downs. In the district of Battle the Oolitic rocks were proved to be more than 1700 feet thick, and the great and increasing thickness of the successive rocks of the Wealden formation above them, which form the surface of the ground between Netherfield and the North Downs, rendered it undesirable to repeat the experiment within the Wealden area proper, where the Wealden rocks presented a total thickness of more than 1000 feet, in addition to that of the Oolites. My attention, therefore, was directed to the line along the North Downs, where Godwin-Austen believed that the Wealden beds abruptly terminated against the ridge of coal-measures and older rocks, and where, therefore, there would be a greater chance of success.

10. The evidence, also, of the French, Belgian, and Westphalian coal-fields pointed in the direction of the North Downs.

The Carboniferous and older rocks, which we have hitherto traced only as far as the area of London from their western outcrops in Somerset, Gloucestershire, and South Wales, reappear at the surface in Northern France, Belgium, and Westphalia, and contain most valuable coal-fields, which are long, narrow, and deep. These extend from the district of the Ruhr on the east, through Aachen, Liège, Namur, Charleroi, Mons, and Valenciennes. The enormous value of the last field led, during the last hundred years, to numerous borings through the newer rocks, which have extended the western range of the coal-measures upwards of 95 miles away from its disappearance under the Oolites and chalk, as far as Flechinelle, south of Aire, or to within 30 miles of Calais. It occupies throughout this distance a narrow trough or syncline, 11 miles across at Douchy, and about half a mile at its western termination. It is represented still further to the west by the faulted and folded coal-fields of Hardinghen and Marquise, which are within about 12 miles of Calais. The coal-measure shales and sandstones found in a boring at Calais, at a depth of 1104 feet from the surface, in 1850,* reveal the existence of another coal-field in the same general line of strike, and making for Dover and the North Downs.

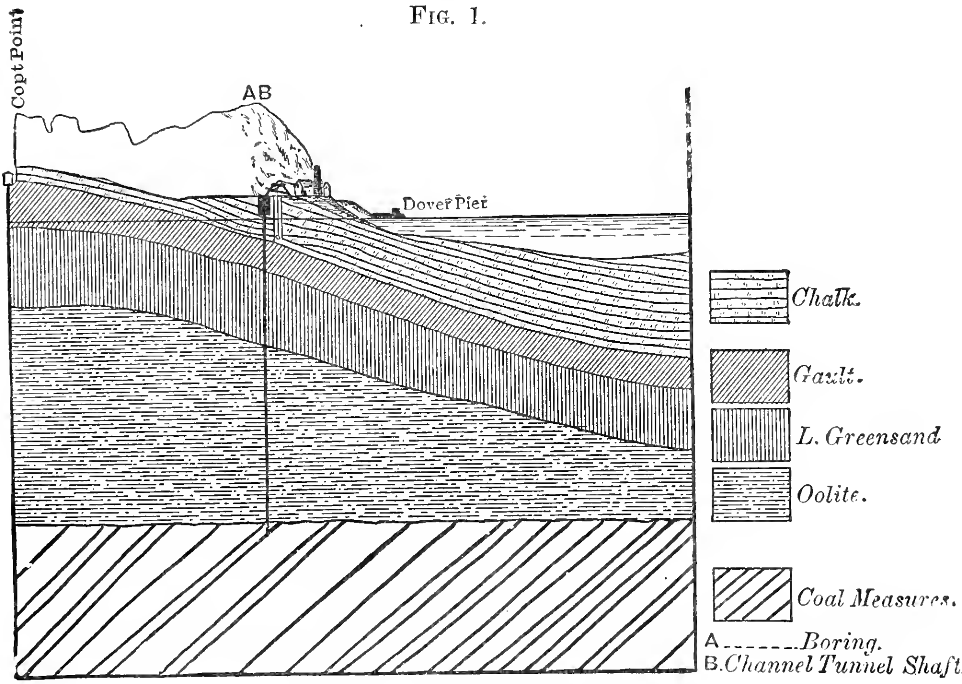
11. We have seen that the range of the coal-measures has been pushed farther and farther to the west by experimental borings, until they have been proved to exist underneath Calais. The opposite shores of the Straits of Dover, therefore, presented the best locality for a trial still further to the west. In choosing a site, the Channel Tunnel works, close to Shakespeare Cliff, Dover, appeared to me to present great advantages, which I embodied in a report to Sir Edward W. Watkin, in 1886. The site is within view of Calais, and not more than six miles to the south of a spot where about 4 cwt. of bituminous material was found imbedded in the chalk in making a tunnel, which, according to Godwin-Austen, had been probably derived from the coal-measures below.

Prestwich also had pointed out, in 1873, in dealing with the question of a tunnel between England and France, that the older

* This fact is doubted by Gosselet. I am, however, informed by Prestwich that both he and Elie de Beaumont identified them as coal-measures at the time, and I see no reason for doubting the accuracy of those two eminent observers. The cores were, unfortunately, lost in the first Paris Exhibition.

rocks were within such easy reach at Dover, that they could be utilised for the making of a submarine tunnel. Sir Edward Watkin acted with his usual energy, and the work was begun in 1886, and has been carried on down to the present time, under my advice, and at the expense of the Channel Tunnel Company. The boring operations have been under the direction of Mr. F. Brady, the Chief Engineer of the South-Eastern Railway, to whose ability we owe the completion of the work to its present point, under circumstances of

Fig. 1.



Boring at Shakespeare Cliff.

great difficulty. A shaft has been sunk (A) [See Fig. 1] on the west side of the Shakespeare Cliff, close to the shaft of the Channel Tunnel (B) to a depth of 44 feet, and from this a bore-hole has been made to a depth of 1180 feet.

Section at Shakespeare Cliff, Dover.

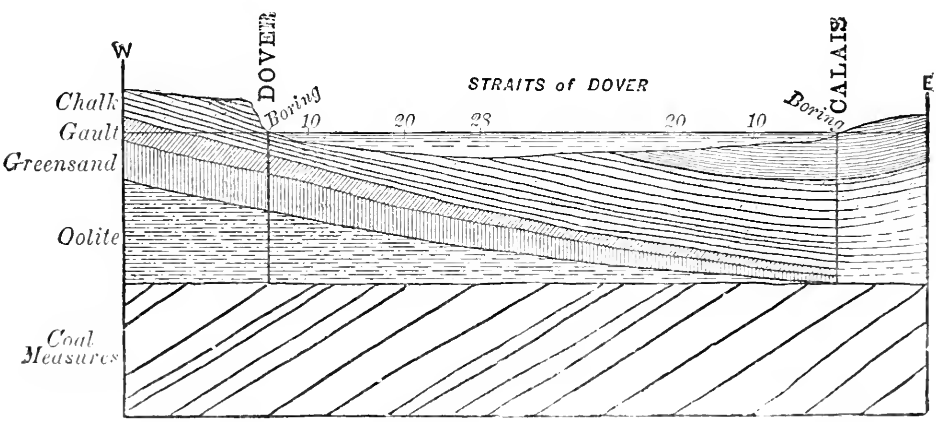
	Feet.
Lower grey chalk, and chalk marl	500
Glauconite marl	
Gault	
Neocomian	
Portlandian	660
Kimmeridgian	
Corallian	
Oxfordian	
Callovian	
Bathonian	
Coal measures, sandstones, and shales and clays, with one seam of coal	70

The coal-measures were struck at a depth of 1204 feet from the surface, or 1160 feet from the top of the bore-hole, and a seam of good blazing coal was met with 20 feet lower.

12. This discovery proves up to the hilt the truth of Godwin-Austen's views as to the range of the coal-measures along the line of the North Downs, and as to the thinning off of the Oolitic and Wealden strata against the buried ridge. The former are less than one-third of their thickness at Netherfield, and the latter are wholly unrepresented. It establishes the existence of a coal-field in South-eastern England, at a depth well within the limits of working at a profit. The principal coal-pits in this country are worked at depths ranging from over 1000 to 2800 feet, and one at Charleroi, in Belgium, is worked to a depth of 3412 feet.

The Dover coal-field probably forms part of the same narrow trough as the Calais measures, prolonged westward under the Channel further to the south than Godwin-Austen drew it in 1858. Whether it is a trough similar to that which extends through Northern France for more than 100 miles from east to west, as Godwin-Austen has drawn it in the diagram on the wall, reaching as far to the west as Reading, or whether it is a small, faulted, insignificant fragment of a field, such as that of Marquise and

Fig. 2.



Probable Range of Coal Measures between Dover and Calais.

Hardinghen, remains to be proved. It is, however, one of a chain of coal-fields, which will, in my opinion, ultimately be proved to extend under the newer rocks between Dover and Somerset, along the line of the North Downs, in long narrow east and west troughs. It is probably a continuation beneath the Straits of Dover of the coal measures struck at Calais. (See Fig. 2.)

The further question as to the value of these fields may be answered by the amount of coal in the fields which are now being worked in Westphalia, Belgium, France, and Somersetshire. The Westphalian coal-field contains 294 feet of workable coal, distributed in 117 seams; that of Mons, 250 feet, in 110 seams; and that of Somerset, 98 feet, in 55 seams. The North French coal-field in 1887 yielded 7,119,633 tons, and gave employment at the pits to 29,000 men, and is rapidly increasing its output.

It may be inferred that the buried coal-fields which await the explorer in the North Downs are in all probability not inferior to these. Godwin-Austen, in his memorable paper before the Geological Society, in 1855, said that if one of these buried fields had once been struck in South-eastern England, their exploration would be an easy matter. It has been struck at Dover, and the necessary base is laid down for further discoveries, which in all probability will restore to South-eastern England the manufactures which have long since fled away to the coal districts of the West and North, and which will put off by many years the evil day when the energy stored up in the shape of coal in these islands shall have been spent.

[W. B. D.]

WEEKLY EVENING MEETING,

Friday, March 7, 1890.

Sir James Crichton Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

Francis Gotch, Esq. Hon. M.A. Oxon. B.A. B.Sc.

Electrical Relations of the Brain and Spinal Cord.

(Abstract.)

(1) The lecturer first described the anatomical structure of the nerve fibres and nerve cells found in the various parts of the mammalian nervous system. He then drew attention to the only physical indication of the passage of a nervous impulse along a nerve fibre, viz. the development in each successive portion of the nerve of an electrical effect. This electrical indication was then demonstrated to the audience by connecting the surface and cross-section of one portion of an isolated frog's nerve with the terminals of a sensitive reflecting galvanometer, and exciting a series of nerve impulses by applying rapidly recurring stimuli to a more distal portion of the same nerve.

(2) The anatomical plan and minute structure of the spinal cord and brain were then described, and the condition of the brain in different mammals was depicted. Special attention was drawn to the important recent additions which had been made to our knowledge of the course of nerve fibres through these complex organs, by the employment of histological methods which differentiated between degenerated and sound nerve fibres, and between partially and completely developed nerve fibres. Observations made along such lines had, it was pointed out, grouped together certain fibres as having common centres both of nutrition and of growth.

(3) The results of the investigations of the last ten years into the physiological relations of the brain and spinal cord were then referred to, and the extent of our knowledge of cerebral localisation determined. The indirect nature of the evidence as to the actual passage of nerve impulses in either direction along the nerve fibres composing the spinal cord was next alluded to—this evidence being the arrival of the nerve impulses at outlying muscles.

(4) Details were then given of the application of the method previously used to determine the electrical changes in the nerves in order to ascertain what changes of a similar kind were present in the spinal cord. Experiments made for the first time by V. Horsley and the lecturer were cited to show that such electrical effects were produced when (*a*) the so-called motor regions of the brain, (*b*) the columns in the spinal cord, and (*c*) the entering sensory spinal nerves were stimulated; and evidence was adduced to prove that the electrical effects thus obtained were true indications of the passage of nerve impulses along the nerve fibres in the particular region of the cord investigated.

(5) The physiological relations of the brain, spinal cord, and spinal nerves as determined by the newly discovered electrical relations of these organs were then touched upon; and a series of experimental investigations still in progress were referred to which seemed to warrant the belief that a basis had been reached for the construction of a scheme of physiological localisation in the fibres of the cord for both efferent (motor), and afferent (sensory) fibres, such as would be in harmony with the known anatomical relations of the central nervous system.

[F. G.]

WEEKLY EVENING MEETING,

Friday, April 18, 1890.

Sir Frederick Abel, C.B. D.C.L. F.R.S. Vice-President, in the Chair.

Sir Frederick Bramwell, Bart. D.C.L. M. Inst. C.E. F.R.S. *Hon. Sec.* and *V.P.R.I.*

Welding by Electricity.

There are certain technical words which relate to operations of a character so decisive, that the words have been adopted into ordinary language, such words as "grafting," "safety valve," "stereotyping," "welding." I dare say that every one who speaks of "grafting," or of a "safety valve," knows something of the operation or of the function of the apparatus of which he is speaking; perhaps he is not quite so clear about "stereotyping." But when it comes to "welding," I doubt whether many persons know what the term really means, as we engineers understand it, or could tell how many and what metals are capable of being united by welding; and I also doubt whether there are many who could distinguish between "fusing and burning together" and true "welding."

I do not myself know how to give a definition of "welding" as it is understood by practical men; but Dr. Percy did know, and if you will pardon me I will read a few lines from his book, which state more clearly than I could hope to do in words of my own, the meaning of welding, as ordinarily practised. He says:*—"Iron has one remarkable and very important property, namely, that of continuing soft and more or less pasty through a considerable range of temperature below its melting-point. It is sufficiently soft at a bright red-heat to admit of being forged with facility, as every one knows; and, at about a white-heat, it is so pasty that when two pieces at this temperature are pressed together they unite intimately and firmly. This is what occurs in the common process of *welding*. Generally metals seem to pass *quickly* from the solid to the liquid state, and so far from being pasty and cohesive at the temperature of incipient fusion, they are extremely brittle, and in some cases easily pulverisable. But, admitting that there is a particular temperature at which a metal becomes pasty, its range is so limited in the case of the common metals, that it would scarcely be possible to hit upon it with any certainty in practice; or, if it were possible, its duration would be too short for the performance of the

* Percy's 'Metallurgy,' Iron and Steel, 1864, pp. 5 and 6

necessary manipulations in welding." This, to my mind, is a concise and complete description of welding as we engineers understand it.

In the Arts, I suppose there are practically, only two easily weldable metals—iron (with its variant, steel, now so commonly substituted for it) and platinum. I had hoped to have practised before you to-night, welding in an ordinary fire, in order to show you the metals which could be welded by this means, and those which could not; and then to show you that metals which could not be welded by the ordinary fire could be easily welded by electricity. But I must ask you to take my word in respect to these matters because we have not room, in this extraordinary structure which has been put up to prevent the sparks flying about, for a fire-heating implement of any kind along with the workmen and the anvil; and therefore the idea had to be given up. I am sorry, for I had hoped there would have been time for all; but probably you have all seen the art of welding practised in the ordinary manner.

Now there are several kinds of welds, and I cannot do better than show you some of them as used in former days to weld the tyres of railway carriage and engine wheels. During the last quarter of a century such tyres have not been welded, but have been made in the circular or hoop form, without welding, still their former mode of manufacture will serve to illustrate the different kinds of welds. The most commonly used kind was that known as a scarf weld. In this the two portions of the tyre before being brought together were made with inclined surfaces. Preparatory to this being done the ends of the bar were thickened by beating them endways—technically known as "upsetting." Then they were "scarfed" or thinned down in a regular incline; the object of this was twofold: one to increase the amount of the surfaces brought into contact, and by thus magnifying these surfaces to increase the strength of the joint—the other to bring the two faces into a good position and shape for being operated upon by the hammer of the workman.

Another form of weld is that known as "a double-wedge weld"; in this case each end of the bar is cut to an obtuse double bevel, so that when the ends are brought together and laid upon the anvil, there are two V-shaped cavities,—one above the centre of the bar, and the other below it,—two separate wedge-shaped pieces are prepared to fill these cavities and the whole is brought to a welding heat. You can imagine that, if a bar thus prepared and fitted with the wedges is laid down on its edge, and is hammered upon its top edge, that these two separate wedge pieces will be forced into the cavities in the ends of the bar, and a weld will thus be made. But in the later days of the manufacture of tyres by welding, at any rate for passenger carriage wheels, the weld was made by what is called a "butt" weld. In this case the ends of the bar were cut perfectly square, were put into the fire, having a screwed clamp placed round about the tyre, and, being heated to the welding heat, the pressure of the screw was exerted, and one end of the bar was forced against the other, with the result that the surfaces were welded and that there was made a projection all round the weld (owing to the plastic condition of the metal), which projection was afterwards beaten down on the anvil. That is the kind of weld that you will see practised to-night electrically. I bring the matter forward now to show you that a butt weld is old in itself.

Now the heating of these pieces of metal was done in a "smith's fire," and the smith's fire, as made in London with Newcastle coal, was really a work of art. The smith succeeded in building up a perfect "grotto" of small coal, and coal dust, beaten together and moistened. In this grotto the thing to be welded could be put. The object of that was as far as possible to obtain the heat, while preventing the introduction of dirt between the surfaces to be united; for one of the greatest difficulties in welding is that there is a danger of foreign matter being introduced between the surfaces, thus preventing a good union being effected. In this way the heating and welding were done in former times, and when proper care and skill were exercised the welds were extremely good. To my mind, there is no more interesting work than that of the smith. It is one of the few things left, in which skill of eye and hand, and the intuitive knowledge born of experience, are all that the man has to trust to, and in which the result of the work is in no wise due to dies and moulds, which have in some other departments of handicraft pretty well superseded the skill of the man. It has always been to me, and is still, a source of pleasure, to see a smith at work. I must qualify this remark about the use of dies a little, because there are those present who know that we do in these days, even in smiths' work, use dies which get rid to some extent of the necessity for his skilled labour.

It is obvious that in all welds where the heat is obtained from the ordinary fire the metal must be heated from the outside. Under that condition you are sometimes subjected to the difficulty which at times occurs in the unskilful cooking of a joint of meat, the outside being burnt, while the inside is raw. But this difficulty is, you will find, entirely obviated in the case of electric welding.

Now, the desiderata in heating for welding are—uniformity of heating throughout the sectional area of the metal, regulation of the heat, freedom from the possibility of introduction of dirt, arising either from particles of fuel or from the presence of sulphur in the coal, or any matter of that sort; and also, facility of inspection during heating. This last point is of great importance, because in the ordinary method of welding by heating in a forge, the work has to be frequently taken out to see how the heat is progressing, and in this taking it out, and in putting it back, the risk is run of doing that which you wish to avoid, i. e. introducing dirt between the surfaces.

Probably the majority of the present audience are aware, that the heating effect of an electric current depends upon the quantity of that current and not upon its pressure, or voltage, or, to use the common term, its electromotive force. It also depends upon the electrical resistance of the material through which the current is passing. This resistance is very different in various substances, and varies even in the same substance under varying conditions, as I shall show you hereafter.

You have all seen over and over again the experiment of heating a wire which seemed, before heating, to be the same from end to end, that is to say, all of one diameter and of one appearance; but on passing an electric current through it you found that it was made up of two different materials; for while it was white-hot in alternate sections, it was dull in the intermediate portions. Those parts which remained dull did so because they were made of a metal (probably of Silver) which allowed the electric current to pass without much opposition, as compared with the parts which glowed, these being probably of Platinum, this metal offering a greater resistance to the current, and thus generating greater heat. I have placed in the "jaws" or "holders" of the electric welding machine before you, a compound wire made of a length of copper, a length of iron, and a length of German silver, or, as a matter of fact, the German silver is between the other two, all being of equal diameter. On passing the electric current through, I trust you will find that the German silver becomes hotter than the iron, because the resistance of an equal sectional area of it is in round numbers double that of iron, and that the copper does not apparently become hot at all, because its resistance is only one-sixth that of the iron, or one-twelfth that of German silver; in stating these proportions I am referring to ordinary atmospheric temperature, for when metals are heated an entirely new set of resistances come into play, these varying considerably with variations in their temperature. That upper arch is the German silver. It is, as you see, very hot; the left-hand portion is less hot, and the right-hand part is apparently unheated. Those metals are, as I have said, iron, German silver, copper.

I told you just now that the electrical resistance of metals and of other bodies alters with their temperature. This alteration is different with different metals; but in the case of the metal with which I am concerned to-night—iron—the variation is very considerable. Many persons have studied this question of the changes of resistance due to the increase of temperature, and among them Dr. Hopkinson. He has kindly furnished me with the results of his experiments, and I exhibit them to you on this diagram in the form of a curve.

From this we find that if the resistance of iron at 32° Fahr. be taken as unity, at 1832° Fahr. the resistance has gone up to over eleven times. You see the way in which it rises, and the peculiar kink there is in the curve at about 1400° or 1500°, when the resistance is about ten times what it is at 32°. This fact of the large increase of electrical resistance with the increase of temperature is a matter of the utmost importance in welding by electricity, as I hope to show you later on.

I have said that the heating effect of an electric current depends upon the quantity of the current, and upon the drop or reduction in electrical pressure. We have four lamps here upon the table, and I think you will see when I turn on the current they are glowing uniformly; not giving much light, however, for they are glowing very badly, but all of them uniformly bad, no one better than the other. Now I will ask you to remember that if we are introducing a given quantity of electric current here, at the first lamp, at a pressure of say 100 volts, and it is leaving the fourth lamp at zero, we are introducing the same quantity of electricity here at the second lamp as at the first, but the pressure is only 75 volts, we have therefore dropped 25 volts between the two. We are also introducing the same quantity of current to the third lamp, but at 50 volts, we therefore drop another 25 volts. We are introducing it to the fourth lamp at 25, and using this pressure up in this lamp, we come down to zero. What we have done is this: we have destroyed an equal amount of electrical energy in every lamp by these reductions, and have turned it into heat, making the lamps glow, and it is, as you will have seen, a matter of absolute indifference as regards heating effect, whether we have done this by taking 25 out of 100 and leaving 75, or by taking 25 out of 25 leaving zero. If we change the switch and throw one of the lamps out of circuit, we have now the same initial electrical pressure, i. e. 100 volts, but there are only three resistances instead of four, and we are consequently now dropping by 33 volts at each lamp. You observe the increase in brightness, but still the three lamps all glow alike. If we switch another lamp out we have only two lamps' resistance to overcome, and are dropping by 50 instead of by 33 volts. We therefore get a further increase of brightness. We take the third lamp out, and may thus destroy the one remaining, for we now have the whole drop of 100 volts occurring at this one lamp, and you see the intense glow that results, although fortunately it has not given way.

But the increases in the heating effect have not varied in the mere ratios of 25 to $33\frac{1}{3}$, 25 to 50, or 25 to 100; but have varied as the squares of these ratios, and have done so for this very simple reason. During our experiment, we have always commenced with the same electrical pressure of 100 volts; but when we used only three lamps in the circuit, instead of the four which I first showed you, the resistance was only that of the three lamps, i. e. three-fourths of the four, but the pressure being the same the current became four-third times that which it originally was when each of the four lamps was used. The drop in voltage was $33\frac{1}{3}$ volts for each lamp, instead of 25, that is to say, this was also four-third times as much as before. The disappearance of electrical energy was therefore four-third times four-thirds = sixteen-ninths, or, in other words, each of the three lamps was heated to $1\frac{7}{9}$th times the heat which was generated in each one of the four lamps. When we had only two lamps in the circuit, the resistance was one-half that which it was when we had the four, thus the current was doubled, the drop of pressure per lamp was also doubled, giving, therefore, the double of the double, or four times the heating effect; while when we had only one lamp in the circuit the current and the drop were each four times as great as when the four lamps were in, so that in this case we had four times four, or sixteen times the heat generated.

Here is another way of showing this effect. We have in this machine two hoops, side by side and shaped like small croquet hoops. They are made of similar iron wire, but one is double the length of the other. The electrical pressure being the same at the two ends of each of the two hoops, we shall have quantities of currents coming through which will be in proportion to the lengths of these hoops. That is to say, you will have half the current coming through the long one which comes through the short one. The short one therefore ought to glow more brightly, because it will be hotter than the long one. Now I turn on the current and the short one begins to glow, but you cannot see any light at present from the long one. Now I can see a feeble light appearing in the long one, but probably those who are not so near as I am cannot do so yet. I am sorry that we have not sufficient horse-power in our engine here to enable us to do that which I did at the Institution of Civil Engineers—to go on with an increased current till the short one is fused. Now the long one is glowing fairly, and the short hoop is very bright indeed. There you see an instance where, the electric potential being the same at each end of two bars, but one being double the length of the other, and carrying therefore only half the amount of current, shows hardly any light at all, while the other has a considerable amount of luminosity.

It is obvious that if we were using a perfect conductor of electricity we could have no electric heating whatever, because a perfect conductor would not destroy any of the electrical energy of the current passing through it, and, therefore, no heat would be produced. It is equally obvious that if we had substances absolutely impermeable to electricity, so that no current could pass through it, we could not heat such a substance. What we want, therefore, is something between the two. Fortunately for us, both iron and steel hold a very happy position in respect of their electrical conducting power, or to use the converse term which I have hitherto employed—their resistance. At the ordinary temperature of 60° Fahr. a piece of wrought iron, 1 foot long and 1 square inch in section, would need half a volt to drive 10,000 amperes through it, and in doing this 3700 foot-pounds of electrical energy would be destroyed in every second of time, equal, therefore, to the production of $4\frac{3}{4}$ units of heat in the conductor. If we had a similar length and area of German silver, as it is so much worse as a conductor, we should in the same time destroy rather more than double the foot-pounds of electrical energy, namely, 7700. This is equivalent to the production of a little over ten units of heat in each second. A similar length and area of silver would, however, destroy only some 515 foot-pounds of electrical energy in the same time, giving only two-thirds of a unit of heat. Iron (which is nearly as good a conductor as silver) is therefore, as you will see, happily placed between silver and copper on the one hand, and German silver on the other.

It is extremely likely that at the temperature at which welding can be performed, the resistance of iron to the passage of an electric current is increased to very much more than eleven-fold that which it had at 32° Fahr., because, probably, the welding temperature is about 3000° Fahr., while, as we saw from Dr. Hopkinson's curve, at 1532° Fahr., we get eleven times the resistance there is at freezing-point. But assume the electrical resistance to be increased only to eleven times that which the iron had when cold. What follows? Why this: that, a piece of iron 1 foot long, and having a section of 1 square inch, would, under these circumstances, destroy in a second of time 40,700 foot-pounds of electrical energy. But, as you see, the bar which is being heated, is much shorter, than a foot. It is only about 2 inches, and thus it only destroys about one-sixth of this, or about 6600 foot-pounds of electrical energy per second of time, equal to about nine units of heat, or a little more. But the specific heat of wrought iron being only ·11379—water, as you know, being unity—these ten units would raise one pound weight of iron 90 degrees in each second. But the portion heated up is only about two-thirds of a pound, and it would be heated, therefore, 135° Fahr. each second; but, as I have told you, as the temperature increases, the resistance, and therefore the heating effect, increases In a lengthened trial with a machine dealing with pieces of good bar iron having a sectional area of about 1 square inch, the maximum heat developed per second of time was 18 units, and the welding heat was reached in 22 seconds.

Now I shall have to refer, as an illustration of electrical phenomena, to a very old friend for this purpose, viz. water. Suppose it is a question of working a hydraulic lift, or anything of that kind. If you have 100 gallons of water, multiplied by 50 lbs. of pressure, you get 5000 gallon-pounds. If you multiply 50 gallons by 100 lbs. of pressure, you equally get 5000 gallon-pounds. Similarly, if you multiply 100 amperes of electrical current by 50 volts of pressure of electrical current, you get 5000 watts, which is the equivalent in this illustration of the gallon-pounds of the water; and if you multiply 50 amperes by 100 volts, you equally get 5000 watts. From what I have told you as to the resistance of metals, it is clear that for welding purposes we want the electrical energy in the form of large quantity and of low pressure. So that if I have at my disposal a total energy of 5,000,000 watts, it may for some purposes suit me to have it in the form of 1000 amperes of quantity by 5000 volts of pressure, but for welding I should undoubtedly prefer to have it in the form of larger quantity and low pressure, say, 5,000,000 amperes of quantity by 1 volt of pressure; but whatever the form may be, there are still 5,000,000 watts.

Now let us take another illustration, a monetary one. Suppose I wish to send five pounds of money by post; it would obviously be best that I should send it in the form of a *5l.* note. It weighs less than five sovereigns, and takes up less room. Take this as an illustration of the greatest "pressure" and the least "quantity." Suppose, however, I want to give 100 Sunday school children each a shilling, I do not want either a *5l.* note, or even five sovereigns; I want 100 shillings. I should have then a comparatively great weight, which it would not be so convenient to send by post, but which is, however, in a suitable form for my purpose of distribution to the children. This is my five pounds sterling in the form illustrating large quantity and low pressure.

Similarly in the case of electricity. Depending upon the construction of the dynamo, and upon its velocity of revolution, you can produce your electricity either in the form of high voltage and of small quantity, or, if the construction is varied, of low voltage and of large quantity—either the *5l.* note or the shillings. But if you produce it in the condition of large quantity and of low pressure, and you desire to transmit it to the smith for him to use it in welding machines, you will find that form of current to be very inconvenient, because it clearly involves the employment of conductors so ample in sectional area as to admit of all this large quantity being brought through them to the iron, to heat it up to the welding point, without the waste of electrical energy due to useless heating up of the conductors themselves. The conductors must therefore be very large, and of very excellent conducting material. Therefore it is desirable to produce the electricity in the *5l.* note form in the first instance, and convert it into the shilling form, or it may be even into the form of farthings, after it has been transported to the very machine in which it is to be used.

Now how is this change to be made? Who is to be our money changer? Who is to change the electricity of small quantity and of high pressure into that of large quantity and low pressure? Some half century ago Ruhmkorff invented the coil by which, as you have all seen, the low potential or pressure of a few cells of a battery, incapable of making an appreciable spark, is translated into small quantity and large voltage, capable of leaping through considerable distances. We have before us here, at my right hand, a Ruhmkorff coil, which contains 70 yards of primary wire, weighing 6 lbs. The secondary coil is 8 miles in length, and weighs 12 lbs. The sectional areas of these two wires are as 100 to 1. We have a battery here of five cells. We will put it to work, in conjunction with the coil, in the first instance simply to give us a spark. You will see now it has converted the low potential of the five cells into that which is capable of leaping across a space of $1\frac{1}{4}$ inches, and you can, with suitable arrangements, get results a great deal higher than that. You will see that there we have transformed our 100 shillings into a 5*l.* note. We are now about to transport the 5*l.* note across these wires [they might have been even finer], and we are putting this current into the secondary coil of another Ruhmkorff coil. We shall by this arrangement change the 5*l.* note into the 100 shillings again, and deliver them through that little incandescent lamp, which will glow, because we have lowered the pressure, and thereby increased the quantity sufficiently to enable us to get a current suitable for that purpose. This lamp, which is glowing, is, as you can see, on the 100 shilling circuit; the other lamp, to which I should have previously called your attention, is on the 5*l.* note circuit, and although the same number of watts are passing through, it is dull, and does not glow. Similarly, if we had put a fine wire from the secondary coil of the first Ruhmkorff to the secondary coil of the second Ruhmkorff, that fine wire would have remained absolutely cold. We have taken a piece of the same wire, and put it in position there; as you will see when I turn on the current, I can melt it. That shows the different effect for heating purposes of a large or of a small quantity of current, and shows, too, that you can vary your electricity, so as to have it in whichever form you please.

And now, I think, the time has come to describe and to show you the electrical welding machine itself in work. Unhappily, we have not sufficient power at our command in this building to work the large machine. We can only work the second sized one. The drawings that I have here are drawings of the large machine; but if you will allow me, I will describe the machine from the machine itself.

This is the machine, and in these two pairs of jaws are fixed the pieces to be welded together: in this case pieces of wire rope. The pieces of rope are grasped by the two jaws, which are made of gun-metal. They are in electrical communication with two conductors, and these two conductors are the terminals of a hollow copper core that passes through coils at the back of the machine, these coils being similar to those of a Ruhmkorff coil. Inside these we have a cylinder built up of a number of sheet-iron discs insulated one from another, and round about these there go 70 convolutions of wire, and therefore of the current from the dynamo. That is, the 5*l.* note form of electricity passes from the dynamo through these 70 convolutions of wire. But by the time the current has passed through, the pressure has exhausted itself in producing the shilling state in the copper core and in the conductors connected with it. One of the jaws is movable, and can be forced forward by this screw arrangement; the other jaw is fixed, and forms the abutment. This other apparatus is one by which the strength of the current can be regulated as desired by the operator; it consists of a number of lengths of wire, the regulation of the current being performed by the operator switching into or out of the circuit of wire (through which the 5*l.* note current flows to the machine), a greater or less number of these lengths, thus delivering to the welding machine a varying amount of electrical energy for it to convert into the welding current.

I have already, when dealing with the weld itself, called your attention to the important part played by the increase in electrical resistance due to increase in temperature, but I now wish you to see how valuable this increase of resistance is when considered in relation to the question of obtaining uniformity of temperature over the whole of the surfaces to be united.

These surfaces, when first brought into contact, are rough, and thus only a very small portion of them—that is to say, the extreme prominences—come together, and, as a consequence, the current which is passing is confined to these points. As these become heated, however, by the passage of the current, they soften; the continued pressure applied by means of the screw flattens them, and thus enlarges the area of contact. But, as I have shown you, the hottest parts are the worst conductors, and thus the greater quantity of electricity passes through the less hot parts of the enlarged area of contact, raises their temperature, and the flattening of these by the continued pressure causes further surfaces to come together, till all are in contact; while the current, still seeking out the coolest parts as offering less resistance to its passage, raises their temperature until a uniform heat and a uniform resistance are established, and then this heating goes on still increasing as the current is continued, in consequence of the increased electrical resistance due to the increased temperature.

I think you will agree with me that this increase of electrical resistance, which follows from the rise of temperature, is most valuable in enabling the necessary welding heat to be produced by the passage of an electric current.

You will observe presently, when we work the machine, that the careful operator, in order to avoid burning the small surface of the prominences which first come into contact, takes great care to apply the current very gently.

I will now ask to have a piece of cast steel welded to another piece of cast steel. I should explain that the machine has been put on to a turn-table, with the object of moving it round to face different parts of the room, so as to afford a better view to the audience. I do not know how many of you are aware of the difficulty of welding cast steel as compared with the welding of iron. This difficulty depends largely on the amount of carbon in the steel. An extremely mild steel, such as is used for gun making, is easily weldable, containing as it does only a very low percentage of carbon; while tool steel, containing over one per cent. of carbon, presents great welding difficulties. We have here two pieces of tool steel, which it would be almost impossible to weld in the ordinary manner. The humming noise which you hear when the current is turned on to the machine is produced by our coil (our money changer) at work.

I may tell you that I shall have to talk about a number of welds made at Fanshawe Street by two electricians, who, not being smiths, of course worked at a disadvantage; but the competent smith who is now before you was then taken to the machine, and at the fourth attempt he made a satisfactory weld, and has been working the machine ever since. You see that the two pieces of steel have been welded together, and then bent at an acute angle, thus showing roughly the satisfactory nature of the weld. As time is running short, I will ask the smith next to show you the welding of a tube. These are two pieces of ordinary steel tube which will now be welded in the machine. I do not know whether I can manage to direct this machine so as to be seen by every individual in the room; but I hope that I shall succeed in enabling most of you to see completely through the tube when it has been welded, and thus to show you that the internal circularity of the tube has in no way suffered by the welding.

Though I believe that the function of this machine will not be the performance of ordinary work, but that it will be applied to welding difficult sections or difficult metals, I thought it well to have it tested, by making 80 welds in $1\frac{1}{8}$th inch round iron bars. These were made, as I have said, by two electricians, not smiths, and they took an average of $2\frac{1}{4}$ minutes to make each weld, equal to 135 seconds, and the time was roughly divided thus:—Fixing the iron and heating up to full heat at one operation, 26 seconds; full heat to taking out of jaws, 11 seconds; work on anvil, 15 seconds; re-putting in to full hot, 21 seconds; full hot to taking out again, 10 seconds; retaking out to completion, 32 seconds; completion and putting in next piece, 20 seconds; making a total, as I have said, of 135 seconds. Then two smiths were put to make welds in the ordinary manner, that is to say, scarf welds in an ordinary smith's fire. They made 44 welds in three hours and a little over, or practically the same time as was taken in making the 80 welds (nearly twice the number) in the machine. Then all the welds were sent to a well-known tester of metals, Mr. Kirkaldy, and he tested about one-half of them, with the following results, which I think it important you should hear:—

RESULTS OF MR. KIRKALDY'S TESTS.

	Per Square Inch of Original Sectional Area.
Average strength of the bars before welding	52,642 lbs.
Average strength of those which broke at the weld when welded electrically	48,215 „
Average strength of those which broke at the weld when the welds were made by hand	46,899 „

From these tests we thus have a right to say that this electrical butt welding gives at least an equal tensile strength with the scarf welding done by hand.

It is probable, as I have said, that the great value of this invention will not be for common work, but for difficult sections and for refractory metals. I have shown you the butt welding of wire rope and could show you work done on T-iron and on various other forms of metal. But there is not time to do so, and I, of all men, should not offend by exceeding the allotted hour, for if I did I could not as Secretary, call others to account. The most difficult metals can be dealt with in this machine. You will remember Dr. Percy tells us that the great difficulty in welding most metals is to find out the critical point of temperature and to maintain it. I wish now to prove to you that the current is under absolute control in the machine, and the object to be welded being under continuous observation during the operation, one is enabled to deal with any one of the refractory metals in the required way, and so to get a union, by bringing the surfaces into the necessary pasty condition of temperature and at the right moment to operate upon them.

We have here a much smaller welder, which is automatic in its action. It is intended for welding together pieces of wire, &c. By the time the two portions are sufficiently heated and are pressed together, the machine is automatically thrown out of gear, and the operation is completed. Two pieces of copper wire are in now, and, if you observe the machine you will see that when the work is done, it of itself, stops the current. There is the welded wire, and I think you will admit that the work is good; for you see I cannot by bending it backwards and forwards break it at the weld. Now we will try a piece of aluminium. That is commonly supposed to be a very difficult metal to unite, but this machine will do it easily. Here it is, welded, and you see that it is perfectly competent to be bent without breaking at the weld. And now a piece of German silver, this being the metal which, you will remember, gave such a high resistance when we used it before.

I regret that time does not admit of my showing you other different kinds of work done in the machines, and that I must bring my remarks to a conclusion by saying:—I think it is obvious that a machine which gives us this power of heating any metal, with absolute control over the heat, and that affords such thorough facility for inspecting the work during the heating, must have many uses in the Arts. Indeed, there can be no doubt that the existence of such a machine will of itself give rise to a large number of new uses.

[F. B.]

GENERAL MONTHLY MEETING,

Monday, July 7, 1890.

Sir James Crichton Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

Thomas Townsend Bucknill, Esq. Q.C.
Edward A. Harvey, Esq.
Malcolm Morris, Esq. F.R.C.S.
William Thomas Rabbits, Esq. F.L.S.

were elected Members of the Royal Institution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz:—

FROM

The Governor-General of India—Geological Survey of India: Records, Vol. XXIII. Part 2. 4to. 1890.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta: Rendiconti. 1° Semestre, Vol. VI. Fasc. 7. 8vo. 1890.
Academy of Natural Sciences. Philadelphia—Proceedings, 1890, Part 3. 8vo.
American Philosophical Society—Transactions, Vol. XVI. Part 3. 4to. 1890.
Astronomical Society, Royal—Monthly Notices, Vol. L. No. 7. 8vo. 1890.
Bankers, Institute of—Journal, Vol. XI. Part 6. 8vo. 1890.
Bischoffsheim, M. R. L.—Annales de l'Observatoire de Nice. Tome III. Text and Atlas. 4to and fol. 1890.
British Architects, Royal Institute of—Proceedings, 1889–90, Nos. 16, 17. 4to.
Chemical Society—Journal for June, 1890. 8vo.
Civil Engineers' Institution—Proceedings, Vol. C. 8vo. 1890.
Cornwall Polytechnic Society, Royal—Annual Report for 1889. 8vo. 1890.
Cracovie, l'Academie des Sciences—Bulletin, 1890, No. 5. 8vo.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I.—Journal of the Royal Microscopical Society, 1890, Part 3. 8vo.
East India Association—Journal, Vol. XXII. No. 2. 8vo. 1889.
Editors—American Journal of Science for June, 1890. 8vo.
Analyst for June, 1890. 8vo.
Athenæum for June, 1890. 4to.
Brewers' Journal for June, 1890. 4to.
Chemical News for June, 1890. 4to.
Chemist and Druggist for June, 1890. 8vo.
Electrical Engineer for June, 1890. fol.
Engineer for June, 1890. fol.
Engineering for June, 1890. fol.
Horological Journal for June, 1890. 8vo.
Industries for June, 1890. fol.
Iron for June, 1890. 4to.
Ironmongery for June, 1890. 4to.
Murray's Magazine for June, 1890. 8vo.
Nature for June, 1890. 4to.
Photographic News for June, 1890. 8vo.
Revue Scientifique for June, 1890. 4to.
Telegraphic Journal for June, 1890. fol.
Zoophilist for June, 1890. 4to.

Electrical Engineers, Institution of—Journal, No. 87. 8vo. 1890.
Florence Biblioteca Nazionale Centrale—Bolletino, Nos. 107, 108. 8vo. 1890.
Franklin Institute—Journal, No. 774. 8vo. 1890.
Geographical Society, Royal—Proceedings, New Series, Vol. XII. Nos. 6, 7. 8vo. 1890.
Geological Institute, Imperial, Vienna—Abhandlungen, Band XV. Heft 2. fol. 1890.
Johns Hopkins University—University Circulars, No. 81. 4to. 1890.
American Chemical Journal, Vol. XI. No. 7. 8vo. 1889.
American Journal of Philology, Vol. X. Nos. 2, 3. 8vo. 1889.
Studies in Historical and Political Science, 7th Series, Nos. 21, 22. 8vo. 1889.
Annual Report. 8vo. 1889.
Laboratory Club—Transactions, Vol. III. No. 6. 8vo. 1890.
Linnean Society—Proceedings, May, 1890. 8vo.
Manchester Geological Society—Transactions, Vol. XX. Parts 18, 19. 8vo. 1890.
Mechanical Engineers' Institution—Proceedings, 1890, No. 1. 8vo.
Meteorological Office—Weekly Weather Reports, Nos. 22–26. 4to. 1890.
Ministry of Public Works, Rome—Giornale del Genio Civile, Seria Quinta, Vol. IV. Nos. 2, 3. And Disegni. fol. 1890.
New York Academy of Sciences—Transactions, Vol. IX. Parts, 1, 2. 8vo. 1890.
Annals, Vol. XV. Nos. 1, 2, 3. 8vo. 1889.
Norwegian North Atlantic Expedition, Editorial Committee—Danielssen, D.C. Actinida, Part 19. fol. 1890.
Odontological Society of Great Britain—Transactions, Vol. XXII. No. 7. New Series. 8vo. 1890.
Pennsylvania Geological Survey—Atlases, A.A. Parts 2–4. 8vo. 1889.
Pharmaceutical Society of Great Britain—Journal, June, 1890. 8vo.
Popoff, Constantine, Esq. (the Translator)—Boyhood, Adolescence, and Youth. By L. Tolstoi. 8vo. 1890.
Rathbone, E. P. Esq. (the Editor)—The Witwatersrand Mining and Metallurgical Review, No. 5. 8vo. 1890.
Richardson, B. W. (the Author)—The Asclepiad, No. 26. 8vo. 1890.
Rio de Janeiro Observatory—Revista, No. 5. 8vo. 1890.
Royal Society of London—Proceedings, Nos. 289, 290. 8vo. 1890.
Selborne Society—Nature Notes, Vol. I. No. 6. 8vo. 1890.
Smithsonian Institution—Contributions to Knowledge, Vol. XXVI. fol. 1890.
Society of Architects—Proceedings, Vol. II. No. 11. 8vo. 1890.
Society of Arts—Journal for June, 1890. 8vo.
St. Pétersbourg Académie Impériales des Sciences—Mémoires, Tome XXXVII. Nos. 6, 7. 4to. 1890.
Vereins zur Beförderung des Gewerbfleises in Preussen—Verhandlungen, 1890: Heft 6. 4to.
Victoria Institute—Transactions, No. 93. 8vo. 1890.
Wisconsin Academy of Sciences, &c.—Transactions, Vol. VII. 1883-7. 8vo. 1889.
Zoological Society of London—Proceedings, 1890, Part I. 8vo.

GENERAL MONTHLY MEETING,

Monday, November 3, 1890.

SIR JAMES CRICHTON BROWNE, M.D. LL.D. F.R.S. Treasurer and Vice-President in the Chair.

J. Viriamu Jones, Esq. M.A.
C. N. Nicholson, Esq. M.A.
John Hartley Perks, Esq. J.P.
The Hon. Sir James Stirling (*Justice of the Supreme Court*),

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to Dr. J. A Fleming, *M.R.I.* for the presentation on behalf of Professor Elihu Thomson of the apparatus employed by Professor Thomson in his experiments on Electro-magnetic Repulsion; and to Hervey Pechell, Esq. *M.R.I.* for his present of an old engraving (1809) of the Library of the Royal Institution.

The Managers reported, That at their Meeting held this day they had elected Victor Horsley, Esq. F.R.S. *M.R.I.* Fullerian Professor of Physiology for three years (the appointment dating from January 12, 1891).

The Managers further reported, That the Royal Institution was now connected with the National Telephone Company's Exchange System, No. 3669.

THE PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Governor-General of India—Geological Survey of India: Records, Vol. XXIII. Part 3. 4to. 1890.
Memoirs, Vol. XXIV. Part 2. 4to. 1890.
Madras Government—South Indian Inscriptions. By E. Hultzsch. Vol. I. fol. 1890.
Academy of Natural Sciences, Philadelphia—Proceedings, 1890, Part 1. 8vo.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta: Rendiconti. 1° Semestre, Vol. VI. Fasc. 8–11. 8vo. 1890.
Atti, Anno 42, Sess. 4–7. 4to. 1889.
Memorie, Vol. V. 4to. 1888.
Agricultural Society of England, Royal—Journal, 3rd Series, Vol. I. Parts 2, 3. 8vo. 1890.
General Index, Vol. I.-XXV. 8vo. 1890.
American Academy of Arts and Sciences—Proceedings, Vol. XXIV. 8vo. 1889.
American Association for the Advancement of Science—Proceedings, 38th Meeting held at Toronto, 1889. 8vo. 1890.
American Philosophical Society—Proceedings, Nos. 131–133. 8vo. 1890.
Antiquaries, Society of—Proceedings, Vol. XIII. No. 1. 8vo. 1890.
Index to Archæologia, Vols. I.-L. fol. 1889.

Aristotelian Society—Proceedings, Vol. I. No. 3, Part 2. 8vo. 1890.
Asiatic Society of Bengal—Journal, Vol. LVII. Part 2, No. 5; Vol. LVIII. Part 1, Supplement; Vol. LIX. Part 1, Nos. 1, 2; Part 2, Nos. 1, 2. 8vo. 1890.
Proceedings, 1890, Parts 1–3. 8vo.
Astronomical Society, Royal—Monthly Notices, Vol. L. No. 8. 8vo. 1890.
Memoirs, Vol. XLIX. Part 2. 4to. 1890.
Bankers, Institute of—Journal, Vol. XI. Parts 7, 8. 8vo. 1890.
Barnard, Frank, Esq. (the Author)—Picturesque Life in Shetland. fol. 1890.
Bavarian Academy of Sciences—Abhandlungen, Band XVII. Abth. 1. 4to. 1890.
Sitzungsberichte, 1890, Heft 1–3. 8vo.
Bell and Sons, Messrs. G. (the Publishers)—School Calendar, 1890. 12mo.
Bischoffsheim, M. R. L.—Annales de l'Observatoire de Nice. Tome II. 4to. 1887.
British Architects, Royal Institute of—Proceedings, 1889–90, Nos. 18-20; 1890–1, No. 1. 4to.
British Museum—Catalogue of Oriental Coins, Vol. IX. (1876–88). 8vo. 1889.
Catalogue of the Cuneiform Tablets. By C. Bezold. Vol. I. 8vo. 1889.
British Museum (Natural History)—Catalogue of Birds, Vols. XIII. XV. XVIII. 8vo. 1890.
Catalogue of Fossil Reptilia and Amphibia, Part 4. 8vo. 1890.
Guide to Galleries of Geology and Palæontology, Parts 1 and 2. 8vo. 1890.
Buckton, G. B. F.R.S. M.R.I. (the Author)—Monograph of the British Cicadæ or Tettigidæ, Parts 3, 4. 8vo. 1890.
California, University of—Publications, 1888–90. 8vo.
Cambridge University Press, Syndics of—Scientific Papers of James Clerk Maxwell. Edited by W. D. Niven. 2 vols. fol. 1890.
Canadian Institute—Proceedings, 3rd Series, Vol. VII. Fas. 2. 8vo. 1890.
Chemical Industry, Society of—Journal, Vol. IX. Nos. 6–9. 8vo. 1890.
Chemical Society—Journal for July to October, 1890. 8vo.
Chief Signal Officer, U.S. Army—Annual Report for 1889. 8vo. 1890.
Civil Engineers' Institution—Proceedings, Vol. CI. CII. 8vo. 1890.
City of London College—Calendar, 1890–91. 8vo. 1890.
Clinical Society—Transactions, Vol. XXIII. 8vo. 1890.
Colonial Institute, Royal—Proceedings, Vol. XXI. 8vo. 1890.
Cracovie, l'Academie des Sciences—Bulletin, 1890, Nos. 6, 7. 8vo.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I.—Journal of the Royal Microscopical Society, 1890, Parts 4, 5. 8vo.
Devonshire Association for Advancement of Science, Literature, and Art—Report and Transactions, Vol. XXII. 8vo. 1890.
Devonshire Domesday, Part VII. 8vo. 1890.
East India Association—Journal, Vol. XXII. Nos. 3, 4, 5. 8vo. 1889.
Editors—American Journal of Science for July–October, 1890. 8vo.
Analyst for July–October, 1890. 8vo.
Athenæum for July–October, 1890. 4to.
Brewers' Journal for July–October, 1890. 4to.
Chemical News for July–October, 1890. 4to.
Chemist and Druggist for July–October, 1890. 8vo.
Electrical Engineer for July–October, 1890. fol.
Engineer for July–October, 1890. fol.
Engineering for July–October, 1890. fol.
Horological Journal for July–October, 1890. 8vo.
Industries for July–October, 1890. fol.
Iron for July–October, 1890. 4to.
Ironmongery for July–October, 1890. 4to.
Murray's Magazine for July–October, 1890. 8vo.
Nature for July–October, 1890. 4to.
Open Court for July–October, 1890. 4to.
Photographic News for July–October, 1890. 8vo.
Public Health for July–October, 1890. 8vo.
Revue Scientifique for July–October, 1890. 4to.

Editors—cont.—Telegraphic Journal for July–October, 1890. fol.
Zoophilist for July–October, 1890. 4to.
Edmunds, Lewis, Esq. D.Sc. F.C.S. M.R.I. (*the Author*)—Law and Practice of Letters Patent for Inventions. 8vo. 1890.
Electrical Engineers, Institution of—Journal, No. 89. 8vo. 1890.
Engineering, The Editor of—Metallurgy of Silver, Gold, &c. in United States. By T. Egleston. Vol. II. 4to. 1890.
Fleming, J. A. Esq. M.I.E.E. M.R.I. (*the Author*)—Electrical Papers. 8vo. 1874–89.
Alternate Current Transformer, Vol. I. 8vo. 1889.
Florence Biblioteca Nazionale Centrale—Bolletino, Nos. 109–116. 8vo. 1890.
Franklin Institute—Journal, Nos. 775–778. 8vo. 1890.
Geographical Society, Royal—Proceedings, New Series, Vol. XII. Nos. 8–10. 8vo. 1890.
Supplementary Papers, Vol. III. Part 1. 8vo. 1890.
Geological Institute, Imperial, Vienna—Verhandlungen, 1890, Nos. 6–9. 8vo.
Geological Society—Quarterly Journal, No. 183. 8vo. 1890.
Goppelsroeder, Dr. F. (*the Author*)—Ueber Feuerbestattung. 8vo. 1890.
Harlem, Société Hollandaise des Sciences—Archives Néerlandaises, Tome XXIV. Liv. 2, 3. 8vo. 1890.
Horticultural Society, Royal—Journal, Vol. XII. No. 2. 8vo. 1890.
Iowa Laboratories of Natural History—Bulletin, Vol. I. Nos. 3 and 4. 8vo. 1890.
Iron and Steel Institute—Journal for 1890, Vol. I. 8vo.
Johns Hopkins University—University Circulars, No. 82. 4to. 1890.
American Chemical Journal, Vol. XI. No. 8; Vol. XII. Nos. 1–5; General Index, Vols. I.–X. 8vo. 1889–90.
American Journal of Philology, Vol. X. No. 4; Vol. XI. No. 1. 8vo. 1889–90.
Studies in Historical and Political Science, 8th Series, Nos. 1–4. 8vo. 1890.
Kerslake, Thomas, Esq. (*the Author*)—Richard, the King of Englishmen. 8vo. 1890.
Laboratory Club—Transactions, Vol. III. No. 7. 8vo. 1890.
Langley, S. P. and F. W. Very—On the Cheapest Form of Light. 8vo. 1890.
Linnean Society—Journal, Nos. 124, 125, 145, 146, 175, 183, 184. 8vo. 1890.
Lisbon Academy of Sciences—Historia do Infante D. Duarte. Por J. Ramos Coelho. Tomo II. 8vo. 1890.
Madras Government Central Museum—Report, 1889–90. fol. 1890.
Maiden, J. H. Esq. F.L.S. F.C.S. (*the Author*)—Wattles and Wattle Barks. 8vo. 1890.
Manchester Literary and Philosophical Society—Memoirs and Proceedings, Vol. III. N.S. 8vo. 1890.
Manchester Steam Users' Association—Report on Red-hot Furnace Crown Experiments. 8vo. 1889.
Manila Universidad de Sto. Tomás—Discurso, Por F. J. M. Ruiz. 4to. 1890.
Mechanical Engineers' Institution—Proceedings, 1890, No. 2. 8vo.
Mensbrugghe, M. G. Van der (*the Author*)—La Surface Commune a deux Liquides, 1e and 2e Partie. 8vo. 1890.
Meteorological Office—Weekly Weather Reports, Nos. 27–43. 4to. 1890.
Quarterly Weather Report, No. 50. fol. 1890.
Variability of Temperature of British Isles, 1869–1883. By R. H. Scott. (Proceed. R.S.) 8vo. 1890.
Meteorological Society, Royal—Quarterly Journal, No. 75. 8vo. 1890.
Meteorological Record, Nos. 36, 37. 8vo. 1890.
Minister of Finance, Halifax—Dictionary of Languages of Micmac Indians. By Rev. S. Rand. 4to. 1888.
Ministry of Public Works, Rome—Giornale del Genio Civile, Seria Quinta, Vol. IV. Nos. 4, 5, 6. And Designi. fol. 1890.
Mitchell, C. Pitfield, Esq. M.R.I. (*the Author*)—The Philosophy of Tumour Disease. 8vo. 1890.
Musical Association—Proceedings, 16th Session, 1889–90. 8vo. 1890.
Odontological Society of Great Britain—Transactions, Vol. XXII. No. 8. New Series. 8vo. 1890.

New South Wales Agent-General—History of Progress of New South Wales. By T. Coghlan. 8vo. 1889.
North of England Institute of Mining and Mechanical Engineers—Report of Use of Explosives in Mines, Part 1. 8vo. 1890.
Norwegischen Commission der Europäischen Gradmessung—Geodätische Arbeiten, Heft 6 and 7. 4to. 1888–90.
Pharmaceutical Society of Great Britain—Journal, July–October, 1890. 8vo.
Photographic Society—Journal, Vol. XIV. No. 9; Vol. XV. No. 1. 8vo. 1890.
Preussische Akademie der Wissenschaften—Sitzungsberichte, Nos. I.–XIX. 8vo. 1890.
Rathbone, E. P. Esq. (*the Editor*)—The Witwatersrand Mining and Metallurgical Review, Nos. 6–9. 8vo. 1890.
Richardson, B. W. (*the Author*)—The Asclepiad, No. 27. 8vo. 1890.
Rio de Janeiro, Observatoire Impérial de—Annales, Tome IV. Parts 1, 2. fol. 1889.
Annuario, Tomes V. and VI. 12mo. 1889-90.
Revista, Nos. 6, 7, 8. 8vo. 1890.
Royal College of Surgeons in England—Calendar, 1890. 8vo.
Royal Dublin Society—Proceedings, Vol. VII. Parts 7–9. 8vo. 1890.
Royal Institution of Cornwall—Journal, Vol. X. No. 1. 8vo. 1890.
Royal Society of Antiquaries of Ireland—Journal, Vol. I. (5th Series), No. 2. 8vo. 1890.
Royal Society of Canada—Proceedings and Transactions, Vol. VII. 4to. 1889–90.
Royal Society of Edinburgh—Transactions, Vol. XXIII. Part 3; Vol. XXV. 4to. 1888–90.
Royal Society of London—Proceedings, Nos. 291–294. 8vo. 1890.
Saxon Society of Sciences, Royal—Mathematisch-physische Classe:
Abhandlung. Band XVI. Nos. 1, 2. 8vo. 1890.
Berichte, 1890, No. 1. 8vo. 1890.
Philologisch-historischen Classe:
Abhandlung. Band XI. No. 7. 8vo. 1890.
Seismological Society of Japan—Transactions, Vol. XIII. Part 2. 8vo. 1890.
Selborne Society—Nature Notes, Vol. I. Nos. 7–10. 8vo. 1890.
Smithsonian Institution—Annual Report, 1886, Part 2; 1887, Parts 1, 2. 8vo. 1889.
Society of Arts—Journal for July–October, 1890. 8vo.
Statistical Society—Journal, Vol. LIII. Parts 2, 3. 8vo. 1890.
St. Pétersbourg Académie Impériales des Sciences—Mémoires, Tome XXXVII. Nos. 8–10. 4to. 1890.
Tasmania Royal Society—Proceedings for 1889. 8vo. 1890.
United Service Institution, Royal—Journal, No. 153. 8vo. 1890.
United States Geological Survey—Eighth Annual Report, 1886–7. 4to. 1890.
Monographs, Vol. XV. XVI. 4to. 1889.
Bulletins, Nos. 54–57. 8vo. 1889–90.
United States Navy—General Information Series, No. 9. 8vo. 1890.
Upsal Royal Society of Sciences—Nova Acta, Series 3, Vol. XIV. Fas. 1. 4to. 1890.
Catalogue Méthodique, 1744–1889. 4to. 1890.
Upsal University—Bulletin de L'Observatoire Météorologique, Vol. XXI. 4to. 1889–90.
Uruguay Consulate General—Catalogues, Reports, &c. of International Exhibition of Mining and Metallurgy, 1890, Parts 1 and 2. 8vo.
Vereins zur Beförderung des Gewerbfleises in Preussen—Verhandlungen, 1890: Heft 7, 8. 4to.
Victoria Institute—Transactions, No. 94. 8vo. 1890.
Wagner Free Institute of Sciences—Transactions, Vol. III. 4to. 1890.
Wild, Dr. H.—Annalen der Physikalischen Central—Observatoriums, 1889, Theil I. 4to. 1890.
Zoological Society of London—Proceedings, 1890, Parts 2, 3. 8vo.

GENERAL MONTHLY MEETING,

Monday, December 1, 1890.

Sir James Crichton Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

Charles Arthur Aikin, Esq. F.R.C.S.
Louis Brennan, Esq.
A. M. Dunlop, Esq.
Henry Gourlay, Esq.
John Rose Innes, Esq. B.Sc. B.A.
Maurice Marcus, Esq.
Charles Gibson Millar, Esq.
George Danford P. Thomas, M.D. M.R.C.S.

were elected Members of the Royal Institution.

The following Lecture Arrangements were announced:—

Professor Victor Horsley, F.R.S. B.S. F.R.C.S. *M.R.I.* Fullerian Professor of Physiology, R.I. Nine Lectures on The Structure and Functions of the Nervous System: Part I. The Spinal Cord, and Ganglia. On *Tuesdays*, Jan. 20, 27, Feb. 3, 10, 17, 24, March 3, 10, 17.

Hall Caine, Esq. Three Lectures on The Little Manx Nation. On *Thursdays*, Jan. 22, 29, Feb. 5.

Professor C. Hubert H. Parry, Mus. Doc. M.A. Professor of Musical History and Composition at the Royal College of Music. Three Lectures on The Position of Lulli, Purcell, and Scarlatti in the History of the Opera (with Musical Illustrations). On *Thursdays*, Feb. 12, 19, 26.

Professor C. Meymott Tidy, M.B. F.C.S. *M.R.I.* Professor of Chemistry and of Forensic Medicine at the London Hospital. Three Lectures on Modern Chemistry in relation to Sanitation. On *Thursdays*, March 5, 12, 19.

W. Martin Conway, Esq. M.A. F.S.A. Three Lectures on Pre-Greek Schools of Art. On *Saturdays*, Jan. 24, 31, Feb. 7.

The Right Hon. Lord Rayleigh, M.A. D.C.L. LL.D. F.R.S. *M.R.I.* Professor of Natural Philosophy, R.I. Six Lectures on the Forces of Cohesion. On *Saturdays*, Feb. 14, 21, 28, March 7, 14, 21.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta: Rendiconti. 2° Semestre, Vol. VI. Fasc. 5. 8vo. 1890.
Astronomical Society, Royal—Monthly Notices, Vol. L. No. 9 and Appendix. 8vo. 1890.
Bankers, Institute of—Journal, Vol. XI. Part 9. 8vo. 1890.

Birmingham Philosophical Society—Proceedings, Vol. VII. Part 1. 8vo. 1889–90.
British Architects, Royal Institute of—Proceedings, 1890–1, Nos. 2, 3. 4to.
Calendar, 1890–1. 8vo.
Cambridge Philosophical Society—Proceedings, Vol. VII. Part 2. 8vo. 1890.
Chemical Industry, Society of—Journal, Vol. IX. No. 10. 8vo. 1890.
Chemical Society—Journal for November, 1890. 8vo.
Cracovie l'Academie des Sciences—Bulletin, 1890, No. 8. 8vo.
Dax: Societé de Borda—Bulletin, quinzième année. 1r Tremestre. 8vo. 1890.
Editors—American Journal of Science for November, 1890. 8vo.
Analyst for November, 1890. 8vo.
Athenæum for November, 1890. 4to.
Brewers' Journal for November, 1890. 4to.
Chemical News for November, 1890. 4to.
Chemist and Druggist for November, 1890. 8vo.
Electrical Engineer for November, 1890. fol.
Engineer for November, 1890. fol.
Engineering for November, 1890. fol.
Horological Journal for November, 1890. 8vo.
Industries for November, 1890. fol.
Iron for November, 1890. 4to.
Ironmongery for November, 1890. 4to.
Murray's Magazine for November, 1890. 8vo.
Nature for November, 1890. 4to.
Open Court for November, 1890. 4to.
Photographic News for November, 1890. 8vo.
Public Health for November, 1890. 8vo.
Revue Scientifique for November, 1890. 4to.
Telegraphic Journal for November, 1890. fol.
Zoophilist for November, 1890. 4to.
Fleming, J. A. Esq. M.A. D.Sc. M.I.E.E. M.R.I. (the Author)—On Prof. E. Thomson's Electro-Magnetic Induction Experiments. 8vo. 1890.
Franklin Institute—Journal, No. 779. 8vo. 1890.
Geographical Society, Royal—Proceedings, New Series, Vol. XII. No. 11. 8vo. 1890.
Geological Society—Quarterly Journal, No. 184. 8vo. 1890.
Georgofili, Reale Accademia—Atti, Vol. XIII. Disp. 2a. 8vo. 1890.
Glasgow Philosophical Society—Proceedings, Vol. XXI. 8vo. 1889–90.
Houghton, George W. W., Esq., (the Author)—The Coaches of Colonial New York. 1890.
Johns Hopkins University—University Circulars, No. 83. 4to. 1890.
Lewins, R. M.D.—Induction and Deduction, by C. C. W. Naden. 8vo. 1890.
Linnean Society—Journal, Nos. 185, 186, 189, 191. 8vo. 1890.
Maryland Medical and Chirurgical Faculty—Transactions, 92nd Session. 8vo. 1890.
Meteorological Office—Weekly Weather Reports, Nos. 44, 45. 4to. 1890.
Meteorological Society, Royal—Quarterly Journal, No. 76. 8vo. 1890.
Ministry of Public Works, Rome—Giornale del Genio Civile, Seria Quinta, Vol. IV. Nos. 7, 8. And Designi. fol. 1890.
New South Wales, Agent General—Official Record of the Australian Federation Conference, 1890. 8vo. 1890.
Wealth and Progress of New South Wales. 1888–9. By J. Coghlan. 8vo. 1889.
North of England Institute of Mining and Mechanical Engineers—Report on Use of Explosives in Mines, Part 2. 8vo. 1890.
Odontological Society of Great Britain—Transactions, Vol. XXIII. No. 1. New Series. 8vo. 1890.
Pharmaceutical Society of Great Britain—Journal, November, 1890. 8vo.
Physical Society of London—Proceedings, Vol. X. Part 4. 8vo. 1890.
Preussische Akademie der Wissenschaften—Sitzungsberichte, Nos. XX.–XL. 8vo. 1890.

Richardson, B. W. M.D. F.R.S. M.R.I. (*the Author*)—The Asclepiad, No. 28. 8vo. 1890.
Rio de Janeiro, Observatoire Imperial de—Revista, No. 9. 8vo. 1890.
Sanitary Institute—Transactions, Vol. X. 8vo. 1890.
Selborne Society—Nature Notes, Vol. I. No. 11. 8vo. 1890.
Société Archéologique du midi de la France—Bulletin. Serie in 8vo, No. 4. 1889. Memoires. 2e Serie. Tome XIV. 1886 à 1889.
Society of Architects—Proceedings, Vol. III. No. 1. 8vo. 1890.
Society of Arts—Journal for November, 1890. 8vo.
Stopes, H. Esq. F.G.S. F.R.H.S. &c.—Indication of Retrogression in Prehistoric Civilization. 4to. 1890.
United Service Institution, Royal—Journal, No. 154. 8vo. 1890.
Veneto, L'Ateneo—Rivista, Serie XIII. Fasc. 4–6; Serie XIV. Fasc. 1–6. 4to. 1889–90.

WEEKLY EVENING MEETING,

Friday, June 13, 1890.

Sir James Crichton Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

Professor Silvanus P. Thompson, D.Sc. *M.R.I.*

The Physical Foundation of Music.

Something in the constitution of the human mind impels us to search, to examine, to analyse. Even music—the art which appeals above all to the emotions—cannot remain exempt. We receive the impression on our senses, and forthwith are impelled to the enquiry: Why does this move us? How can the mere movement or tremor of the air enter thus into our senses? The instinct to analyse will not let us alone until we have found some sort of an explanation—a mental resting-place—enabling us to account to ourselves for the things that would otherwise seem an inscrutable mystery.

Now, in music, there are three main questions for which in this way an answer has been sought:—

(1) Why is it that the ear is pleased by a succession of sounds belonging to a certain particular set called a scale?

(2) Why is it that when two (or more) musical sounds are simultaneously sounded, the ear finds some combinations agreeable and others disagreeable?

(3) Why is it that a note sounded on a musical instrument of one sort is different from, and is distinguishable from, the same note sounded with equal loudness upon an instrument of another sort?

In brief, we desire to know the origin of *melody*, the cause of *harmony*, and the nature of *timbre.*

The theories which have been framed to account for each of these three features of music are based on a double foundation—partly physical, partly physiological. With the physiological aspect of this foundation we have to-night nothing to do, being concerned only with the physical aspect. What, then, are the physical foundations of melody, of harmony, and of timbre? Demonstrable by experiment they must be, in common with all other physical facts, otherwise they cannot be accepted as proven. What are the facts, and how can they be demonstrated?

To Pythagoras and the Greeks it was known that the notes of the melodic scale corresponded in a curiously-perfect way to certain numerical relations between the lengths of the stretched strings. Modern research has transferred these numerical relations to the frequency of the vibrations executed by the moving body, and *pitch* is now a matter assignable in definite numbers. In the philosophical pitch the middle c' between the bass and treble staves is defined as consisting of 256 complete vibrations. In concert pitch the usual number given to the same note is 268 or 270. Further, the notes of the major scale are always related to one another in the following definite ratios:—

$$1 : \frac{9}{8} : \frac{5}{4} : \frac{4}{3} : \frac{3}{2} : \frac{5}{3} : \frac{15}{8} : 2$$

In the execution of music the performer does not always adhere rigidly to these ratios. For pure melodic purposes, the actual scale is nearer to that called the Pythagorean; but for the purpose of harmony in any one key the above ratios must be precisely observed. But, obviously, a scale is wanted which will serve both purposes, melodic and harmonic; hence the same scale is attempted to be used for melody as for harmony. Further, the requirements of modern music, ever since the time of Bach, have necessitated a further modification of the scale to enable the performer to modulate into harmonies, not in one key, but in all; so that he may enjoy the tonal relations of contrasted keys, modulating from key to key. In order that this may be done, and yet that all the keys may be played from one key-board, musicians have been content to spoil the exact consonances in individual keys by departing from these exact ratios by the device of tempering all the keys, so that twelve successive fifths shall exactly equal seven successive octaves.

I am not here, however, to fight over again the battle of the temperaments, nor do I purpose to enter upon a discussion of the origin of melody, which, indeed, I believe to be associative rather than physical. I shall confine myself to two matters only—the *cause of harmony* and the *nature of timbres.*

Returning, then, to the ratios of the vibration-numbers of the major scale, we may note that two of these, namely, the ratios 9 : 8 and 15 : 8, which correspond to the intervals called the major whole tone and the seventh, are dissonant—or, at least, are usually so regarded. It will also be noticed that these particular fractions are more complex than those that represent the consonant intervals. This naturally raises the question: *Why is it that the consonant intervals should be represented by ratios made up of the numbers* 1 *to* 6, *and by no others?*

To this problem the only answer for long was the entirely evasive and metaphysical one that the mind instinctively delights in order and number. The true answer, or rather the first approximation to a true answer, was only given about forty years ago, when von Helmholtz, as the result of his ever-memorable researches on the sensations of tone, returned the reply: *Because only by fulfilling numerical relations which are at once exact and simple, can the "beats" be avoided, which are the cause of dissonance.* The phenomenon of beats is so well known that I may assume the term to be familiar. An excellent mode of making beats audible to a large audience is to place upon a wind chest two organ-pipes tuned to $ut_2 = 128$, and then flatten one of them slightly by holding a finger in front of its mouth. Von Helmholtz's theory of dissonance may be briefly summarised by saying that any two notes are discordant if their vibration numbers are such that they produce beats, maximum discordance occurring when the beats occur at about 33 per second; beats if either fewer than these, or more numerous, being less disagreeable than beats at this frequency. It is an immediate consequence that the degree of dissonance of any given interval will depend on its position on the scale. For example, the interval of the major whole tone, represented by the ratio 9 : 8, produces four beats per second at the bottom of the pianoforte keyboard, 32 beats per second at the middle of the keyboard, and 256 beats per second at the top. Such an interval ought to be discordant, therefore, in the middle octaves of the scale only. Von Helmholtz expresses elsewhere the opinion that beats only occur between two tones when the intervals between these tones are within a minor third of one another.

To this view of von Helmholtz it was at first objected that if that were the whole truth, all intervals should be equally harmonious provided one got far enough away from being in a bad unison: fifths, augmented fifths, and sixths minor and major ought all to be equally harmonious. This no musician will allow. To account for this von Helmholtz makes the further supposition that the beats occur, not simply between the fundamental or prime tones, but also between the upper partials which usually accompany prime tones. This leads me to say a word about *upper partial-tones* and *harmonics*. I believe many musicians use these two terms as synonymous; but they ought to be carefully distinguished. The term harmonics ought to be rigidly reserved to denote higher tones which stand in definite harmonic relations to the fundamental tone. The great mathematician Fourier first showed that any truly periodic function, however complex, could be analysed out and expressed as the sum of a certain series of periodic functions having frequencies related to that of the fundamental or first member of the series as the simple numbers 2, 3, 4, 5, &c. Thirty years later G. S. Ohm suggested that the human ear actually performs such an analysis, by virtue of its mechanical structures, upon every complex sound of a periodic character, resolving it into a fundamental tone, the octave of that tone, the twelfth, the double octave, &c. Von Helmholtz, arming himself with a series of tuned resonators, sought to pick up and recognise as members of a Fourier-series, the higher harmonics of the tones of various instruments. In his researches he goes over the ground previously traversed by Rameau, Smith, and Young, who had all observed the co-existence in the tones of musical instruments, of higher partial tones. These higher tones correspond to higher modes of vibration, in which the vibratile organ—string, reed, or air column,—subdivides into two, three, four, or more parts. Such parts naturally possess greater frequency of vibration, and their higher tones, when they co-exist along with the lower or fundamental tone, are denominated *upper partial tones*, thereby signifying that they are higher in the scale, and that they correspond to vibrations *in parts*. It is to be regretted that Professor Tyndall in his Lectures on Sound, rendered von Helmholtz's *Oberpartialtöne* by the term *overtones*, omitting the most significant half of the word. To avoid all confusion in the use of such a term I shall rather follow Kœnig in speaking of these as *sounds of subdivision*. And I must protest emphatically against calling these sounds harmonics, for the simple reason that in many cases they are very inharmonious. It is a matter to which I shall recur hereafter.

Returning to the subject of beats, the question arises: What becomes of the beats when they occur so rapidly that they cease to produce a discontinuous sensation upon the ear? The view which I have to put before you to-night in the name of Dr. Kœnig is that they blend to make a tone of their own. Earlier acousticians have propounded, in accordance with this view, that the *grave harmonic* of Tartini (a sound which corresponds to a frequency of vibration which is the difference between those of the two tones producing it) is due to this cause. Von Helmholtz has taken a different view, denying that the beats can blend to form a sound, giving reasons presently to be examined. Von Helmholtz considered that he had discovered a new species of combinational tone, namely one corresponding in frequency to the *sum* of the frequencies of the two tones, whereas that discovered by Tartini (and before him by Sorge) corresponded to their *difference*. Accordingly he includes under the term of combinational tones the differential tone of Tartini and the summational tone which he considered himself to have discovered. To the existence of such combinational tones he ascribed a very important part in determining the character, harmonious or otherwise, of chords; and to them also he attributes the ability of the ear to discriminate between the degrees of harmoniousness possessed by such intervals (fifths, sixths, &c.) as consist of two tones too widely apart on the scale to give beats of a discontinuous character. He also considers that such combinational tones are chiefly effective in producing beats, the summational tones of the primaries beating with their upper partial tones; and that this is the way in which they make an interval more or less harmonious.

The whole fabric of the theory of harmony as laid down by Von Helmholtz is thus seen to repose upon the presence or absence of beats; and the beats themselves are in turn made to depend not upon the mere interval between two notes but upon the timbres also of those notes, as to what upper partials they contain, and whether those partials can beat with the summational tone of the primaries. It becomes then of the utmost importance to ascertain the precise facts about the beats and about the supposed combinational tones. What the numbers of beats are in any given case: whether they do or do not correspond to the alleged differential and summational tones: these are vital to the theory of harmony. Equally vital is it to know what the timbres of sounds are, and whether they can be accurately or adequately represented by the sum of a set of pure harmonics corresponding to the terms of a Fourier-series.

And here let me take the opportunity of saying that the views which I am about to propound, and which for to-night I must be considered to adopt, are those which have been put forth as the result of a quarter of a century of patient work by Dr. Rudolph Kœnig of Paris.

Dr. Kœnig, whose recent visit to this country will be remembered by some here present, is, as is well known, the constructor of the finest and most accurate acoustical instruments in the world, and is not only a constructor but an investigator of great distinction, and author of numerous memoirs on acoustics which have from time to time appeared in the *Annalen* of Poggendorff, and in those of Wiedemann, and elsewhere. The splendid apparatus around me belongs to him, and forms but a very small part of the collection which adorns his *atelier* on the Quai d'Anjou. He lives and works in seclusion, surrounded by his instruments, even as our own Faraday lived and worked amongst his electric and magnetic apparatus. His great tonometer, now nearly completed, comprises a set of standard tuning forks, adjusted each one by his own hands, ranging from 20 vibrations per second up to nearly 40,000, with perfect continuity, many of the forks being furnished with sliding adjustments, so as to give by actual marks upon them any desired number of vibrations within their own limits. Besides this colossal masterpiece, Dr. Kœnig's collection includes several large wave-sirens, and innumerable pieces of apparatus in which his ingenious manometric flames are adapted to acoustical investigation. There also stands his tonometric clock; a timepiece governed, not by a pendulum, but by a standard tuning-fork, the rate of vibration of which it accurately records. Lest I should forget it at a later stage, let me here return my most cordial thanks to Dr. Kœnig for the extreme kindness and courtesy with which he has put at my disposal for this discourse all the apparatus wherewith to illustrate the various points in his researches.

In investigating beats and combinational tones Dr. Kœnig deemed it of the highest importance to work with instruments producing the purest tones; not with harmonium reeds or with polyphonic sirens, the tones of which are avowedly complex in timbre, but with massive steel tuning-forks, the pendular movements of which are of the simplest possible character. Massive tuning-forks properly excited by bowing with a violoncello bow, or, in the case of those of high pitch, by striking them with an ivory mallet, emit tones remarkably free from all sounds of subdivision, and of so truly pendular a character (unless over-excited) that none of the harmonics corresponding to the members of a Fourier-series can be detected. No living soul has had a tithe of the experience of Dr. Kœnig in the handling of tuning-forks. Tens of thousands of them have passed through his hands. He is accustomed to tune them himself, making use of the phenomenon of beats to test their accuracy. He has traced out the phenomena of beats through every possible degree of pitch, even beyond the ordinary limits of audibility, with a thoroughness utterly impossible to surpass or to equal. Hence, when he states the results of his experience, it is idle to contest the facts gathered on such an unique basis.

The results of Dr. Kœnig's observations on beats are easily stated. He has observed primary beats, as well as beats of secondary and higher orders, from the interference of two simple tones simultaneously sounded. When two simple tones interfere, the primary beats always belong to one or other of two sets, called an *inferior* and a *superior* set, corresponding respectively in number to the two remainders, positive and negative, to be found by dividing the frequency of the higher tone by that of the lower.

This mode of stating the facts is a little strange to those trained in English modes of expressing arithmetical calculations: but an example or two will make it plain. Let there be as the two primary sounds two low tones having the respective frequencies of 40 vibrations and 74 vibrations. What are the two remainders, positive and negative, which result from dividing the higher number, 74 by the lower number 40? Our English way of stating it is to say that 40 goes into 74 once, and leaves over a (positive) remainder of 34. But it is equally correct to say that 40 goes into 74 twice, all but 6: or that there is a negative remainder of 6. Well, Dr. Kœnig finds that when these two tuning-forks are tried, the ear can distinguish two sets of beats, one rapid, at 34 per second, and one slow, at 6 per second.

Again, if the forks chosen are of frequencies 100 and 512, we may calculate thus: 100 goes into 512 five times, plus 12; or 100 goes into 512 six times, minus 88. In this actual case the 12 beats belonging to the inferior set would be well heard: the 88 beats belonging to the superior set would probably be almost indistinguishable. As a rule the inferior beat is heard best when its number is *less* than half the frequency of the lower primary, whilst, when its number is *greater*, the superior beat is then better heard. Dr. Kœnig has never been able to hear any primary beat which did not fall within the arithmetical rule which I have previously stated.

I will now illustrate to you the beats, inferior and superior, as produced by these two massive tuning-forks, each weighing about 50 pounds, and each provided with a large resonating cavity consisting of a metal cylinder with an adjustable piston. One of them is tuned to the note $ut_1 = 64$. The other also sounds ut_1; but by sliding down its prongs the adjustable weights of gun-metal, and screwing in the piston, I can raise its pitch a whole tone to $re_1 = 72$. I excite them with the 'cello bow, first separately, that you may hear their individual tones, then together. At once you hear an intolerable beating—the beats coming 8 per second. This is the inferior beat, corresponding to the positive remainder, the superior beat you cannot hear. I raise the note of the second fork from re_1 to $mi_1 = 80$; and the beats quicken to 16 per second. Raising it to $fa_1 = 85\frac{1}{3}$, and then to $sol_1 = 96$, while the first fork is still kept at ut_1, the beats increase in rapidity, but are fainter in distinctness. If I now substitute for the second fork a similar one which begins with sol_1, and raise its pitch to $la_1 = 106\frac{2}{3}$ you may be able to hear two beats, the inferior one rapid and faint at $42\frac{2}{3}$ per second, and the superior one slower, but also faint, at $21\frac{1}{3}$ per second. Still raising the pitch to the true seventh tone $= 112$, the rapid inferior beat has died out, but now you hear the superior strongly at 16 per second. I raise it once more to $si_1 = 120$ (the seventh of the ordinary scale) and the beats are still stronger and slower at 8 per second. Finally I bring the pitch up to the octave $ut_2 = 128$, to find that all beats have disappeared; there is a perfectly smooth consonance. The facts so observed are tabulated for you as follows:—

TABLE I.

Primary Beats.

Primary Tones.		Ratio.	Inferior Beats.	Superior Beats.
ut_1 64	re_1 72	8 : 9	8	—
ut_1 64	mi_1 80	4 : 5	16	—
ut_1 64	fa_1 $85\frac{1}{3}$	3 : 4	$21\frac{1}{3}$	—
ut_1 64	sol_1 96	2 : 3	32	32
ut_1 64	la_1 $106\frac{2}{3}$	3 : 5	$42\frac{2}{3}$	$21\frac{1}{3}$
ut_1 64	(7) 112	4 : 7	—	16
ut_1 64	si_1 120	8 : 15	—	8
ut_1 64	ut_2 128	1 : 2	—	0

Suppose now, keeping the lower fork unaltered, we raise the pitch of the higher note (taking a new fork that starts at the octave) from ut_2 to sol_2 by gradual steps, we shall find that there begins a new set of primary beats—an inferior set, which are at first slow, then get more rapid and become undistinguishable; but these are succeeded by another set which gradually emerge from the intolerable *vacarme*, and, though rapid at first and indistinct, grow slower and stronger as the pitch is raised, until, when it reaches sol_2, the frequency of which is exactly three times that of ut_1, all beats again vanish. This range between the octave and the twelfth tone may be called the second "period," to distinguish it from the period from unison to the first octave, which was our first period. Similarly, the range from the twelfth tone to the second octave is the third period, and from thence to the major third above is the fourth period, and so forth. In each period, up to the sixth or seventh of such periods, a set of inferior and a set of superior beats may be observed; and in every case the frequency of the beats corresponds, as I have said, to one or other of the two remainders of the frequencies of the two tones. No beat has ever been observed corresponding to the sum of the frequencies, even when using the slowest forks. None has ever been observed corresponding to the difference of the frequencies, save in the first period; where, of course, the positive remainder is simply the difference of the two numbers.

That you may hear for yourselves the beats belonging to one of the higher periods, I will take a pair of forks which will give us some of the superior beats in the fourth period. One of the forks is the great ut_1, 64 as previously used. The other is $mi_3 = 320$; their ratio being $1:5$. Sounded together they give a pure consonance, but if the smaller one is loaded with small pellets of wax to lower its pitch slightly, and I then bow it, at once you hear beats. It was in studying the beats of these higher periods that Dr. Kœnig made the observation that whereas the beats of an imperfect unison are heard as alternate silences and sounds, the beats of the imperfect consonances of higher periods—twelfth tone, double octave, &c.—consist mainly in variations in the loudness of the lower of the two primary tones; an observation which was independently made by Mr. Bosanquet, of Oxford.

Passing from the beats themselves, I approach the question, what becomes of the beats when they occur too rapidly to produce on the ear a discontinuous sensation? On this matter there have been several conflicting opinions: some holding with Lagrange and Dr. Thomas Young, that they blend into a separate tone; others, with von Helmholtz, maintaining that the combinational tones cannot be so explained, and arise from a different cause. Let it be observed that, even if beat-tones exist, it is quite possible for beats and beat-tones to be simultaneously heard. A similar co-existence of a continuous and discontinuous sensation is afforded by the familiar experiment of producing a tone by pressing a card against the periphery of a rapidly rotating toothed wheel. There is a certain speed at which the individual impulses begin to blend into a continuous low tone, while yet there are distinguishable the discontinuous impulses; the degree of distinctness of the two co-existing sounds being dependent on the manner in which the card is pressed against the wheel—that is to say, on the nature of the individual impulses themselves. The opponents of the view that beats blend into a tone, state plainly enough that, in their opinion, a mere succession of alternate sounds and silences cannot blend into a tone different from that of the beating tone. Having said that the beats cannot blend, they then add that they do not blend; for, say they, the combinational tones are a purely subjective phenomenon. Lastly, they say that if even the beats blend they will not so explain the existence of combinational tones, because the combinational tones have frequencies which do not correspond to the number of the beats.

In the teeth of all these views and opinions, Dr. Kœnig—without dogmatising as to how or why it is—emphatically affirms that beats do produce *beat-tones;* and he has pursued the matter down to a point that leaves no room for doubting the general truth of the fact. The alleged discrepancy between the frequency of the observed combinational tones and that of the beats disappears when closely scrutinised. Those who count the beats by merely taking the difference between the frequencies of the two primary tones, instead of calculating the two remainders, will assuredly find that their numbers do not agree in pitch with the actual sounds heard. But that is the fault of their miscalculation. Those who use harmonium reeds or polyphonic sirens instead of tuning-forks to produce their primary tones must not expect from such impure sources to reproduce the effects to be obtained from pure tones. And those who say that the beats calculated truly from the two remainders will not account for the summational tones, have unfortunately something to unlearn—namely, that, when pure tones are used, under no circumstances is a tone ever heard, the frequency of which is the sum of the frequencies of the two primary tones.

The apparatus before you enables me to demonstrate, in a manner audible, I trust, to the whole assembly in this theatre, the existence of the beat-tones. My first illustrations relate to tones of primary beats, some belonging to the inferior, others to the superior set, in the first period.

I take here the fork $ut_6 = 2048$, five octaves higher than the great ut_1. To excite it, I may either bow it or strike it with this ivory mallet. With it I will take the fork one note higher, $re_6 = 2304$. When we took the same interval with ut_1 and re_1, the number of beats was 8. The *ut* and *re* of the next octave higher would have given us 16 beats, that of the next 32, that of the next 64, of the fourth octave 128, and that of the fifth octave higher 256. But 256 per second is a rapidity far too great for the ear to hear as separate sounds. If there were 256 separate impulses, they would blend to give us the note $ut_3 = 256$. They are not *impulses*, but *beats:* nevertheless, they blend. I strike the ut_6, then the re_6, both shrill sounds when you hear them separately; but when I strike them in quick succession one after the other, at the moment when the mallet strikes the second fork you hear this clear ut_3 sounding out. I am not going to waste your time in a disputation as to whether the sound you hear is objective or subjective. It is enough that you hear it, pure and unmistakable in pitch. It is the grave harmonic; and the number 256, which is its frequency, corresponds to the positive remainder when you divide 2304 by 2048.

Now let me give you a beat-tone belonging to the superior set: it also will be a grave harmonic, if you so please to call it; but its frequency will correspond neither to the difference nor to the sum of the frequencies of the two primary tones. I take $ut_6 = 2048$ as previously, and with it $si_6 = 3840$. Let us calculate what the superior beats ought to be. 2048 goes into 3840 twice, less 256. Then, 256 being the negative remainder, we ought to hear from these two forks the beat-tone of 256 vibrations, which is ut_3, the same note as in our last experiment. I strike the forks, and you hear the result. The beat-tone, which is neither a differential tone nor a summational tone, corresponds to the calculated number of beats.

If I take $ut_6 = 2048$ and $sol_6 = 3072$, the two remainders both come out at 1024, which is ut_5. Let me sound ut_5 itself, separately, on an ut_5 fork, that you may know what sound to listen for. Its sound has died away; and now I strike ut_6 and sol_6, when at once you hear ut_5 ringing out. That sound which you all heard corresponds in frequency to the calculated number of beats. That is enough for my present purpose.

The next illustration is a little more complex. I select a case in which the beat-tones corresponding to the inferior and the superior beats will both be present. We shall have four tones altogether—two primary tones and two beat-tones. The forks I select are $ut_6 = 2048$ as before, and a fork which is tuned to vibrate exactly 11 times as rapidly as ut_3—it is the 11th harmonic of that note, but does not correspond precisely to any note of the diatonic scale. It has 2816 vibrations, and is related to ut_6 as $11:8$. The two remainders will now be 768 and 1280, which are the respective frequencies of sol_4 and mi_5. I will first sound those notes on two other forks, that you may know beforehand what to listen for. Now, on striking the two shrill forks in rapid succession, the *two* beat-tones are heard.

If I select, instead of the 11th harmonic, the 13th harmonic of ut_3, vibrating 3328 times in the second, to be sounded along with ut_6, I shall produce the same two beat-tones as in the preceding case; but $mi_5 = 1280$ is now the inferior one, corresponding to the positive remainder, while $sol_4 = 768$ is the superior tone, corresponding to the negative remainder. It is certainly a striking corroboration of Dr. Kœnig's view that the beat-tones actually heard in these last two experiments should come out precisely alike, though on the old view, that the combination tones were simply the summational and differential tones, one would have been led to expect the sounds in the two experiments to be quite different.

One other example I will give you of a beat-tone belonging to the second period. The two primary notes are given by the forks ut_5 = 1024 and re_6 = 2304. The beat-tone which you hear is ut_3 = 256, which corresponds to the positive remainder.

I may here mention that a mathematical investigation,* which only appeared a month ago from the pen of Professor W. Voigt, entirely confirms the views of Dr. Kœnig as to the non-existence of the supposed summational tones, and the existence of those which accord with the two remainders.

It will be convenient to draw up in tabular form the results just obtained. These may be considered as abbreviations of the much more extended tables drawn up by Dr. Kœnig, which hang upon the walls, and which are to be found in his book, 'Quelques Expériences d'Acoustique.'

TABLE II.

Sounds of Primary Beats.

Primary Tones.		Ratio.	Inferior Beat-tone.	Superior Beat-tone.
ut_6 2048	re_6 2304	8 : 9	1 ut_3 256	—
ut_6 2048	si_6 3840	8 : 15	—	1 ut_3 256
ut_6 2048	sol_6 3072	8 : 12	4 ut_5 1024	4 ut_5 1024
ut_6 2048	(11th) 2816	8 : 11	3 sol_4 768	5 mi_5 1280
ut_6 2048	(13th) 3328	8 : 13	5 mi_5 1280	3 sol_4 768
ut_5 1024	re_6 2304	4 : 9	1 ut_3 256	—

So far we have been dealing with primary beats and beat-tones; but there are also secondary beats and secondary beat-tones, which are produced by the interference of primary beat-tones. An example of a secondary beat is afforded by the following experiment. Recurring to the preceding table of experiments, it may be observed that when the two shrill notes ut_6, sol_6, giving the interval of the fifth, are sounded together, the inferior and superior beat-tones are both present, and of the same pitch. If, now, one of the two forks is lightly loaded with pellets of wax to put it out of adjustment, we

* "Ueber den Zusammenklang zweier einfacher Töne." 'Göttinger Nachrichten,' No. 5, 1890.

shall get beats, not between the primary tones, but between the beat-tones. Suppose we add enough wax to reduce the vibration of sol_6 from 3072 to 3070. Then the positive remainder is 1022, and the negative remainder is 1026; the former being ut_5 flattened two vibrations, the latter the same note sharpened to an equal amount. As a result there will be heard four beats per second—secondary beats. Similarly the intervals 2:5, 2:7, if slightly mistuned, will, like the fifth, yield secondary beats. Or, to put it in another way, there may be secondary beats from the (mistuned) beat-tones that are related (as in our experiment) in the ratio 1:1, or from those in the ratios 3:4, 3:5, 4:7, and so forth.

I have given you an example of secondary beats; now for an example of a secondary beat-tone. This is afforded by one of the previous experiments, in which were sounded ut_6, and the 11th harmonic of ut_3. In this experiment, as in that which followed with the 13th harmonic, two (primary) beat-tones were produced, of 768 and 1280 vibrations respectively. These are related to one another by the intervals 3:5. If we treat these as tones that can themselves interfere, they will give us for their positive remainder the number 256, which is the frequency of ut_4. As a matter of fact, if you listen carefully, you may, now that your attention has been drawn to it, hear that note, in addition to the two primary tones and the two beat-tones to which you listened previously.

In von Helmholtz's "Tonempfindungen," he expresses the opinion that the distinctness with which beats are heard depends upon the narrowness of the interval between the primary tones, saying that they must be nearer together than a minor third. But, as we have seen, using bass sounds of a sufficient degree of intensity and purity, as is the case with those of the massive forks, beats can be heard with every interval from the mistuned unison up to the mistuned octave. Even the interval of the fifth, ut_1 to sol_1, gave strongly-marked beats of 32 per second. When this number is attained or exceeded, the ear usually begins to receive also the effect of a very low, continuous tone, the beats and the beat-tone being simultaneously perceptible up to about 60 or 70 beats, or as a roughness up to 128 per second. If, using forks of higher pitches but of narrower interval, one produces the same number of beats, the beat-tone is usually more distinct. Doubtless this arises from the greater true intensity of the sounds of higher pitch. With the object of pursuing this matter still more closely, Dr. Kœnig constructed a series of 12 forks of extremely high pitch, all within the range of half a tone, the lowest giving si_6 and the highest ut_7. The frequencies, and the beats and beat-tones given by seven of them, are recorded in Table III.

The first of these intervals is a diatonic semitone; the second of them is a quarter-tone; the third is an eighth of a tone; nevertheless, a sensitive ear will readily detect a difference of pitch between the two separate sounds. The last of the intervals is about half a comma.

TABLE III.

Frequencies of Forks.	Ratio.	Beats (Calcd.).	Resulting Sound.
ut_7 and si_6 / 4096 and 3840 ..	16 : 15	256	ut_3
,, 3968 ..	32 : 31	128	ut_2
,, 4032 ..	64 : 63	64	ut_1
,, 4048 ..	256 : 253	48	sol_{-1}
,, 4056 ..	512 : 507	40	mi_{-1}
,, 4064 ..	128 : 127	32	ut_{-1}
,, 4070 ..	158 : 157	26	—

These forks are excited by striking them with a steel hammer. Some of the resulting beat-tones will be heard all over the theatre; but, in the case of the very low tones of 40 and 32 vibrations, only those who are close at hand will hear them. The case in which there are 26 beats is curious. Most hearers are doubtful whether they perceive a tone or not. There is a curious *fluttering* effect, as though a tone were there, but not continuously.

We have seen, then, that the beat-tones correspond in pitch to the number of the beats; that they can themselves interfere, and give secondary beats; and that the same number of beats will always give the same beat-tone irrespectively of the interval between the two primary tones. What better proofs could one desire to support the view that the beat-tones are caused, as Dr. Young supposed, by the same cause as the beats, and not, as von Helmholtz maintains, by some other cause? Yet there are some further points in evidence which are of significance, and lend additional weight to the proofs already adduced.

Beats behave like primary impulses in the following respect, that when they come with a frequency between 32 and 128 per second, they may be heard, according to circumstances, either discontinuously or blending into a continuous sensation.

It has been objected that, whereas beats imply interference between two separate modes of vibration arising in two separate organs, combination-tones, whether summational, or differential, or any other, must take their origin from some one organ or portion of vibratile matter vibrating in a single but more complex mode. To this objection an experimental answer has been returned by Dr. Kœnig in the following way. He takes a prismatic bar of steel, about 9 inches in length, and files it to a rectangular section, so as to give, when it is struck at the middle of a face to evoke transversal vibrations, a sound of some well-defined pitch. By carefully adjusting the sides of the rectangular section in proper proportions, the same steel bar can be made to give two different notes when struck in the two directions respectively parallel to the long and short sides of the rectangle. A set of such tuned steel bars are here before you. Taking one tuned to the note of $ut_6 = 2048$, with $re_6 = 2304$, I give you the notes separately by striking the bar with a small steel hammer when it is lying on two little bridges of wood, first on one face, then on the other face. If, now, I strike it on the corner, so as to evoke both notes at once, you immediately hear the strong boom of $ut_3 = 256$, the inferior beat-tone. If I take a second bar tuned to ut_6 and $si_6 = 3840$, you hear also ut_3, this time the superior beat-tone. If I take a bar tuned to ut_6 and the 11th harmonic of ut_3 (in the ratio 8 : 11), you hear the two beat-tones sol_4 and mi_5 (in ratios of 3 and 5 respectively) precisely as you did when two separate forks were used instead of one tuned bar.

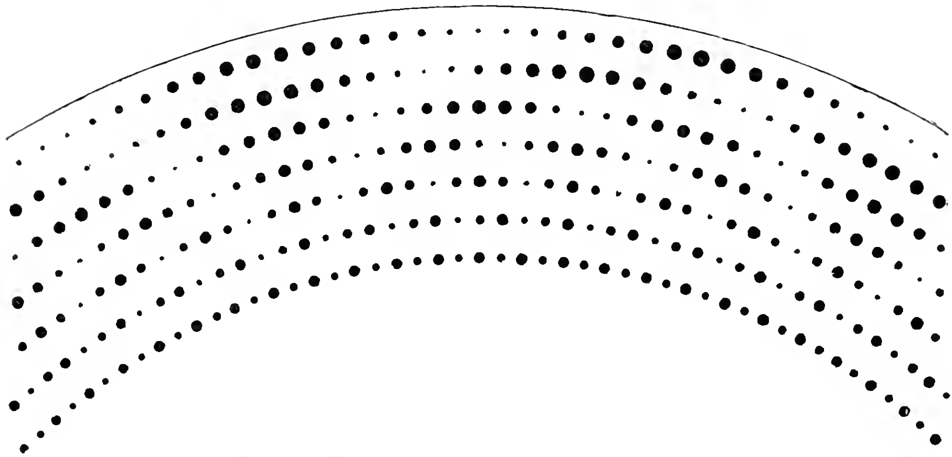
Dr. Kœnig goes beyond the mere statement that beats blend to a tone, and lays down the wider proposition that any series of maxima and minima of sounds of any pitch, if isochronous and similar, will always produce a tone the pitch of which corresponds simply to the frequency of such maxima and minima. A series of beats may be regarded as such maxima and minima of sound; but there are other ways of producing the effect than by beats. Let me illustrate some of these to you.

If a shrill note, produced by a small organ-pipe or reed, be conveyed along a tube, the end of which terminates behind a rotating disk pierced with large, equidistant apertures, the sound will be periodically stopped and transmitted, giving rise, if the intermittences are slow enough, to effects which closely resemble beats, but which, if the rotation is sufficiently rapid, blend to a tone of definite pitch. Dr. Kœnig uses a large zinc disk with 16 holes, each about 1 inch in diameter. In one set of experiments this disk was driven at 8 revolutions per second, giving rise to 128 intermittences. The forks used were all of different pitches from $ut_3 = 256$ to $ut_7 = 4096$. In all cases there was heard the low note ut_2 corresponding to 128 vibrations per second. In another series of experiments, using forks ut_2 and ut_3, the number of intermittences was varied from 128 to 256 by increasing the speed, when the low note rose also from ut_2 to ut_3.

From these experiments it is but a step to the next, in which the intensity of a tone is caused to vary in a periodic manner. For this purpose Dr. Kœnig has constructed a siren-disk (Fig. 1), pierced with holes arranged at equal distances around seven concentric circles; but the sizes of the holes are made to vary periodically from small to large. In each circle are 192 equidistant holes, and the number of maxima in the respective circles was 12, 16, 24, 32, 48, 64, and 96. On rotating this disk, and blowing from behind through a small tube opposite the outermost circle, there are heard, if the rotation is slow, a note corresponding to the number of holes passing per second, and a beat corresponding to the number of maxima per second. With more rapid rotation two notes are heard—a shrill one, and another

4 octaves lower in pitch, the latter being the beat-tone. On moving the pipe so that wind is blown successively through each ring of apertures, there is heard a shrill note, which is the same in each case, and a second note (corresponding to the successive beat-tones) which rises by intervals of fourths and fifths from circle to circle.

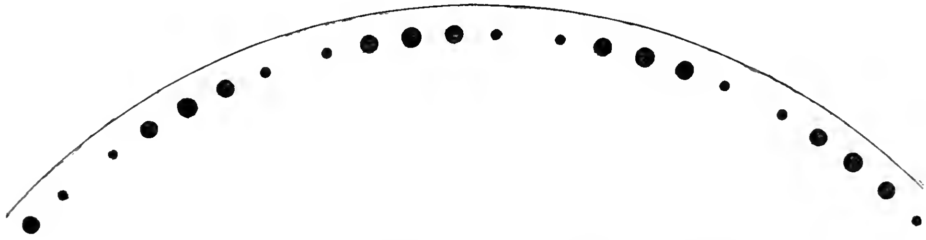
FIG. 1.



Siren Disk, with apertures varying periodically in size.

These attempts to produce artificially the mechanism of beats were, however, open to criticism; for in them the phase of the individual vibrations during one maximum is the same as that of the individual vibrations in the next succeeding maximum; whereas in the actual beats produced by the interference of two tones the phases of the individual vibrations in two successive maxima differ by half a vibration; as may be seen by simple inspection of the

FIG. 2.

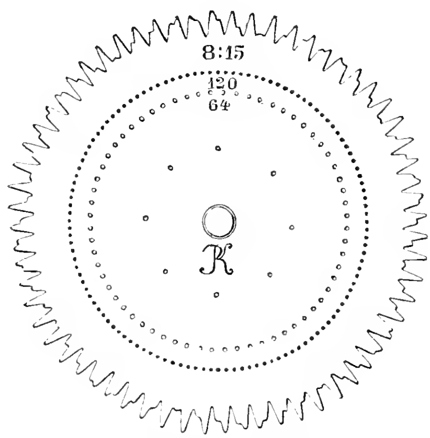


Siren Disk, pierced to imitate mechanism of beats.

curves corresponding to a series of beats. When this difference was pointed out to Dr. Kœnig, he constructed a new siren-disk (Fig. 2), having a similar series of holes of varying size, but spaced out so as to correspond to a difference of half a wave between the sets. With this disk, beats are distinctly produced with slow rotation, and a beat-tone when the rotation is more rapid.

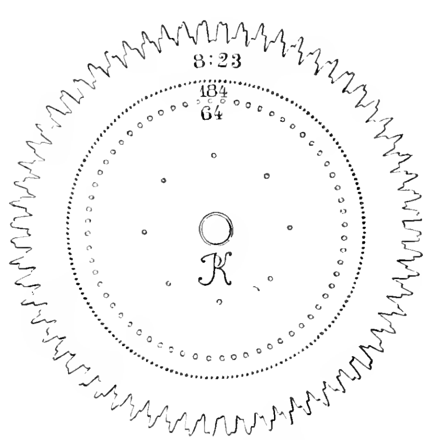
Finding this result from the spacing out of apertures to correspond in position and magnitude to the individual wavelets of a complex train of waves, it occurred to Dr. Kœnig that the phenomena of beats and beat-tones might be still more fully reproduced if the edge of the disk were cut away into a wave-form corresponding precisely to the case of the resultant wave produced by the composition of two interfering waves. Accordingly, he calculated the wave-forms for the cases of several intervals, and, having set out these

Fig. 3.



Wave-disk giving beats and beat-tone.

Fig. 4.



Wave-disk, giving beats and beat-tone.

curves around the periphery of a brass plate, cut away the edge of the plate to the form of the desired wave. Two such wave-disks, looking rather like circular saws with irregular teeth, are depicted in Figs. 3 and 4. These correspond to the respective intervals 8:15

and 8 : 23. A number of such wave-disks corresponding to other intervals lie upon the table ; these two will, however, suffice. In the first of these the curve is that which would be obtained by setting out around the periphery a series of 120 simple sinusoidal waves, and a second set of 64 waves, and then compounding them into one resultant wave. In order to permit of a comparison being made with the simple component sounds, two concentric rings of holes have been also pierced with 120 and 64 holes respectively. Regarding these two numbers as the frequency of two primary tones, there ought to result beats of frequency 8 (being the negative remainder corresponding to the superior beat). An interior set of 8 holes is also pierced, to enable a comparison to be made. To experiment with such wave-disks they are mounted upon a smoothly running whirling-table, and wind from a suitable wind-chest is blown against the waved edge from behind, through a narrow slit set radially. In this way the air-pressures in front of the wave-edge are varied by the rush of air between the teeth. It is a question not yet decided how far these pressures correspond to the values of the ordinates of the curves. This question, which involves the validity of the entire principle of the wave-siren, cannot here be considered in detail. Suffice it to say that for present purposes the results are amply convincing.

The wave-disk (Fig. 3) has been clamped upon the whirling-table, which an assistant sets into rotation at a moderate speed. I blow first through a small pipe through one of the rows of holes, then through the other. The two low notes sound out separately, just a seventh apart. Then I blow through the pipe with a slotted mouth-piece against the waved edge; at once you hear the two low notes interfering, and making beats. On increasing the speed of rotation the two notes become shrill, and the beats blend into a beat-tone. Notice the pitch of that beat-tone: it is precisely the same as that which I now produce by blowing through the small pipe against the ring of 8 holes. With the other wave-disk, having 184 and 64 holes in the two primary circles, giving a wave-form corresponding to the interval 8 : 23, the effects are of the same kind, and when driven at the same speed it gives the same beat-tone as the former wave-disk. It will be noted that in each of these two cases the frequency of the beat-tone is neither the difference nor the sum of the frequencies of the two primary tones.

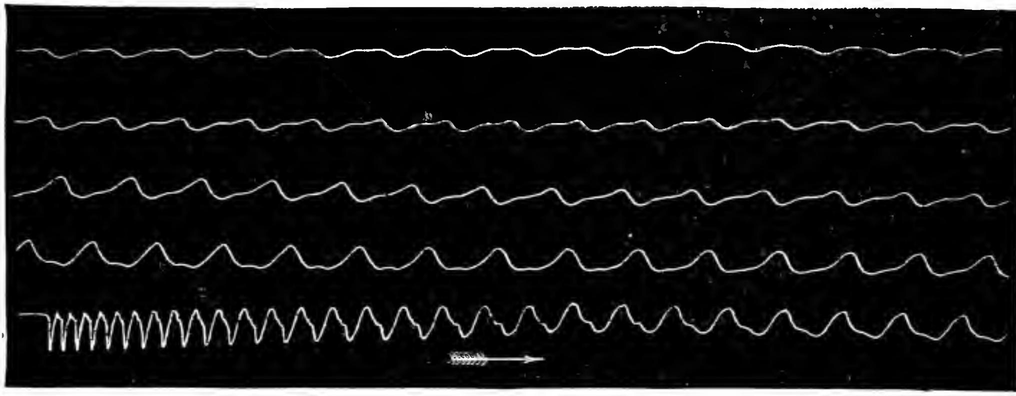
A final proof, if such were needed, is afforded by an experiment, which, though of a striking character, will not necessarily be heard by all persons present, being only well heard by those who sit in certain positions. If a shrill tuning-fork is excited by a blow of the steel mallet, and held opposite a flat wall, part of the waves which it emits strike on the surface, and are reflected. This reflected system of waves, as it passes out into the room, interfere with the direct system. As a result, if the fork, held in the hand, be moved toward the wall or from it, a series of maxima and minima of sound will successively reach an ear situated in space at any point near the line of motion and will be heard as series of beats; the rapidity with which they succeed one another being proportional to the velocity of the movement of the fork, the fork I am using is ut_6, which gives well-marked beats, slow when I move my arm slowly, quick when I move it quickly. There are limits to the speed at which the human arm can be moved, and the quickest speed that I can give to mine fails to make the beats blend to a tone. But if I take sol_6, vibrating $1\frac{1}{2}$ times as fast, and strike it, and move it away from the wall with the fastest speed that my arm will permit, the beats blend into a short low growl, a non-uniform tone of low pitch, but still having true continuity.

This first portion of my discourse may then be summarised by saying that in all circumstances where beats, either natural or artificial, can be produced with sufficient rapidity, they blend to form a beat-tone of a pitch corresponding to their frequency.

I now pass to the further part of the researches of Dr. Kœnig which relates to the timbre of sounds. Prior to the researches of Dr. Kœnig, it had been supposed that in the reception by the ear of sounds of complex timbre the ear took no account of, and indeed was incapable of perceiving, any differences in phase in the constituent partial tones. For example, in the case of a note and its octave sounded together, it was supposed and believed that the sensation in the ear, when the difference in phase of the two components was equivalent to one-half of the more rapid wave, was the same as when that difference of phase was one-quarter, or three-quarters, or zero. I had myself, in the year 1876, shown reason for holding that the ear does nevertheless take cognizance of such differences of phase. Moreover, the peculiar rolling or revolving effect to be noticed in slow beats is a proof that the ear perceives some difference due to difference of phase. Dr. Kœnig is, however, the first to put this matter on a distinct basis of observation. That such differences of phase occur in the tones of musical instruments is certain: they arise inevitably in every case where the sounds of subdivision are such that they do not agree rigidly with the theoretical harmonics. Fig. 5 depicts a graphic record taken by Dr. Kœnig from a vibrating steel wire, in which a note and its octave had been simultaneously excited. The two sounds were scarcely preceptibly different from their true interval, but the higher note was just sufficiently sharper than the true harmonic octave to gain about one wave in 180. The graphic trace has in Fig. 5 been split up into 5 pieces to facilitate insertion in the text. It will be seen that as the phase gradually changes, the form of the waves undergoes a slow change from wave to wave.

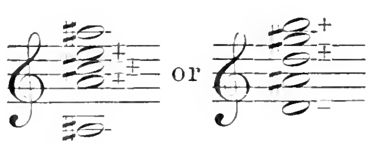
Now, it is usually assumed that in the vibrations of symmetrical systems, such as stretched cords and open columns of air, the sounds of subdivision agree with the theoretical harmonics. For example, it is assumed that when a stretched string breaks up into a nodal vibration of four parts, each of a quarter its length, the vibration is precisely four times as rapid as the fundamental vibration of the string as a whole. This would be true if the string were absolutely uniform, homogeneous, and devoid of rigidity. Strings never are so; and even if uniform and homogeneous, seeing that the rigidity

FIG. 5.



Graphic Record of Vibrations of Steel Wires.

of a string has the effect of making a short piece stiffer in proportion than a long piece, cannot emit true harmonics as the sounds of subdivision. In horns and open organ-pipes the width of the column (which is usually neglected in simple calculations) affects the frequency of the nodal modes of vibration. Wertheim found the partial tones of pipes higher than the supposed harmonics. Dr. Kœnig found with an open organ-pipe, about $7\frac{1}{2}$ feet long, that the eighth partial tone (or sound of subdivision) was a whole major tone higher than the theoretical eighth harmonic, and nearly agreed with the ninth harmonic of the fundamental tone! Further, there are the researches of Lord Rayleigh on the tones of bells, in which the sounds of subdivision are most extraordinarily inharmonious; affording us probably the reason why concerted hand-bell music is so unendurable. I do not know what the musicians present would say to such chords * as



They are given by Lord Rayleigh as representing respectively the sounds emitted by two of the bells of the peal at Terling.

* The signs + or − signify that the actual tones were respectively a little sharper or a little flatter than the note as written in the staff notation.

These things being so, it is manifestly insufficient to assume, as von Helmholtz does in his great work, that all timbres possess a purely periodic character; with the necessary corollary that all timbres consist merely in the presence, with greater or less intensity, of one or more members of a series of higher tones corresponding to the terms of a Fourier-series of harmonics. When, therefore, following ideas based on this assumption, von Helmholtz constructs a series of resonators, accurately tuned to correspond to the terms of a Fourier-series (the first being tuned to some fundamental tone, the second to one of a frequency exactly twice as great, the third to a frequency exactly three times, and so forth), and applies such resonators to analyse the timbres of various musical and vocal sounds, he is trying to make Nature fit to an ideal system which Nature does not herself follow. He is trying to make his resonators pick up things which in many cases do not exist—upper partial tones which are exact harmonics. If they are not exact harmonics, even though they exist, his tuned resonator does not hear them, or only hears them imperfectly, and he is thereby led into an erroneous appreciation of the sound under examination.

Further, when in pursuance of this dominant idea he constructs a system of electro-magnetic tuning-forks, accurately tuned to give forth the true mathematical harmonics of a fixed series, thinking therewith to reproduce artificially the timbres not only of the various musical instruments but even of the vowel sounds, he fails to reproduce the supposed effects. The failure is inherent in the instrument; for it cannot reproduce those natural timbres which do not fall within the circumscribed limits of its imposed mathematical principle. Nature does not sort men out into rigidly defined sets, one set exactly four feet high, another set exactly five feet high, another exactly six feet high. Neither does she, in the vibrations of strings, reeds, and air-columns impose rigid mathematical relations between the fundamental notes and the sounds of subdivision, though in many cases such mathematical relations are approximately attained. Harmony depends, beyond contest, on the approximate fulfilment of exact mathematical relations, and it is the grand achievement of von Helmholtz to have shown us why this is so. But the question of timbre involves the more subtle question of the minuter details of vibration by virtue of which the sound of a note in one instrument differs from that of the same note in an instrument of another kind, and depends therefore on the mechanism of the small vibrating parts. In these matters of delicate detail the natural departures from mathematical relations assert themselves. He who neglects these departures, or tries to square them to his preconceived theory, misses one of their most important characteristics, and can only render an imperfect account of them.

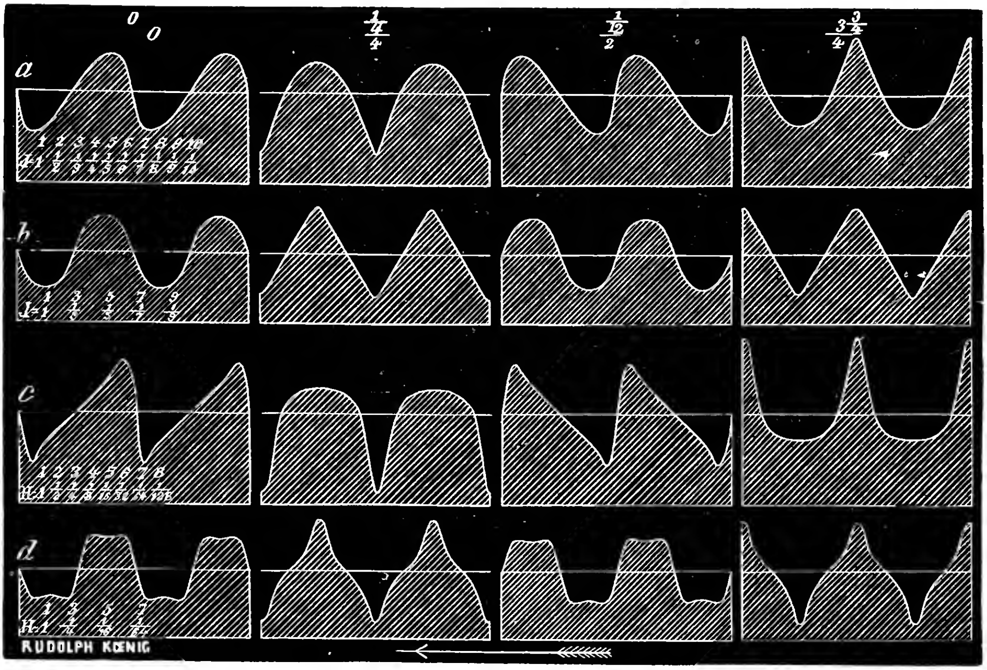
Nothing is more certain than that in the tones of instruments, particularly in those of such instruments as the harp and the pianoforte, in which the impulse, once given, is not sustained, the relations between the component partial tones are continually changing, both in relative intensity and in phase. The wavelets, as they follow one another, are ever changing their forms; in other words, the motions are not truly periodic—their main form may recur, but with modifications ever changing.

To estimate the part played in such phenomena by mere differences of phase—to evaluate, in fact, the influence of phase of the constituents upon the integral effect of a compound sound—Dr. Kœnig had recourse to the *wave-siren*, an earlier invention of his own, of which the wave-disks which have already been shown are examples.

In the first place, Dr. Kœnig proceeded synthetically to construct the wave-forms for tones consisting of the resultant of a set of pure harmonics of gradually decreasing intensity. The composition of complex wave-form out of simple waves belonging to a Fourier-series has long been a familiar subject to students of acoustics; and instruments have been devised by Wheatstone and others to produce them mechanically. Of such devices one of the most elegant is the curve-drawing machine of Mr. A. Stroh, here on the table, which he has kindly lent me, together with a number of curves produced by its means. With this beautiful little machine it is possible to draw curves compounded of any of the first eight waves of a harmonic series, in various phases and of various amplitudes.

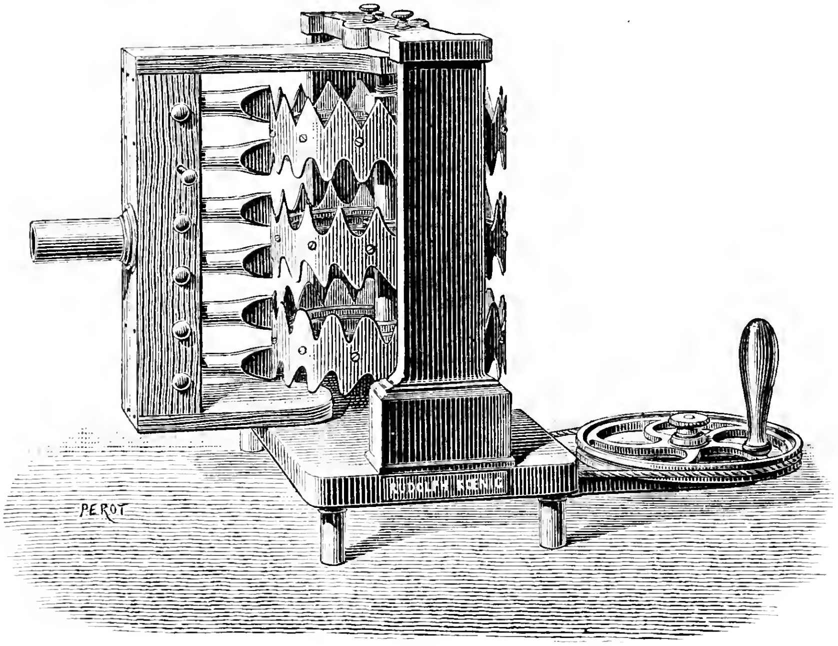
In Dr. Kœnig's synthetic study he began by drawing to scale the separate waves of the different orders. The curves of these, up to the tenth member of the series, were carefully compounded graphically: first with zero difference of phase, then with all the upper members shifted on one quarter, then with a difference of a half-wave, then with with a difference of three-quarters. The results are shown in the top line of curves in Fig. 6, wherein it will be noticed that the curve for difference of phase = $\frac{1}{2}$ is like that for zero difference, but reversed, left for right; and that the curve for difference of phase = $\frac{3}{4}$ is like that for difference = $\frac{1}{4}$, but inverted. Now, according to von Helmholtz, the sounds of all these four curves should be precisely alike, in spite of their differences of form and position. To test the matter, these carefully-plotted curves were set out upon the circumference of a cylindrical band of thin metal, the edge being then cut away, leaving the unshaded portion, the curve being repeated half a dozen times, and meeting itself after passing round the circumference. For convenience, the four curves to be compared are set out upon the separate rims of two such metallic cylindrical hoops, which are mounted upon one axis, to which a rapid motion of rotation can be imparted, as shown in Fig. 7. Against the dentellated edges of these rims, wind can be blown through narrow slits connected to the wind-chamber of an organ-table. In the apparatus (Fig. 7) the four curves in question are the four lowest of the set of six. It will be obvious that, as these curves pass in front of the slits from which wind issues, the maximum displacement of air will result

Fig. 6.



Synthesis of Wave-forms.

Fig. 7.



Wave-forms set out to act as Sirens.

when the slit is least covered, or when the point of greatest depression of the curve crosses the front of the slit. The negative ordinates of the curves correspond therefore approximately to condensations. Air is now being supplied to the slits; and when I open one or other of the valves which control the air-passages, you hear one or other of the sounds. It must be audible to every one present that the sound is louder and more forcible with a difference of phase of $\frac{1}{4}$ than in any other case, that produced with $\frac{3}{4}$ difference being gentle and soft in tones, whilst the curves of phase 0 and $\frac{1}{2}$ yield tones of intermediate quality. Dr. Kœnig found that, if he merely combined together in various phases a note and its octave (which was indeed the instance examined by me binaurally in 1876), the loudest resultant sound is given when the phase difference of the combination is $\frac{1}{4}$, and the mildest when it is $\frac{3}{4}$.

Returning to Fig. 6, in the second line are shown the curves which result from the superposition of the odd members only of a harmonic series of decreasing amplitude. On comparing together the curves of the four separate phases, it is seen that the form is identical for phases 0 and $\frac{1}{2}$, which show rounded waves, whilst for phases $\frac{1}{4}$ and $\frac{3}{4}$ the forms are also identical, but with sharply angular outline. These two varieties of curve are set out on the two edges of the highest metallic circumference in the apparatus depicted in Fig. 7. The angular waves are found to yield a louder and more strident tone than the rounded waves, though according to von Helmholtz, their tones should be alike.

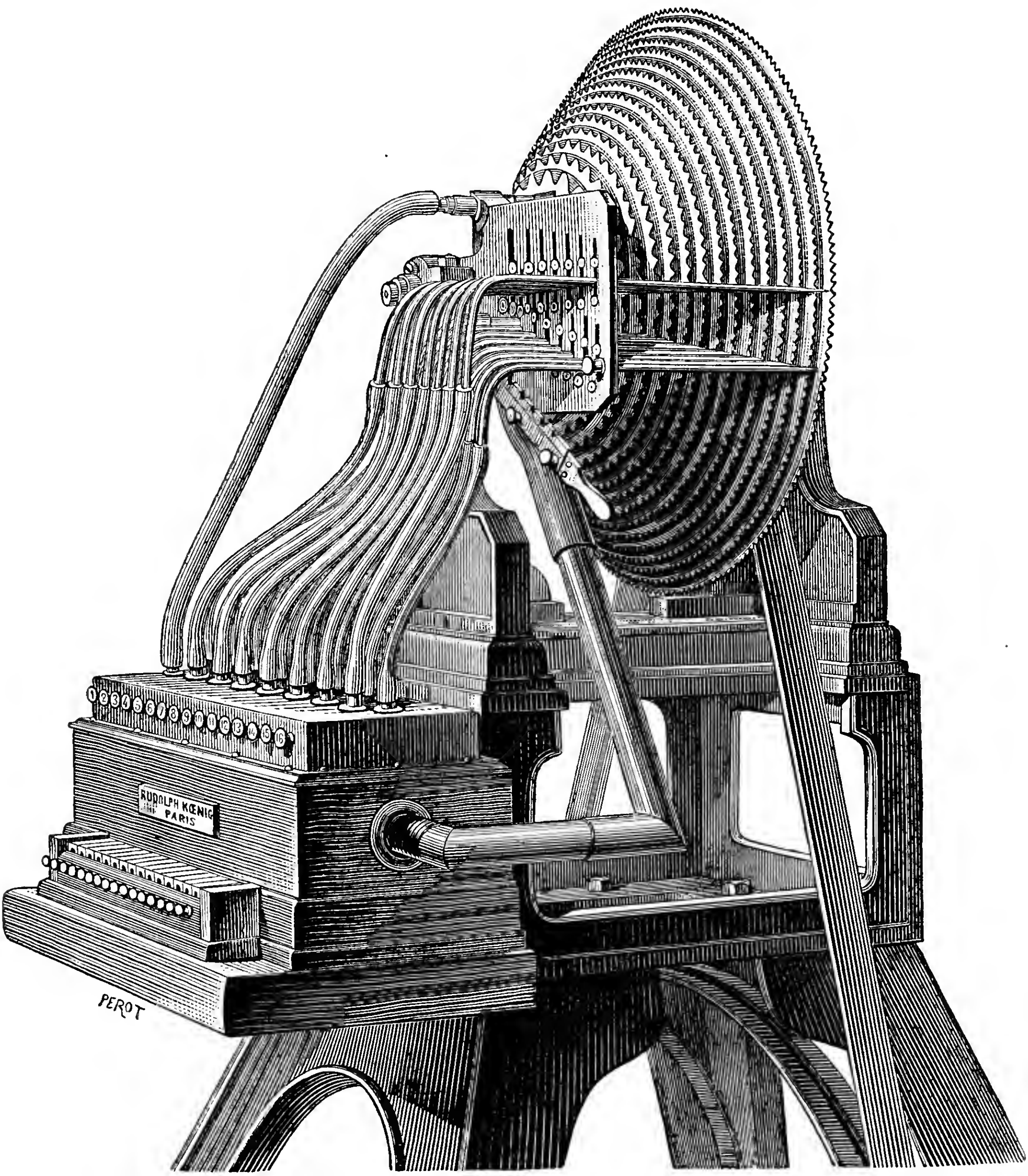
A much more elaborate form of compound wave-siren (Fig. 8) was constructed by Dr. Kœnig for the synthetic study of these phase-relations. Upon a single axis, one behind the other is mounted a series of 16 brass disks, cut at their edges into sinusoidal wave-forms. These represent a harmonic series of 16 members of decreasing amplitude, there being just sixteen times as many small sinuosities on the edge of the largest disk as there are of large sinuosities on that of the smallest disk. A photograph of the apparatus* is now thrown upon the screen. Against the edge of each of the 16 wave-disks wind can be separately blown through a slit. This instrument, therefore, furnishes a fundamental sound with its first fifteen pure harmonics. It is clear that any desired combination can be obtained by opening the appropriate stops on the wind-chest; and there are ingenious arrangements to vary the phases of any of the separate tones by shifting the positions of the slits.

The brass tubes, which terminate in 15 mouth-piece slits, are connected to the wind-chest by flexible rubber tubes. The mouth-piece tubes are so mounted that they can be displaced laterally in curved slots concentric with the disks. By the aid of templates

* It is described fully by Dr. Kœnig in his volume 'Quelques Expériences,' and was figured and described in 'Nature,' vol. xxvi. p, 277.

cut out in comb-fashion, and screwed, as shown in Fig. 12, to a lever handle, the mouth-pieces, or any set of them can be displaced at will, producing any pre-arranged difference of phase. Fig. 9 shows the

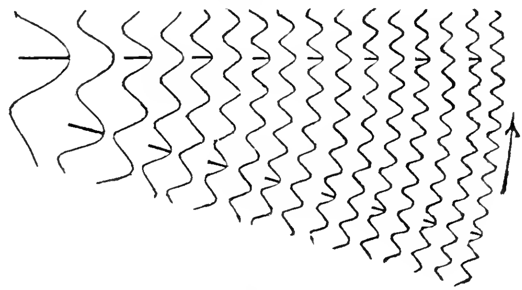
Fig. 8.



Kœnig's Compound Wave-siren for synthetic researches on the quality of Compound Tones.

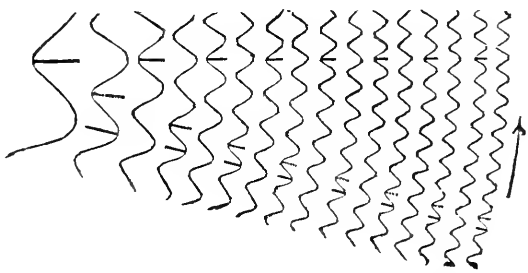
way in which the 15 movable slits are arranged with respect to the wave-disks and to the one fixed slit of the fundamental note; they are set in two radial lines for convenience of grouping, and so that each is opposite the crest of the wave of its own wave-disk; all the slits being simultaneously closed. This corresponds in Dr. Kœnig's nomenclature to a phase of $\frac{3}{4}$; minimum flow of air occurring

FIG. 9.



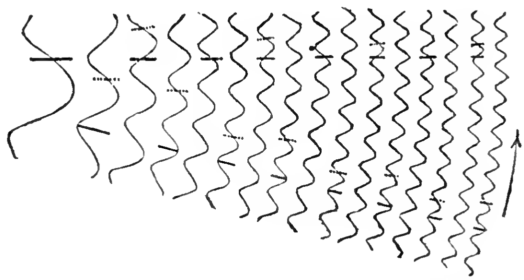
Positions of the Slits in front of the Wave-disks for combining the Sounds with Phase-difference $\frac{3}{4}$.

FIG. 10.



Position of the Slits for Phase-difference $\frac{1}{4}$.

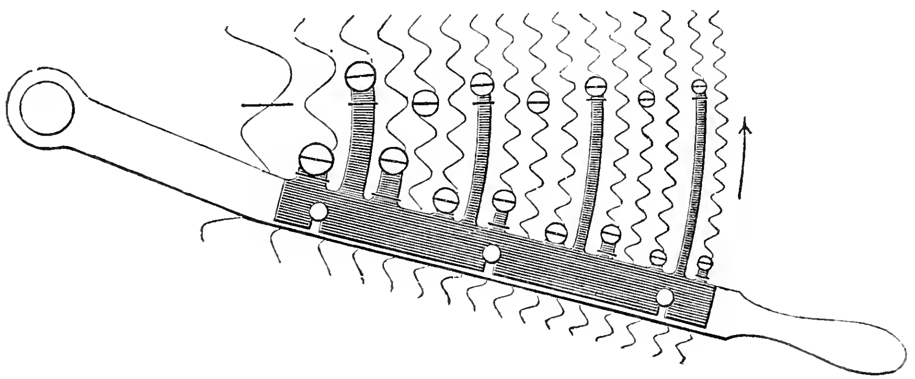
FIG. 11.



Position of the Slits for Phase-difference $\frac{1}{2}$.

simultaneously for all the components. Suppose now it is desired to change the phase so that the slits shall all be open simultaneously, all that is necessary is to move forward the slits of alternate members of the series, as shown in Fig. 10. This is done by a special template. Fig. 11 shows the positions required for phase of $\frac{1}{2}$. Fig. 12 shows the template for zero phase. To produce this there will be no movement required for the fourth, eighth, and twelfth members of the movable set, but the intermediate ones will

Fig. 12.



Position of the Slits for Phase-difference 0.

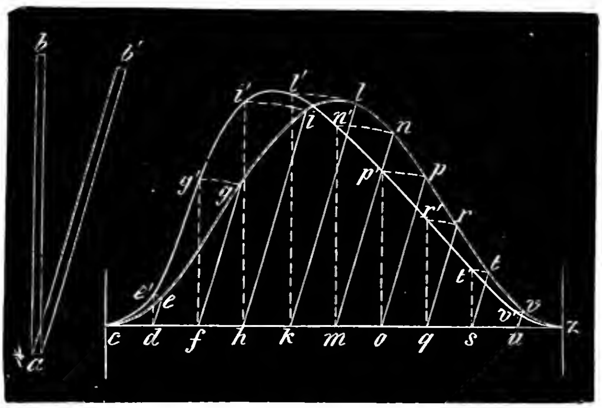
need to be shifted by $\frac{1}{4}$, $\frac{1}{2}$, and $\frac{3}{4}$ of their respective waves. When this set of positions is attained the condensation is increasing at the same moment for all the component waves, and reaches its mean values simultaneously. In experimenting the practice is to listen first to the combined sound with the undisplaced slits, and then, suddenly raising the lever, observe the change in the resultant sound.

The following are the chief results obtained with this instrument. If we first take simply the fundamental tone and its octave together, the total resultant sound has the greatest intensity when the difference of phase $\delta = \frac{1}{4}$ (i. e. when the maximum displacement of air occurs at the same instant for both waves); and at the same time the whole character of the sound becomes somewhat graver, as if the fundamental tone predominated more than in other phases. The intensity is least when $\delta = \frac{3}{4}$. If, however, attention is concentrated on the octave note while the phase is changed, its intensity seems about the same for $\delta = \frac{1}{4}$ as for $\delta = \frac{3}{4}$, but weaker in all other positions. The compound tones formed only of odd members of the series have always more power and brilliancy of tone for phase differences of $\frac{1}{4}$ and $\frac{3}{4}$, than for 0 and $\frac{1}{2}$; but the quality for $\frac{1}{4}$ is always the same as for $\frac{3}{4}$, and the quality for 0 is always the same as for $\frac{1}{2}$. This corresponds to the peculiarity of the corresponding wave-form, of which the fourth line of curves in Fig. 6 is an example. For compound tones corresponding to the whole series, odd and even, there is, in every case, minimum intensity, brilliancy, and stridence with

$\delta = \frac{3}{4}$, and maximum with $\delta = \frac{1}{4}$. Inspection of the first and third lines of curves in Fig. 6 shows that in these wave-forms that phase which is the most forcible is that in which the maximum displacement, and resulting condensation, is sudden and brief.

Observing that wave-forms in which the waves are symmetrical—steeper on one side than on the other—are produced as the resultant of a whole series of compounded partial tones, it occurred to Dr. Kœnig to produce from a perfect and symmetrical sinusoidal wave-curve a complex sound by the very simple device of turning into an oblique position the slit through which the wind was blown against it. In Fig. 13 is drawn a simple symmetrical wave-form, *e g l n p r t v*. If a

FIG. 13.



Effect of Tilting the Slit.

series of such wave-forms is passed in front of a vertical slit, such as *ab*, a perfectly simple tone, devoid of upper partials, is heard. But by inclining the slit, as at *ab'*, the same effect is produced as if the wave-form had been changed to the oblique outline *e'g'l'n'p'r't'v'*, the slit all the while remaining upright. But this oblique form is precisely like that obtained as resultant of a decreasing series of partial tones (Fig. 6, *a*). If the slit be inclined in the same direction as the forward movement of the waves, the quality produced is the same as if all the partial tones coincided at their origin, or with $\delta = 0$; while if inclined in the opposite direction the quality is that corresponding to $\delta = \frac{1}{2}$. It is easy to examine whether the change of phase produces any effect on the sound. Before you is a simple wave-disk, and air is being blown across its edge through a slit. On tilting the slit forward to give $\delta = 0$, you hear a purer and more perfect sound; and on tilting it back, giving $\delta = \frac{1}{2}$, a sound that is more nasal and forcible.

All the preceding experiments agree then in showing that differences of phase do produce a distinct effect upon the quality of compound tones: what then must we say as to the effect on the timbre of the presence of upper partial tones or sounds of subdivision that do not agree with any of the true harmonics? A mistuned harmonic—if the term is permissible—may be looked upon as a harmonic which is undergoing continual change of phase. The mistuned octave which yielded the graphic curve of Fig. 5 is a case in point. The wavelets are continually changing their form. It is certain that in a very large number of musical sounds, instrumental and vocal, such is the case.

It was whilst experimenting with his large compound wave-siren that Dr. Kœnig was struck by the circumstance that under no conditions, and by no combination of pure harmonics in any proportion of intensity or phase could he reproduce any really strident timbres of sound, like those of harmonium reeds, trumpets, and the like; nor could he produce satisfactory vowel qualities of tone. Still less can these be produced satisfactorily by von Helmholtz's apparatus with electro-magnetic tuning-forks, in which there is no mode of varying the phases of the components. The question was therefore ripe for investigation, whether, for the production of that which the ear can recognise as a timbre, a definite unitary quality of tone, it was necessary to suppose that all the successive wavelets should be of similar form. Or, if the forms of the successive wavelets are continually changing, is it possible for the ear still to grasp the result as a unitary sensation?

If the ear could always separate impure harmonic or absolutely anharmonic partials from their fundamental tone, or if it always heard pure harmonics as an indistinguishable part of the unity of the timbre of a fundamental, then we might draw a hard and fast line between mere mixtures of sound and timbres, even as the chemist distinguishes between mere mixtures and true chemical compounds. But this is not so: sometimes the ear cannot unravel from the integral sensation the inharmonious partial; on the other hand, it can often distinguish the presence of truly harmonious ones. Naturally, something will depend on the training of the ear; as is the case with the conductor of an orchestra, who will pick out single tones from a mixture of sounds which to less perfectly trained ears may blend into a unitary sensation.

Dr. Kœnig accordingly determined to make at least an attempt to determine synthetically how far the ear can so act, by building up specific combinations of perturbed harmonics or anharmonic partials, giving rise to waves that are multiform, as distinguished from the uniform waves of a true periodic motion. The wave-siren presented a means of carrying this attempt to a result. On the table before me lie a number of wave-disks constructed with this aim. These I will now set into rotation by aid of a silent-running water-motor, and will blow against them by means of a wind-chest, which supplies air to the slit at a sufficiently great and steady pressure. But I ought to warn you that these experiments are intended for the laboratory rather than for the lecture theatre, and only those who sit in the immediate front of the apparatus will hear the resultant sounds properly.

Upon the edge of the first of the series there has been cut a curve graphically compounded of 24 waves as a fundamental, together with a set of four perturbed harmonics of equal intensity. The first harmonic consists of 49 waves ($2 \times 24 + 1$); the second of 75 waves ($3 \times 24 + 3$); the third of 101 ($4 \times 24 + 5$); the fourth of 127 ($5 \times 24 + 7$). The resulting curve possesses 24 waves, no two of them alike in form, and some highly irregular in contour. The effect of blowing air through a slit against this disk is to produce a disagreeable sound, quite lacking in unitary character, and indeed suggesting intermittence.

The second wave-disk is constructed with the same perturbed harmonics, but with their amplitudes diminishing in order. This disk produces similar effects, but with more approach to a unitary character.

In the third disk there are also 24 fundamental waves, but there are no harmonics of the lower terms, the superposed ripples being perturbed harmonics of the fifth, sixth, and seventh orders. Their numbers are $6 \times 24 + 6$; $7 \times 24 + 7$; and $8 \times 24 + 8$; being, in fact, three harmonics of a fundamental 25. This disk gives a distinctly dual sort of sound; for the ear hears the fundamental quite separate from the higher tones, which set themselves to blend to a unitary effect. There is also an intermittence corresponding to each revolution of the disk, like a beat.

The fourth disk resembles the preceding; but the gap between the fundamental and the three perturbed harmonics has been filled by the addition of three true harmonics. This disk is the first in this research which gives a real timbre, though it is a peculiar one: it preserves, however, a unitary character, even when the slit is tilted in either direction. The 24 waves in this disk all rake forward like the teeth of a circular saw, but with multiform ripples upon them. The quality of tone becomes more crisp when the slit is tilted so as to slope across the teeth, and more smooth when in the reverse direction.

The fifth disk, which is larger, has 40 waves at its edge; these are cut with curves of all sorts, taken haphazard from various combinations of pure harmonics in all sorts of proportions and varieties, no two being alike, the maxima and minima of the separate waves being neither isochronous nor of equal amplitude. This disk gives an entirely unmusical effect, amid which a fundamental tone is heard, accompanied by a sort of rattling sound made up of intermittent and barely recognisable tones.

The sixth disk is derived from the preceding by selecting eight only of the waves, and repeating them five times around the periphery. In this case each set of eight acts as a single long curve, giving beats, with a slow rotation, and a low tone (accompanied always by the rattling mixture of higher tones) when the speed is increased.

The seventh disk was constructed by taking 24 waves of perfect sinusoidal form, and superposing upon them a series of small ripples of miscellaneous shapes and irregular sizes, but without essentially departing from the main outline. This disk gives a timbre in which nothing can be separated from the fundamental tone, either with vertical or tilted slit.

The eighth and last disk consists of another set of 24 perfect waves, from the sides of which irregular ripples have been carved away by hand, with the file, leaving, however, the summits and the deepest parts of the hollows untouched, so that the maxima and minima are isochronous and of equal amplitude. This disk gives also a definite timbre of its own, a little raucous in quality, but still distinctly having a musical unity about it.

We have every reason, therefore, to conclude that the ear will recognise as possessing true musical quality, as a timbre, a combination in which the constituents of the sound vary in their relative intensity and phase from wave to wave.

What, then, is *a timbre?* Dr. Kœnig would be the first to recognise that these experiments, though of deepest interest, do not afford a final answer to the question. We may not yet be in a position to frame a new definition as to what constitutes a timbre, but we may at least conclude that, whenever that definition can be framed, it will at least include several varieties, including the non-periodic kinds with multiform waves, as well as those that are truly periodic with uniform waves. We must not on that account, however, rush to the conclusion that the theory of von Helmholtz as to the nature of timbre has been overthrown. The corrections introduced into lunar theory by Hansen and Newcomb have not overturned the splendid generalisations of Newton. What we can and must confess is that we now know that the acoustic theory of von Helmholtz is, like the lunar theory of Newton, correct only as a first approximation. It has been the distinctive merit of Dr. Kœnig to indicate to us the magnitude of the correcting terms, and to supply us not only with a rich store of experimental facts but with the means of prosecuting the research synthetically beyond the point to which he himself has attained.

Fascinating as is the pursuit of such questions, one cannot conclude these researches without pausing to enquire how much nearer they have brought us to the ultimate explanation of the power which music exercises upon us. And it must be confessed frankly that the discovery of the physical foundations of the science leaves us very much where we were before. For music, though a science, is before all an art, and can be interpreted only by the artist. Science has nothing to say concerning the vast range of musical impressions, which are purely associative in their character. No analysis, however searching, will explain away the thrill that runs through us as we listen to some simple phrase or motif which recalls the stately prelude, the inspiring theme, the passionate andante, the gay barcarolle, the massive triumphal march, or the wailing miserere. The horn of Siegfried summons us to Brünhilde's rock, quite irrespective of the upper partial tones which accompany its fundamental tone. We may try to analyse our sensations with endless metaphysical refinements: we may investigate their physical causes by the most careful dissection of the waves and wavelets with which the musician's hand and instrument flood our listening ears—and we are not a whit nearer either to composing music ourselves or to comprehending its intrinsic beauty and power. We might as well suppose that we could become painters by going through a course of chemical analysis of the paints employed by a Watts or a Herkomer. True, a knowledge of the chemistry of pigments will assist the artist by sparing him blunders and giving him something more than empirical rules to guide him in the mixing of his paints. So likewise, a knowledge of the physical basis of music may help the musician by lifting him above merely empirical rules, which, like that forbidding consecutive fifths, are founded on no rational basis, being deliberately violated by the builder of every organ, and set aside by every great composer. But, make him a musician, never. Analysis, though it is an instinctive faculty of the mind, is not art. Of some arts indeed, it may be said that analysis is death; but only of those which have been based on falsehood or superstition. Art that is true fears nothing from analysis; it is beyond and above its reach. And music, the most refined, the most subtle, the most spiritual of the arts, defies analysis more effectually than any. Our enquiry leaves its emotional and spiritual power untouched, unchanged.

Some things there are which lose their charm when touched by the finger of enquiry: their spell is snapt; their magic vanishes into thin air. Not so is it with music:—

> "For music, which is as a voice,
> A low voice calling fancy, as a friend,
> To the green woods in the gay summer-time,
> Seeing we know emotions strange by it
> Not else to be revealed . . . .
> . . . is earnest of a Heaven."

[S. P. T.]

Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, January 23, 1891.

Sir Frederick Bramwell, Bart. D.C.L. F.R.S. Honorary Secretary and Vice President, in the Chair.

The Right Hon. Sir Edward Fry, Lord Justice of Appeal, F.R.S. F.S.A. F.L.S. *M.R.I.*

British Mosses.

(Abstract.)

I cannot without an apology address the Royal Institution on this subject. I can make no pretence to speak with authority; I speak only as a learner who has devoted to the subject some leisure from amidst avocations of a very different kind. But the pleasure I have derived from the study, the sense, whenever I am in the country, that I am surrounded with a world of variety and beauty of which I was formerly only dimly conscious, and the hope of communicating some of this pleasure to others may, I hope, furnish some apology for my venturing to speak on the subject.

Classification.—Without entering into any question as to the best classification of the mosses, or the relative systematic value of the different groups, the following table, which is arranged in an ascending rank, will be sufficient to show the position of the mosses in the vegetable kingdom, and the principal groups into which they may be divided:—

TABLE A.

		Series.	Orders.	Examples.
Vascular Cryptogams				
Muscineæ	i. Musci	Pleurocarpæ		Hypnum
		Acrocarpæ	Stegocarpæ	Polytrichum
			Cleistocarpæ	Phascum
		Anomaleæ	Schizocarpæ	Andræa
			Holocarpæ	Archidium
	ii. Sphagnaceæ			
	iii. Hepaticeæ	Jungermanniaceæ		
		Marchantiaceæ		
Algæ, &c.				

From this table it will be gathered that the mosses, using that word in its wide signification, stand at the head of the cellular cryptogams, and that above them are the vascular cryptogams, of which the ferns are one of the best-known groups. From these vascular cryptogams the mosses are, however, separated by a distance which Goebel has described as a chasm "the widest with which we are acquainted in the whole vegetable kingdom."

From the table it will be further seen that the larger group of the Muscineæ divides itself into three principal smaller groups; the Hepaticæ or liverworts, the Sphagnaceæ or turf mosses, and the Musci or true mosses—urn-mosses, as they have been called, from the form of their capsule. Passing over the other subdivisions, it may be observed that the Acrocarpous mosses are those which carry their capsules at the end of the axis of growth, whilst the Pleurocarpous mosses bear their fructification on stalks, more or less long, proceeding from the sides of the axis. Amongst these Pleurocarpous mosses occurs the old genus Hypnum (broken up by modern systematists into several genera), the largest of all the genera in these islands or in Europe—a vast group which occupies amongst mosses something like the place which the Agarics occupy amongst the Fungi.

Number of British Species.—If we were to try and ascertain the number of the British Muscineæ from the systematists of some few years ago, like Hooker and Wilson, the species would number between 500 and 600; but according to the views of more recent writers, the number would probably rise to something between 800 and 900. The true mosses are the most numerous, the turf-mosses by far the fewest.

Date of Flora.—What is the date of this moss flora of Britain? Two ancient collections enable us to give some reply to the question. In an interglacial bed near Crofthead, in Renfrewshire, eleven species of moss were discovered, and with one possible exception all are well-defined British species of the present day. If we take Mr. Wallace's chronology, and hold that 80,000 years have passed since the Glacial epoch disappeared, and 200,000 years since the Glacial epoch was at its maximum, we may perhaps give from 100,000 to 150,000 years for the age of this little collection. Out of the eleven mosses discovered, seven belong to the genus Hypnum, or the family Hypnaceæ. This collection, then, is evidence, so far as it goes, (1) that the existing moss flora is as old as the interglacial epoch; (2) that the Hypnaceæ were as dominant then as now: and (3) that the specific forms have remained constant since that epoch.

Another collection of fourteen mosses has been discovered in a drift in the Clyde valley above the Boulder drift, and tends to confirm the previous conclusions; as all the species are existing, all now inhabit the valley of the Clyde, and the Hypnaceæ are still predominant, though not in so great a proportion as in the Renfrewshire bed.

The fossil remains of mosses are not numerous, nor for the most part very ancient. Heer inferred their existence in the Liassic period, from the presence of remains of a group of small Coleoptera, the existing members of which now live amongst mosses—an inference which seems not very strong. But recently the remains of a moss have been found in the Carboniferous strata at Commentry, in France. It appears to be closely allied to the extant Polytrichum, the most highly-developed genus of mosses; so that we have here a phenomenon like that which occurs in reference to the Equisetaceæ and Lycopodiaceæ, viz. that the earliest fossil species known belong to very highly-developed forms of the group.

Life-History.—The following table (p. 240) is intended to illustrate the life-history of a moss in its fullest and in its abbreviated courses, and to bring this history into comparison with that of the ferns.

Attention should first be drawn to the second column, which shows the life history in its fullest form. It will be seen that it starts with a spore and returns to a spore.

From (1) the *spore*, which is a simple cell, proceeds (2) the *protonema*, a line of cells, extending by transverse divisions, so that it consists of single cells joined end to end to one another—an organism indistinguishable from the hypha of an Alga. At points this hypha throws off lateral branches which are always of less diameter than the principal ones. There is thus produced a tangled mat of fibres, running on or near the surface of the ground, and often coloured by chlorophyll. It is the green stuff so often seen in flower-pots which have been allowed to get too damp. At points in the primary hypha individual cells begin to divide in a new fashion—not by transverse septa as before, but by septa differently inclined, so as to produce the rudiments of leaves; and the direction of growth changes from horizontal to vertical. Thus is formed (3) the *bud*, which by growth gives rise to (4) the *moss plant;* on this plant, sometimes in close proximity to one another, sometimes in different parts of the same plant, sometimes on different plants, are formed (*a*) the female cell or archegonium, and (*b*) the antheridia or male organs, the antherozoids proceeding from which seek and find and fertilise the archegonium. This completes the first part of the life of the plant, the oophytic generation which results in a single sexual cell, viz. the fertilised archegonium. From this cell arises the next generation, consisting of the *sporogone* or stem bearing the capsule and the capsule itself, in which without fertilisation are produced spores. The plant has thus started with the spore, an asexual cell, reached the point where its whole future is gathered up in a sexual cell, which has produced an organism again producing an asexual cell: we started with a spore, and have returned to a spore; we have travelled round a circle, divisible into two parts or generations, one sexual, the other asexual; and we have therefore a case of alternation of generations. To make this statement more clear, it may be observed that a generation is here spoken of as that part of the life of an organism which intervenes between the two points at which its whole future is gathered up into one cell; that such a cell is sexual when it is the result of the combination of two previously existing and independent cells; that such a cell is asexual when it is not the result of such combination; that an alternation of generation exists, whenever in the complete cycle of existence or life-history there are two points at which the whole organism is reduced to a single cell, and when the forms of the organism in the two intervals of its development are

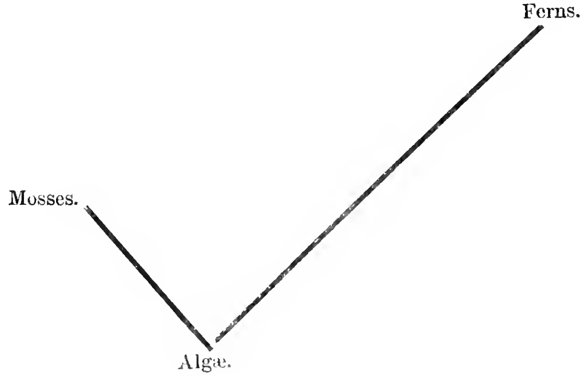
TABLE B.—LIFE-HISTORY.

GENERATION.	FERN.		MOSS.					
Oophyte, or Generation with Sexes	Spore		Spore		Gemma			
	Prothallus	Archegonia Antheridia	Protonema		Protonema	Protonema		
			Bud		Bud	Bud	Bud	Moss Plant
			Moss Plant	Archegonia Antheridia	Moss Plant	Moss Plant	Moss Plant	Moss Plant
					Gemma	Protonema	Bud	Moss Plant
Sporophyte, or Generation without Sexes.		Fern Plant		Sporogone				
		Sporange		Spore				
		Spore						

different. In the mosses, where the sporogone co-exists with and is organically connected with what I have called the moss plant, it is evident that the two generations are not such, according to the more popular notion of that word; they are not independent, nor necessarily successive.

A comparison of the first and second columns of the last table reveals at once the likeness and the unlikeness of the life-histories of the moss and of the fern. In each case the spore produces a growth of the form and nature entirely unlike the mother-plant—in one case a hypha, in the other a thallus. But whilst in the moss the protonema produces the moss plant, in the fern the prothallus itself is the home of the male and female organs, and of the sexual process, so that the fern plant belongs to the sporophytic and the moss plant to the oophytic generation; the fern plant is the result of the sexual union, whilst the moss plant is produced from an asexual spore; the fern plant produces spores asexually, the moss plant produces the sporogone as the result of the sexual union.

The observations which arise in connection with this comparison are numerous. (1) It is the belief of botanists, ever since the investigations of Hofmeister, that not mosses and ferns only, but all the phanerogams, go through an alternation of generations consisting of the oophytic and sporophytic generations. (2) It appears that the mosses and the Characeæ are the only groups of plants in which the conspicuous and vegetative organism—the plant, in ordinary parlance—belongs to the oophytic generation: (3) That, in consequence, the plant of the moss is in no sense the ancestor of the plant of the fern, or of the phanerogams, but belongs to a different generation from these; and further, that the leaves, the stem, and the epidermis of the moss have no genetic connection with the leaves, the stem, or the epiderms of our flowering plants, whilst the fibro-vascular bundles of the sporogone of the Polytrichum, and the stomata on the apophyses of some mosses will belong to the same generation which, in the vascular cryptogams and phanerogams, produces similar organs. (4) That the great chasm in the systematic arrangement of the vegetable kingdom between the mosses and the ferns is thus accounted for by their belonging to different generations, so that the ferns are not in any sense descendants of the mosses, but only collateral relatives, as thus—



(5) That, consequently, the mosses not only represent the highest development known of the cellular cryptogams, but the highest point in one line of development, in which the oophytic generation took the lead in importance; whilst the vascular cryptogams and phanerogams are the results of another and more successful line of development, in which the sporophytic generation took the lead as the prominent part in the life-history.

The appearance of similar organs in two independent lines of development—i. e. of the leaves, stem, and epidermis—in the mosses, and then in the ferns, without any relation of descent, is a thing well worthy of being pondered over by those who study evolution: it may suggest that the two lines of development, though independent, are governed by some principle which brings about such like results: it may be compared with the liknesses which occur in the animal kingdom between the placental and marsupial mammals.

The remaining columns of the table above given will best be understood after a study of the next succeeding table.

Modes of Reproduction.—Hitherto we have considered only the reproduction from a spore produced in the special organ for their production—the spore capsule. But, in fact, one of the most striking peculiarities of the mosses is the vast variety of their modes of reproduction.

TABLE C.—MODES OF REPRODUCTION.

A.—*With Protonema.*

i. Spores.	in capsule	
ii. Gemmæ	on end of leaf	*Leptodontium gemmascens.* *Orthotrichum phyllanthum.* *Grimmia Hartmani.*
	on midrib	*Tortula papillosa.*
	in axils of leaves	*Bryum.*
	in balls	*Aulacomnion.*
	in cups	*Tetraphis.*
iii. Protonema	from rhizoids	*Phascum.* *Polytrichum.*
	from aërial rhizoids ..	*Dicranum undulatum.*
	from terminal leaves ..	*Oncophorus glaucus.*
	from base of leaf	*Funaria hygrometrica.*
	from midrib	*Orthotrichum Lyelii.*
	from margin	*Buxbaumia aphylla.*
	from stems	*Dicranum undulatum.*
	from calyptra	*Conomitrium julianum.*

B.—*Without Protonema.*

iv. Leaf-buds	on rhizoids	*Grimmia pulvinata.*
v. Leaf-buds	on aërial rhizoids	*Dicranum undulatum.*
vi. Bulbs	on stem	*Bryum annotinum.*
vii. Young Plants ..	at ends of branches	*Sphagnum cuspidatum.*
viii. Leafy Branches ..	becoming detached	*Conomitrium julianum.* *Cinclidotus aquaticus.*
ix. Rooting of main axis	.. ..	*Mnium undulatum.*

In the above table, which is probably far from exhaustive, I have endeavoured to exhibit many of these modes of reproduction, dividing them into those cases in which it takes place with protonema, and those cases in which it takes place without.

Weismann's Theory.—The consideration of this table is not without its interest in reference to Prof. Weismann's theory of the division of the cells and plasma of organisms into two kinds: the germ cells and germ plasma endowed with a natural immortality, and the somatic cells and somatic plasma with no such endowment. That the mosses are a difficulty in the acceptance of the theory as a universal truth, the Professor himself admits. The evidence of the mosses seems to amount at least to this: that in this whole group, the highest in this line of development, where the oophytic generation produces the principal plant, and where there are highly specialised organs for the production of spores or germ cells—that in this whole group either there is no effectual separation between the two kinds of plasma, or that the germ plasma is so widely diffused amongst the somatic plasma that every portion of the plant is capable of reproducing the entire organism.

Comparison with Zoological Embryology.—The table will further offer us some points of comparison with animal embryology.

In that branch of physiology, one of the most remarkable facts is what has been called recapitulation, i. e. the summary in the life of the individual of the life of the race, so that the development of the individual tells the development of the race—e. g. the gills of the tadpole tell us of the descent of the Batrachians from gill-breathing animals.

So here we cannot doubt that the protonema of the moss tells us of the descent of the whole group of mosses from the Algæ.

Another remarkable fact in animal embryology is the co-existence in exceptional cases of the mature and the immature form; so the axolotl retains both gills and lungs throughout its life. In like manner it happens with some mosses, e. g. the *Phascum*, retains its algoid protonema throughout its life.

Again, in zoological embryology, an attempt is often found, to use the language of Prof. Milnes Marshall,* "to escape from the necessity of recapitulating, and to substitute for the ancestral process a more direct method."

In like manner the preceding tables will show to how great an extent Nature has adopted the system of short-circuiting in the reproduction of the mosses; for in every mode of reproduction, except that through sporogone and spore, it will be observed that a shorter circuit is travelled, e.g. the *Orthotrichum phyllanthum* produces cells at the end of its leaves, which, falling to the ground, throw out a protonema which produces a bud, and then a moss plant, and then a cell at the end of the leaf, and the whole sporophytic

---

* Address to Biological Section of British Association ('Nature,' vol. xlii. p. 478).

generation is evaded; and so on in gradually shortening circles (see Table B), till we get the case of a Sphagnum, which produces a little Sphagnum plant at the end of its leaves without protonema—whether without bud, I do not know. In every case Nature seems to leave out the sexual reproduction if she can help it, and directs her whole attention to the production of the vegetative organism—the moss plant in the popular sense—which she never omits.

Another point of comparison arises, but this time it is one of contrast between the embryology of the two kingdoms.

In animals, to again quote Prof. Milnes Marshall, "Recapitulation is not seen in all forms of development, but only in sexual development, or at least only in development from the egg. In the several forms of asexual development, of which budding is the most frequent and the most familiar, there is no repetition of ancestral phases, neither is there in cases of regeneration of lost parts."

In mosses, on the contrary, the table last given shows that in most of the modes of reproduction, the ancestral form, the algoid protonema, is retained and reproduced, whereas in the growth from a sexual cell, i. e. in the sporogone, the ancestral form entirely disappears.

The peristome, or girdle of teeth round the orifice of the capsule, assumes very varying forms, often of great beauty and interest. In some of the mosses it is absent, in some it consists of one ring of teeth, in many of two rings, and in one foreign genus (Dawsonia) there are as many as four circles of teeth.

The object served by this complicated structure is not, perhaps, very certain, but it seems to be intended to secure the retention or exclusion of the spores from the spore sac in such conditions of the atmosphere as will best conduce to their germination. In the gymnostomous mosses (i. e. those without peristome) it is observed that the spores sometimes germinate within the capsule, an event which is probably adverse to the prospects of the race.

In some genera, as e. g. Bartramia, the teeth of the peristome are erect in dry weather and convergent in wet weather; and in such cases it seems probable that the spores require dry weather when first emitted. In other genera, as e. g. Bryum, the teeth are convergent in dry weather and expanded in wet weather: in those cases it is probable that the spores require wet weather when first emitted.

The motion of the teeth of the peristome appears to be due to the action of a ring of specialised cells which surrounds the mouth of the capsule at the base of the teeth; and the opposite ways in which these cells act in the same condition of moisture in different genera, is a remarkable circumstance.

To anyone who studies the subject, the immense variety as well as beauty of the peristomes of mosses becomes very impressive. If the sole end be the protection and extrusion of the spores in the proper weather respectively, why is there this infinite wealth and variety of form and of colour? The question can be asked, but hardly can be answered, and the mind of the beholder is left, as it so often is, when contemplating the richness of Nature, in a state of admiration and wonder and ignorance.

Sphagnaceæ.—Vast tracts of land in this country and throughout Northern Europe and America are covered with plants of this group, and large tracts which are now fertile agricultural land, where they have entirely ceased to grow, have in former times been occupied by them. The bogs of Ireland, which are mainly constituted of turf moss, were computed in 1819 by the Bog Commissioners to occupy 2,830,000 acres. No moss has probably ever, at least in the present state of the globe, played so large a part as the Sphagnum or peat moss.

Structure.—It is to the peculiar structure of the peat moss that this great part on the theatre of the globe is to be attributed.

Leaves.—In the young leaves the component cells are all alike; then by a differential growth we are presented with square cells surrounded by four narrow and oblong ones; then chlorophyll forms in these narrow cells, but is absent from the square cells; from these the contents disappear, and water or water-like fluid occupies the whole cell; subsequently annular and spiral threads develop on the walls of the square cells. The intimate structure of the leaf thus enables it to absorb great quantities of water.

But again, the shape of the leaves is in many species adapted to the retention of water. By a retardation of the lateral as compared with the mesial growth, the leaf assumes a boat shape. Often the edges of the leaves are turned over; the leaf thus affords means of holding water. Again, the lateral branches grow in groups from the stem, and some of these branches are generally pendent, and in close proximity to the stem, so that an immense capillary attraction is exerted by them.

Again, the *stem* itself is surrounded or rather is more than half occupied by large water-holding cells, and pitchers of a very peculiar form.

Again, the mode of growth of the plant, abandoning its moorings on the soil and throwing out roots into the water, and growing successively year after year, enables it not only to attain great growth, but also, when the occasion demands, to keep pace with the rise of the water in which it may be growing, "the individual thus becoming," it has been said, "in a manner immortal, and supplying a perpetual fund of decomposing vegetable matter."*

Physical Results from Structure.—The result of these peculiarities is that the entire plant of any species of Sphagnum is a perfect sponge. When dry it is capable (as may easily be found by experiment) of rapidly absorbing moisture, and carrying it upwards through the plant; and when growing in vast beds it acts thus on a great scale. Everyone who knows Scotland must know how on many a

* Macculock, 'Western Islands,' p. 130.

steep mountain-side, or on the bottom and sides of a gorge, these beds will hold up a great body of water against the force of gravity; and again, the Irish bogs are described as often ascending from the edges towards the interior, sometimes by a gradual, and sometimes by a sudden ascent, so that at times the bog is so high that it reaches the height of the church steeples of the adjoining country, without any rising ground intervening.

These peculiarities in the structure of Sphagnum have produced considerable physical effects.

(1) Everyone knows the different effects of rain falling on a land of bare rock or sand, like the Sinaitic desert, and on a retentive soil; in the one case it produces a freshet or a flood, that leaves no trace behind; in the other it is held for a while in suspense, and only gradually passes into the streams. The glaciers and the Sphagnum beds of the mountains of Europe alike act as compensation reservoirs —receive large quantities of moisture as it falls, and retain it till the drier season comes, when it gradually passes away in part; but for these reservoirs, many of the rivers would exhibit a far greater shrinkage in summer and autumn than is now the case.

But (2) the Sphagnum beds have become peat, and have gradually filled up the ancient lakes and morasses, and turned water into dry land. It is true that the peat appears under some circumstances to be formed by other vegetables than Sphagnum, and in all cases it has probably some other plants or roots growing amongst it. Mr. Darwin tells us that in Terra del Fuego and the Chonos Archipelago, peat is formed by two phanerogamous plants, of which one at least seems endowed with an immortality something like that of the Sphagnum; and the peat of the fens of Lincolnshire is formed mainly of *Hypnum fluitans*. But Sphagnum appears to be the main constituent of peat in Ireland, Scotland, and, so far as my researches have gone, in England; the peculiar spiral threads of the cells of the Sphagnum leaf being easily detected in the peat so long as it retains traces of its organic origin.

Ancient Forests.—The peat mosses, and the sea-shores of our islands, and of the adjoining mainland, reveal, as it is very well known, traces of ancient forests. Many parts of England, nearly all the mainland of Scotland, the Hebrides, the Orkneys, and the Shetlands, Ireland, and Denmark, the shores of both sides of the English Channel, Normandy, Brittany, the Channel Islands, and Holland, and the shores of Norway, all bear evidence to the presence of these primæval forests; and what is more, to the successive existence of forests, each in succession living above the buried remains of the earlier ones.

What is the cause of the disappearance of these ancient forests one after the other? To this question various answers have been proposed.

The Romans, it has been suggested, in their inroads, cut ways through the forests and laid waste the land. But, wide as was the spread of the wings of the Roman eagle, the phenomenon in question is of far wider extension. They never conquered Denmark, or Norway, or Ireland, or the islands of Scotland: in Scotland, and even in England, their operations could never have covered the whole country; and as regards some of our peat mosses, we know that they must have existed long before the Roman invasion; for at least on the borders of Sedgmoor we have traces of their using peat for fuel as it is used there at the present day.

Still humbler agents have been invoked, in the supposition that the beaver and other rodents were the authors of the destruction of the forests. So far as I can judge, the cause suggested seems inadequate to the effect.

Again, changes in climate have been suggested. But, although there may be some evidence from the succession of the trees of a gradual amelioration in the climate, we know of no evidence of changes of so sudden and violent a character as would destroy the existing forests over large areas. Moreover, with few exceptions, the trees of the destroyed forests are such as are now found wild, or will grow easily in the spots where they lie buried.

The overthrow by storms has, again, been suggested as the cause of this wholesale destruction; and the fact that in some of the peat bogs of the West of Scotland the trees that have fallen lie to the north or north-east, and in some of those in Holland to the south-east, in the direction of the prevailing winds in those countries respectively, affords some reason to believe that wind has given the *coup de grace* to the dying trees, and determined the direction of their fall. But it is much more likely that this was the work of the wind, than that successive forests should have been swept from the face of vast tracts of Europe by the agency of wind alone. Moreover, in some cases the trunks as well as the bases and roots of the trees are found standing or buried in the bogs.

Allowing that some or all of these agencies may have had their part in the destruction of the forests, I believe that the growth of Sphagnum has been the greatest factor in the work of destruction. "To the chilling effect of the wet bog mosses in their upward growth must be attributed," says Mr. James Geikie, "the overthrow of by far the greater portion of the buried timber in our peat bogs" (Trans. Roy. Soc. Edin., xxiv. 380).

But, it will be said, assuming that this may be the case with one growth of forest, how about the successive destruction of successive forests? The answer is, I believe, to be found in the curious change which peat undergoes, and which converts it from a substance highly absorbent of water into one impervious to it.

The section exposed by a peat-cutting in, I believe, almost all cases exhibits two kinds of peat, the one known variously as red peat—or red bog, or fibrous bog, or in Somersetshire as white turf—which lies at the top, and the other, a black peat, which lies at the bottom. The red peat retains visible traces of the Sphagnum of which it is mainly composed, and is highly absorbent of moisture; whilst the black peat has lost all, or nearly all, traces of the minute structure of the cells, and is not only unabsorbent of moisture, but is impervious to it. In fact, it constitutes an insoluble substance which is said to be scarcely subject to decay, so that it is used in Holland for the foundations of houses, and is found unchanged after ages, and when the buildings have fallen into decay. It is even said to have remained unchanged after three months' boiling in a steam-engine boiler. The broad difference between these two kinds of peat may easily be ascertained by anyone who will subject the two kinds to the action of water.

If we now take a section of a peat bog, with a succession of forests one above another, the history of the formation will be, I believe, much as follows:—

We must get a water-tight bottom—sometimes this is a stiff clay, sometimes a pan, i. e. a stratum of sand or gravel made into a solid plate by the infiltration of insoluble iron oxides, themselves often due to decaying vegetable matter. The necessity of this water-tight bottom is well shown by the fact that in places in the Irish bogs where a limestone subsoil occurs the bog become shallow and dry.

If on this clay bottom or sandy or gravel soil a forest arises, it may flourish for a considerable period, until the natural drainage of the area is stopped, whether by the choking up of the course of the effluent stream, or from the aggregation of vegetable matter, or from the fall in the course of nature of the trunks of the trees themselves. Everyone who will consider how much care our rivers require in order to make them flow with regularity to the sea—who thinks for instance, of the works in the Thames valley, or in the upper valleys of the Rhine—will see how often and how easily, in a country in the condition of nature, stagnant waters will arise. In the morass thus formed the Sphagnum has grown, years after years, and if it has not destroyed the old trees it has prevented the growth of young ones. The stools of the trees buried in the antiseptic waters of the Sphagnum pools have been preserved, whilst the fallen trunks have, except when preserved by the like circumstance, rotted, and added their remains to the peat which the Sphagnum has been producing. It has been observed in several places in Scotland, that the underside of fallen trees which would be protected from decay by the tannin of the Sphagnum is preserved, whilst the upper side has decayed or rotted away. Year by year the process of decay on the lower parts of the Sphagnum goes on until the water grows shallower and at last disappears, leaving the original morass choked and filled up by the Sphagnum and the plants which it has nourished. On the top of this soil have grown first the heathy and bog shrubs which first succeed the Sphagnum, and in time, as the soil has grown more solid, forest trees. This is our second forest. This first peat deposit, or the lower part of it at all events, having been turned into the black peat impervious to water, plays the same part in the next stage that the clay or pan did in the earlier stage. Again, the drainage of this second level gets stopped, and the forest bottom is loaded with stagnant water, the home of the Sphagnum; together, the water and the Sphagnum kill the forest trees, which share the fate of their predecessors. The same history is gone through again—the Sphagnum filling up the morass and turning the water into dry land until it supports the third forest, and so on to the end.

N.B.—The discourse was illustrated by diagrams.

[E. F.]

WEEKLY EVENING MEETING,

Friday, January 30, 1891.

EDWARD FRANKLAND, Esq. D.C.L. LL.D. F.R.S. Vice-President, in the Chair.

PROFESSOR JOHN W. JUDD, F.R.S. F.G.S.

The Rejuvenescence of Crystals.

VERY soon after the invention of the microscope, the value of that instrument in investigating the phenomena of crystallisation began to be recognised. The study of crystal-morphology and crystallogenesis was initiated in this country by the observations of Robert Boyle; and since his day, a host of investigators—among whom may be especially mentioned Leeuenhoek and Vogelsang in Holland, Link and Frankenheim in Germany, and Pasteur and Senarmont in France—have added largely to our knowledge of the origin and development of crystalline structures. Nor can it be said with justice that this field of investigation, opened up as it was by English pioneers, has been ignobly abandoned to others; for the credit of British science has been fully maintained by the numerous and brilliant discoveries in this department of knowledge of Brewster and Sorby.

There is no branch of science which is more dependent for its progress on a knowledge of the phenomena of crystallisation than geology. In seeking to explain the complicated phenomena exhibited by the crystalline masses composing the earth's crust, the geologist is constantly compelled to appeal to the physicist and chemist—from them alone can he hope to obtain the light of experiment and the leading of analogy whereby he may hope to solve the problems which confront him.

But if geology owes much to the researches of those physicists and chemists who have devoted their studies to the phenomena of crystallisation, the debt has been more than repaid through the new light which has been thrown on these questions by the investigation of naturally-formed crystals by mineralogists and geologists.

In no class of physical operations is *time* such an important factor as in crystallisation; and Nature, in producing her inimitable examples of crystalline bodies, has been unsparing in her expenditure of time. Hence it is not surprising to find that some of the most wonderful phenomena of crystallisation can best be studied—some indeed can only be studied—in those exquisite specimens of Nature's handiwork which have been slowly elaborated by her during periods which must be measured in millions of years.

I propose to-night to direct your attention to a very curious case in which a strikingly complicated group of phenomena is presented in a crystalline mass; and these phenomena, which have been revealed to the student of natural crystals, are of such a kind that we can scarcely hope to reproduce them in our test-tubes and crucibles.

But if we cannot expect to imitate all the effects which have in this case been slowly wrought out in Nature's laboratory, we can at least investigate and analyse them; and, in this way, it may be possible to show that phenomena like those in question must result from the possession by crystals of certain definite properties. Each of these properties, we shall see, may be severally illustrated and experimentally investigated, not only in natural products, but in the artificially-formed crystals of our laboratories.

In order to lead up to the explanation of the curious phenomena exhibited by the rock mass in question, the first property of crystals to which I have to refer may be enunciated as follows:—

Crystals possess the power of resuming their growth after interruption; and there appears to be no limit to the time after which this resumption of growth may take place.

It is a familiar observation that if a crystal be taken from a solution and put aside, it will, if restored after a longer or shorter interval to the same or a similar solution, continue to increase as before. But geology affords innumerable instances in which this renewal of growth in crystals has taken place after millions of years must have elapsed. Still more curious is the fact, of which abundant proof can be given, that a crystal formed by one method may, after a prolonged interval, continue its growth under totally different conditions and by a very different method. Thus crystals of quartz, which have clearly been formed in a molten magma, and contain enclosures of glass, may continue their growth when brought in contact with solutions of silica at ordinary temperatures. In the same way, crystals of felspar which have been formed in a mass of incandescent lava, may increase in size when solvent agents bring to them the necessary materials from an enveloping mass of glass, even after the whole mass has become cold and solid.

It is this power of resuming growth after interruption, which leads to the formation of zoned crystals, like the fine specimen of amethyst enclosed in colourless quartz, which was presented to the Royal Institution seventy years ago by Mr. Snodgrass.

The growth of crystals, like that of plants and animals, is determined by their environment; the chief conditions affecting their development being temperature, rate of growth, the supply of materials (which may vary in quality as well as quantity), and the presence of certain foreign bodies.

It is a very curious circumstance that the form assumed by a crystal may be completely altered by the presence of infinitesimal traces of certain foreign substances—foreign substances, be it remarked, which do not enter in any way into the composition of the crystallising mass. Thus there are certain crystals which can only be formed in the presence of water, fluorides, or other salts. Such foreign bodies, which exercise an influence on a crystallising substance without entering into its composition, have been called by the French geologists "mineralisers." Their action seems to curiously resemble that of diastase, and of the bodies known to chemists as "ferments," so many of which are now proved to be of organic origin.

Studied according to their mode of formation, zoned crystals fall naturally into several different classes.

In the first place, we have the cases in which the successive shells or zones differ only in colour or some other accidental character. Sometimes such differently coloured shells of the crystal are sharply cut off from one another; while, in other instances, they graduate imperceptibly one into the other.

A second class of zoned crystals includes those in which we find clear evidence that there have been pauses, or at all events changes in the rate of their growth. The interruption in growth may be indicated in several different ways. One of the commonest of these is the formation of cavities filled with gaseous, liquid, or vitreous material,—according to the way the crystal has been formed, by volatilisation, by solution, or by fusion; the production of these cavities indicating rapid or irregular growth. Not unfrequently, it is clear that the crystal, after growing to a certain size, has been corroded or partially resorbed in the mass in which it is being formed, before its increase was resumed. In other cases, a pause in the growth of the crystal is indicated by the formation of minute foreign crystals, or the deposition of uncrystallised material along certain zonal planes in the crystal.

Some very interesting varieties of minerals—like the Cotterite of Ireland, the red quartz of Cumberland, and the spotted amethyst of Lake Superior—can be shown to owe their peculiarities to thin bands of foreign matter zonally included in them during their growth.

A curious class of zoned crystals arises when there is a change in the *habit* of a crystal during its growth. Thus, as Lavalle showed in 1851,* if an octahedron of alum be allowed to grow to a certain size in a solution of that substance, and then a quantity of alkaline carbonate be added to the liquid, the octahedral crystal, without change in the length of its axes, will be gradually transformed into a cube. In the same way, a scalenohedron of calcite may be found enclosed in a prismatic crystal of the same mineral, the length of the vertical axis being the same in both crystals.

By far the most numerous and important class of zoned crystals is that which includes the forms where the successive zones are of different, though analogous, chemical composition. In the case of the alums and garnets, we may have various *isomorphous* compounds forming the successive zones in the same crystal; while in substances crystallising in other systems than the cubic, we find *plesiomorphous* compounds forming the different enclosing shells. Such cases are

* Bull. Géol. Soc. Paris, 2nd ser. vol. viii. pp. 610–13.

illustrated by many artificial crystals, and by the tourmalines, the epidotes, and the felspars among minerals. The separate zones, consisting of different materials, are sometimes separated by well-marked planes, but in other cases they shade imperceptibly into one another.

In connection with this subject, it may be well to point out that zoned crystals may be formed of two substances which do not crystallise in the same system. Thus crystals of the monoclinic angite may be found surrounded by a zone of the rhombic enstatite; and crystals of a triclinic felspar may be found enlarged by an outer shell of monoclinic felspar.

Still more curious is the fact that, where there is similarity in crystalline form, and an approximation in the dominant angles (plesiomorphism), we may have zoning and intergrowth in the crystals of substances which possess no chemical analogy whatever. Thus, as Senarmont showed in 1856, a cleavage-rhomb of the natural calcic carbonate (calcite), when placed in a solution of the sodic nitrate, becomes enveloped in a zone of this latter substance; and Tschermak has proved that the compound crystal thus formed behaves like a homogeneous one, if tested by its cleavage, by its susceptibility to twin lamellation, or by the figures produced by etching. In the same way, zircons, which are composed of the two oxides of silicon and zirconium, are found grown in composite crystals with xenotime, a phosphate of the metals of the cerium and yttrium groups.

These, and many other similar facts which might be adduced, point to the conclusion that the beautiful theory of isomorphism, as originally propounded by Mitscherlich, stands in need of much revision as to many important details; it may be indeed of complete reconstruction in the light of modern observation and experiment.

The second property of crystals to which I must direct your attention is the following:—

If a crystal be broken or mutilated in any way whatever, it possesses the power of repairing its injuries during subsequent growth.

As long ago as 1836, Frankenheim showed that if a drop of a saturated solution be allowed to evaporate on the stage of a microscope, the following interesting observations may be made upon the growing crystals. When they are broken up by a rod, each fragment tends to re-form as a perfect crystal; and if the crystals be caused to be partially redissolved by the addition of a minute drop of the mother-liquor, further evaporation causes them to resume their original development.*

In 1842 Hermann Jordan showed that crystals taken from a solution and mutilated, gradually became repaired or healed when replaced in the solution.† Jordan's observations, which were

* Pogg. Ann., Bd. xxxvii. (1836).
† Müller, Archiv for 1842, pp. 46–56.

published in a medical journal, do not however seem to have attracted much attention from the physicists and chemists of the day.

Lavalle, between the years 1850 and 1853,* and Kopp in the year 1855,† made a number of valuable observations bearing on this interesting property of crystals. In 1856 the subject was more thoroughly studied by three investigators, who published their results almost simultaneously—these were Marbach,‡ Pasteur,§ and Senarmont.‖ They showed that crystals taken from a solution and mutilated in various ways, upon being restored to the liquid, became completely repaired during subsequent growth.

As long ago as 1851, Lavalle had asserted that when one solid angle of an octahedron of alum is removed, the crystal tends to reproduce the same mutilation on the opposite angle when its growth is resumed. The phenomena observed have, by some subsequent writers, however, been explained in another way to that suggested by the author of this experiment. In the same way the curious experiments performed at a subsequent date by Karl von Hauer—experiments which led him to conclude that hemihedrism and other peculiarities in crystal growth might be induced by mutilation,¶— have been asserted by other physicists and chemists not to justify the startling conclusions drawn from them at the time. It must be admitted that new experiments bearing on this interesting question are, at the present time, greatly needed.

In 1881 Loir demonstated two very important facts with regard to growing crystals of alum.** *First*, that if the injuries in such a crystal be not too deep, it does not resume growth over its general surface until those injuries have been repaired. *Secondly*, that the injured surfaces of crystals grow more rapidly than natural faces. This was proved by placing artificially cut octahedra and natural crystals of the same size in a solution, and comparing their weight after a certain time had elapsed.

The important results of this capacity of crystals for undergoing healing and enlargement, and their application to the explanation of interesting geological phenomena, has been pointed out by many authors. Sorby has shown that, in the so-called crystalline sand-grains, we

---

* Bull. Géol. Soc. Paris, 2nd ser., vol. viii. (1851), pp. 610–13; Moigno, Cosmos, ii. (1853) pp. 454–6; Compt. Rend., xxxvi. (1853), pp. 493–5.

† Liebig. Ann., xciv. (1855), pp. 118–25.

‡ Compt. Rend., xliii. (1856), pp. 705–6, 800–2.

§ Ibid., pp. 795–800.

‖ Ibid., p. 799.

¶ Wien. Sitzungsber., xxxix. (1860), pp. 611–22; Erdmann, Journ. Prakt. Chem., lxxxi., pp. 356–62; Wien. Geol. Verhandl., xii. pp. 212–3, &c. Compare Frankenheim, Pogg. Ann., cxiii. (1861); Fr. Scharff, Pogg. Ann., cix. (1860), pp. 529–38; Neues Jahrb. für Min., &c., 1876, p. 24; and W. Sauber, Liebig. Ann., cxxiv. (1862), pp. 78–82; also W. Ostwald, Lehrbuch d. Allg. Chem. (1885), Bd. i., p. 738; and O. Lehmann, Molekular Physik (1888), Bd. i. p. 312.

** Compt. Rend., Bd. xcii. p. 1166.

have broken and worn crystals of quartz, which, after many vicissitudes and the lapse of millions of years, have grown again, and been enveloped in a newly formed quartz-crystal. Bonney has shown how the same phenomena are exhibited in the case of mica, Becke and Whitman Cross in the case of hornblende, and Merrill in the case of angite. In the felspars of certain rocks, it has been proved that crystals that have been rounded, cracked, corroded, and internally altered—which have, in short, suffered both mechanical and chemical injuries—may be repaired and enlarged with material that differs considerably in chemical composition from the original crystal.

It is impossible to avoid a comparison between these phenomena of the inorganic world and those so familiar to the biologist. It is only in the lowest forms of animal life that we find an unlimited power of repairing injuries; in the rhizopods and some other groups, a small fragment may grow into a perfect organism. In plants the same phenomenon is exhibited much more commonly, and in forms belonging to groups high up in the vegetable series. Thus parts of a plant, such as buds, bulbs, slips, and grafts, may—sometimes after a long interval—be made to grow up into new and perfect individuals. But in the mineral kingdom we find the same principle carried to a much farther extent. We know in fact no limit to the minuteness of fragments which may, under favourable conditions, grow into perfect crystals; no bounds as to the time during which the crystalline growth may be suspended in the case of any particular individual.

The next property of crystals which I must illustrate, in order to explain the particular case to which I am calling your attention to-night, is the following:—

Two crystals of totally different substances may be developed within the space bounded by certain planes, becoming almost inextricably intergrown, though each retains its distinct individuality.

This property is a consequence of the fact that the substance of a crystal is not necessarily continuous within the space enclosed by its bounding planes. Crystals often exhibit cavities filled with air and other foreign substances. In the calcite crystals found in the Fontainbleau sandstone, less than 40 per cent. of their mass consists of calcic carbonate, while more than 60 per cent. is made up of grains of quartz-sand caught up during crystallisation. In the rock called "graphic granite" we have the minerals orthoclase and quartz intergrown in such a way that the more or less isolated parts of each can be shown, by their optical characters, to be parts of great mutually interpenetrant crystals. Similar relations are shown in the so-called micrographic or micropegmatitic intergrowth of the same minerals which are so beautifully exhibited in the rock under our consideration this evening.

There is still another property of crystals that must be kept in mind if we would explain the phenomena exhibited by this interesting rock.

A crystal may undergo the most profound internal changes, and these may lead to great modifications of the optical and other physical properties of the mineral: yet so long as a small—often a very small—proportion of its molecules remain intact, the crystal may retain, not only its outward form, but its capacity for growing and repairing injuries.

Crystals, like ourselves, grow old. Not only do they suffer from external injuries, mechanical fractures, and chemical corrosion, but from actions which affect the whole of their internal structure. Under the action of the great pressures in the earth's crust the minerals of deep-seated rocks are completely permeated by fluids which chemically react upon them. In this way negative crystals are formed in their substance (similar to the beautiful "ice-flowers" which are formed when a block of ice is traversed by a beam from the sun or an electric lamp), and these become filled with secondary products. As the result of this action, crystals, once perfectly clear and translucent, have acquired cloudy, opalescent, iridescent, avanturine, and "schiller" characters, and minerals thus modified abound in the rocks that have at any period of their history been deep-seated. As the destruction of their internal structure goes on, the crystals gradually lose more and more of their distinctive optical and their physical properties, retaining, however, their external form; till at last—when the last of the original molecules has been transformed or replaced by others—they pass into those mineral corpses known to us as "pseudomorphs."

But while crystals resemble ourselves in "growing old," and at last undergoing dissolution, they exhibit the remarkable power of growing young again, which we, alas! never do. This is in consequence of the following remarkable attribute of crystalline structures:—

It does not matter how far internal change and disintegration may have gone on in a crystal; if only a certain small proportion of the unaltered molecules remain, the crystal may renew its youth and resume its growth.

When old and much-altered crystals begin to grow again, the newly-formed material exhibits none of those marks of "senility" to which I have referred. The sand-grains that have been battered and worn into microscopic pebbles, and have been rendered cloudy by the development of millions of secondary fluid-cavities, may have clear and fresh quartz deposited upon them to form crystals, with exquisitely perfect faces and angles. The white, clouded, and altered felspar-crystals may, in the same way, be enveloped by a zone of clear and transparent material, which has been added millions of years after the first formation and the subsequent alteration of the original crystals. In these, and many similar examples which might be cited, the kernel, representing the original crystal, may exhibit evidence of having undergone the most profound chemical and physical modification, while the outer shell displays all the normal characteristics of the mineral.

We are now in a position to explain the particular case which I have thought of sufficient importance to claim your attention to-night.

In the Island of Mull, in the Inner Hebrides, there exist masses of granite of Tertiary age which are of very great interest to the geologist and mineralogist. In many places this granite exhibits beautiful illustrations of the curious intergrowths of quartz and felspar of which I have already spoken. Such parts of the rock often abound with cavities (druses), which I believe are not of original, but of secondary origin. At all events, it can be shown that these cavities have been localities in which crystal growth has gone on—they constitute indeed veritable laboratories of synthetic mineralogy.

Now in such cavities, the interpenetrant crystals of quartz and felspar in the rock have found a space where they may grow and complete their outward form; and it is curious to see how sometimes the quartz has prevailed over the felspar, and a pure quartz-crystal has been produced; while at other times the opposite effect has resulted, and a pure felspar individual has grown up. In these last cases, however much the original felspar may have been altered (kaolinised and rendered opaque), it is found to be completed by a zone of absolutely clear and unaltered felspar-substance. The result is that the cavities of the granite are lined with a series of projecting crystals of quartz and clear felspar, the relations of which to the similar materials in an altered condition composing the substance of the solid rock are worthy of the most careful observation and reflection. These relations can be fully made out when thin sections of the rock are examined under the microscope by the aid of polarised light, and they speak eloquently of the possession by the crystals in question of all those curious peculiarities of which I have reminded you this evening.

By problems such as those which we have endeavoured to solve to-night, the geologist is beset at every step. The crust of our globe is built up of crystals and crystal-fragments—of crystals in every stage of development, of growth, and of variation—of crystals, undergoing change, decay, and dissolution. Hence the study of the natural history of crystals must always constitute one of the main foundations of geological science; and the future progress of that science must depend on how far the experiments carried on in laboratories can be made to illustrate and explain our observations in the field.

[J. W. J.]

GENERAL MONTHLY MEETING,

Monday, February 2, 1891.

Sir James Crichton Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

Lady Abel,
John Aird, Esq. M.P.
Henry Graham Harris, Esq. M. Inst. C.E.
Sydney Turner Klein, Esq. F.L.S. F.R.H.S.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to the Committee of Subscribers to the Bowman Presentation Portrait for an Engraving of the Portrait of Sir William Bowman, Bart.

The Special Thanks of the Members were returned for the following Donation to the Fund for the Promotion of Experimental Research:—

F. B. Wiggins, Esq. £5

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Secretary of State for India—Report on Public Instruction in Bengal, 1889–90. fol.

The Governor-General of India—Geological Survey of India: Records, Vol. XXIII. Part 4. 4to. 1890.

The Lords of the Admiralty—Nautical Almanack, 1894. 8vo. 1890.
Greenwich Observations for 1888. 4to. 1890.
Greenwich Spectroscopic and Photographic Results, 1888–9. 4to. 1889–90.

Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta: Rendiconti. 2° Semestre, Vol. VI. Fasc. 6–11. 8vo. 1890.
Atti, Anno 43, Sess. 3ª. 4to. 1890.

Astronomical Society, Royal—Monthly Notices, Vol. LI. Nos. 1, 2. 8vo. 1891.

Australian Museum, Sydney—Report for 1889. fol. 1890.

Bankers, Institute of—Journal, Vol. XI. Part 10. 8vo. 1890.

Basel Naturforschende Gesellschaft—Verhandlungen, Band IX. Heft 1. 8vo. 1890.

Bickers & Son, Messrs. (the Publishers)—Constance Naden. A Memoir. 8vo. 1890.

Boston Society of Natural History—Proceedings, Vol. XXIV. Parts 3, 4. 8vo. 1890.
Memoirs, Vol. IV. Nos. 7, 8, 9. 4to. 1890.

British Architects, Royal Institute of—Proceedings, 1890–1, Nos. 4–7. 4to.
Transactions, Vol. VI. 4to. 1890.

Canada, Geological and Natural History Survey of—Catalogue of Canadian Plants, Part 5. 8vo. 1890.
List of Canadian Hepaticæ. 8vo. 1890.
Chemical Industry, Society of—Journal, Vol. IX. No. 11. 8vo. 1890.
Chemical Society—Journal for Dec. 1890 and Jan. 1891. 8vo.
Colliery Guardian, Editor of—Map showing lines of Equal Magnetic Declination for January 1st, 1891.
Cracovie, l'Academie des Sciences—Bulletin, 1890, Nos. 9, 10. 8vo.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I.—Journal of the Royal Microscopical Society, 1890, Part 6. 8vo.
Dawson, G. M. Esq. LL.D. F.G.S. (the Author)—Later Physiographical Geology of the Rocky Mountain region. 4to. 1890.
Editors—American Journal of Science for Dec. 1890 and Jan. 1891. 8vo.
Analyst for Dec. 1890 and Jan. 1891. 8vo.
Athenæum for Dec. 1890 and Jan. 1891. 4to.
Brewers' Journal for Dec. 1890 and Jan. 1891. 4to.
Chemical News for Dec. 1890 and Jan. 1891. 4to.
Chemist and Druggist for Dec. 1890 and Jan. 1891. 8vo.
Electrical Engineer for Dec. 1890 and Jan. 1891. fol.
Engineer for Dec. 1890 and Jan. 1891. fol.
Engineering for Dec. 1890 and Jan. 1891. fol.
Horological Journal for Dec. 1890 and Jan. 1891. 8vo.
Industries for Dec. 1890 and Jan. 1891. fol.
Iron for Dec. 1890 and Jan. 1891. 4to.
Ironmongery for Dec. 1890 and Jan. 1891. 4to.
Monist, Vol. I. Nos. 1, 2. 8vo. 1891.
Murray's Magazine for Dec. 1890 and Jan. 1891. 8vo.
Nature for Dec. 1890 and Jan. 1891. 4to.
Open Court for Dec. 1890 and Jan. 1891. 4to.
Optical Magic Lantern Journal, Vol. II. No. 20. 8vo. 1891.
Photographic News for Dec. 1890 and Jan. 1891. 8vo.
Public Health for Dec. 1890 and Jan. 1891. 8vo.
Revue Scientifique for Dec. 1890 and Jan. 1891. 4to.
Telegraphic Journal for Dec. 1890 and Jan. 1891. fol.
Zoophilist for Dec. 1890 and Jan. 1891. 4to.
Electrical Engineers, Institution of—Journal, Nos. 90, 91. 8vo. 1890.
Florence Biblioteca Nazionale Centrale—Bolletino, Nos. 118–121. 8vo. 1889–90.
Fournet, A. Esq. (the Author)—The Philosophy of Sight. 8vo. 1889.
Franklin Institute—Journal, Nos. 780, 781. 8vo. 1890–1.
Geographical Society, Royal—Proceedings, New Series, Vol. XII. No. 12; Vol. XIII. No. 1. 8vo. 1890–1.
Supplementary Papers, Vol. IV. 8vo. 1890.
Geological Institute, Imperial, Vienna—Verhandlungen, 1890. Nos. 10–13. 8vo.
Horticultural Society, Royal—Journal, Vol. XII. No. 3. 8vo. 1890
Institute of Brewing—Transactions, Vol. IV. No. 1. 8vo. 1890.
Johns Hopkins University—University Circulars, No. 84. 4to. 1890.
Kew Observatory—Report, 1890. 8vo.
Lincoln's Inn, Hon. Society of—Catalogue of Lincoln's Inn Library. Supplementary Volume. 8vo. 1890.
Linnean Society—Journal, Nos. 147, 192. 8vo. 1890.
Manchester Geological Society—Transactions, Vol. XXI. Part 1. 8vo. 1890–1.
Manchester Steam Users' Association—Boiler Explosions Act, 1882. Report, Nos. 351–429. 4to. 1890.
Mechanical Engineers' Institution—Proceedings, 1890, No. 3. 8vo.
Medical and Chirurgical Society, Royal—Transactions, Vol. LXXIII. 8vo. 1890.
Meteorological Office—Weekly Weather Reports, Vol. VII. Nos. 48–53; Vol. VIII. Nos. 1, 2. 4to. 1890.
Meteorological Observations at Stations of Second Order. fol. 1886 and 1890.
Report of Meteorological Council, 31st March, 1890. 8vo.

Meteorological Society, Royal—Library Catalogue. 8vo. 1891.
Ministry of Public Works, Rome—Giornale del Genio Civile, 1890, Fasc. 9, 10, 11. And Designi. fol. 1890.
Murray, J. Esq. (the Publisher)—Dictionary of Greek and Roman Antiquities. Edited by W. Smith and others. Vol. I. 8vo. 1890.
Odontological Society of Great Britain—Transactions, Vol. XXIII. No. 2. New Series. 8vo. 1890.
Pharmaceutical Society of Great Britain—Journal, Dec. 1890 and Jan. 1891. 8vo.
Photographic Society—Journal, Vol. XV. Nos. 2, 3. 8vo. 1890.
Radcliffe Observatory—Observations, Vol. XLIV. 8vo. 1890.
Rathbone, E. P. Esq. (the Editor)—The Witwatersrand Mining and Metallurgical Review, Nos. 10, 11. 8vo. 1890.
Rio de Janeiro, Observatoire Imperial de—Revista, Nos. 10–12. 8vo. 1890.
Royal Society of Antiquaries of Ireland—Journal, Vol. I. (5th Series), No. 3. 8vo. 1890.
Royal Historical Society—Walter of Henley's Husbandry. Trans. by E. Lamond. 8vo. 1890.
Royal Irish Academy—Cunningham Memoirs, No. 6. 4to. 1890.
Royal Society of London—Proceedings, No. 295. 8vo. 1890.
Saxon Society of Sciences, Royal—Philologisch-historischen Classe:
Abhandlung. Band XII. No. 1. 8vo. 1890.
Berichte, 1890, No. 1. 8vo. 1890.
Seismological Society of Japan—Transactions, Vol. XV. Part 2. 8vo. 1890.
Selborne Society—Nature Notes, Vol. I. No. 12; Vol. II. No. 13. 8vo. 1890.
Society of Architects—Proceedings, Vol. III. Nos. 2–4. 8vo. 1890.
Society of Arts—Journal for Dec. 1890 and Jan. 1891. 8vo.
St. Pétersbourg Académie Impériales des Sciences—Mémoires, Tome XXXVII. Nos. 11–13; Tome XXVIII. No. 1. 4to. 1890.
Bulletin, Tome XXIV. No. 1. 4to. 1890.
Surgeon-General's Office, U.S. Army—Index Catalogue of the Library, Vol. XI. 4to. 1890.
United Service Institution, Royal—Journal, No. 155. 8vo. 1891.
United States Department of Agriculture—North American Fauna, Nos. 3, 4. 8vo. 1890.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1890: Heft 9–10. 4to.
Wild, Dr. H. (the Director)—Repertorium für Meteorologie, Band XIII. 4to. 1890.

WEEKLY EVENING MEETING,

Friday, February 6, 1891.

Sir Frederick Bramwell, Bart. D.C.L. F.R.S. Honorary Secretary and Vice-President, in the Chair.

The Right Hon. Lord Rayleigh, M.A. D.C.L. F.R.S. *M.R.I.*

PROFESSOR OF NATURAL PHILOSOPHY R.I.

Some Applications of Photography.

One of the subjects to which I propose to invite your attention this evening is the application of instantaneous photography to the illustration of certain mechanical phenomena which pass so quickly as to elude ordinary means of observation. The expression "instantaneous photography" is perhaps not quite a defensible one, because no photography can be really instantaneous—some time must always be occupied. One of the simplest and most commonly used methods of obtaining very short exposures is by the use of movable shutters, for which purpose many ingenious mechanical devices have been invented. About two years ago we had a lecture from Prof. Muybridge, in which he showed us the application of this method—and a remarkably interesting application it was—to the examination of the various positions assumed by a horse in his several gaits. Other means, however, may be employed to the same end, and one of them depends upon the production of an instantaneous light. It will obviously come to the same thing whether the light to which we expose the plates be instantaneous, or whether by a mechanical device we allow the plate to be submitted to a continuous light for only a very short time. A good deal of use has been made in this way of what is known as the magnesium flash light. A cloud of magnesium powder is ignited, and blazes up quickly with a bright light of very short duration. Now I want to compare that mode of illumination with another, in order to be able to judge of the relative degree of instantaneity, if I may use such an expression. We will illumine for a short time a revolving disc, composed of black and white sectors; and the result will depend upon how quick the motion is as compared with the duration of the light. If the light could be truly instantaneous, it would of necessity show the disc apparently stationary. I believe that the duration of this light is variously estimated at from one-tenth to one-fiftieth of a second; and as the arrangement that I have here is one of the slowest, we may assume that the time occupied will be about a tenth of a second. I will say the words one, two, three, and at the word three Mr. Gordon will project the powder into the flame of a spirit lamp, and the flash will be produced. Please give your attention to the disc, for the question is whether the present uniform grey will be displaced by a perception of the individual black and white sectors. [Experiment.] You see the flash was *not* instantaneous enough to resolve the grey into its components.

I want now to contrast with that mode of illumination one obtained by means of an electric spark. We have here an arrangement by which we can charge Leyden jars from a Wimshurst machine. When the charge is sufficient, a spark will pass inside a lantern, and the light proceeding from it will be condensed and thrown upon the same revolving disc as before. The test will be very much more severe; but severe as it is, I think we shall find that the electric flash will bear it. The teeth on the outside of the disc are very numerous, and we will make them revolve as fast as we can, but we shall find that under the electric light they will appear to be absolutely stationary. [Experiment.] You will agree that the outlines of the black and white sectors are seen perfectly sharp.

Now, by means of this arrangement we might investigate a limit to the duration of the spark, because with a little care we could determine how fast the teeth are travelling—what space they pass through in a second of time. For this purpose it would not be safe to calculate from the multiplying gear on the assumption of no slip. A better way would be to direct a current of air upon the teeth themselves, and make them give rise to a musical note, as in the so-called siren. From the appearance of the disc under the spark we might safely say, I think, that the duration of the light is less than a tenth of the time occupied by a single tooth in passing. But the spark is in reality much more instantaneous than can be proved by the means at present at our command. In order to determine its duration, it would be necessary to have recourse to that powerful weapon the revolving mirror; and I do not, therefore, propose to go further into the matter to-night.

Experiments of this kind were made some twenty years ago by Prof. Rood, of New York, both on the duration of the discharge of a Leyden jar, and also on that of lightning. Prof. Rood found that the result depended somewhat upon the circumstances of the case; the discharge of a small jar being generally more instantaneous than that of a larger one. He proved that in certain cases the duration of the principal part of the light was as low as one twenty-five-millionth part of a second of time. That is a statement which probably conveys very little of its real meaning. A million seconds is about twelve days and nights. Twenty-five million seconds is nearly a year. So that the time occupied by the spark in Prof. Rood's experiment is about the same fraction of one second that one second is of a year. In many other cases the duration was somewhat greater; but in all his experiments it was well under the one-millionth part of a second. In certain cases you may have multiple sparks. I do not refer to the oscillating discharges of which Prof. Lodge gave us so

Plate I.

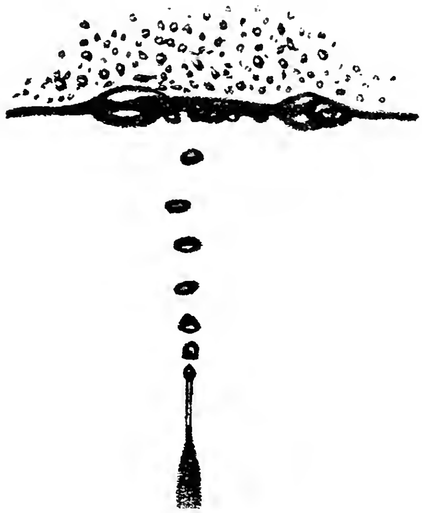


Fig 1.

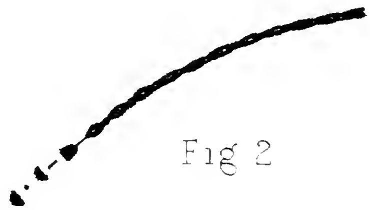


Fig 2

Fig 3.

Fig 4.

interesting an account last year; Prof. Rood's multiple discharge was not of that character. It consisted of several detached overflows of his Leyden jar when charged by the Rhumkorff coil. One number mentioned for the total duration was one six-thousandth part of a second; but the individual discharges had the degree of instantaneity of which I have spoken.

It is not a difficult matter to adapt the electrical spark to instantaneous photography. We will put the lantern into its proper position, excite the electric sparks within it, causing them to be condensed by the condenser of the lantern on to the photographic lens. We will then put the object in front of the lantern-condenser, remove the cap from the lens, expose the plate to the spark when it comes, and thus obtain an instantaneous view of whatever may be going on. I propose to go through the operation of taking such a photograph presently. I will not attempt any of the more difficult things of which I shall speak, but will take a comparatively easy subject,—a stream of bubbles of gas passing up through a liquid. In order that you may see what this looks like when observed in the ordinary way, we have arranged it here for projection upon the screen. [Experiment.] The gas issues from the nozzle, and comes up in a stream, but so fast that you cannot fairly see the bubbles. If, however, we take an instantaneous picture, we shall find that the stream is decomposed into its constituent parts. We arrange the trough of liquid in front of the lantern which contains the spark-making apparatus —[Experiment]—and we will expose a plate, though I hardly expect a good result in a lecture. A photographer's lamp provides some yellow light to enable us to see when other light is excluded. There goes the spark; the plate is exposed, and the thing is done. We will develop the plate, and see what it is good for; and if it turns out fit to show, we will have it on the screen within the hour.

In the meantime, we will project on the screen some slides taken in the same way and with the same subject. [Photograph shown.] That is an instantaneous photograph of a stream of bubbles. You see that the bubbles form at the nozzle from the very first moment, contrasting in that respect with the behaviour of jets of water, projected into air. [Fig. 1, Plate I.]

The latter is our next subject. This is the reservoir from which the water is supplied. It issues from a nozzle of drawn-out glass, and at the moment of issue it consists of a cylindrical body of water. The cylindrical form is unstable, however, and the water rapidly breaks up into drops, which succeed one another so rapidly that they can hardly be detected by ordinary vision. But by means of instantaneous photography the individual drops can be made evident. I will first project the jet itself on the screen, in order that you may appreciate the subject which we shall see presently represented by photography. [Experiment.] Along the first part of its length the jet of water is continuous. After a certain point it breaks into drops, but you cannot see them because of their rapidity. If we act on the jet with a vibrating body, such as a tuning fork, the breaking into drops occurs still earlier, the drops are more regular, and assume a curious periodic appearance, investigated by Savart. I have some photographs of jets of that nature. Taken as described, they do not differ much in appearance from those obtained by Chichester Bell, and by Mr. Boys. We get what we may regard as simply shadows of the jet obtained by instantaneous illumination; so that these photographs show little more than the outlines of the subject. They show a little more, on account of the lens-like action of the cylinder and of the drops. Here we have an instantaneous view of a jet similar to the one we were looking at just now. [Fig. 2, Plate I.] This is the continuous part; it gradually ripples itself as it comes along; the ripples increase; then the contraction becomes a kind of ligament connecting consecutive drops; the ligament next gives way, and we have the individual drops completely formed. The small points of light are the result of the lens-like action of the drops. [Other instantaneous views also shown.]

The pictures can usually be improved by diffusing somewhat the light of the spark with which they are taken. In front of the ordinary condensing lens of the magic lantern we slide in a piece of ground glass, slightly oiled, and we then get better pictures showing more shading. [Photograph shown.] Here is one done in that way; you would hardly believe it to be water resolved into drops under the action of a tremor. It looks more like mercury. You will notice the long ligament trying to break up into drops on its own account, but not succeeding. [Fig. 3, Plate I.]

There is another, with the ligament extremely prolonged. In this case it sometimes gathers itself into two drops. [Fig. 4, Plate I.]

[A number of photographs showing slight variations were exhibited.]

The mechanical cause of this breaking into drops is, I need hardly remind you, the surface tension or capillary force of the liquid surface. The elongated cylinder is an unstable form, and tends to become alternately swollen and contracted. In speaking on this subject I have often been embarrassed for want of an appropriate word to describe the condition in question. But a few days ago, during a biological discussion, I found that there is a recognised, if not a very pleasant, word. The cylindrical jet may be said to become *varicose*, and the varicosity goes on increasing with time, until eventually it leads to absolute disruption.

There is another class of unstable jets presenting many points of analogy with the capillary ones, and yet in many respects quite distinct from them. I refer to the phenomena of sensitive flames. The flame, however, is not the essential part of the matter, but rather an indicator of what has happened. Any jet of fluid playing into a stationary environment is sensitive, and the most convenient form for our present purpose is a jet of coloured in uncoloured water. In this case we shall use a solution of permanganate of potash playing into

Fig 5.

Fig 6.

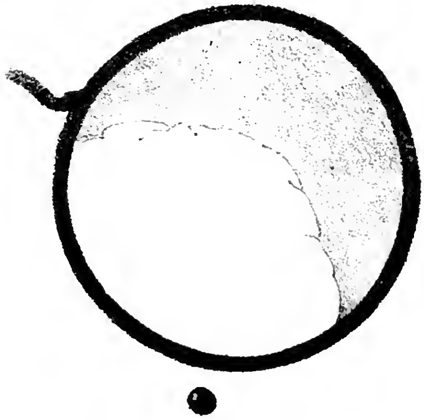


Fig 7

an atmosphere of other water containing acid and sulphate of iron, which exercises a decolourising effect on the permanganate, and so retards the general clouding up of the whole mass by accumulation of colour. [Experiment.] Mr. Gordon will release the clip, and we shall get a jet of permanganate playing into the liquid. If everything were perfectly steady, we might see a line of purple liquid extending to the bottom of the trough; but in this theatre it is almost impossible to get anything steady. The instability to which the jet is subject now manifests itself, and we get a breaking away into clouds something like smoke from chimneys. A heavy tuning fork vibrating at ten to the second acts upon it with great advantage, and regularises the disruption. A little more pressure will increase the instability, and the jet goes suddenly into confusion, although at first, near the nozzle, it is pretty regular.

It may now be asked "What is the jet doing?" That is just the question which the instantaneons method enables us to answer. For this purpose the permanganate which we have used to make the jet visible is not of much service. It is too transparent to the photographic rays, and so it was replaced by bichromate of potash. Here the opposite difficulty arises; for the bichromate is invisible by the yellow light in which the adjustments have to be made. I was eventually reduced to mixing the two materials together, the one serving to render the jet visible to the eye and the other to the photographic plate. Here is an instantaneous picture of such a jet as was before you a moment ago, only under the action of a regular vibrator. It is *sinuous*, turning first in one direction and then in the other. The original cylinder, which is the natural form of the jet as it issues from the nozzle, curves itself gently as it passes along through the water. It thus becomes sinuous, and the amount of the sinuosity increases, until in some cases the consecutive folds come into collision with one another. [Several photographs of sinuous jets were shown, two of which are reproduced in Figs. 5, 6, Plate II.]

The comparison of the two classes of jets is of great interest. There is an analogy as regards the instability, the vibrations caused by disturbance gradually increasing as the distance from the nozzle increases; but there is a great difference as to the nature of the deviation from the equilibrium condition, and as to the kind of force best adapted to bring it about. The one gives way by becoming varicose; the other by becoming sinuous. The only forces capable of producing varicosity are symmetrical forces, which act alike all round. To produce sinuosity, we want exactly the reverse—a force which acts upon the jet transversely and unsymmetrically.

I will now pass on to another subject for instantaneous photography, namely, the soap film. Everybody knows that if you blow a soap bubble it will break—generally before you wish. The process of breaking is exceedingly rapid, and difficult to trace by the unaided eye. If we can get a soap film on this ring, we will project it upon the screen and then break it before your eyes, so as to enable you to form your own impressions as to the rapidity of the operation. For some time it has been my ambition to photograph a soap bubble in the act of breaking. I was prepared for difficulty, believing that the time occupied was less than the twentieth of a second. But it turns out to be a good deal less even than that. Accordingly the subject is far more difficult to deal with than are those jets of water or coloured liquids, which one can photograph at any moment that the spark happens to come.

There is the film, seen by reflected light. One of the first difficulties we have to contend with is that it is not easy to break the film exactly when we wish. We will drop a shot through it. The shot has gone through, as you see, but it has not broken the film; and when the film is a thick one, you may drop a shot through almost any number of times from a moderate height without producing any effect. You would suppose that the shot in going through would necessarily make a hole, and end the life of the film. The shot goes through, however, without making a hole. The operation can be traced, not very well with a shot, but with a ball of cork stuck on the end of a pin, and pushed through. A dry shot does not readily break the film; and as it was necessary for our purpose to effect the rupture in a well defined manner, here was a difficulty which we had to overcome. We found, after a few trials, that we could get over it by wetting the shot with alcohol.

We will try again with dry shot. Three shots have gone through and nothing has happened. Now we will try one wetted with alcohol, and I expect it will break the film at once. There! It has gone!

The apparatus for executing the photography of a breaking soap film will of necessity be more complicated than before, because we have to time the spark exactly with the breaking of the film. The device I have used is to drop two balls simultaneously, so that one should determine the spark and the other rupture the film. The most obvious plan was to hang iron balls to two electro-magnets, and cause them to drop by breaking the circuit, so that both were let go at the same moment. The method was not quite a success, however, because there was apt to be a little hesitation in letting go the balls. So we adopted another plan. The balls were not held by electro-magnetism but by springs (Fig. 8) pressing laterally, and these were pulled off by electro-magnets. The proper moment for putting down the key and so liberating the balls, is indicated by the tap of the beam of an attracted disc electrometer as it strikes against the upper stop. One falling ball determines the spark, by filling up most of the interval between two fixed ones submitted to the necessary electric pressure. Another ball, or rather shot, wetted with alcohol, is let go at the same moment, and breaks the film on its passage through it. By varying the distances dropped through, the occurrence of one event may be adjusted relatively to the other. The spark which passes to the falling ball is, however, not the one which illuminates the photographic plate. The latter occurs within the lantern, and forms part of a circuit in connection with the *outer* coatings of the Leyden jars,* the whole arrangement being similar to that adopted by Prof. Lodge in his experiments upon alternative paths of discharge. Fig. 8 will give a general idea of the disposition of the apparatus. [Several photographs of breaking films were shown upon the screen; one of these is reproduced in Fig. 7, Plate II.] †

This work proved more difficult than I had expected; and the evidence of our photographs supplies the explanation, namely, that the rupture of the film is an extraordinarily rapid operation. It was found that the whole difference between being too early and too late was represented by a displacement of the falling ball, though less than a diameter, viz. $\frac{1}{4}$ inch nearly. The drop which we gave was about a foot. The speed of the ball would thus be about 100 inches per second; therefore the whole difference between being too soon and too late is represented by $\frac{1}{300}$ second. Success is impossible, unless the spark can be got to occur within the limits of this short interval.

Prof. Dewar has directed my attention to the fact that Dupré, a good many years ago, calculated the speed of rupture of a film. We know that the energy of the film is in proportion to its area. When a film is partially broken, some of the area is gone, and the corresponding potential energy is expended in generating the velocity of the thickened edge, which bounds the still unbroken portion. The speed, then, at which the edge will go depends upon the thickness of the film. Dupré took a rather extreme case, and calculated a velocity of 32 metres per second. Here, with a greater thickness, our velocity was, perhaps, 16 yards a second, agreeing fairly well with Dupré's theory.

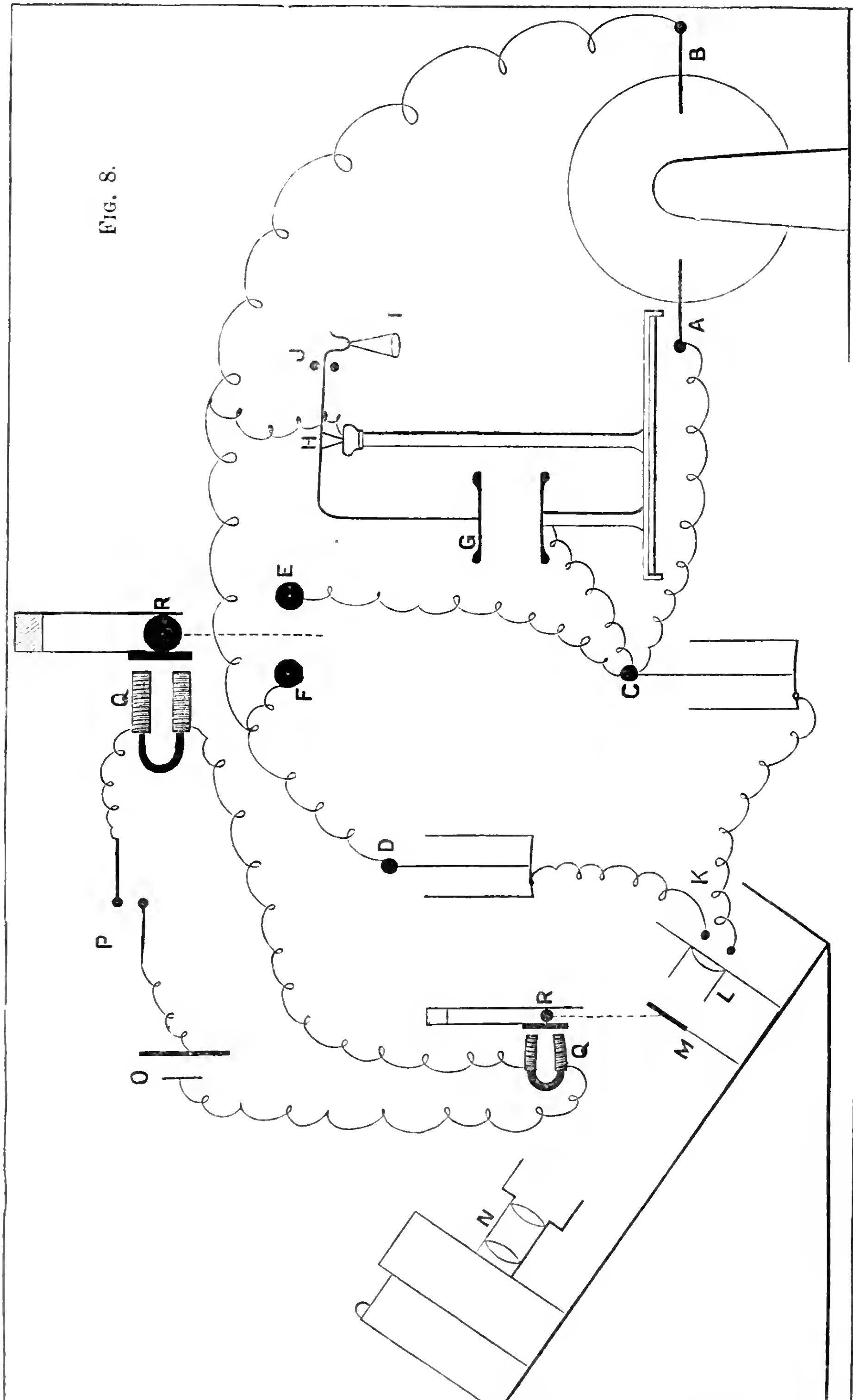
I now pass on to another subject with which I have lately been engaged, namely, the connection between aperture and the definition of optical images. It has long been known to astronomers and to those who study optics that the definition of an optical instrument is proportional to the aperture employed; but I do not think that the theory is as widely appreciated as it should be. I do not know whether, in the presence of my colleague, I may venture to say that I fear the spectroscopists are among the worst sinners in this respect. They constantly speak of the dispersion of their instruments as if that by itself could give any idea of the power employed. You may have a spectroscope of any degree of dispersion, and yet of resolving power insufficient to separate even the D lines. What is the reason of this? Why is it that we cannot get as high a definition as we please with a limited aperture? Some people say that the reason

---

* In practice there were two sets of three jars each.

† The appearance of the breaking bubble, as *seen* under instantaneous illumination, was first described by Marangoni and Stephanelli, Nuovo Cimento, 1873.

Fig. 8.



why large telescopes are necessary, is, because it is only by their means that we can get enough light. That may be in some cases a sufficient reason, but that it is inadequate in others will be apparent, if we consider the case of the sun. Here we do not want more light, but rather are anxious to get rid of a light already excessive. The principal *raison d'être* of large telescopes, is, that without a large aperture definition is bad, however perfect the lenses may be. In accordance with the historical development of the science of optics, the student is told that the lens collects the rays from one point to a focus at another; but when he has made further advance in the science he finds that this is not so. The truth is that we are in the habit of regarding this subject in a distorted manner. The difficulty is not to explain why optical images are imperfect, no matter how good the lens employed, but rather how it is that they manage to be as good as they are. In reality the optical image of even a mathematical point has a considerable extension; light coming from one point cannot be concentrated into another point by any arrangement. There must be diffusion, and the reason is not hard to see in a general way. Consider what happens at the mathematical focus, where, if anywhere, the light should all be concentrated. At that point all the rays coming from the original radiant point arrive in the same phase. The different paths of the rays are all rendered optically equal, the greater actual distance that some of them have to travel, being compensated for in the case of those which come through the centre by an optical retardation due to the substitution of glass for air; so that all the rays arrive at the same time.* If we take a point not quite at the mathematical focus but near it, it is obvious that there must be a good deal of light there also. The only reason for any diminution at the second point lies in the discrepancies of phase which now occur; and these can only enter by degrees. Once grant that the image of a mathematical point is a diffused patch of light, and it follows that

---

Description of Fig. 8.

A, B, Electrodes of Wimshurst machine.
C, D, Terminals of interior coatings of Leyden jars.
E, F, Balls on insulating supports between which the discharge is taken.
G, Attracted disc of electrometer.
H, Knife edge.
I, Scale pan.
J, Stops limiting movement of beam.
K, Sparking balls in connection with exterior coatings of jars. [These exterior coatings are to be joined by an imperfect conductor, such as a table.]
L, Lantern condenser.
M, Soap film.
N, Photographic camera.
O, Daniell cell.
P, Key.
Q, Electromagnets.
R, Balls.

---

* On this principle we may readily calculate the focal lengths of lenses without use of the law of sines. See 'Phil. Mag.,' Dec. 1879.

there must be a limit to definition. The images of the components of a close double point will overlap; and if the distance between the centres do not exceed the diameter of the representative patches of light, there can be no distinct resolution. Now their diameter varies inversely as the aperture; and thus the resolving power is directly as the aperture.

My object to-night is to show you by actual examples that this is so. I have prepared a series of photographs of a grating consisting of parallel copper wires separated by intervals equal to their own diameter, and such that the distance from centre to centre is $\frac{1}{10}$-inch. The grating was backed by a paraffin lamp and large condensing lens; and the photographs were taken in the usual way, except that the lens employed was a telescopic object glass, and was stopped by a screen perforated with a narrow adjustable slit, parallel to the wires.* In each case the exposure was inversely as the aperture employed. The first [thrown upon the screen], is a picture done by an aperture of eight hundredths of an inch, and the definition is tolerably good. The next, with six hundredths, is rather worse. In the third case, I think that everyone can see that the definition is deteriorating; that was done by an aperture of four hundredths of an inch. The next is one done by an aperture of three hundreths of an inch, and you can see that the lines are getting washed out. In focussing the plate for this photograph, I saw that the lines had entirely disappeared, and I was surprised, on developing the plate, to find them still visible. That was in virtue of the shorter wave-length of the light operative in photography as compared with vision. In the last example, the aperture was only two-and-a-half hundredths of an inch, and the effect of the contraction has been to wash away the image altogether, although, so far as ordinary optical imperfections are concerned, the lens was acting more favourably with the smaller aperture than with the larger ones.

This experiment may be easily made with very simple apparatus; and I have arranged that each one of my audience may be able to repeat it by means of the piece of gauze and perforated card which have been distributed. The piece of gauze should be placed against the window so as to be backed by the sky, or in front of a lamp provided with a ground-glass or opal globe. You then look at the gauze through the pin-holes. Using the smaller hole, and gradually drawing back from the gauze, you will find that you lose definition and ultimately all sight of the wires. That will happen at a distance of about $4\frac{1}{2}$ feet from the gauze. If, when looking through the smaller hole, you have just lost the wires, you shift the card so as to bring the larger hole into operation, you will see the wires again perfectly.

That is one side of the question. However perfect your lens may be, you cannot get good definition if the aperture is too much

* The distance between the grating and the telescope lens was 12 ft. 3 in.

restricted. On the other hand if the aperture is much restricted, then the lens is of no use, and you will get as good an image without it as with it.

I have not time to deal with this matter as I could wish, but I will illustrate it by projecting on the screen the image of a piece of gauze as formed by a narrow aperture parallel to one set of wires. There is no lens whatever between the gauze and the screen. [Experiment.] There is the image—if we can dignify it by such a name—of the gauze as formed by an aperture which is somewhat large. Now, as the aperture is gradually narrowed, we will trace the effect upon the definition of the wires parallel to it. The definition is improving; and now it looks tolerably good. But I will go on, and you will see that the definition will get bad again. Now, the aperture has been further narrowed, and the lines are getting washed out. Again, a little more, and they are gone. Perhaps you may think that the explanation lies in the faintness of the light. We cannot avoid the loss of light which accompanies the contraction of aperture, but to prove that the result is not so to be explained, I will now put in a lens. This will bring the other set of wires into view, and prove that there was plenty of light to enable us to see the first set if the definition had been good enough. Too small an aperture, then, is as bad as one which is too large; and if the aperture is sufficiently small, the image is no worse without a lens than with one.

What, then, is the best size of the aperture? That is the important question in dealing with pin-hole photography. It was first considered by Prof. Petzval, of Vienna, and he arrived at the result indicated by the formula, $2\,r^2 = f\,\lambda$, where $2\,r$ is the diameter of the aperture, λ the wave-length of light, and f the focal length, or rather simply the distance between the aperture and the screen upon which the image is formed.

His reasoning, however, though ingenious, is not sound, regarded as an attempt at an accurate solution of the question. In fact it is only lately that the mathematical problem of the diffraction of light by circular holes has been sufficiently worked out to enable the question to be solved. The mathematician to whom we owe this achievement is Prof. Lommel. I have adapted his results to the problem of pin-hole photography. [A series of curves * were shown, exhibiting to the eye the distribution of illumination in the images obtainable with various apertures.] The general conclusion is that the hole may advantageously be enlarged beyond that given by Petzval's rule. A suitable radius is $r = \sqrt{(f\lambda)}$.

I will not detain you further than just to show you one application of pin-hole photography on a different scale from the usual. The definition improves as the aperture increases; but in the absence of a lens the augmented aperture entails a greatly extended focal length.

* 'Phil. Mag.' Feb. 1891.

The limits of an ordinary portable camera are thus soon passed. The original of the transparency now to be thrown upon the screen was taken in an ordinary room, carefully darkened. The aperture (in the shutter) was ·07 inch, and the distance of the 12 × 10 plate from the aperture was 7 feet. The resulting picture of a group of cedars shows nearly as much detail as could be seen direct from the place in question.

[R.]

WEEKLY EVENING MEETING,

Friday, February 13, 1891.

William Huggins, Esq. D.C.L. LL.D. F.R.S. Vice-President, in the Chair.

Professor Arthur Schuster, Ph.D. F.R.S.

Recent Total Solar Eclipses.

Successful observations of solar eclipses began with the invention of the spectroscope. Though valuable results had been personally obtained, especially by De la Rue in 1860, the bulk of the observations made before 1868 are of a kind which at present can be carried on without the help of an eclipse. The steady progress of eclipse work of late years is not, however, altogether due to the spectroscope, but in great part to the science of photography, which of late years has advanced with rapid strides. In order to judge of the amount of information actually obtained, it is well to bear in mind the short time which has been at our disposal; the aggregate time during which eclipse observations have been carried on since the construction of the spectroscope hardly exceeds half an hour.

The primary object of an eclipse expedition is to investigate the regions of space which are in the vicinity of the sun, and we may hope thereby ultimately to obtain important information concerning the constitution of interstellar space.

The lecturer explained the peculiar difficulties of eclipse observations, the preparations for which often have to be conducted under great disadvantages, especially when, as happened in the West Indian eclipse of 1886, the weather is of such an unsettled character that up to the last minute of the eclipse it was uncertain whether anything could be seen at all.

Photographs of the solar corona as observed in different eclipses were thrown on the screen, and attention was drawn to the great differences in its general outline and character. Two types of corona may be distinguished; one of them principally appearing at a time of sunspot maximum, while the other is chiefly seen when there are few spots on the luminary.

The corona of sunspot minimum is characterised by long streamers spreading chiefly in directions which are not much inclined to the solar equator. In addition to these extensions, curved lines are noticed which seem to converge to two points on the solar surface which are not far removed from the solar poles. If a line is drawn through the two points of convergence, the corona is roughly symmetrical with respect to it. No such symmetry can be noticed in the corona which appears at times of maximum sunspots. The streamers extend irregularly all round the body of the sun, but do not reach so far as the long extension of the first type.

With respect to the nature of the corona, there are four alternatives. It consists of matter either (1), forming a regular atmosphere round the sun; or (2), matter projected from the sun; (3), matter falling into the sun; or finally (4), matter circulating round the sun with planetary velocity.

The choice between these observations must be made by careful analysis of the light received by us from the corona. We possess well-known methods to distinguish between light which is sent out from bodies which are self-luminous, and light which is reflected; and we may even detect violent motions of luminous matter.

The lecturer then explained in detail the photographs of the spectrum of the corona and prominences as photographed in the West Indian eclipse. The principal results may be summarised as follows:—

(1) The greater part of the light sent out by the solar corona is due to matter which is self-luminous, and probably in a solid or liquid condition; the maximum of luminous intensity is displaced towards the red end of the spectrum as compared with sunlight, showing that the temperature of the luminous matter is lower than that of the solar surface.

(2) A comparatively small part of the light is reflected sunlight. The relative importance of reflected and independent light seems to differ in different eclipses, a point which will no doubt receive careful attention on future occasions.

(3) Hydrogen and calcium, which are the main constituents of solar prominences, do not form part of the normal spectrum of the corona. The hydrogen lines are visible only in the parts overlying strong prominences. The violet calcium lines known as H and K, though visible everywhere, are stronger on that side of the corona which has many prominences at its base.

If this result is confirmed on future occasions it would prove that the matter of the corona is partly formed by substances thrown out from the body of the sun.

(4) The gaseous constituents of the corona, which seem rich in spectroscopic lines, cannot at present be identified with any terrestrial elements.

(5) The matter of the corona does not revolve with planetary velocity round the sun.

Photometric measurements have been made during the last eclipse by Professor Thorpe with instruments designed by Captain Abney, and one of their results allows us to compare the corona in this respect with the one observed by Langley in 1878. Thorpe and Abney find the luminous intensity between eight and nine minutes of arc away from the sun's limb to be about the twentieth part of the intensity of moonlight, while Langley found that at a distance of three minutes the corona radiated with one-tenth the intrinsic brightness of the moon.

Considering (1), that the brightness of the corona diminishes with the distance from the sun's limb; (2), that Langley observed at the top of Pike's Peak, at an elevation of 14,000 feet in a very dry atmosphere, while Thorpe's observations were taken at sea-level under unfavourable circumstances; also (3), that the type of corona was different on the two occasions; the results agree to a remarkable degree, and show that the eye estimates which have suggested enormous differences in the brilliancy of the corona in different eclipses are not to be trusted.

Returning to the four alternatives respecting the constitution of the corona, we may at once reject the first and fourth; for it may be proved that the sun could have no regular atmosphere to the extent indicated by the outlines of the corona, and spectroscopic results exclude the hypothesis that the bulk of its matter revolves with planetary velocity; though probably there is some meteoric material which does revolve round the sun.

Dr. Huggins, in a lecture delivered in the Royal Institution,* has suggested a theory of the corona, according to which its luminosity is due to electrical discharges, the matter conveying the discharge being projected from the sun by electrical repulsion. The author agrees with Dr. Huggins in the idea that electrical discharges are probably the cause of the streamers which form the most prominent feature of the solar corona. But before we can form any definite ideas as to the precise way in which these discharges are brought about, we must first settle the very important question whether the planetary space contains sufficient matter to be a conductor of electricity. Our present knowledge regarding electrical discharges entitles us to say that a body which is at the high temperature of the sun, surrounded by gaseous matter, cannot keep any appreciable charge of electricity, and we have some evidence for saying that once a discharge is set up in interplanetary space there is sufficient matter present to convey the discharge, so that the lecturer feels bound to believe in a direct electric connection between the sun and the planets. If then, as is probable, electric discharges take place near the sun, there must be some cause which keeps up the difference in electrical potential between the sun and outside space. The form of the corona suggests a further hypothesis, which, extravagant as it may appear at present, may yet prove to be true. Is the sun a magnet? We know that a body at such a high temperature cannot be magnetisable, but may not a revolving body act like a magnet, and may not the earth's magnetism be similarly due to the earth's revolution about its axis? It can be shown that although a revolving body may act like a magnet sufficiently to account for

* 'Proceedings,' Royal Institution, 1885.

terrestrial magnetism, our instrumental appliances would yet be quite insufficient to allow us to detect the phenomenon in bodies set into rotation artificially on the surface of the earth, so that there is no *a priori* reason against the hypothesis. Owing to the large mass of the sun the magnetic forces at his surface would be much stronger than those at the surface of the earth, and we should expect the outline of the corona to show the influence of these magnetic forces if the streamers of the corona are caused by electric discharges. The form of the corona at a time of minimum sunspot is as a matter of fact very similar to what we should expect if the sun was a magnet, discharging negative electricity near its poles. The author has shown, in his Bakerian lecture of 1884, that a magnet introduced into a hollow negative electrode drives the discharges away from the poles of the magnet, concentrating at places where the field is weakest. It is also known that the negative discharge near a magnet tends to take up the shape of the lines of force, and Bigelow has recently drawn attention to the similarity between the polar rays of the corona and the lines of force due to a magnetised sphere.

All these questions are at present in a purely speculative state, but it is only by keeping an ambitious programme that we may hope to make good use of the eclipses which are yet to come. The relation between matter and the luminiferous ether is the great question of the day, and Cosmical Physics is likely to contribute largely to its solution.

[A. S.]

WEEKLY EVENING MEETING,

Friday, February 20, 1891.

Sir James Crichton Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

Edward E. Klein, M.D. F.R.S. Lecturer on Physiology at St. Bartholomew's Hospital.

Infectious Diseases, their Nature, Cause, and Mode of Spread.

We read in Homer that "Phœbus Apollo, offended by mortals, sent a pernicious plague into the camp of the Greeks; the wrathful god with his arrows hit first mules, then dogs, and then also the Greeks themselves, and the funeral pyres burned without end." If we expressed this in less poetical language, but more in conformity with our modern realistic notions, we would say that the deity of health and cleanliness, having been offended by mortals, sent his poisonous but imperceptible darts or bacilli into them, and caused an epidemic of a fatal disease, communicable to man and animals.

In whatever form we meet with this simile—whether an epidemic be ascribed to a wrathful Providence, or to a sorcerer or a witch that put their spells on man or on cattle, thereby causing numbers of them to sicken and to die; whether this happened amongst the nations of old, or amongst the modern Zulus, whether amongst the peasants of Spain or in Italy—we now know that it always means that the offended deity of cleanliness, and the outraged laws of health, avenge themselves on mortals by the invasion of armies of imperceptible enemies, which we do not call arrows, nor sorcerers' or witches' spells or incantations, but microbes.

From Homer's Trojan epidemic among the Greeks to the epidemic in the camp of Cambyses, from the plagues carried and spread by the Crusaders of old to the plagues carried and spread in modern times by pilgrims to and from Mecca, the plagues following the ancient armies and those of more recent times, the plagues attacking a country debilitated by famine or by superstition have been in the past, and will be in the future, due in a great measure to neglect and ignorance, on the part either of individuals or of a whole population, of the principles of the laws of health: and it is chiefly to this neglect, ignorance, and indolence, that the spread and visitations of epidemic infectious disorders must be ascribed. It is therefore, with justice, that these disorders are called preventable diseases, and one cannot imagine a greater contrast than that between the knowledge we possess at the present time of communicable diseases, as to their cause, mode of spread and prevention, and the views of former generations as to their spontaneous origin.

Although the notion that all epidemic diseases are communicable, i.e. spread from one individual to another, is not a new one, since many writers of former generations have had clear ideas about them, yet the actual demonstration of the fact that the different infectious or communicable diseases are due to definite species of microbes, which, having invaded a human or animal organism, are capable therein of multiplying and of causing a particular infectious illness; further, the identification of these living germs in the blood and tissues of an invaded individual, and the recognition of their many and intricate migrations outside the animal body; the study of the microbes in artificial cultures, i.e. outside the human or animal body; further, the best means to do battle with them, to neutralise them, to prevent their growth and to destroy them; then the *modus operandi* of the different species, each appertaining to, and causing, a definite kind of disorder—in short, all that is exact and precise in the knowledge of the causation, nature, and prevention of infectious diseases, is an outcome of investigations carried on during the last twenty-five years. Modern research has not only definitely demonstrated these microbes, it has also shown that a number of diseases not previously suspected as communicable have a similar cause to the above, and are therefore now classed amongst them. It need hardly be emphasised that a knowledge of the causes must lead, and as a matter of fact has led, to a clearer and better understanding of the recognition, prevention, and treatment of these disorders, an understanding obviously directed towards, and followed by, the alleviation and diminution of disease and death in man and animals.

I may point to a few special examples to illustrate these propositions. The disease known as splenic apoplexy or malignant anthrax is a disease affecting man and brutes. In some countries the losses to agriculturists and farmers owing to the fatal character of the disease in sheep and cattle is enormous. In man it is chiefly known amongst wool-sorters and those engaged in the handling of hides. This disease has been definitely proved to be due to a bacillus, the *Bacillus anthracis,* which, after its entry into the system of an animal or human being, multiplies very rapidly in the blood and spleen, and, as a rule, produces a fatal result, at any rate in sheep and cattle. Now, the bacillus having been proved to be always associated with this disease, anthrax, it was then shown that this bacillus can grow and multiply also outside the animal body: its characters in artificial media have been carefully studied and noted, so that it can be easily recognised; and by the pure cultures of the bacillus the disease can be again reproduced in a suitable animal. Such cultures have been subjected to a number of experiments with heat, chemicals, or antiseptics; the chemical function of the *Bacillus anthracis* has been and is being accurately studied in order to give us an insight into the mode in which it is capable of producing the disease; it has been further shown by Koch that the bacilli are capable of forming seeds or spores which possess a very high degree of resistance to various inimical conditions, such as heat, cold, chemicals, &c., and that it is precisely these spores, entering the system by the alimentary canal through food or water, or by the respiratory organs through the air, to which the disease in most instances is due. Further, it has been shown that a trace of the blood of an animal affected with or dead from the disease, when introduced into an abrasion of the skin of man or animal, produces at first a local effect (carbuncle) followed by a general and often fatal infection. But the most important result of the cultivation of the bacillus outside the body, in artificial media, was the discovery that if subjected to or grown at abnormally high temperatures, 42°·5 C., i. e. above the temperature of the animal body, its power to produce fatal disease—that is, its virulence—becomes attenuated, so much so that, while the so-altered bacilli on inoculation into sheep or cattle produce a mild and transitory illness, they nevertheless furnish these animals with immunity against a fatal infection.

The recognition and identification of the *Bacillus anthracis* as the true cause of the disease, splenic fever or splenic apoplexy, the knowledge of its characters in the blood and spleen of man and animals, and of its peculiarities in artificial cultures, have enabled us to make a precise diagnosis of the disease, which previously was not always easy or even possible. The knowledge of its forming spores when grown under certain conditions, and of the manner in which experimentally the disease can be reproduced in animals by the bacillus and its spores, has led to a complete understanding of the means and ways in which the disease spreads both in animals and from them on to man; and last, but not least, the methods of the protective inoculations first indicated and practised by Pasteur have been solely the result of the studies in the laboratory of the cultures of the *Bacillus anthracis*, and of experiments with them on living animals. I could add here a number of other diseases—such as glanders, fowl cholera and fowl enteritis, erysipelas, scarlet fever and diphtheria in man, actinomycosis in man and cattle, swine fever and swine erysipelas, grouse disease, symptomatic charbon in cattle, and other diseases of animals—which have been brought to a fairly advanced understanding by methods such as those indicated above; and hereby not only in the diagnosis and recognition, but also in the treatment and prevention of these disorders, an immense amount of valuable progress has been achieved.

[1. Demonstration: lantern slides of anthrax, fowl cholera, fowl enteritis, grouse disease, typhoid, cholera, pneumonia, diphtheria, actinomycosis, scarlatina, and glanders.]

As examples of the second proposition, viz. that the modern methods of study of disease germs, of their nature and action on living animals, have led to the recognition as communicable diseases of some disorders which previously were not known or even suspected to be of this character, I may mention amongst several the disease known as tuberculosis or consumption, tetanus or lock-jaw, and acute pneumonia. Not until Klencke and Villemin had shown by direct experiment on animals that tuberculosis is inoculable was it grouped amongst the infectious diseases. Since these experiments were first published a large amount of work has been done, proving conclusively that tuberculous material—that is, portions of the organs containing the tubercular deposits (e. g. lung, lymph gland, spleen, &c.)—by inoculation, by feeding, or by introducing it into the respiratory tract, can set up typical tuberculosis in the experimental animals; the tubercular deposits in these experimental animals again are endowed with the power to propagate the disease in other animals. Further, it was shown that the disease in cattle called "Perlsucht" was in all respects comparable to tuberculosis in man, and it is accordingly now always called tuberculosis.

A further, and perhaps the greatest, step was then made by Koch's discovery in 1882 of the tubercle bacillus, and his furnishing the absolute proof of its being the true cause of the disease. The demonstration and identification of this microbe is now practised, I might almost say, by every tyro, and it is of immense help to diagnosis. In former years, and before 1882, the diagnosis of tuberculosis was not by any means an easy matter in many cases of chronic lung disease; since that year every physician in such cases examines the expectoration of the patient, and the demonstration of the tubercle bacilli makes the diagnosis of tuberculosis absolutely certain. Not only in medical, but also in many surgical cases, e. g. certain forms of chronic disease of bones and joints, particularly in children, the demonstration of the tubercle bacilli is of essential importance, and by these means diseases like lupus of the skin, scrofula, and certain diseases of bones and joints not previously known as tuberculosis, are now proved to be so. The same applies to animals; wherever in a diseased organ of man or animal the tubercle bacilli can be demonstrated, the disease must be pronounced as tuberculosis.

The proof that the tubercle bacillus is the actual cause of the tubercular disease was established by Koch beyond possibility of doubt. Cultures in artificial media were made from a particle of a tubercular tissue, either of a human being or of cattle affected with tuberculosis, or of an experimental animal tubercular by ingestion, or by injection with tuberculous matter, and in all cases crops of the tubercle bacilli were obtained. Such cultures were then carried on from subculture to subculture, through many generations, outside the animal body; with a mere trace of any of these subcultures, however far removed from the original source, susceptible animals were infected, and all without fail developed tuberculosis, with the tubercle bacilli in the morbid deposits of their organs. The discovery of the tubercle bacilli and the demonstration that they are constantly present in the tubercular deposits of the typical tuberculosis, and the proof by experiment on living animals that they are the actual cause of the disease, are not all that we have learned, for it has also been shown that certain diseases, like the dreaded and disfiguring disease known as lupus—at any rate some forms of it—and further the disease scrofula, so often present in children, are really of the nature of tuberculosis, the former in the skin, the latter in the lymph glands.

Now see what an enormous step in advance this constitutes :—

(1) We can now diagnose tuberculosis with much greater accuracy in man and animals, even in cases in which this was formerly difficult or impossible.

(2) We have accepted rightly that all forms of tuberculosis are infectious or communicable diseases, communicable by inoculation, by ingestion, i.e. by food, or by respiration, i.e. by air.

(3) We have learned to recognise that, as in other infectious disorders, there exists a risk to those susceptible to tuberculosis, of contracting the disease from a tubercular source, and it is the recognition of these facts which ought to regulate all efforts to prevent its spread.

Tetanus or lockjaw, not previously known to be so, has likewise been fully demonstrated to be an infectious disease: we now know that it is due to a bacillus having its natural habitat in certain garden earth; that this bacillus forms spores, that these spores gaining access to an abrasion or wound of the skin in man or animals are capable of germinating there and multiplying, and of producing a chemical poison which is absorbed into the system, and sets up the acute complex nervous disorder called lockjaw. The recognition of the disease as an infectious disease and caused by a specific microbe has taught us at the same time the manner in which the disease is contracted, and thereby the way in which the disease is preventable.

[2. Demonstration: lantern slides of tubercle and tetanus.]

The study of disease germs by the new and accurate methods of bacteriology has also led to a clearer and better understanding of the manner in which at any rate some of the infectious diseases spread. While it was understood previous to the identification of their precise cause that some spread directly from individual to individual (e. g. small-pox, scarlet fever, diphtheria), others were known to be capable of being conveyed from one individual to another indirectly, i. e. through adhering to dust, or being conveyed by water, milk, or by food-stuffs (e. g. cholera, typhoid fever). But we are now in a position to define and demonstrate more accurately the mode in which infection can and does take place in many of the infectious diseases. By these means we have learned to recognise that the popular distinction between strictly contagious and strictly infectious diseases—the former comprising those diseases which spread as it were only by contact with a diseased individual, while in the latter diseases no direct contact is required in order to produce infection, the disease being conveyed to distant points by the instrumentality of air, water, or food—is only to a very small extent correct. Take, for instance, a disease like diphtheria, which was formerly considered a good example of a strictly contagious disorder; we know now that diphtheria, like typhoid fever or scarlet fever, can be, and, as a matter of fact, is, often conveyed from an infected source to great distances by the instrumentality of milk. In malignant anthrax, another disease in which the contagium is conveyable by direct contact, e. g. in the case of an abrasion or wound on the skin coming in contact with the blood of an animal dead of anthrax, we know that the spores of the anthrax bacilli can be, and, as a matter of fact, in many instances, are, conveyed to an animal or a human being by the air, water, or food. The bacilli of tubercle, finding entrance through a superficial wound in the skin or mucous membrane, or through ingestion of food, or through the air, can in a susceptible human being or an animal produce tuberculosis either locally or generally. The difference as regards mode of spread between different diseases resolves itself merely into the question, which is, under natural conditions, the most common mode of entry of the disease germ into the new host? In one set of cases, e. g. typhoid fever, cholera, the portal by which the disease germ generally enters is the alimentary canal; in another set an abrasion or wound of the skin is the portal, as in hydrophobia, tetanus, and septicæmia; in another set the respiratory organs, or perhaps the alimentary canal, or both, are the paths of entrance of the disease germ, as in small-pox, relapsing fever, malarial fever; and in a still further set the portal is just as often the respiratory tract as the alimentary canal, or a wound of the skin, as in anthrax, tuberculosis. But this does not mean that the virus is necessarily limited to one particular portal, or that it must be directly conveyed from its source to the individual that it is to invade. All this depends on the fact whether or not the microbe has the power to retain its vitality and virulence outside the animal or human body.

Anthrax bacilli are killed by drying; they gradually die off if they do not find sufficient nutriment in the medium into which they happen to be transferred; they are killed by exposure to heat far below boiling-point; they are killed by weak carbolic acid. But if these anthrax bacilli have been able to form spores, these latter retain their vitality and virulence when dried, when no nutriment is offered to them, and even when they are exposed for a few seconds to the heat of boiling water, or when they are exposed to the action of strong solutions of carbolic acid. Similarly, the bacillus of diphtheria is killed by drying, also by weak solutions of carbolic acid; it is killed when kept for a few days in pure water, on account of not finding sufficient nutriment; fortunately the diphtheria bacillus is killed in a few minutes at temperatures above 60° or 65° C., for this bacillus does not form spores. The same is the case with the microbe of scarlet fever.

The tubercle bacillus forms spores; these are not killed by drying, they are killed by the heat of boiling water of sufficiently long duration, two or more minutes; they are not killed by strong carbolic acid.

While, therefore, we know in these cases on what the conditions of infection depend, we have also learned to understand the conditions which favour or prevent the infection.

Not all infectious diseases which have been studied are due to Bacteria: in some the microbe has not been discovered, e. g. hydrophobia, small-pox, yellow fever, typhus fever, measles, whooping-cough; in others it has been shown that the disease is due to a microbe which belongs, not to the Bacteria, but to the group of those simplest animal organisms known as Protozoa. Dysentery and tropical abscess of the liver are due to Amœbæ; intermittent fever or ague is due to a protozoon called Hæmoplasmodium; a chronic infectious disease prevalent amongst rodents, and characterised by deposits in the intestine, liver, and muscular tissue, is due to certain forms known as Coccidia, or Psorospermia. A chronic infectious disease in cattle and man known as actinomycosis is due to a fungus, the morphology of which indicates that it probably belongs to the higher fungi; certain species of moulds (e. g. certain species of Aspergillus and Mucor) are also known to be capable of producing definite infectious chronic disorders; and so also is thrush of the tongue of infants; ringworm and certain other diseases of the hair and skin are known to be due to microbes allied to the higher fungi.

The microbes causing disease which have been studied best, are those belonging to the groups of Bacteria or Schizomycetes or fission fungi (they multiply by simple division or fission); most species of these have been cultivated in pure cultures, and the new crops have been utilized for further experiments on animals under conditions variable at the will of the experimenter.

[3. Demonstration: cultures of Bacteria in plates and in tubes.]

One of the earliest and most important discoveries was that made by Pasteur as to the possibility of attenuating in action an otherwise virulent microbe—that is to say, he succeeded in so growing the microbes, that when introduced into a suitable animal they caused only a mild and transitory illness, which attack, though mild, is nevertheless capable of making this animal resist a second virulent attack. Jenner, by inoculating vaccine, inoculated a mild or attenuated small-pox, and by so doing protected the individual against a virulent small-pox. Pasteur succeeded in producing such an attenuated virus for two infectious diseases—chicken cholera and splenic apoplexy or anthrax; later on also for a third—swine erysipelas. For the first two he produced cultures grown under certain unfavourable conditions, which owing to these conditions lose their virulence, and when inoculated fail to produce the fatal disease, which they would produce if they were grown under normal conditions. What they produce is a transitory mild attack of the disease, but sufficient to protect the animal against a virulent form; thus in anthrax he showed that by growing the *Bacillus anthracis* at a temperature of $42^{\circ}\cdot 5$ to 43° C. for one week, the bacilli become slightly weakened in action; growing them for a fortnight at that temperature, they become still more weakened, so much so, that if this culture (*première vaccine*) be injected into sheep or cattle (animals very susceptible to anthrax) the effect produced is slight; then injecting the culture which has been growing only eight days at $42^{\circ}\cdot 5$, the effect is a little more pronounced, but not sufficient to endanger the life of the animal. Such an animal, however, may be regarded as having passed through a slight attack of anthrax, and as being now protected against a second attack, however virulent the material injected. In the case of swine erysipelas, Pasteur found that the microbe of this disease, transmitted through several rabbits successively, yields a material which is capable of producing in the pig a slight attack of swine erysipelas, sufficient to protect the animal against a second attack of the fatal form. Passing the anthrax virus from however virulent a source through the mouse, it becomes attenuated, and is then capable of producing in sheep only a mild form of disease protective against the fatal disorder. Attenuation of the microbes has been brought about outside the body by growing them under a variety of conditions somewhat unfavourable to the microbe.

Attenuation of the action of the anthrax microbes has been produced by adding to the cultures some slightly obnoxious material (e. g. mercuric bichloride 1 : 40,000), by which the growth is somewhat interfered with; or subjecting an otherwise virulent culture for a short time to higher degrees of temperature (anthrax to 56° C. for five minutes; fowl enteritis, twenty minutes, 55° C.); or exposing them for short periods to some obnoxious chemical substance (e. g. anthrax to carbolic acid, anthrax to bichloride of mercury 1 : 25,000 for twenty minutes); or the microbes are passed through, i. e. are grown in the body of certain species of animals, whereby the microbes become weakened as regards other species (swine erysipelas, anthrax, diphtheria, and tetanus); finally, some microbes become attenuated spontaneously, as it were, by growing them in successive generations outside the animal body, e. g. the pneumonia microbe, the erysipelas microbe, and others. However good the nutritive medium, these microbes gradually lose their virulence as cultivation is carried on from subculture to subculture; in diphtheria the culture which was virulent at first loses its virulence as the same culture becomes several weeks old.

All these facts are of considerable importance, inasmuch as they enable us to understand how, in epidemics, the virulence of the microbe gradually wears off and becomes ultimately nil, and because they indicate the ways of attenuating microbes for the object of protective inoculations.

Another important step in the study of Bacteria was this: it was shown that they have, besides their special morphological and cultural characters, definite chemical characters. Specific chemical characters (specific ferment actions) of Bacteria have been known for a long time through the earlier researches of Pasteur—e. g. the Bacteria causing the acetic acid fermentation of alcohol, the mucoid fermentation, e. g. when beer becomes ropy, the lactic acid fermentation of milk-sugar, when milk becomes spontaneously sour, &c.

Similarly it has been shown that when animal or vegetable matter undergoes the change known as putrefaction or putrid decomposition, substances are produced which resemble alkaloids in many ways, and which, introduced into the circulation of man or animals, act poisonously, the degree of action depending, *cæteris paribus*, on the dose, i. e. the amount introduced. These alkaloids—called ptomaines of Selmi—have been carefully investigated and analyzed by Brieger; they are different in nature according to the organism that produces them, and according to the material in which this organism grows: neurin, cadaverin, cholin, &c., are the names given to these substances.

Recent research has shown that pathogenic Bacteria, i. e. those associated with, and constituting the cause of specific diseases, are capable of elaborating poisonous substances—toxalbumins or toxins, as they are called—not only in artificial culture media, but also within the human or animal body affected with the particular pathogenic microbe. Thus, in anthrax or splenic apoplexy, Hankin and Sidney Martin have shown this to be the case; in diphtheria (Fraenkel and Brieger), in tetanus (Kitisato), similar toxins have been demonstrated. We can already assert with certainty that a microbe that causes a particular disease causes the whole range of symptoms characterising the disease by means of a particular poisonous substance or substances it elaborates in and from the tissues of the affected individual.

Another important fact ascertained about some of the toxic substances produced by the different pathogenic Bacteria was this: that if, after they are elaborated in an artificial culture fluid, and, by certain methods of filtration, are separated from the Bacteria and injected into a suitable animal, they are capable of producing the same disease as their microbes, the rapidity and intensity varying with the amount introduced; so that it became evident that also in the human and animal body the intensity of the particular disease depends, amongst other things, on the amount of poisonous substance elaborated by the Bacteria in the tissues. A further important step made was this: that if the poisonous substance be introduced in such doses that only slight disturbance would follow, and the dose be repeated several times, the body of the animal eventually becomes refractory to the growth and multiplication of the particular Bacteria.

Wooldridge's researches on septicæmia and on anthrax, Roux's researches on septicæmia and diphtheria, Beumer and Peiper, Salmon, and many others, have shown the same for a variety of infectious diseases: in all these instances it has been proved that, when the chemical products of a specific microbe, elaborated in an artificial culture medium or in the animal body, are injected into a healthy animal, this latter is rendered refractory against the specific microbe, so that, if the specific microbe be introduced into such a prepared animal, the microbe cannot grow and multiply, and cannot therefore produce the disease. Pasteur's brilliant researches on protective inoculation against hydrophobia are based on this principle.

The same explanation applies also to diseases like scarlet fever, anthrax, fowl cholera, swine fever, certain forms of septicæmia, in which a first even mild attack is sufficient to protect the animal against a second attack, however large the number, and however great the virulence of the particular Bacteria introduced.

For it must be obvious that it is practically the same, whether the protective amount of the toxic substance is produced by the Bacteria in the animal body, as is the case during a mild first attack of the disease, or whether the protective amount of toxin is elaborated outside the body, i. e. in an artificial culture, and is then introduced into the animal body. In both instances the effect is the same, viz. the animal body is hereby rendered capable of withstanding the growth and multiplication of the particular Bacteria when a new invasion takes place.

What is the cause of this immunity or refractory condition?

In order to explain this, I wish first to draw your attention to the familiar fact that different species of animals, and even different individuals of the same species, offer a different degree of resistance to the different infectious diseases. Whereas splenic fever or anthrax is communicable to man, rodents, and herbivorous animals, it is only with difficulty communicable to carnivorous animals or birds; cholera and typhoid fever are not communicable to any but man; diphtheria is communicable to the human species, to guinea-pigs, cats, and cows, it is not communicable to some other animals; tubercle or consumption is communicable to man and herbivorous animals, in a less degree to carnivorous animals, though these also take it but in a smaller intensity; certain other diseases are common to animals, but are not communicable to the human species.

If we inquire into the cause of this different susceptibility, we find some very striking facts. Take anthrax: cold-blooded animals, e. g. frogs, are unsusceptible as long as they are in their natural conditions of temperature; but if a frog be kept at the temperature of a warm-blooded animal, it is found susceptible to anthrax (Petruschki). Birds are not susceptible to anthrax, but if its temperature be lowered a few degrees it becomes susceptible to anthrax (Pasteur).

Or take another instance: rats are not susceptible to anthrax, but if the animals be kept for some time under severe muscular exercise, they become susceptible to the disease. Tame mice are unsusceptible to glanders, but if phlorizin is administered to them for some days, whereby a deposit of sugar takes place in their tissues, they become susceptible to glanders. The susceptibility and unsusceptibility are expressed by saying that the living tissues in an animal offer in the one case a favourable, in the other an unfavourable, soil for the growth and multiplication of the microbe, and that this unfavourable condition can be altered in a variety of ways, e.g. temperature, muscular fatigue, sugar in the tissues, &c. But also a primary favourable condition can be rendered unfavourable: for instance, a human being that has passed through one attack of scarlatina offers tissues unfavourable for the growth of scarlatina microbes; attenuated anthrax protects against virulent anthrax; an animal that has been first treated with repeated small doses of the chemical products of a peculiar microbe becomes unsusceptible to that microbe.

In order to explain the whole group of phenomena of refractory state, immunity, and protection, a theory has been put forward which is as simple as it is fascinating. There can be truly no greater satisfaction and no greater aim in any branch of science than to express a great number of facts and phenomena by the simplest possible formula; the greatest minds and the most successful philosophers have achieved this. Now, in regard to the numerous and extremely complex phenomena that we have under consideration, a simple formula has been put forward which is supposed to cover all the facts and to explain all the phenomena; this formula is comprised in a single word, "phagocyte." This word is put forth whenever and wherever a difficulty arises in explaining or understanding the complex problems involved in the intimate pathological processes, the refractory condition, the unsusceptibility to and immunity from an infectious disease. To any and every question referring to infectious diseases the answer is simply "phagocyte." By a "phagocyte" is understood one of those elementary microscopic corpuscles abounding in the animal and human body, possessed of spontaneous or amœboid movement, and occurring in the blood as white blood-cells or leucocytes; in the lymph and lymph-glands and most tissues, as lymph corpuscles; in all acute and chronic pathological processes, as pus-cells or round cells. The cells, by their protoplasmic or amœboid movement, have the power to take up into their interior all manner of minute particles or granules, living or non-living; it seems as if these granules and particles were being swallowed up, eaten, and destroyed by the cells—hence the name of eating globule, or "phagocyte," given to them.

These cells—white blood-cells, lymph-cells, or round cells—are supposed to have the important function to act as the sanitary police against the invading Bacteria, to be always on the look-out for them, and where they meet them to at once engage in battle with them; that is to say, to do as the giants do—to eat their victims. If the phagocytes are victorious—that is, if they succeed in eating up the Bacteria—no harm is done to the animal body; no disease is produced; but if, for one reason or another, the Bacteria succeed in evading the grasp of the phagocyte police, then the Bacteria grow and multiply, and cause the disease. Sometimes this latter result follows on account of the phagocytes not being capable of moving sufficiently briskly to the places of mischief, or, for some inherent reason, not being able to cope with the Bacteria, or being altogether indifferent to the presence of the enemy; when this is the case in an animal or human body, the phagocytes being powerless to destroy the Bacteria, we are supposed to be dealing with a body that is susceptible to the disease; but when the phagocytes do their duty, then the body is unsusceptible to the disease. Again, when an animal or human being, by a mild first attack, or by protective inoculation of one kind or another, becomes unsusceptible to a second attack, this is explained by saying that, though the phagocytes have not done, or have not been able to do, their duty during that first attack, they have now been rendered capable of doing it.

Now, if you ask what is the evidence on which this theory of phagocytosis is based, you will find that it is of the most slender kind, and you will further find that there is an overwhelming number of observations which directly negatives this theory of *universal* phagocytosis, and, moreover, proves conclusively that if phagocytosis has any share in producing a refractory condition on the part of animals towards a particular infectious disease—be that a primary unsusceptibility or an acquired immunity against a second attack—this share is of a remarkably small degree. The whole theory was started by Metschnikoff by the interesting and fundamental observation that, if anthrax bacilli are introduced into the dorsal lymph-sac of the frog—an animal unsusceptible to anthrax while living under normal conditions—the bacilli become inclosed in the lymph-cells, and are gradually broken up; they do not multiply, and do not therefore set up the disease anthrax. This observation, which is easily verified, was the starting-point for the theory. Metschnikoff and others have described similar appearances in other conditions of refractory states.

Now the above observation is explained by Metschnikoff in this way: the lymph-cells are acting the part of guardians, swallowing up the bacilli and preventing them from entering the circulation, and thereby preventing the outbreak of the disease. It must seem very extraordinary that this should be really a true explanation of the refractory state of the frog towards anthrax, considering that the bacilli, like other minute particles, when injected into the lymph-sac, would be absorbed and brought into the circulation in a few minutes, nay, seconds—at any rate some hours before the phagocytes have got into the lymph-sac in sufficient numbers to do battle with the bacilli. That the bacilli really enter the circulation in this and other cases, but are destroyed by the blood, not by the leucocytes, but by the fluid part of the blood—the plasma—has been abundantly proved; and it has likewise been proved that the fluid part of the blood and of the lymph in general has a remarkable germicidal action, independent of any cellular element, leucocytes, or other cells. The observation of Metschnikoff admits of an explanation different from that given by him; it may mean, and probably does mean, that the bacilli cannot exist in the fluid of the lymph and of the blood; they re destroyed here, but they *take refuge in the leucocytes or lymph-cells* *in which they can live and exist*—for a time, at any rate—these cells offering to them more favourable conditions of existence.

In the case of the normal frog, this is also only of a temporary nature, since the substance of the lymph-cells suspended in the lymph or in the blood-plasma becomes likewise permeated with the germicidal influence of the fluid lymph and the fluid plasma, and hence the bacilli soon die, even in the substance of the cells. This explanation is in perfect harmony with the large number of carefully conducted experiments of a host of workers (Fodor, Nutall, Buchner, Petruschki, Lubarsch, and others), according to whom the refractory condition of an animal to a particular infectious disease is due to a chemical germicidal action of the tissue-juices, the lymph, or blood plasma, and independent of any cellular elements. The more pronounced this germicidal action of the juices is, the more refractory the animal.

Hence we find that, for instance, when in an animal even a very small number of bacteria introduced are sufficient to produce disease, the germicidal action of the living blood fluid is very small indeed; but when a considerable number of bacilli are required to produce infection, as in animals possessing only a slight refractory power, this germicidal action of the blood and tissue fluid is greater, and it is greatest of all in those bodies in which not even a large number of bacilli can produce infection, as is the case in animals possessed of immunity. As stated just now, this part of the subject, as to the germicidal action of the fluid of the tissues and the blood, has been worked out very carefully, and it has been shown that, as regards the destruction of bacteria, the leucocytes of the lymph and blood, or other similar cells, might just as well be absent altogether. Quite recently the whole argument has been clenched by showing* that if an animal susceptible to a particular disease be first infected with the bacilli causing this disease, and then into such an infected animal the cell-free serum or plasma of the blood of an animal refractory to that disease be injected, the development of the disease in the former animal is stopped or prevented. Thus mice are very susceptible to anthrax; if they are infected with anthrax bacilli they die of virulent anthrax within thirty-six to forty-eight hours; but if simultaneously with, or soon after, the introduction of the virulent anthrax bacilli, blood of frog or blood of dog (both animals very refractory to anthrax) be injected into these mice, no fatal anthrax follows. Guinea-pigs, very susceptible to diphtheria, when infected with virulent culture of the diphtheria bacilli, die in the course of a day or two, but rats are little or not at all susceptible to the diphtheria bacilli; and therefore if the guinea-pigs, after infection with the diphtheria bacilli, are injected with rat's blood they recover, this blood being a powerful destroyer of the diphtheria bacilli. Tetanus is easily communicable to mice, in which it produces fatal tetanus in

* Ogata and Tasuhara, 'Mitth. der Med. Facultät d. Kais. Japan Universität,' Tokio.

one to three days, but it is not communicable to rabbits; mice infected with the tetanus bacilli and then injected with rabbit's blood do not become affected with tetanus and remain alive.*

While on the one hand, then, the tissue juices and the blood, independent of the cellular elements, possess this germicidal action —small or *nil* in susceptible, larger in animals less susceptible, and largest in unsusceptible animals—there is, on the other hand, a considerable body of evidence to show that the least germicidal action seems to be possessed by those very cells themselves which figure in the theory as the destroyers of Bacteria, as phagocytes; that is to say, that of all the tissues the so-called phagocytes are the materials offering to the Bacteria the best means of existence. Even in cases in which the lymph and blood fluid have against particular Bacteria the greatest germicidal power, the so-called phagocytes are for a time the last refuges for the Bacteria. I will illustrate this by a number of examples both of acute and chronic infectious diseases, as gonorrhœa, Egyptian ophthalmia, Koch's mouse septicæmia, leprosy, and tuberculosis; this latter is particularly instructive, as it demonstrates the absurdity of the alleged phagocytosis of the cells of the spleen in tuberculosis, for it is the latter cells in which the tubercle bacilli thrive well, and which they choose pre-eminently.

[4. Demonstration: tubercle cells and leprosy cells.]

Nay, more than this: non-pathogenic Bacteria cannot exist in the normal blood and in the tissues, in the wall of the alimentary canal, in the tonsils, in the tongue; they are destroyed and are therefore absent in the living tissue. But they can, for a time at any rate, exist in the cells of those parts, and in these, and these only, they are met with; these cells are therefore just the reverse of phagocytes, being the last refuges of the Bacteria.

These facts seem to show that cells containing in their substance living Bacteria is no evidence whatever of a battle going on between the cells and the Bacteria, but rather the reverse. The assumption of the presence in the so-called phagocytes and similar cells of a "defensive proteid" seems therefore opposed by these facts. The cells seem to possess a particular chemical attraction for the Bacteria, just as is possessed by certain chemical substances; such attraction is spoken of as *positive chemotaxis* in contradistinction to *negative chemotaxis*—that is, the opposite or repulsive interaction between Bacteria and certain substances. This line of inquiry is of quite recent date, and promises to produce important and interesting results.

From all this we conclude, then, that in some cases the blood and tissues are, or include, a natural antidote; in others the antidote is not present naturally, and is only furnished by the Bacteria themselves, and still in others the tissues, though possessed of this antidote, may lose it owing to altered conditions.

Another point worth considering is the peculiar inimical action

* Behring and Kitisato, 'Deutsche Med. Woch.,' 1890, No. 49.

exerted by one microbe on the other: this practically means that the products of one microbe either prevent the growth of another microbe or neutralise its toxic action. It is perfectly well established that while the products of one microbe exert an inimical action, when present in sufficient amount, on the growth and life of the same microbe (the protective inoculation by chemical products of the Bacteria cited above), the accumulation of the products of the particular microbe interferes with, and eventually altogether stops the further growth of its microbe. Outside the body this is easily proved in artificial cultivations. Inside the body it is proved by those cases in which recovery takes place.

It has been shown that while some pathogenic microbes can well thrive side by side in the same culture, inside or outside the body, there are others where the growth of one is antagonistic to the action of the other: erysipelas and anthrax (v. Emmerich), swine erysipelas and swine fever, anthrax and *Bacillus pyocyaneus* (Charrin), anthrax and *Staphylococcus aureus*; this is due to the fact that the chemical products of one species inhibit or neutralise the other species. That this antagonism is really of a chemical nature is shown in the case of anthrax and *Bacillus pyocyaneus*; if the chemical products of this latter microbe be injected into the animal simultaneously or soon after inoculation with the anthrax bacilli, no anthrax disease ensues, the anthrax bacilli do not multiply and do not produce the disease. Apart from specific antiseptics, there exist, then, in the battle against the action of pathogenic microbes which have already entered the system of an animal, the following means: (1) the chemical antagonism offered by the healthy tissues themselves—in some cases this is nought, in others very great and powerful, alterable by various conditions; (2) the germicidal action of the blood and tissue juices of unsusceptible animals on the multiplication of pathogenic Bacteria within a susceptible animal; (3) the antagonism existing between the Bacteria and their own chemical products; (4) the antagonism of one species and its chemical products against another species. These principles have, then, to be borne in mind as indicating the ways in which the microbes can be prevented from producing eventually the disease in a body into which they have found access. Pasteur's hydrophobia inoculations, and many of the recently published results of curative inoculations against tetanus, against anthrax, and against diphtheria, are illustrations of these principles.

The principle on which Koch's antituberculous lymph acts is apparently of a different nature. Koch has found by experiment on guinea-pigs that if the chemical products or an extract of the substance of the tubercle bacilli be injected into a body affected with tuberculosis, the tubercular tissue becomes necrotic, while the tubercle bacilli themselves remain unaffected; at the same time a remarkable reactive inflammation sets in, by which the necrotic tissue becomes eliminated, either spontaneously, e. g. where the tubercular process is superficial, as in lupus of the skin, or it may be removed by surgical aid, as in tuberculosis of the bones and joints. All who have followed the numerous cases treated in this manner must agree that it is an immense advance on all previous methods of treatment of some forms of lupus and of bone tuberculosis.

After having shown you what an ernormous amount of accurate knowledge about the nature and causation, about the prevention and treatment of infectious diseases has been gained by the experimental method and by this alone, it will hardly be credited that a number of persons, as well-meaning as they are ill-instructed, are still maintaining the contrary; it is they who have succeeded in inducing Parliament to pass a law restricting, if not in some cases altogether prohibiting, the use of that method. This law is interfering with research in this country to a large extent, and has even put a stigma on those who are engaged in elucidating truths that are for the benefit of mankind, and of the animals themselves. What can be of greater benefit in the battle against diseases than the knowledge of their causes and the devising of means for their prevention and treatment?

Fortunately for progress in general, this country is the only one in which such restrictions disfigure the statute-book; other countries, more enlightened and able to recognise the value of researches of this kind, have wisely resisted the clamour for restrictions.

In connection with all this knowledge, of which I have only been able to give you an outline, I have heard it stated that "ignorance" (meaning the ignorance of former times) "is bliss" as compared with the present knowledge of the dangers surrounding us; but I have also heard it stated that the wise man, knowing and recognising the nature of these dangers, has the possibility given him of avoiding and preventing them, and no truer words have been spoken than these, that "he who cures a disease may be the skilfullest, but he that prevents it is the safest, physician."

My best thanks are due to my friend Mr. Andrew Pringle for the admirable photographs prepared by him of the microscopic slides illustrating the different pathogenic microbes exhibited, and to my friend and pupil Mr. Bousfield for the fine lantern slides of tube cultivations.

WEEKLY EVENING MEETING,

Friday, February 27, 1891.

Sir Frederick Abel, K.C.B. D.C.L. F.R.S. Vice-President, in the Chair.

Percy Fitzgerald, Esq. M.A. F.S.A.

The Art of Acting.

[No Abstract.]

GENERAL MONTHLY MEETING,

Monday, March 2, 1891.

William Crookes, Esq. F.R.S. Vice-President, in the Chair.

Miss Emily Aston, B.Sc.
Henry Daw Ellis, Esq. M.A.
Francis Fowke, Esq.
Augustus William Gadesden, Esq. J.P.
Miss Emily Dora Locock,
Signor Alberto Randegger,
The Hon. Frederick Hamilton Russell,
John Wilson Walter, Esq.

were elected Members of the Royal Institution.

The following Arrangements for the Lectures after Easter were announced:—

J. Scott Keltie, Esq. F.R.G.S.—Three Lectures on The Geography of Africa, with special reference to the Exploration, Commercial Development, and Political Partition of the Continent; on Tuesdays, April 7, 14, 21.

Edward E. Klein, M.D. F.R.S.—Three Lectures on Bacteria: their Nature and Functions (The Tyndall Lectures); on Tuesdays, April 28, May 5, 12.

William Archer, Esq.—Four Lectures on Four Stages of Stage History: 1. The Betterton Period; 2. The Cibber Period; 3. The Garrick Period; 4. The Kemble Period; on Tuesdays, May 19, 26, June 2, 9.

Professor Dewar, M.A. F.R.S. *M.R.I.*—Six Lectures on Recent Spectroscopic Investigations; on Thursdays, April 9, 16, 23, 30, May 7, 14.

A. C. Mackenzie, Esq. Mus. Doc.—Four Lectures on The Orchestra Considered in Connection with the Development of the Overture; on Thursdays, May 21, 28, June 4, 11.

Professor Silvanus P. Thompson, D.Sc. B.A. *M.R.I.*—Four Lectures on The Dynamo; on Saturdays, April 11, 18, 25, May 2.

H. Graham Harris, Esq. M. Inst. C.E. *M.R.I.*—Three Lectures on The Artificial Production of Cold; on Saturdays, May 9, 16, 23.

Professor A. H. Church, M.A. F.R.S. *M.R.I.*—Three Lectures on The Scientific Study of Decorative Colour; on Saturdays, May 30, June 6, 13.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The New Zealand Government—Statistics of the Colony of New Zealand for the year 1889. fol. 1890.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta: Rendiconti. 2° Semestre, Vol. VI. Fasc. 12. 8vo. 1890.
Agricultural Society of England, Royal—Journal, Third Series, Vol. I. Part 4. 8vo. 1890.
Astronomical Society, Royal—Monthly Notices, Vol. LI. No. 3. 8vo. 1891.
Bankers, Institute of—Journal, Vol. XII. Parts 1, 2. 8vo. 1891.
Bavarian Academy of Sciences—Sitzungsberichte, 1890, Heft 4. 8vo. 1891.
British Architects, Royal Institute of—Proceedings, 1890–1, Nos. 8, 9. 4to.
Buckton, G. B. Esq. F.R.S. M.R.I. (the Author)—Monograph of the British Cicadae, Part 5. 8vo. 1891.
Chemical Industry, Society of—Journal, Vol. X. No. 1. 8vo. 1891.
Chemical Society—Journal for February, 1891. 8vo.
Clark, Latimer, Esq. F.R.S. M.R.I. (the Author)—Dictionary of Metric and other useful Measures. 8vo. 1891.
Cracovie, l'Academie des Sciences—Bulletin, 1891, No. 1. 8vo.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I.—Journal of the Royal Microscopical Society, 1891, Part 1. 8vo.
Editors—American Journal of Science for February, 1891. 8vo.
Analyst for February, 1891. 8vo.
Athenæum for February, 1891. 4to.
Brewers' Journal for February, 1891. 4to.
Chemical News for February, 1891. 4to.
Chemist and Druggist for February, 1891. 8vo.
Electrical Engineer for February, 1891. fol.
Engineer for February, 1891. fol.
Engineering for February, 1891. fol.
Horological Journal for February, 1891. 8vo.
Industries for February, 1891. fol.
Iron for February, 1891. 4to.
Ironmongery for February, 1891. 4to.
Murray's Magazine for February, 1891. 8vo.
Nature for February, 1891. 4to.
Open Court for February, 1891. 4to.
Photographic News for February, 1891. 8vo.
Public Health for February, 1891. 8vo.
Revue Scientifique for February, 1891. 4to.
Telegraphic Journal for February, 1891. fol.
Zoophilist for February, 1891. 4to.
Florence Biblioteca Nazionale Centrale—Bolletino, Nos. 122, 123. 8vo. 1891.
Franklin Institute—Journal, No. 782. 8vo. 1891.
Geographical Society, Royal—Proceedings, New Series, Vol. XIII. No. 2. 8vo. 1891.
Geological Institute, Imperial, Vienna—Verhandlungen, 1890, Nos. 14–18; 1891, No. 1. 8vo.
Geological Society—Quarterly Journal, No. 185. 8vo. 1891.
Harlem, Société Hollandaise des Sciences—Archives Neerlandaises, Tome XXIV. Livraisons 4 and 5. 8vo. 1891.
Johns Hopkins University—University Circulars, No. 85. 4to. 1891.
Junior Engineering Society—Address, No. 9. 8vo. 1890.
M'Kendrick, J. G. M.D. LL.D. F.R.S. (the Author)—Chronological Tables of Scientific Men. 8vo. 1890.
Madras Government—Madras Meridian Circle Observations for 1868–1870. 4to. 1890.

Manchester Literary and Philosophical Society—Memoirs and Proceedings, Vol. IV. Nos. 1, 2. 8vo. 1890–91.
Mechanical Engineers' Institution—Proceedings, 1890, No. 4. 8vo.
Meteorological Office—Weekly Weather Reports, Vol. VIII. No. 3. 4to. 1891.
Meteorological Observations at Stations of Second Order. fol. 1890.
Meteorological Society, Royal—Meteorological Record, Vol. X. No. 38. 8vo. 1890.
Quarterly Journal, No. 77. 8vo. 1891.
Middlesex Hospital—Reports for 1889. 8vo. 1891.
Ministry of Public Works, Rome—Giornale del Genio Civile, 1890, Fasc. 12. And Designi. fol. 1890.
Mivart, J. St. George, M.D. F.R.S. M.R.I. (the Author)—Introduction générale à l'étude de la Nature. 8vo. 1891.
Norwegian North Atlantic Expedition, Editorial Committee—Sars, G. O. Pycnogonidea. fol. Christiania, 1891.
Numismatic Society—Chronicle and Journal, Part IV. 8vo. 1890.
Odontological Society of Great Britain—Transactions, Vol. XXIII. No. 3. New Series. 8vo. 1891.
Pharmaceutical Society of Great Britain—Journal, February, 1891. 8vo.
Calendar, 1891. 8vo.
Photographic Society—Journal, Vol. XV. No. 4. 8vo. 1891.
Rathbone, E. P. Esq (the Editor)—The Witwatersrand Mining and Metallurgical Review, No. 12. 8vo. 1890.
Rio de Janeiro, Observatoire Imperial de—Revista, No. 1. 8vo. 1891.
Royal College of Physicians, Edinburgh—Reports from the Laboratory, Vol. III. 8vo. 1891.
Royal Irish Academy—Proceedings, Third Series, Vol. I. No. 4. 8vo. 1891.
Transactions, Vol. XXIX. Part 14. 4to. 1891.
Royal Observatory, Edinburgh—Catalogue of Crawford Library. 4to. 1890.
Royal Society of London—Philosophical Transactions, Vol. CLXXXI. 4to. 1891.
Saxon Society of Sciences, Royal—Philologisch-historischen Classe:
Abhandlung. Band XII. No. 2. 8vo. 1891.
Selborne Society—Nature Notes, Vol. II. No. 14. 8vo. 1891.
Society of Architects—Proceedings, Vol. III. Nos. 5, 6. 8vo. 1891.
Society of Arts—Journal for February, 1891. 8vo.
St. Bartholomew's Hospital—Reports, Vol. XXVI. 8vo. 1890.
Statistical Society, Royal—Journal, Vol. LIII. Part 4. 8vo. 1890.
United Service Institution, Royal—Journal, No. 156. 8vo. 1891.
United States Geological Survey—Ninth Annual Report, 1887–88. 4to. 1889.
Mineral Resources of the United States. 8vo. 1890.
Monographs, Vol. I. 4to. 1890.
Bulletins, Nos. 58–61, 63, 64, 66. 8vo. 1890.
Vereins zur Beförderung des Gewerbfleises in Preussen—Verhandlungen, 1891: Heft 1. 4to.

WEEKLY EVENING MEETING,

Friday, March 6, 1891.

William Crookes, Esq. F.R.S. Vice-President, in the Chair.

Professor J. A. Fleming, M.A. D.Sc. *M.R.I.*

Electro-magnetic Repulsion.

§ 1. On the 2nd day of October, 1820, Ampère presented to the Royal Academy of Sciences in Paris an important memoir, in which he summed up the results of his own and Arago's previous investigations in the new science of electro-magnetism, and crowned that labour by the announcement of his great discovery of the dynamical action between conductors conveying electric currents.* Respecting that achievement, when developed in its experimental and mathematical completeness, no less a writer than Clerk Maxwell calls it "one of the most brilliant in the history of physical science." Our wonder at what was then accomplished is increased when we remember that hardly more than two months before that date John Christian Oersted had startled the scientific world by the announcement of the discovery of the magnetic qualities of the space near a current-traversed conductor. Oersted called the actions going on around the conductor the "electric conflict," and in his first paper,† in describing the newly-observed facts, he says:—"It is sufficiently evident that the *electric conflict* is not confined to the conductor, but is dispersed pretty widely in the circumjacent space." "We may likewise collect," he adds, "that this conflict performs circles round the wire, for without this condition it seems impossible that one part of the wire when placed below the magnetic needle should drive its pole to the east, and when placed above it to the west." These few words are taken from the original paper which stimulated the philosophic thought of Ampère and his contemporaries, and started into existence a wave of discovery, placing us in possession of the facts which form our starting-point to-night.

§ 2. It will be unnecessary to spend more than a moment or two

---

* 'Mémoire présenté à l'Académie Royale des Sciences le 2 octobre 1820, où se trouve compris le résumé de ce qui avait été lu à la même Académie les 18me et 25me septembre 1820, sur les effets des courants électriques,' par M. Ampère. See vol. xv. 'Annales de Chimie,' 1820.

† In the 'Annals of Philosophy' for October 1820, vol. xvi. p. 274, is to be found an English translation of Oersted's original Latin essay, dated July 21, 1820, describing his immortal discovery. This paper is entitled "Experiments on the Effect of a Current of Electricity on the Magnetic Needle," by John Christian Oersted, Knight of the Order of Danneborg, Professor of Natural Philosophy in Copenhagen.

in illustrating the now familiar interactions of two circuits traversed by currents in the same or opposite directions. Holding a circular coil traversed by a continuous electric current near to a similar circuit free to move, we are well aware that when the circuits are parallel to each other there is an attractive force between them if the currents in adjacent parts of the circuits flow in the same direction, and a repulsive effect if they flow in the opposite. This is the electro-dynamic action discovered by Ampère and utilised in the construction of instruments for the measurement of electric currents in practical work. If one such conducting coil, such as that of the electro-magnet now before me, is traversed by an alternating current, and the other is simply a closed circuit or coil placed a little distance off, but in its field, I feel sure I am free to assume that all here are aware of the effect which will be produced. The closed circuit becomes the seat of an alternating induced current, which, if our inducing current is sufficiently powerful, can be made to render itself evident by illuminating a small incandescent lamp placed in the secondary circuit.* Notice, however, that in performing the experiment the secondary circuit must be so placed that the magnetic lines of force of the primary coil perforate through the secondary circuit. If the secondary circuit is held in such a position that the reversal of direction of the primary current causes no reversal of direction of the magnetic field traversing the secondary circuit, because it is not linked with any of the lines of force of the primary, the secondary circuit is no longer the seat of any induced current.

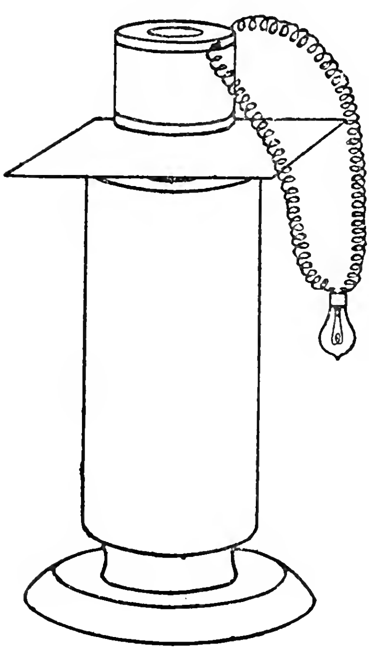
Note also that this electro-magnetic induction, whilst taking place across space, is not interfered with by the interposition of a non-conducting screen. The magnetic induction passes freely through an inch board or a plate of glass, but if we interpose a thick copper sheet (Fig. 1), we cut off the secondary current or screen the secondary circuit from the inductive action of the primary. The secondary coil is, in fact, an electro-magnetic eye which can *see through* an inch of wood, but to which a sheet of zinc is semi-transparent, and a thick sheet of copper quite opaque, as far as the electro-magnetic radiation of the wave-length employed is concerned. The rapid heating of this copper screen makes us aware that the secondary currents are now being induced in the copper sheet in the form of

* The experiments here referred to were mostly performed with an alternating-current magnet, having a core of divided iron about 3 inches in diameter and 12 inches long, excited by a Siemens alternating-current dynamo, kindly lent for the occasion by Messrs. Siemens Bros., giving a current at an electromotive force of rather less than 200 volts. A small shelf around the core a little above the middle serves as a support for rings, &c., to be projected. The performance of these experiments on a scale suitable for large audiences requires from 10 to 15 horse-power at least, and can hardly be shown well unless the alternator can provide a current of 100 amperes at 100 volts, available at the moment of maximum demand. The magnet of course takes very little actual current until the metal plate or ring is held near it, when the impedance is immediately reduced.

eddy currents, and it screens the secondary circuit because the inductive action of these eddy currents on the side remote from the magnet is exactly equal and opposite to that of the primary circuit on the secondary coil.

§ 3. Connecting together these elementary facts, we are easily able to explain another well-known fact. If a continuous current is sent through the coils of a large electro-magnet, and we magnetise its iron core very powerfully, it is found to be impossible to strike the pole of the magnet a sharp blow by means of a sheet of copper. Holding a sheet of copper over such a magnetic pole, and energising the magnet, the hand holding the copper sheet feels a repulsive action at the moment when the current is put on and an attractive action when it is cut off. If we try to slap the magnet sharply with the copper sheet, it is found that this repulsive force prevents anything like such a sharp blow being given to the pole when the current is on, as can be given when the current is off. Moreover, when a very powerful electro-magnet is employed, it is found that a disc of copper let fall over the pole does not fall down sharply and quickly on to it when the current is flowing through the coils of the magnet, but settles down softly and slowly, as if falling through some sticky fluid. We know that the correct explanation of these facts is to be found in the statement that the motion of the conductor towards the magnetic pole causes eddy electric currents to be generated in it by electromagnetic induction, and that these being in the opposite direction to the energising current of the magnet, cause a repulsive force to exist between the inducing and secondary circuits, which creates the apparent resistance we feel.

Fig. 1.



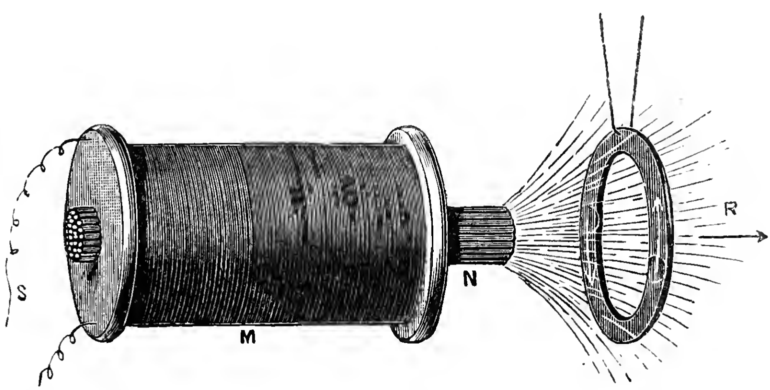
Copper plate interposed between a primary and a secondary coil and shielding the secondary from induction.

In order to exhibit the stress brought into existence between an electro-magnet and a metal sheet held near it when induced currents are set up in the disc, we have arranged the following experiment:—Over the pole of a powerful electro-magnet we have balanced a small disc of copper, the size of a penny, carried on one end of a delicately balanced bar. A mirror attached to that bar serves to reflect on to the screen a ray of light indicating the smallest motion of the copper piece. On magnetising the magnet the copper is suddenly repelled, but comes to rest again immediately in its initial position.

When the magnet is demagnetised the copper experiences a momentary attraction. These attractions and repulsions are obviously due to the Ampèrian stress set up between the magnet and the metal by reason of the induced currents set up in the latter. Impulsive effects of this nature have been particularly studied by Mr. Boys.*

§ 4. Let me, in the next place, ask you to contemplate a circuit, say a closed conducting ring, suspended in front of the pole of an electro-magnet, and that the coils of the electro-magnet are traversed by an alternating current of electricity (Fig. 2). The magnetic field of the magnet is then an alternating field. We shall suppose it to vary in strength, according to a simple periodic law. The closed circuit is therefore subjected to an inductive action, and we know that the induced electromotive force in that circuit is at any instant propor-

FIG. 2.



Copper ring hung in the field of an electro-magnet, and repelled or attracted when the current is put on or cut off.

tional to the rate of change of the magnetic field in which it is immersed. If, therefore, the variation in strength of that field is represented geometrically by the ordinates of a periodic curve, the varying electromotive force acting in the ring circuit is represented by the ordinates of another such curve of equal wave length, shifted a quarter of a wave length behind the first. In the diagram before you the variation of the induced magnetic field, and the induced electromotive force in the circuit, are represented, as usual, by two harmonic curves. This induced electromotive force creates an induced current flowing backwards and forwards in the ring, and we shall, in the first instance, suppose that this current flows in exact synchronism with its electromotive force. The induced current and the inducing magnetic field may therefore be represented as to relative phase and strength by the curves in the diagram (Fig. 3). The dynamical action,

* See Proc. Phys. Soc. of London, vol. vi. p. 218, "A Magneto-electric Phenomenon."

or the force which the ring experiences, is at any instant proportional to the product of the strength of the magnetic field in which the ring is immersed, and to the strength of the induced current created in it. If we multiply together the numerical values of the ordinates of these two curves at any and every point on the horizontal line, and set up a new ordinate at that point representing this product, the extremities of these last ordinates define a curve, which is a curve representing the *force* acting on the secondary circuit, and it is, as you see from the diagram, Fig. 3, a wavy curve having a wave length equal to half that of the first two curves. Moreover, the whole area inclosed between the outline of this force curve and the horizontal line represents to a certain scale the time integral of that force, or

Fig. 3.

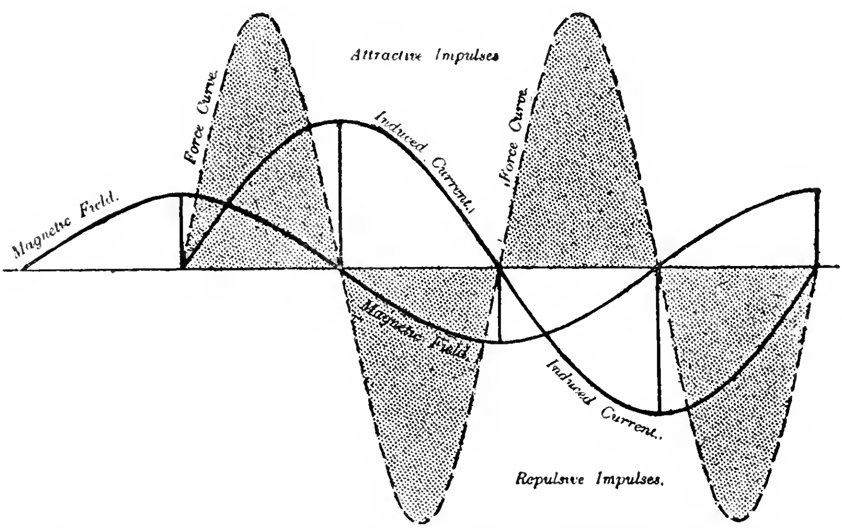


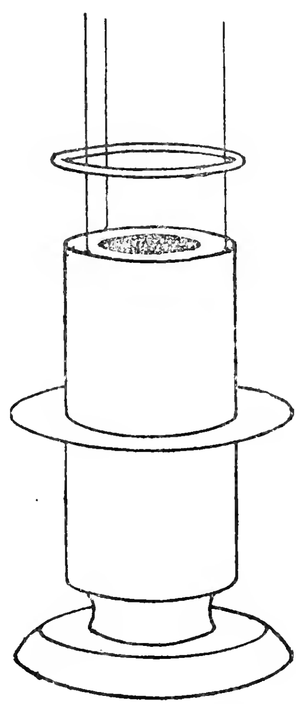
Diagram showing the equality of the attractive and repulsive impulses in a non-inductive circuit when held in an alternating magnetic field.

the *impulse* acting on the secondary circuit, and the theory shows us that, under the assumptions made, the secondary circuit so acted upon experiences in each period of the current four impulses, two positive or repulsive, and two negative or attractive. Hence, it comes to this, that such an ideal conducting circuit held in front of an alternating electro-magnet should experience a rapid alternate series of equal pushes and pulls, or of little impulses to and from the magnet. These equal and opposite impulses in quick succession would neutralise one another, and our supposed circuit would not, on the whole, be subject to any resultant force.

§ 5. Quite otherwise is it, however, when we present a real conducting circuit to the pole of an electro-magnet traversed by a powerful alternating current. We find that under the actual circumstances there is a powerful repulsive action between the pole and the circuit. The exact nature of the electro-magnetic repulsion it is our business now to explore.

Taking in my hands a copper ring, I hold it over the pole of this powerful vertical alternating electro-magnet, and find at once that there is a perceptible and strong repulsion. Letting the ring go, it jumps up into the air, impelled so to do by the electro-magnetic repulsion acting upon it (Fig. 4). All good conducting rings will execute this gymnastic feat, and rings of copper and aluminium are found to be most nimble of all. Rings of zinc and brass are sluggish, and a ring of lead will not jump at all. We owe the discovery of this striking effect to Prof. Elihu Thomson, and he has explored in all directions the consequences and nature of this interesting effect. Why is it that our real copper and metal rings behave so differently, when immersed in an alternating field, to ideal rings of conducting matter? The explanation is not very difficult to find. The real ring possesses a quality, called its inductance, of which we took no account in our examination of the case a moment ago.

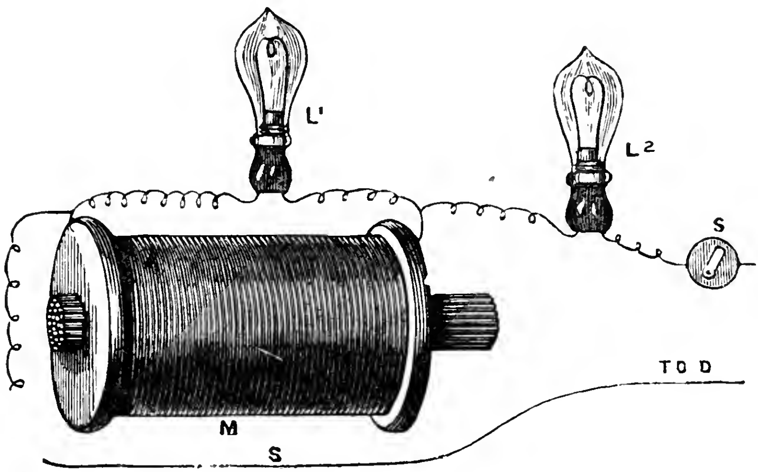
FIG. 4.



Aluminium ring projecting from the pole of an alternating electro-magnet, and floating over the pole when restrained by three strings.

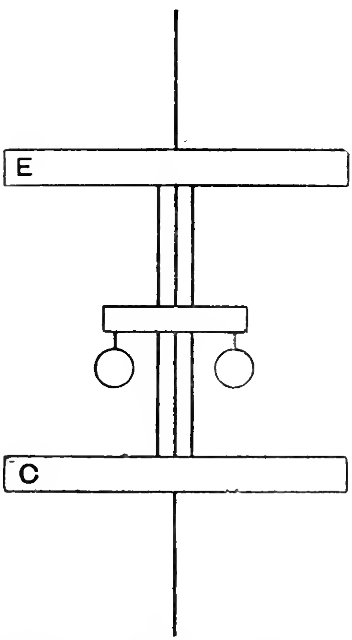
§ 6. Before me lies a very large bobbin of wire (Fig. 5), and the ends of this coil are connected to an incandescent lamp. We send a current of electricity through the bobbin and the lamp, and have arranged matters as in the diagram before you, so that the current divides through the two circuits of lamp and bobbin. Under these circumstances, the divided current is just sufficient to illuminate faintly the lamp. I break the circuit of the battery, and you see that the lamp flashes up for a moment, and we are well aware that this effect is due to the electro-magnetic momentum of the coil, or to its inductance, in virtue of which the current continues to flow on in the bobbin for a short period after the impressed electromotive force is withdrawn. Also, we know that there is a small but definite time required before the current practically reaches its full strength in the bobbin when the electromotive force is again applied. These effects are the self-inductive effects of a coil first noticed by Prof. Joseph Henry in 1832, and subsequently fully examined in the ninth series of Faraday's 'Experimental Researches in Electricity.' Every circuit of any kind—disc, ring, or coil of wire—possesses a certain degree of this quality of self-induction, and, as a natural consequence it follows therefrom that if that circuit is subjected to a periodically varying electromotive force acting upon it, the current produced in it will always be in arrear of the electromotive force in phase. The current

FIG. 5.



Incandescent lamp and coil arranged to exhibit the effect of the inductance of the coil.

FIG. 6.



Mechanical model illustrating the "lag" of the current in an inductive circuit.

induced lags in phase behind the inducing electromotive force. This "lag" of the current behind the impressed harmonically varying electromotive force in consequence of self-induction may be illustrated mechanically thus:—Three light wooden laths are connected together by a flat steel band, and the system hung up by a string like a pair of astatic magnetic needles (Fig. 6). If we take hold of the upper rod and move it backwards and forwards on a horizontal plane, compelling it to execute harmonic oscillations, the upper and lower rods move together synchronously. Let the upper rod symbolise impressed electromotive force, and the lower rod resulting current. Under present circumstances, these two move always in step with one another. Next load down the middle rod or connecting mechanism by means of two lead weights, and give it *inertia*. On again taking hold of the upper rod and causing it to

FIG. 7.

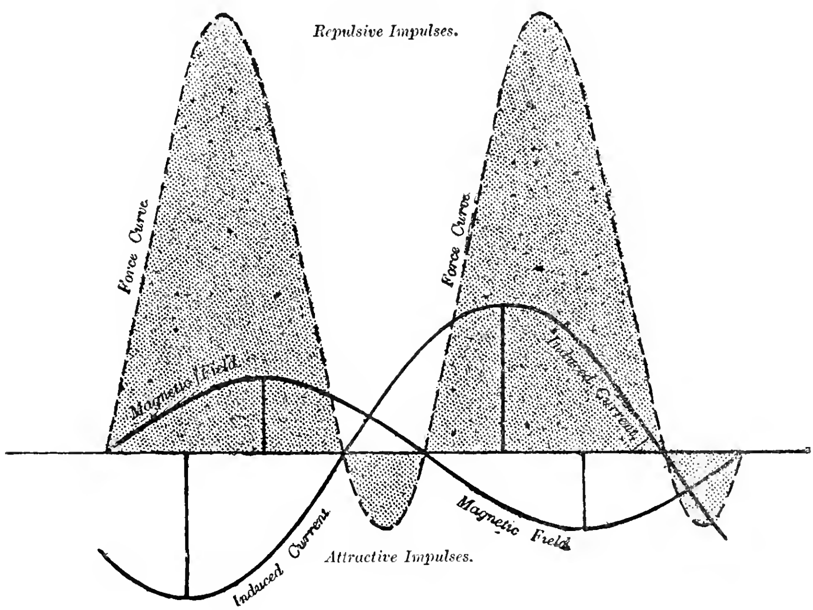


Diagram showing the inequality of the attractive and repulsive impulses in the case of an inductive circuit when held in an alternating magnetic field.

execute forced harmonic oscillations, the lower rod no longer vibrates in tune with it. It lags in phase behind the top one, and we thus illustrate, though not perhaps by a perfect mechanical analogy, the effect of electric inertia in the intermediate mechanism in causing a lag of current behind impressed electromotive force.

§ 7. Returning for an instant to the diagram we considered just now, we must correct it to make it fit in with the facts of nature, and we must represent the periodic curve which stands for the fluctuations of the induced current in the ring as shifted backwards or lagging behind the curve which represents the electromotive force in the circuit brought into existence by the fluctuating magnetic field. Making this change in our diagram (Fig. 7), and forming as before a force curve to represent the impulses on the ring, we now find that owing to the "lag" of the secondary current, one set of the impulses, namely, the positive or repulsive impulses, have been enlarged at the expense of the negative or attractive impulses. Theory, therefore, points out that as a consequence of the self-induction of the ring the balance between the attractive impulses and repulsive impulses is upset, and that the latter predominate. Our real ring behaves, therefore, very differently to our ideal ring. The real ring is strongly repelled, because the resultant action of all the impulses is to produce on the whole an electro-magnetic repulsion. This repulsion is evidence of the self-induction of the circuit exposed to the magnetic field, and it forms a new way of detecting it. But although this is part of the truth, it is not the whole truth. The lag of the induced current in the ring, and hence the predominance of the repulsive impulses, depends on the conductivity of the material of which the ring or circuit is made; and the better this conductivity the greater is that repulsion, because both the induced current and the "lag" are thereby increased. Hence it comes to pass that there are two factors involved in making this repulsive effect, the conductance of the ring or disc and its inductance. For equal conductivities, the greater the self-induction, the greater the repulsion. For equal self-inductions, the greater the conductivity of the circuit so much the more repulsive effect will be produced. Time does not permit me to enlarge on the strict analysis of the effect. Its broad outlines are indicated, perhaps, sufficiently, by what has been said.

§ 8. We can show the effect of the relative conductivity of discs of equal size, and therefore of equal self-induction, by *weighing* similar discs of various metals over an alternating pole. Here, for instance, are discs of copper, zinc, and brass of equal form and size. Placing these discs on the scale pan of a balance, and suspending them over the alternating pole, I am able to prove that the repulsion on the copper disc is greater than that on the zinc, and that on the zinc greater than that on the brass.*

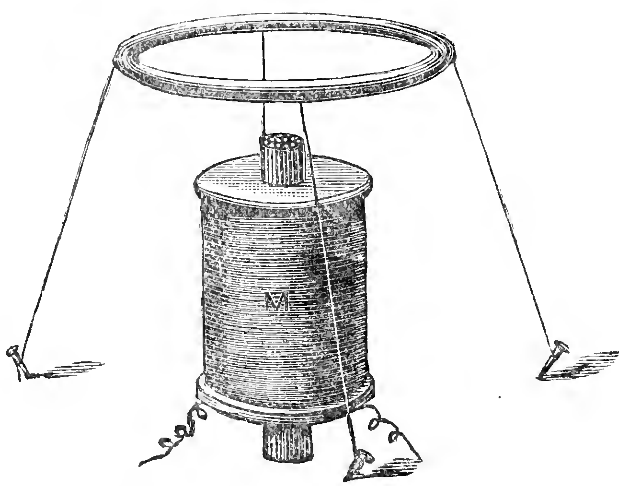
The same result can be illustrated by placing over the pole of our alternating magnet a paper tube. Taking one of the copper rings in my hand, and first exciting the magnet, I let the ring drop down the tube. It falls as if on an invisible cushion that buoys it up, and it remains floating in the air. If rings of different metals and equal size are placed on the tube, they float at different levels like various specific-gravity beads in a liquid. The greater the conductivity of the ring, the greater is the repulsion on it, in any given part of the alternating field, and hence the highly conducting rings will be sustained on a weaker field than the feebler conducting rings, assuming the rings to have about equal weights. Moreover, we are able to show by another experiment the fact that these rings are traversed, when so held, by powerful electric currents. If we press

* Experiments of this kind have been made by M. Borgman. See 'Comptes Rendus,' No. 16, April 21, 1890, p. 849; and also February 3, 1890, vol. cx. p. 233.

down the copper ring upon the zinc or brass ring floating beneath it, the rings are attracted together and the copper ring holds up the zinc. This is obviously because the rings are all traversed by induced currents circulating in the same direction.

§ 9. It is, of course, an immediately obvious corollary, from all that has just been said, that any cutting of a ring or disc which hinders the flow of the induced currents causes the whole of the repulsion effects to vanish. We illustrate this by causing a ring of copper wire to jump off the pole, and then cutting it with pliers, find it has ceased to be capable of giving signs of life. When the metallic masses or circuits which are presented to the alternating magnetic pole are of very low resistance, the electro-magnetic repulsion may become very powerful, many pounds of thrust or push being produced

Fig. 8.



Copper ring "floating" in air over the pole of an alternating-current electro-magnet, when restrained by strings.

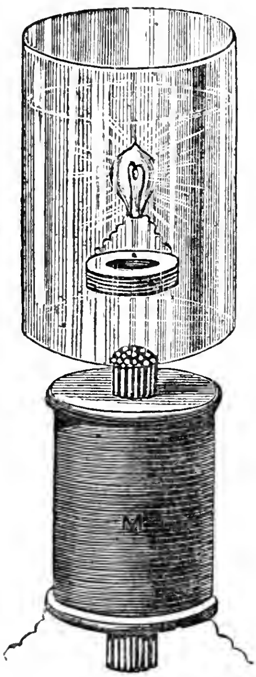
by apparatus of quite moderate size. It is, in fact, quite startling to hold over the pole of such an alternating magnet as I have before me a very thick plate of high conductivity copper. It would greatly surprise any one not aquainted with these principles to be told that a massive copper ring weighing eight or ten pounds could be made to float in the air, but you have ocular demonstration before you that this feat can be performed. The ring needs to be tethered by light strings, to prevent it from being thrown off laterally, although these strings in no way support its weight (Fig. 8).

One of the most beautiful of Prof. Elihu Thomson's experiments exhibits this effect of electro-magnetic repulsion on a closed coil, which is buoyed up in water by a small incandescent lamp in circuit therewith. In the glass vase before you floats a little glow-lamp like a balloon (Fig. 9). The car consists of a coil of insulated wire, and the ends of this coil are connected with the lamp. The whole arrangement is accurately adjusted to just, or only just, float in water.

Placing the vase over an alternating magnetic pole, you see that the magnetic induction creates a current in the coil which lights the lamp, and, moreover, that the electro-magnetic repulsion on the coil causes the lamp and coil to rise upward in the water.

§ 10. We must now pass on to study shortly another class of actions, namely, deflections and rotations produced by electro-magnetic repulsion on highly conducting discs or rings. If the conducting ring or disc which is presented to the alternating pole is constrained by being fixed to an axis around which it can rotate, the action may reduce to a deflective force. Here, for instance, is a flat disc fixed on a transverse axis. On presenting this disc to the pole, the disc is prevented by its constraint from being repelled bodily; so it does the next best thing it can, it sets its plane parallel to the lines of magnetic force, and gets into such a position that the induced currents in it are reduced to a minimum. On this principle, before becoming acquainted with Prof. Elihu Thomson's original work, I devised a little copper disc galvanometer for detecting small alternating currents.

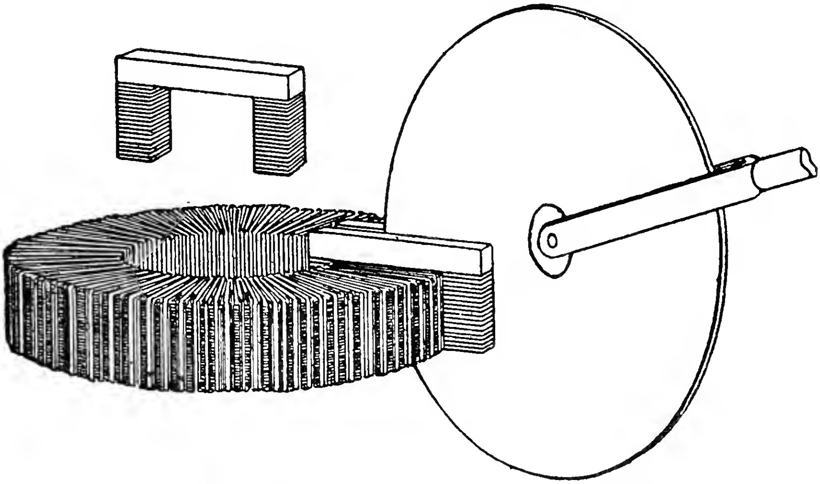
Fig. 9.



Incandescent lamp and secondary coil floating in water and repelled by an alternating-current electro-magnet, placed beneath.

§ 11. More interesting than the deflective actions are those which result in the production of continuous rotation in highly conducting bodies placed in an alternating field. This electro-magnet in front of me, and which has come from Prof. Elihu Thomson for the purposes of this lecture, consists, as you see, of a nearly closed circuit divided iron core wound over with a coil (Fig. 10). The ends of the iron circuit are provided with copper bars, which embrace and cover portions of the polar terminations of the magnet. When the magnet is excited by a periodic current, these secondary circuits become the seat of powerful induced secondary currents. Taking in hand a large copper disc pivoted at the centre and held in a fork, we hold this wheel so that part of the disc is inserted between the jaws of the electro-magnet. Immediately, rapid rotation is produced. The reason is not far to seek. The alternating field induces, both in the closed coils and in the neighbouring portions of the disc, induced currents; these tend to cause the parts of the conductors in which they flow to be pulled into parallelism, and if the polar coils are so placed as to partly shield the poles these attractive actions act unsymmetrically on the disc and pull it continuously round. The action is, perhaps, better illustrated by a simpler experiment. If we hold a pivoted copper disc (Fig. 11) symmetrically over an alternating pole, the action of the pole is one of pure repulsion on the disc, which, however, causes no rotation in it.

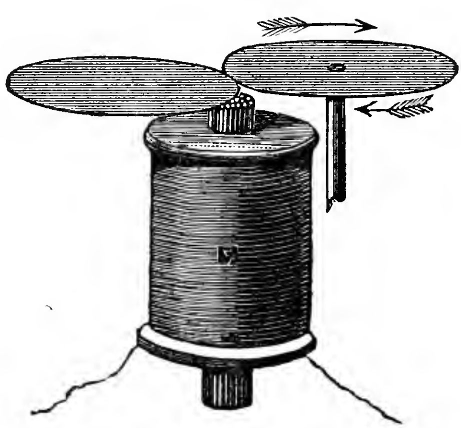
Fig. 10.



Electro-magnet with shaded poles causing a copper disc placed between the jaws to revolve.

When a copper sheet is so placed as to shield or "shade," as Prof. Thomson calls it, part of the magnetic pole, currents are induced both in the fixed plate and in the movable one. The fixed disc shields part of the other from the induction of the pole, and hence causes the induced currents in that plate and disc to be so located that they are in positions to cause continual attraction between one another and continuously pull round the movable disc into fresh positions, so creating regular rotation. This principle of "shading" a pole is employed in constructing the polar coils of the magnet used in our experiment a moment ago, and the experiments present us with a form of self-starting alternating-current motor, although not perhaps a very efficient one in the technical sense. This principle of "shading" a portion of a conductor from the inductive action of the pole, and so causing the eddy currents in it to be located in a portion of its surface and to

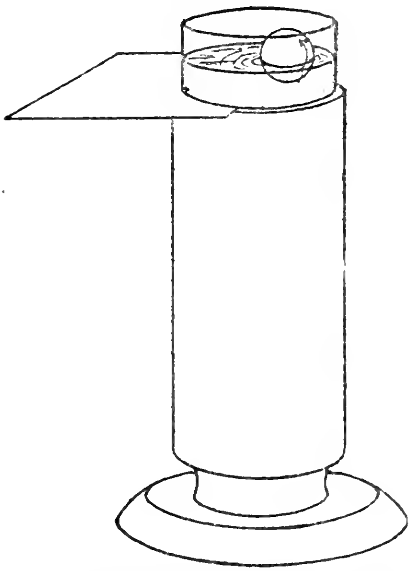
Fig. 11.



Revolution of a shaded copper plate held over an alternate-current magnetic pole.

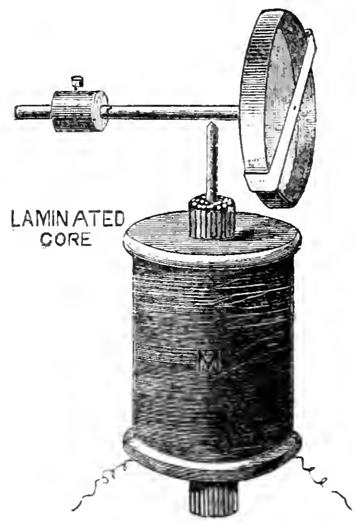
cause attraction between that conductor and the shading conductor is capable of being exhibited in various ways. We place on this copper plate a light hollow copper ball, and support it in a little depression in a copper plate. Holding the arrangement over the alternating magnet, the ball begins to spin round rapidly when the magnet is excited. This rotation is caused by the continual attraction of the eddy currents induced in the fixed plate and in that part of the ball which is not shielded from the pole by the plate. We may vary the experiment, and exhibit many more or less curious and amusing illustrations of it. If we float these copper balls in water (Fig. 12),

Fig. 12.



Hollow copper ball floating in water over an alternate current electro-magnet, and caused to revolve by the interposition of a "shading" plate.

Fig. 13.



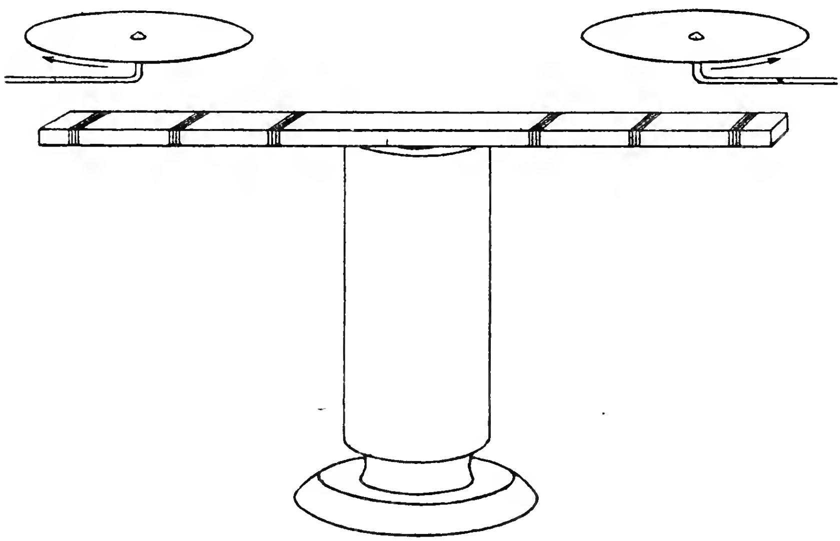
Electro-magnetic gyroscope revolving over the pole of an alternate-current electro-magnet.

and place the glass bowl containing them over the alternating pole, the interposition of a copper sheet between the pole and the balls causes the latter to begin to spin in a highly energetic manner.

§ 12. Prof. Elihu Thomson has invented a novel form of electro-magnetic gyroscope (Fig. 13). We have now suspended over the alternating magnet a gyroscope of the usual form. The wheel of the gyroscope is made of iron, and the tyre of the wheel is a thick copper band. Immediately the magnet is energised, the gyroscope begins to rotate with great rapidity over the pole. In this case the unsymmetrical disposition of the eddy currents in the copper band around the wheel is sufficient by itself to cause the rotation to occur. The phenomenon which, however, lies at the bottom of all these effects is that the self-induction of the secondary circuit causes the eddy currents to be delayed in phase behind the magnetising field, and hence to persist into the period of reversal of that field, and so produce the repulsion between the primary conducting circuit and that part of the secondary conducting circuit in which the eddy currents are set up.

One more experiment in this part of the subject, before we pass on to some other developments of it, shall be placed under your notice. Returning to the use of the electro-magnet, in which the iron circuit is all but complete, we find that when a highly-conducting disc is put between the closely approximated half-shielded jaws of this electro-magnet, and an alternating current employed to excite it, the conducting disc is held up in the air-gap

FIG. 14.

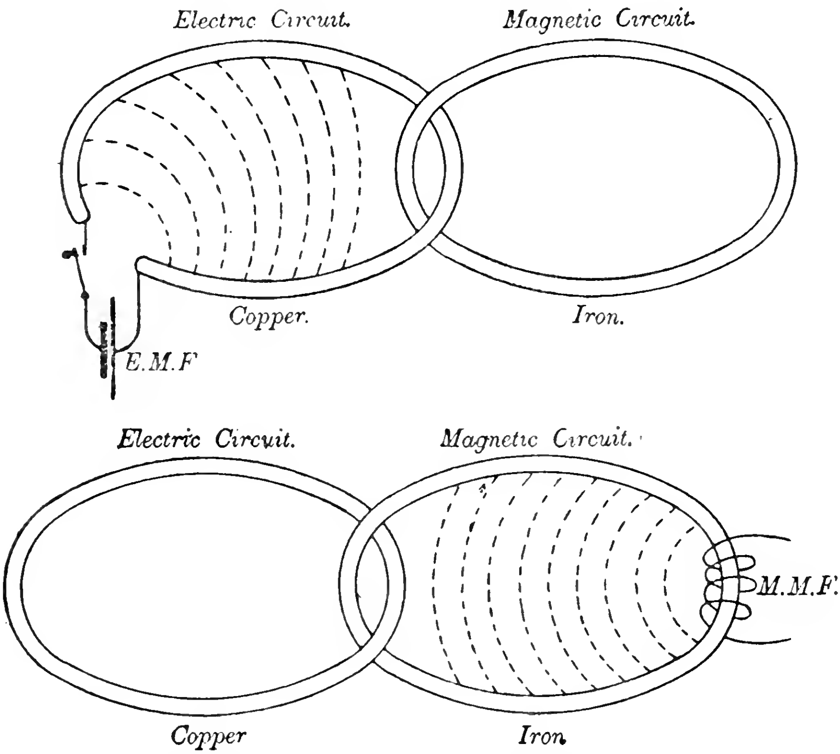


Alternating magnetised iron bar causing revolution of two iron discs held near its extremities.

by reason of the electro-magnetic attraction set up between the disc and the shielding polar plates. If, however, the disc has a relatively poor conductivity, the attraction is not nearly so marked. A good or bad silver coin can be discriminated thereby, because the good silver coin has conductivity enough to be the seat of powerful induced currents, but the bad coin has not.

§ 13. Closely akin to the foregoing, but rather less easy to explain, are the rotations in copper and iron discs which can be caused by the approximation to them of a laminated iron bar alternately magnetised. These actions have been carefully studied by Prof. Elihu Thomson, and applied by him and others in many practical devices. Across the top of this electro-magnet we place a long bar of laminated iron with the plane of the lamination vertical (Fig. 14). This bar is throttled at intervals by copper bands, which form small closed secondary circuits upon it. We excite the magnet, and hold near the bar an iron disc capable of free rotation; it begins to rotate rapidly, as you now see. Not only can this be done with a laminated bar throttled by conducting circuits, but even a solid bar of hard steel will serve the same purpose, and a couple of steel files placed across the poles can cause rapid rotation in pivoted discs of copper or of iron held with their edges close to the bars so alternately magnetised. To elucidate this remarkable action, we must revert for a moment to some fundamental facts. Here are two paper rings interlinked, one of red, the other of blue paper (Fig. 15). Let the red

FIG. 15



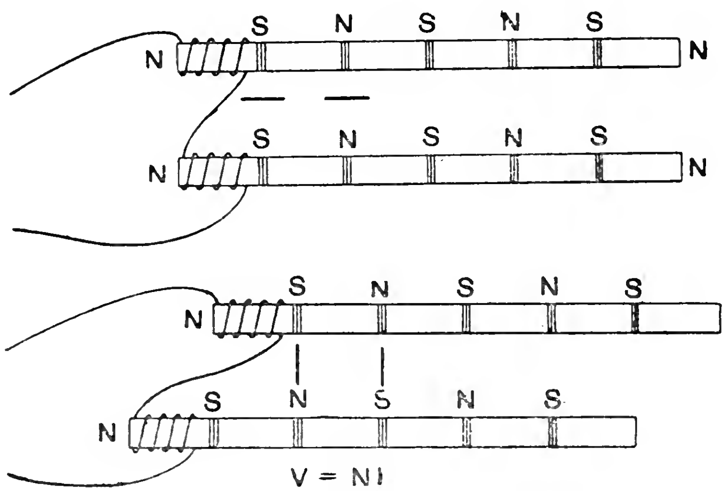
Diagrams illustrating the symmetry in relation between electromotive force and electric current, and magnetomotive force and magnetic induction.

ring stand as a symbol for a copper or conductive circuit. Let the blue ring stand for an iron or magnetic circuit. If we introduce into the conductive circuit an impulsive or alternating *electromotive force*, we are well aware that the interlinked iron circuit, by increasing the self-induction of the conductive circuit, hinders the change of current strength in it by introducing a *back electromotive force* of self-induction. Consider now the iron circuit. If we introduce into that magnetic circuit an impulsive or alternating *magnetomotive force* by interlinking it with some turns of a magnetising current, the effect of the copper or conductive circuit, which is linked with the iron or magnetic current, is similarly to introduce a *back magnetomotive force* into the magnetic circuit by reason of the magnetic field set up by the secondary current generated in that copper or conducting circuit. In other words, the secondary current induced in the copper circuit by any change in the magnetomotive in the iron circuit is in such a direction that it operates to oppose that primary magnetomotive force, chiefly, however, at the spot where the copper circuit passes round the iron. The general result may be stated to be that the action of the interlinked copper circuit is to cause the magnetic induction in the iron circuit to leak across through the air and partly to escape, passing through the secondary circuit. This escape of induction is called magnetic leakage, and the induced current set up in the closed secondary circuit is the cause of this magnetic leakage. There is a symmetry in the relations of magnetomotive force and the magnetic induction and electromotive force and electric current, and we can, as Faraday pointed out long ago, make the symmetry complete, if we suppose the two interlinked magnetic and electric circuits immersed in an imperfectly conducting medium. If, then, we throttle a magnetic circuit, such as a laminated iron bar with copper coils closed upon themselves, and place a magnetising coil at one end, the closed conducting circuits hinder the rise of magnetic induction in the bar; in other words, they give it what may be called *magnetic self-induction.* If the source of magnetism is a rapidly-reversed pole, the consequences of this delay or "lag" in the induction is that a series of alternating magnetic poles are always travelling with retarded speed up the bar, and these may be considered to be represented by tufts of lines of magnetic force which spring out from and move laterally up the bar. If the bar is not laminated and not throttled, the eddy currents set up in the mass of the bar itself act in the same way, and operate to resist the rise of induction in the bar and to delay the propagation of magnetism along it. Hence we must think of such a throttled bar, when embraced by a magnetising coil at one end, as surrounded by laterally moving bunches of lines of magnetic force, which move up the bar. Each reversal of current in the magnetising coil calls into existence a fresh magnetic pole at the one end of the bar, which is, as it were, pushed along the bar to make room for the pole of opposite name, which appears the next instant behind it. When an iron disc is held near such a laminated und throttled bar, these laterally moving lines of force induce poles in the disc which travel after the inducing poles, and hence the disc is continually pulled round. If the disc is a copper disc, the laterally moving lines of magnetic force induce eddy currents in the disc, and these, by the principle already explained, create a repulsion between the pole and the part of the disc in which the eddy currents are set up.

§ 14. The progression of alternate poles along a bar can be investigated by means of an experiment due to Mr. A. Wright.

Two laminated straight iron bars (Fig. 16) are throttled at intervals with secondary circuits, and have wound on one extremity a magnetising coil. The two bars are placed near each other and parallel. The coils are so connected that the poles at any instant in the ends of the two bars are of similar name. An alternate current is sent through the coils joined in series. Under these circumstances a series of alternate poles of similar names run up the bar parallel with one another. A small, soft iron needle hung at any place between the bars sets itself parallel to the bars, because at any instant poles of similar names are abreast of one another at any spot in the length of the bars. If, however, we shift one bar lengthways backwards or forwards through a certain distance, so as to bring opposite

Fig. 16.

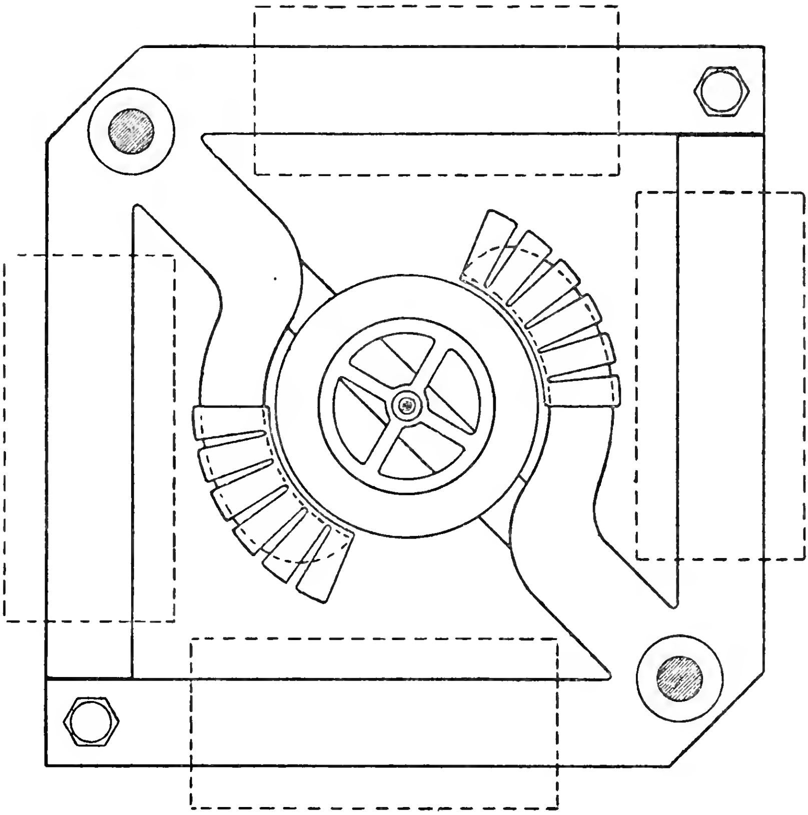


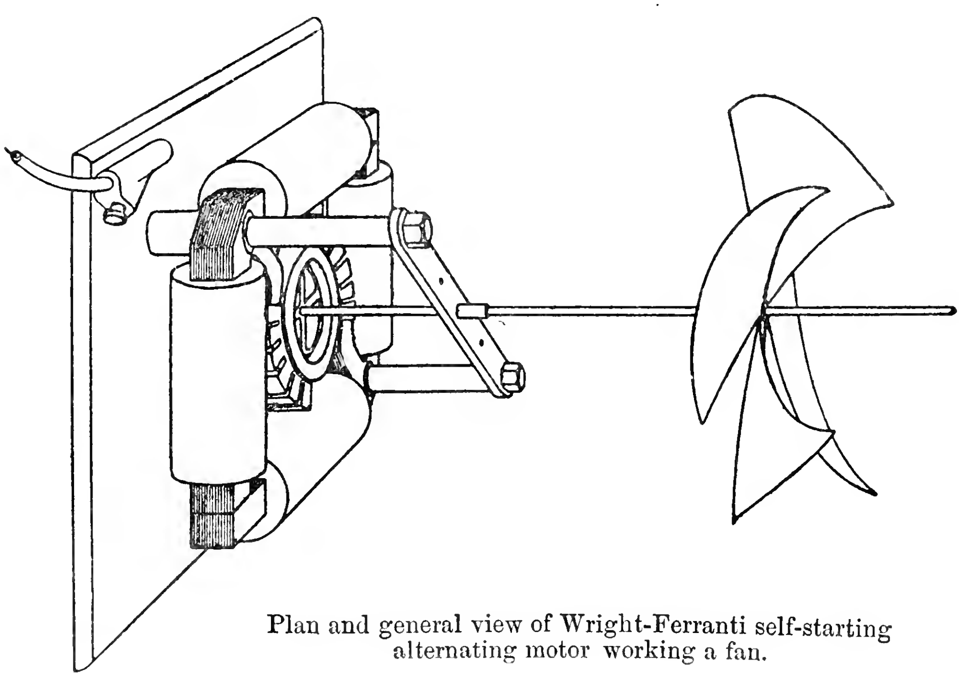
Mr. Wright's experiments with throttled and alternately magnetised bars.

poles abreast of each other throughout their journey up the bars, we shall find a position such that the soft iron needle will set at right angles to the bars when hung at any point in the space between them. The distance by which we have to shift the one bar backwards of the other to effect the change is evidently half a magnetic wave length, and knowing the frequency of the alternations we can readily arrive at a measure of the velocity of propagation of these alternate poles in the bar. This velocity is evidently numerically equal to the product of the frequency and wave length so obtained.

§ 15. A very pretty application of the above principle has been made in the electric meter of Messrs. Wright and Ferranti for measuring alternating currents. Before me stands one of these meters. It consists of a pair of vertical electro-magnets, with laminated iron cores, and each magnet bears at the top a curved horn of laminated iron which is throttled by copper rings. These curved horns, springing from the magnets, embrace and nearly touch

Fig. 17.



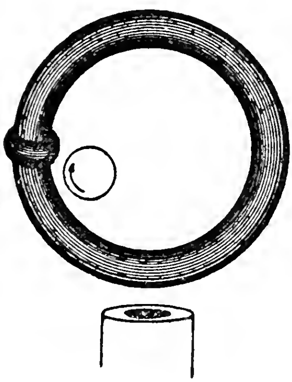


Plan and general view of Wright-Ferranti self-starting alternating motor working a fan.

a light iron-rimmed wheel, free to turn in the centre. The actions just explained drive the wheel round, when the magnet coils are traversed by an alternating current. The iron wheel carries on its shaft a set of mica vanes, which retard the wheel by air friction. Under the opposing influences of this retardation and the electro-magnetic rotation forces, the wheel takes a certain speed corresponding to different current strengths in the magnetic coils, and hence the total number of revolutions of the wheel in a given time, as recorded by a counter, serves to determine the total quantity of alternating current which has passed through the meter. A motor (Fig. 17) working a fan is also here exhibited, the operation of which depends on the same facts. In the case of the motor the iron-rimmed wheel has its tyre closed with copper sheet to aid the action.

§ 16. The rotation of iron discs can be shown also by means of a badly-designed transformer. If a closed laminated iron ring (Fig. 18), like the one before me, is wound with a couple of conducting circuits, such an arrangement constitutes a transformer. If these two circuits are wound on opposite sides of the iron ring, the previous explanations will enable you to perceive that the arrangement will be productive of great magnetic leakage across the iron circuit. In designing transformers for practical work, one condition amongst others which must be held in view is to so arrange the conductive and magnetic circuits that a great magnetic leakage of lines of force across the air does not take place. If, however, this leakage exists, it indicates that the secondary circuit is not getting the full benefit of the induction created by the primary. To detect it we have merely to hold near the iron circuit a little balanced or pivoted iron disc, and if it is set in rapid rotation, as you observe in this case, it indicates that there are laterally-moving lines of magnetic force outside the iron, which have escaped from the iron in consequence of the back magneto-force of the secondary circuit.

Fig. 18.



Magnetic leakage across a throttled iron ring, causing rotation of an iron disc placed near the secondary coil.

§ 17. Time would fail me if I were to attempt to enlarge on the practical applications of the scientific principles which these experiments disclose to us. They are a fertile field both for the investigator seeking to add to the sum total of existing knowledge, or to the inventor in search of applications in electrical technology for such acquired facts. Ingenious minds, and that of Prof. Elihu Thomson foremost amongst them, are busy in seeking to turn these facts to account in the construction of alternating current motors.

One of the simplest of these is shown in principle in the diagram now on the screen (Fig. 19). The coils C are traversed by an alternating current, and are placed on either side of a drum armature wound over with three sets of insulated wire coils, the terminals of the coils coming to insulated sections of a commutator. The functions of this commutator are to keep one coil, B, on short circuit during the time when it is in such positions relatively to the field coils C that the induced current in the closed coil causes it to be repelled by the field coils, and as each successive coil on the armature becomes in turn the active coil, rotation is kept up. A motor, made by Prof. Thomson, based on these principles, but with some additions, is on the table, and on turning the current into it it speedily starts and gets up considerable speed. The details of the actual construction are a little less simple than in the diagram shown, because the

Fig. 19.

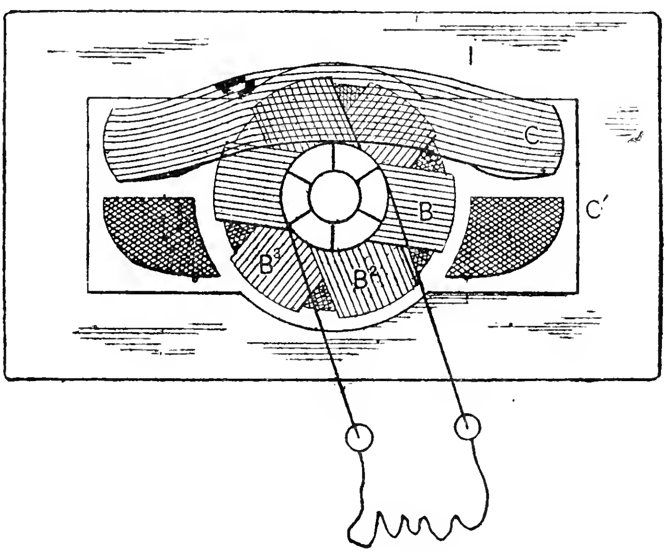


Diagram illustrating elementary form of alternating-current motor.

motor is made to start by sending the external current into the armature by means of a commutator and brushes. When, however, the proper speed is attained, the armature coils are automatically short-circuited, and the motor continues to run in virtue of the current induced by the field-magnet in the armature coils. This is by no means, however, the last word said on this portion of the applications, and I think we may shortly look to Prof. Thomson to give us further and more perfect methods of utilising these facts in the construction of self-starting alternating motors.

§ 18. For the opportunity of exhibiting to you this evening these remarkable experiments, I am personally indebted to Prof. Elihu Thomson, both for constructing and sending the apparatus we have used. Part of these appliances have now an historical value, and have been presented by him to the Royal Institution. For the use of the rest I desire to record my obligations and thanks. Illustrative as these experiments are of important facts in connection with the use of alternating currents, they have a special value at the present time. In the opening year of next century, when we celebrate the centenary of the first practical production by Volta, in 1801, of the electric current, we shall find ourselves in the presence of the fact that almost every large city is ramified by a subterranean network of copper conductors for the distribution of electric energy as a necessary of modern life; and although it may be dangerous to express too confident a view on the direction which the progress of electrical invention may take, yet it does not seem improbable that the alternating current will be doing a considerable share of that work. It is, therefore, not only as a contribution to a comprehension of the vagaries of these alternating currents that the phenomena we have shortly studied are worthy of attention, but also as being, perhaps, the avenue of approach to a further possession of valuable knowledge, enlarging our views, and capable, without doubt, of being minted into the current coin of useful and ingenious applications.*

[J. A. F.]

---

* For the loan of the blocks, illustrating the foregoing reprint, the author is indebted to the Editor of the *Electrician*.

WEEKLY EVENING MEETING,

Friday, March 13, 1891.

Sir James Crichton Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

Felix Semon, M.D. F.R.C.P.

The Culture of the Singing Voice.

The subject selected for to-night's discourse is so large that within the limited time at my disposal it will be obviously impossible to do justice to it in all its branches. Under these circumstances two ways are open: either to cast a hurried glance over the whole subject, or rather to select a few points of special interest from amongst the multitude of questions with which it is beset, and to dwell on these at somewhat greater length. The latter course appears to me the preferable one, and I shall follow it, but before entering upon the subject itself I am anxious to touch upon the reasons which have induced me to select this particular subject, and to define my own position with regard to it.

If it be true, as no doubt generally speaking it is, that occupation with science and art is an ennobling thing, an imperfect study of the physiology of the singing voice constitutes a sorry exception to that rule. Whilst in previous times the culture of the singing voice was conducted upon purely empirical but good rules, such as had gradually and logically developed themselves from the accumulated experience of many schools of singing, ever since the discoveries of modern physiology have been popularised, and especially since the laryngoscope has been introduced and believed to elucidate all questions connected with the production and cultivation of the singing voice, a very bitter war has been raging, in which practically everybody's hand has been against his neighbour. Dogmatic rules, irrespective of actual facts, have been laid down as to the hygiene of the vocal organs, the question of registers has been and is at the present time as hotly discussed as ever, and untenable theories have been raised with regard to the capability of the laryngoscope to decide the most intricate and difficult questions concerned in the production of the singing voice.

Now, I have always taken, since I have occupied myself with these questions, the one view that nothing could be more detrimental to the true interests of the noble art of singing than to be led astray by well-meaning but over-enthusiastic adapters of incomplete physiological facts into a wrong groove under the impression that the rules which were preached were so firmly based upon facts of unimpeachable scientific accuracy that nothing remained to the professors of the art itself but to bow before the superior wisdom of the theorist; and I make bold to say that such a superior wisdom does not exist with regard to this question. Probably I of all persons shall least be suspected of underrating the value and the importance of the laryngoscope, and I firmly hope and believe that with further long continued and careful studies this valuable little instrument will help us in the future to further elucidate a great many questions concerning the production and culture of the singing voice, about which at the present we are still completely in the dark. At the same time it cannot be strongly enough insisted upon that the millenium has not yet come, and that at the present time the claims of the laryngoscope to teach and lay down the rules for really reasonable and scientific training of the singing voice are by no means completely established.

This may be very disappointing to a good many of my hearers, who possibly have expected that I would join the ranks of those who know everything about the culture of the singing voice from physiological principles, but if it be true, as no doubt it is, that the very foundation of every knowledge consists in the fact that one knows the limitation of one's knowledge, I think that some good may be done by simply strictly defending those claims which are justified and refuting those which are based upon insufficient and solitary experience of one or of a few cases. This will be the point of view from which I shall to-night approach my subject. As to my legitimation I only have to add that for a good many years I have had unusual opportunities of seeing the results of faulty training of the singing voice; that I have always taken a special interest in the study of the nervous mechanism by which the production of song is governed, and that I have for many years made experimental researches in that particular line; and finally, that I have had the good luck for many years of having been associated by ties of friendship with a great many of the leading singers and teachers of singing of our age.

Upon such basis I will venture to-night to give a few of the results of my own experiences coupled with results of literary study and of the scientific and important work of an American friend of mine, Dr. French, of Brooklyn, through whose kindness I am in the position to-night of showing you some of the most interesting and remarkable facts concerning the production of the singing voice graphically, and which have been recently elucidated by means of instantaneous photography.

The organ in which the singing voice is produced being the larynx, it will be indispensable to give a short description of the anatomical configuration of that part, which I shall strive to keep as free from technicalities as possible. The larynx consists of a framework of cartilages which are joined to one another by means of ligaments and joints, and which allow to all the parts very free movements towards one another by means of muscles attached to them. Additionally the larynx as a whole can be very freely moved in various directions through the instrumentality of other muscles connecting it with the parts both above and below it. The larynx forms the top of the windpipe, which again is the beginning of the bronchial tubes, which branch off from its lower part and gradually spread into more and more twigs around which are arranged the constituent parts of the lungs, which form the bellows for the blast of air necessary for the performance of vocal functions. Above, the larynx opens into the throat and the cavities of the mouth, nose, and its accessory cavities, and the naso-pharyngeal space, which serve as a resonator for the vocal vibrations which are produced in the larynx itself. The larynx is lined with a mucous membrane contiguous with that of the neighbouring parts. This mucous membrane in the larynx itself forms two folds, situated one above the other; the upper of these two reduplications, which is not itself at all concerned in the formation of sound, retains all the characteristics of common mucous membrane. Formerly these upper folds—as there is one on each side of the larynx—were called the false vocal cords, but this misleading name has latterly almost entirely been given up in favour of the more significant expression, ventricular bands. The lower reduplications are much more important, and are inserted in front into the receding angle of the biggest laryngeal cartilage (the thyroid), which in the male sex forms outwardly the so-called Adam's apple. Posteriorly they are inserted into two small cartilages called the arytenoid cartilages, which, by means of an articulation or joint, can move very freely and in various directions on the surface of the second large laryngeal cartilage, the cricoid. These lower folds of mucous membrane form the so-called true vocal cords and have lost to a great extent the common characteristics of mucous membrane, which are principally replaced by numberless elastic fibres in part running parallel to one another, but in part interwoven in the most various directions with one another. These fibres are of unequal length, some of them being inserted in the most projecting part of the arytenoid cartilages, which has been called the vocal process, whilst other ones extend very considerably further backwards and are inserted along the body of the arytenoid cartilage itself. This elastic tissue being the sounding element, by the vibrations of which primarily sound is engendered, it is very likely, as Signor Manuel Garcia first pointed out, that it is through the unequal length of these fibres that the enormous range of the human voice is rendered possible.

The muscles through which the vocal cords are set in motion, and which indeed regulate the mechanism of the sound produced in the larynx, are subdivided into three groups: the abductors, the adductors, and the tensors of the vocal cords. Of these the two last groups, i.e. the adductors and tensors, are always in unison, whilst their action is antagonistic to that of the abductors. The function of the latter muscles consists in keeping the vocal cords during respiration so far asunder from one another that the narrowing of the tube, which is actually produced by the interpolation of the larynx into the respiratory apparatus, is neutralised to the necessary extent, whilst the adductors and tensors as a rule serve only the voluntary and purposive function of phonation, as in speaking and in singing, and are employed only in a secondary fashion for some of the reflex acts of respiration, such as laughing and coughing.

All these muscles, i. e. the respiratory as well as the phonatory muscles, receive their nerve supply from two small nerves, the superior laryngeal and the recurrent laryngeal nerves. The superior laryngeal only supplies the tensors of the vocal cords, the cricothyroid muscles, with motor fibres, whilst the recurrent is distributed to the adductor as well as to the abductor muscles. It is still an open question whether the recurrent is ultimately derived from the spinal accessory or the vagus nerve, both being cranial nerves, the centres of which are situated in the medulla oblongata. The researches of Ferrier, Duret, Munk, Krause, Horsley, and myself have shown that the medulla is not the ultimate seat from which impulses are distributed along the motor paths just sketched to the laryngeal muscles, but that there is for the purposive function which the larynx serves, viz. for phonation, a distinct centre in the surface or cortex of the brain, situated in the foot of the ascending frontal gyrus, just behind the lower end of the precentral sulcus. It is a very interesting and noteworthy phenomenon, that Professor Horsley and I have only been able to find (except in the cat) a definite area of representation of the action of the vocal cords in the cortex of the brain for the intentional purposive movements of the vocal cords, such as are used in speaking and singing. On stimulation of this area on one side, both vocal cords directly come together (i. e. are adducted), and remain, so long as the stimulation lasts, in the position which they assume when used for either of the last-named purposes. It is never possible, according to our researches, to produce an action of one cord alone, they always act bi-laterally and symmetrically. Equally impossible is it when one looks at the larynx of a human being, of a monkey, or of a dog during the act of phonation, i. e. when the cords are being brought together and put into the proper degree of tension, to make out any difference in time between these two actions. Tension and adduction apparently occur absolutely simultaneously, or at least our retina is not able to distinguish any point of time between the order of execution of these two movements. Only in the cat can it be seen that the mewing is produced by the vocal cords first being brought together, i. e. by the act of adduction being performed, and then, after a measurable interval tension, i. e. elongation of the vocal cords, occurring.

All this may seem rather much of a scientific refinement, and only in remote, if any, connection with the subject of to-night's discourse. In reality, however, it is easy to show that this connection is a very intimate and a very necessary one. Nothing could be more fallacious than the often heard comparison between the teaching of the voice and that of the hand, as in violin or in piano playing. The mechanism which governs the muscles of our fingers, though by no means capable of producing finer variations and differentiations than that of the larynx, yet is infinitely more under the influence of the will than that of the last-named part. We can, by a process of perfectly conscious cerebration, teach not only the muscles of one hand, whilst those of the other remain absolutely quiet, to perform certain movements according to will, but this differentiation goes to such refinement that we can actually educate individual muscles of individual fingers to a degree of independence which does not exist at all in the child or in the uneducated adult. All this is done, I repeat, by a process of conscious cerebration.

Very different, however, from this is the action of the vocal cords in speaking and in singing. Not only is it impossible to the greatest singer to move one vocal cord without the other at the same time executing the same movements (in this respect, also, the vocal cords differ from even the movements of the eyes), but nobody can by a process of conscious cerebration move one laryngeal muscle without the other. This is rendered an absolute fact both by observation of the human being with the laryngoscope and by experiments upon animals.

All the comparisons, therefore, of the development of the laryngeal muscles with those of the hand fall to the ground, and all the elaborate anatomico-physiological directions met with in more than one book of instruction for the student of singing must be referred to the realm of bewildering phantasmagoria. The rational training of the singing voice can only as yet proceed upon the basis of empirical experience, not upon that of theoretical deductions as to the action of the individual adductor muscles and upon equally theoretical directions as to their individual use.

The means by which the movements of the vocal cords and indeed the larynx can be observed during the act of singing is the laryngoscope, first introduced for physiological purposes by Signor Manuel Garcia, and afterwards brought into use for the study of laryngeal disease by Czermak and Türck. This instrument consists of a little round mirror, which, after having been properly warmed, is introduced into the throat of the person under observation in such a manner that it forms an angle of 45° with the horizon, its upper margin resting against the base of the uvula. If now from a powerful source of light horizontal rays are thrown on to the mirror, which is held in the open mouth of the person in the position just described, according to the principles of physiological optics these rays are directed in a vertical direction downwards, and illuminate the larynx, which is just below the point where the mirror is held. The rays are in turn reflected upwards into the mirror and thence into the eye of the observer, which is situated at an equal height and close to the source of illumination. The picture thus resulting during quiet respiration you see here on the screen.

(Demonstration.)

Now, what I am particularly anxious to say as the result of long observation is that whilst you can, as just shown, see in this way the larynx in its entirety, and whilst you can judge with certainty as to any pathological change that may exist therein, not even the most experienced laryngologist can say from mere laryngoscopic examination whether the larynx he sees is in any way that of a singer or not. Of course he will be able to pronounce that, if there are any organic congenital or acquired defects in the anatomical configuration of the part, the owner of this organ will be incapable of producing musical sound, but if he sees merely a normally constituted larynx, it is absolutely impossible for him to say whether this belongs to the greatest singer living or to a person absolutely unendowed with the faculty of producing melodious sounds. I can assure my hearers that the larynges of some of the greatest living singers look so common-place that nobody seeing one of these organs without knowing who its owner is would ever venture for a moment to believe that this could be the organ to which he has been indebted for many a time of the highest artistic pleasure, whilst, on the other hand, magnificent looking larynges are frequently found in the possession of individuals who not only are utterly unmusical, but at the same time incapable of producing anything like an average singing voice.

Nor is it possible, from the mere aspect of a larynx, to say with absolute certainty even so much as what the general character of the singing voice produced by it may be. It is perfectly true that in the majority of cases soprani and tenors have comparatively speaking short and narrow vocal cords, while those of contralti and bassi are broad and long. But to this rule so many exceptions occur that anybody who trusts blindly to this sign will be exposed to very frequent mistakes. To give but one example, I have never seen any larger and longer vocal cords than those of a well-known tenor who has often enchanted London audiences, whilst I am perfectly certain that every laryngologist who was asked without knowing anything about the owner to pronounce merely from the larynx, would pronounce this vocal organ, if at all exercised for singing, to be that of a basso profundo. I mention this point more particularly because it has more than once occurred in my practice that students have been brought to me in order that I should decide from laryngoscopic examination what the true character of their voice was, a demand which, as I think I have just shown, it is absolutely impossible to comply with. But the general gist of the foregoing remarks is to show that an instrument which unfortunately fails to give us any clue as to the very elementary points just mentioned with regard to the character of the voice of the person examined, can certainly not claim to have laid down on its authority the rules for singing in the dogmatic fashion in which this has repeatedly been done of late.

Having so far given a very cursory anatomical outline of the conditions in which we are here interested, I now come to the physiological aspect of the question. The long mooted question whether the human voice was to be compared to a wood, a string, or a reed instrument has at last been definitely decided in favour of the last-named view; though even at present timid attempts are made from time to time to revive the flute or the violin theory. In reality, however, the larynx is best to be compared to an organ pipe, the reed being represented by the two vocal cords, which being anatomically absolutely identical with one another, and being simultaneously put into vibrations by the blast of air coming from the lungs, entirely correspond to the reed of the organ pipe; sound being produced, of course, by the vibrations of the cords, which are communicated to the column of air above and below the vibrating reed. According to the quantity of air coming from the lungs, more or less amplitude is given to the vibrations, as the result of which the tone gets more or less strength. The character of the sound thus emitted is no doubt influenced to a very considerable degree by the configuration of the larynx and the composition of the vibrating reeds, but the exact manner in which this influence is exercised is at the present time still an absolutely unknown entity. What is quite certain, however, is that the character of the voice is very greatly varied by the anatomical configuration of and the changes possible in the throat and mouth.*

According to the laws of acoustics, three fundamental laws come here into question:

1. The number of vibrations of the cords determines the pitch of the note.
2. The amplitude of the vibrations determines, as already mentioned, the strength of the note.
3. The form of the vibrations determines the timbre or quality of the voice.

These laws briefly indicate the most important qualities of the sound, viz. purity, strength, and timbre.

The intra-laryngeal movements, i. e. the proper degree of tension of the vocal cords determining, as has just been said, the pitch of the note, the purely technical training of the voice and the purity of the notes naturally depend upon the movements of the larynx proper, and more particularly upon the intralaryngeal changes during the emission of the sound.

The dynamics of the voice, on the other hand, the crescendo and decrescendo, &c., depend upon the intensity of the movements of the thorax, of the diaphragm, and of the lungs.

Finally, the colour or timbre of the voice is rendered variable by

---

* The brief summary here given I have borrowed mainly from Julius Stockhausen's 'Gesangstechnik und Stimmbildung.'

the different positions of the parts forming the resonator, i.e. the tongue, the lips, the palate, and the epiglottis. What we call "expression" in singing is, therefore, the result of a combination of the action of the bellows on the one, and of the resonator on the other, hand.

(Demonstration.)

There are, of course, almost numberless particular qualities of the voice upon which I should like to enter here at greater length, as the definition and discussion of each of them possess a particular fascination of their own, such as the compass, the volume, the sustained power, the tellingness, the certainty, the freshness, the intonation, facility, &c., of the voice, but time will not allow me to do so. I can only refer my hearers to a most charming little book of Dr. Walsh's, called 'Dramatic Singing,' in which, although I do not agree with everything that the distinguished author states, and especially not with his curious manner of estimating the individual qualities of the voice, they will find a most fascinating description of all those individual qualities, and ample food for thought concerning the almost incredible multitude of points which enter into the composition of dramatic singing, couched in the most elegant and most picturesque language.

Coming now to the question of the culture of the singing voice itself, two elements are absolutely necessary for proficiency, viz. first, a certain amount of natural material, and, second, a good ear. With regard to the first, this ought to be, as it were, a truism, but, indeed, it is not. Often enough people mistake the inclination for the gift, and confound their love of singing with the decision of devoting themselves to it. I see numbers of students deficient in the very elements of vocal material, who nevertheless have formed so grave and momentous a decision as the devotion of their lives to the practice of singing. There is a general tendency, under such circumstances, to attribute the failure to some "disease" of the vocal organs, or, if the word "disease" be not pronounced, at any rate to "weakness." The physician, seeing many of these ailments, cannot help asking himself what want of judgment can have induced such people to fight against impossibilities. As a rule there is no disease at all, but simple deficiency of the indispensable elementary material. I think it an act of kindness to warn such people against an uphill fight, in which, with the rarest exceptions, they cannot be successful. Certainly it is not my desire to discourage ardent lovers of music from training their voices, however small, so long as they merely intend to use them for their own or their immediate friends' pleasure, but matters are widely different when one sees young persons, who in other walks of life might earn a decent livelihood, struggling under the greatest difficulties against unfavourable circumstances of every conceivable sort, and all this in order to cultivate a practically non-existent singing voice. Valuable years are often thus lost, and it is finally with a feeling of despair and bitterness that such people, after lost years of labour, gain the conviction that they would have done much better to devote themselves from the very commencement to a different career. Would that every student of music, or those responsible for the selection of a career, might keep before their minds that in singing there are but few, very few indeed, who ever reach the top of the ladder, and that those who lag behind often enough carry the conviction of the futility of their endeavours throughout their lives!

I trust that the foregoing thoughts will not be interpreted in the sense as if I wished to encourage only those who are endowed with very large and beautiful material to devote themselves to the noble art of singing. In many cases even in originally weak vocal organs by rational training really astounding improvement can be produced; others even with small material may, if husbanding their resources, and if intelligent enough not to aspire to impossibilities, achieve very fair success with limited means. Thus, it is a curious thing to find that often very small voices, i. e. small both in compass and in strength, yet are endowed with that all-important quality of the singing voice, viz. a sympathetic timbre, which is utterly denied to much larger or more flexible voices. Indeed this sympathetic timbre of the voice often goes hand in hand with conditions of an otherwise disqualifying character, such as a certain veil over the notes, a very small compass, a very deficient strength of tone; yet if such people understand the great secret, that it is better, as was said of Henrietta Sonntag, to have a small genre, but to be great in that genre, than to attempt impossibilities, they may on the concert platform be very successful.

Thus I know myself of several singers, both ladies and gentlemen, whose voices are very small indeed, but who, being endowed with the sympathetic quality of timbre, having cultivated their voices in the most rational manner, and limiting their work to the interpretation of a high class of musical lyrics, to such a degree enchant their public that the smallness of the means by which their successes are achieved is completely forgotten in the intellectual delight which they give to their hearers. But the warning note I tried to sound before was merely directed against the loss of valuable time in cases of utter absence of any of those qualities of the voice which could endear its owner to a musical public.

The next indispensable factor for cultivating the singing voice is the possession of a good musical ear. Now, with regard to the musical ear there are almost as many different senses in which that expression may be taken, as there are with regard to the expression "musical" in general. Thus it is perfectly well known that, whilst in some persons the musical ear is by a generous gift of nature, even if entirely untutored, yet endowed with the keenest qualities of perception and of action based upon that perception, other people equally intelligent are entirely deprived of any natural endowment in this particular direction, and have, as the saying goes, absolutely "no ear for music." Now this may mean a great many different things. Some people have no ear for pitch, others not for melody. The former will not hear even the most abominable flat or sharp singing, the latter will never recognise even the most catching melodies however often they may have heard them. Upon the tympana of other ones music makes a directly painful impression. A third class has absolutely no sense of rhythm and cannot distinguish a march from a waltz; again, others, and here we come to the subject now under consideration, though having a keen enough perception of music, and being ready enough to detect faults in others, are utterly unaware either of the quality or of the pitch of their own voices. No doubt many of those present to-night will remember instances within their own experience in which some professional singer or amateur has judged very harshly certain defects in another singer's voice, being apparently utterly unaware that he himself had the faults against which he vociferated, in a much higher degree than the object of his attack. In other cases singers who will most acutely hear any flat singing in another are utterly incapable of apprehending that they themselves sing flat, and finally, there is one class, who, though they themselves are aware of their own singing flat, are quite incapable of correcting the fault and of singing in pitch and in unison with other voices or with accompanying instruments. Instances of these points will be familiar to my hearers.

The causes of all these deficiencies, which are of the most serious importance for the career of any professional singer, are no doubt to be found in the highest cerebral centres; the imperfection of perception being due either to those afferent fibres which carry the impression of sound to the auditory centre or to congenital defect of the centre itself, whilst the impossibility of singing in pitch, though the singer himself is painfully conscious of his not doing so, must be due to some mischief within the paths which lead from the auditory to the phonatory centre. Defects of this sort are in part to some extent remediable through the aid of long continued training. In such cases it must be assumed that certain nerve cells originally not intended or only intended in a minor degree for the conveyance of the impressions now under discussion, have been educated up to higher functions, in the same way in which we see that after the destruction of the speech centre in the left hemisphere, the corresponding part in the right hemisphere may be, though almost always in an imperfect way, educated up to take the original duties of its fellow in the left hemisphere. In the great majority of cases, however, either all training remains without effect, or the results are so small in proportion to the labour spent that the game is hardly worth the candle. This is a point of the very highest importance.

I have already previously mentioned that the training of the voice cannot be compared to the training of the hand for the purposes of piano or violin playing, inasmuch as the training of the latter is mostly performed in the shape of conscious voluntary acts; whilst the training of the voice is of an infinitely more instinctive character and guided mainly by auditory impressions. But few persons have got such an absolute sense of tonality that they are at any moment ready to produce a note in the correct pitch without having first received a hint from some musical instrument. If the guide be taken away, i.e. if the auditory mechanism and the fibres connecting the auditory and phonatory apparatus be acting imperfectly, the whole training will needs be of an infinitely more difficult, very frequently of a finally imperfect character. Proofs of this may practically be seen any day both on the operatic stage and on the concert platform. A good musical ear, therefore, I should say, is an indispensable adjunct for the professional career of a singer.

Supposing now that both the indispensable amount of vocal material and the good ear be present, one question foremost naturally presents itself: when to begin proper training?

With regard to this question, I am decidedly of opinion that serious vocal training should not be begun in either sex as a rule before the sixteenth year of age, though it must be understood that there may be exceptions to this rule, both in favour of an earlier and of a later commencement of vocal studies. The reason of this decided opinion consists in a consideration of the physiological conditions of the larynx during its development. In the period of adolescence the larynx undergoes very considerable changes. In boys especially a very sudden and very considerable enlargement of all the cartilaginous framework occurs, accompanied by more or less acute congestion of the mucous membrane. The considerable elongation of the vocal cords which takes place at the same time, in a *number* of cases undoubtedly goes on so gradually that the muscles governing their movements adapt themselves insensibly to the altered condition of matters, and the transition both of the speaking and of the singing voice may be equally insensible and gradual. In by far the larger number of cases, however, the co-operation between all the factors necessary to produce the voice, especially in singing, often enough even in simple speaking, is not so imperceptibly established. The whole apparatus, as it were, temporarily gets out of gear and only after a considerable period the different elements constituting it learn instinctively to adapt themselves to the suddenly altered anatomical conditions. The practical illustration of what I mean is given by that hated period in the life of many a boy called the breaking of the voice.

Supposing now that a boy had had a sweet child's voice and that this voice had been utilised in choir singing, on the stage, in oratorio, or elsewhere, too often the exigencies of life make it very desirable that such a boy, who has to some extent contributed towards the support of his family by the gift of Nature bestowed upon him, should continue his singing over the period allotted to the child's voice by Nature; whilst in other cases masters who do not know enough of the physiological changes taking place in the larynx might be inclined to continue the training of a favourite pupil's voice into this period of disturbance, during which physiological rest is absolutely wanted. The result in most cases will be lasting loss of voice; inasmuch as all the organs here concerned are of such a delicate nature that once hopelessly overstrained they are not likely ever to return to normal conditions.

I am perfectly well aware that different opinions exist on this question; that some singers as well as some laryngologists have expressed themselves in favour of a continued training through the period of adolescence, and that so and so many cases are quoted in which such a training has been continued throughout this trying period without any lasting harm resulting, the pupil on the contrary finally attaining eminence in the vocal profession. To all this I simply reply that I do not doubt the occurrence of exceptions, but that such exceptions the more confirm the rule, and that if a census were taken with regard to the number of those voices which have been irretrievably ruined by premature vocal training during the period of adolescence in proportion to those in which no harm has resulted, undoubtedly a large excess on the side of harm having resulted would be shown. Indeed a census of this nature has been taken by Messrs. Behnke and Browne in their little work, 'The Child's Voice,' and the result based upon collective investigation, in which a very large number of competent physiological authorities, teachers of singing and singers took part, incontrovertibly points in the same direction in which my observations are going. All I can say is that if I had a child, boy *or* girl, gifted with an exceptionally fine voice, I should not allow it even to make the experiment.

If a voice is really worth training, be it for professional use or for private amusement only, it is certainly worth—provided that external circumstances permit—having from the very beginning a very good teacher. No greater mistake, I think, could be made than to confide so complex an apparatus as the vocal one is, at first for so-called elementary tuition to the tender mercies of a teacher, who has not the faintest idea either of the physiology of the vocal organs or of the recognised and valuable modes of educating this apparatus, and who, by wrong tuition, either hopelessly spoils all the material that has been confided to him, or at any rate engenders bad habits, wrong muscular combinations, wrong habitudes of the sounding board of the voice, which afterwards only with the greatest possible difficulties can be eradicated even by the most competent successor he may have.

The necessity of selecting *from the very beginning* of the pupil's vocal career a really good teacher, is too obvious to be insisted upon at length. A really good master of singing will first of all take infinite pains to ascertain the true character of the pupil's voice. He will not make the mistake, but too often committed, of educating a contralto as a soprano or a baritone as a tenor, simply because there are a few fine high notes in the pupil's voice; nor will he at all purposely develop one part of the range of the pupil's voice at the expense of the others. He will build upon the material that he finds, not impose hypothetical or self-made laws upon it, and he will subordinate his own likings to the natural exigencies of the whole character and nature of the pupil's voice. No more pernicious thing could be imagined than to look upon flexibility as the highest triumph to be achieved in a pupil whose voice is the prototype of a heroic tenor, or, on the other hand, to force a pupil to sing Brahms' or Schubert's most dramatic songs whilst the nature of her voice would show to any perfectly unbiassed ear that her true vocation was that of a coloratura singer.

This brings me to the final part of my discourse, to the vexed question of the registers. I need not say that this question alone could be made the subject, not of one, but of a whole course of lectures, and that it will be impossible in the short space of time still left me to do anything like adequate justice to it, or merely to mention all the points which here demand consideration. At the same time the question is of such transcendental importance for the rational cultivation of the singing voice that it cannot in a discourse on this subject be altogether passed over in silence; and I avail myself of the opportunity of alluding to it the more readily because, as stated in the beginning of my discourse, through the kindness of Dr. French, I am in a position to illustrate the question by the aid of the unassailable testimony of the photographic camera.

It is well known that if any singer, but especially an untutored one, sings the ascending scale, the ear of the musical listener perceives after a series of tones which are different from one another only so far as the *pitch* is concerned, suddenly at one point or another of the gamut a notable difference in *timbre*, *strength*, and *character*. The point at which this change occurs is called the "break in the voice," and this change is much more noticeable in some classes of voices than in others. It is most developed in the tenor voice, in which the transition from the chest to the so-called falsetto voice is obvious even to less musical ears. A break of this character, according to the opinion of many authorities, occurs only once in the range of the voice, and these authorities broadly divide the entire compass into the "chest" and "head" registers. In the opinion of others, however, there are not one but several breaks in the voice, and accordingly not two but three or more registers, the term "register" by common consent being applied to that series of consecutive tones which is produced by one and the same relative position of the laryngeal apparatus, whilst only in the tension and approximation of the vocal cords do minute variations occur.

What each of these registers exactly is, how it is produced, how one is changed into the other, what exactly is the manner in which our will influences the change, &c., we are not yet, I make bold to say, in a position to know precisely, though I am perfectly aware that some authorities think that they know all about it, and that it has been definitely stated which of the adductor muscles is, as it has been called, the "leading" muscle for each register. A theory of this character has been developed in a very attractive little book by Dr. Michael, of Hamburg, entitled 'The Formation of the Registers in Singing.' The author, whilst claiming that each register is distinguished by one of the adductor muscles specially presiding over its functions, positively states that for the production of each sound the co-operation of *all* laryngeal muscles up to a certain degree is necessary, and that on complete disablement of a single muscle only, complete loss of voice must necessarily ensue.

Statements of this character show how dangerous it is to make too bold and absolute assertions in this whole question. According to the nature of things it is quite imaginable that certain of the adductor muscles may be concerned more in the formation of one, others in the formation of another register, but to make, as Dr. Michael has done, *absolute and general* conclusions from a few cases of paralysis of one laryngeal muscle or another in singers, as to the exact function of each of them in the production of the register, is quite inadmissible, as I am in a position to show at once.

A distinguished tenor, whose case is known to several British laryngologists, had the misfortune a few years ago of entirely losing his voice from a tumour in his neck, pressing upon the left recurrent laryngeal nerve and completely paralysing the left vocal cord. Not only the singing but even the speaking voice was entirely lost in the beginning of the illness. Under appropriate treatment the tumour almost disappeared, and certain fibres of the recurrent laryngeal nerve recovered, whilst other ones had already been irretrievably damaged. The result of all this was that his left vocal cord was finally immovably fixed in the position of phonation. In this position it remains up to the present day, i. e. it is not the least abducted when the patient inspires, and there cannot be the least doubt that the left abductor muscle is as completely paralysed and unable to fulfil its natural functions as it possibly could be. Yet this gentleman at the present time is able not merely to sing and to sing high chest as well as falsetto notes, but the voice, according to the statements of many who have heard him before and after his severe illness, has entirely regained its former character, power, and compass.

This case at once disposes of two theories which have been brought forward as if they were unassailable facts, namely, first of the statement just mentioned, and secondly of the frequently heard assertion that in paralysis of the abductors the possibility of producing high notes is lost.

Another point also associated with the question of registers and of the greatest possible importance for the rational cultivation of the singing voice, is that, whether in voices of identical character, say for instance contralto voices, the break always occurs in *one and the same* note of the scale, or whether the exact note on which it takes place varies in different individuals. I hope that the number of the theorists among teachers of singing, who on preconceived ideas believe that it always occurs on one and the same point and who, in accordance with this belief, force the whole natural mechanism of their pupils' voices into their theoretical formulæ, is only a small one. But no doubt such teachers exist, and more than once I have heard statements from pupils who come to consult me, to the effect that ever since they studied with Mr. So-and-so, and since they were told that they had been wrongly taught with regard to the break in their voice, and that they must begin to use the head voice or the falsetto voice, either higher or lower than so far they had been accustomed to take it, they felt a great sense of fatigue after practising, and that they distinctly thought they had suffered with regard to the character of their voice.

There can be nothing more dangerous, I venture to say, than any mistake with regard to this point, i. e. any interference with the laws of nature, which in this question, I have not the least doubt, vary in every individual case. There is no such thing as an absolute point on which the voice breaks, in any class of singers. No doubt the break occurs in one and the same class of voice more or less in the *neighbourhood* of a certain note, for instance, in contralto voices the lower break as a rule occurs about the neighbourhood of E or F on the line, but no doubt there are many voices in which it occurs either at E flat or on the other hand at F sharp, and that the voice in which it occurs at the higher part should now be forced into a lower break because that corresponds with the theoretical ideas of the master, would be a simply unpardonable mistake.

The whole question of the registers is at the present time being so ably treated by my friend, Dr. French, of Brooklyn, who earned general and well deserved applause by a paper he read on that subject on the occasion of last year's International Medical Congress at Berlin, that I only wish I could in conclusion of this discourse read to you verbatim the whole of it and show you all those splendid photographs by means of which he illustrated it. But unfortunately the time still left to me is so short that I must limit myself to giving you a part only of his lecture in his own words, and to more briefly deal with the remainder.

Dr. French at the onset of his enquiries started from the very just idea that the movements of the glottis are often so rapid that the eye cannot appreciate them, or rather so numerous that the mind will not retain them in the order of their occurrence. It is estimated that the human eye can open and shut in the tenth part of a second, but an impression formed upon the retina in that time lacks detail, while an image of the interior of the larynx in all its detail may be fully and clearly impressed upon the sensitive plate in the hundredth part of a second. Those movements which the eye fails to appreciate may easily be defined by taking a series of photographs at different stages, which being viewed consecutively clearly shows such movements in their entirety.

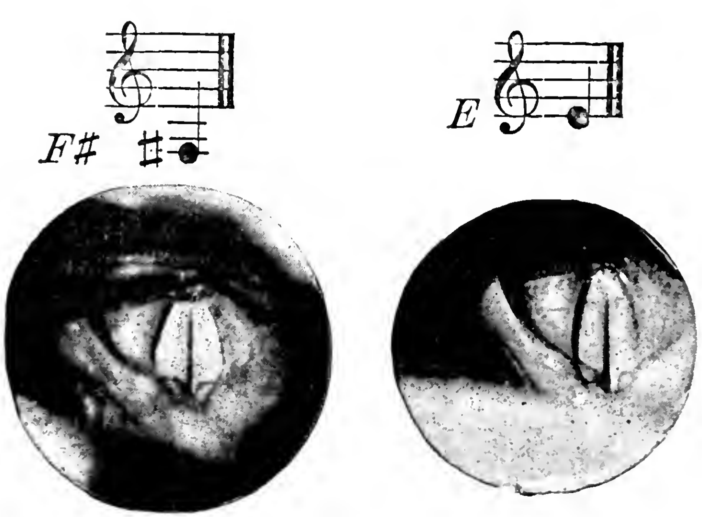
For fully six years Dr. French with unremitting perseverance has perfected the art of taking instantaneous photographs of the larynx in singing. Only those who saw his initial results and those obtained about 1883 and 1884 by contemporaneous workers, such as Messrs. Behnke and Browne, can appreciate the enormous progress he has made within that time and the value of the results thus obtained. In spite of this he has, according to his own statements, not yet permitted himself to formulate a theory of the action of the larynx in singing, for even now, after large numbers of studies have been made by him, he says that the camera is constantly revealing new processes in the action of the vocal cords in every part of the scale, and that the movements of the larynx in a much larger number of subjects must be revealed, grouped, and recorded before definite conclusions can be drawn. The fact that there are relatively but few subjects in whose larynges the anterior insertions of the vocal cords can be seen throughout the range, adds greatly to the difficulties of this investigation. In order to find one satisfactory subject a large number have to be examined, which necessarily takes much time, and renders the progress of the study very slow.

At Berlin Dr. French exhibited a series of photographs taken of the larynges of four female singers, which showed how the changes are made in the action of the glottis from one register to the other in the variations and the pitch of the voice. These series were taken consecutively, and therefore fairly represent the marked variations in the movements of the various structures which occur in different larynges.

The description of the first series I give in Dr. French's own words as follows:—

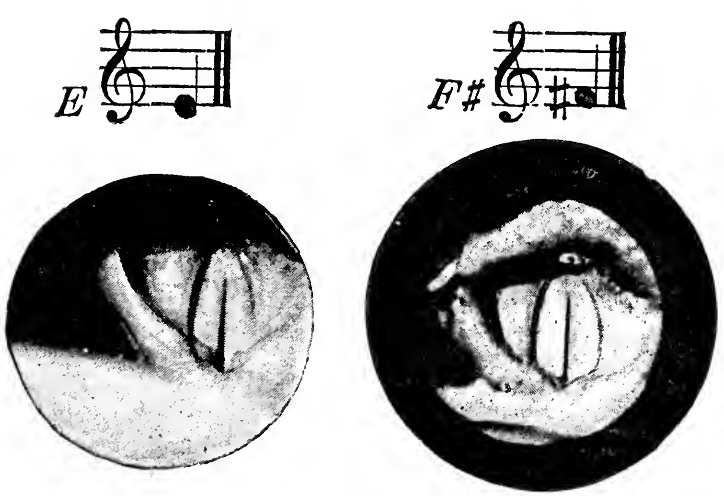
"The first pair of photographs is the first of a series which will be shown of the larynx of a well-known professional contralto singer.

FIG. 1.



The voice is of excellent quality. The first pair was taken while F sharp, treble clef, third line below staff, was being sung; and the second while she was singing E above. These are one of the lowest and highest notes of her lower register. In the photograph representing the lowest note it can be seen that the vocal cords are quite short and wide, and that with the exception of the anterior fourth the ligamentous part of the cartilaginous glottis is open, and the slit between the vocal bands is linear in shape. As the voice ascends the scale the vocal cords increase in length and decrease in width, until at the highest note of the register they may be seen to have become considerably longer. It can also be observed that the ligamentous portion of the glottis is still open to the same relative extent, and that the cartilaginous portion has opened to its full extent. In the photograph representing the lower note the anterior faces of the arytenoid cartilages can be seen. The epiglottis, though not well illuminated, seems to have risen as the voice ascended the scale; the vocal cords have increased in length at least $\frac{1}{8}$th of an inch in seven notes. The compass of the voice of this singer is about two octaves and a half, therefore at that rate of lengthening the vocal cords would increase nearly half an inch if their length was progressively increased while singing up the scale from the lowest to the highest note. This progressive increase in length does not, however, occur, and the reason will be apparent in the next pair of photographs, which show the changes which take place in the larynx at the lower break in the voice, which in this subject occurs at F sharp, treble clef, first space."

Fig. 2.

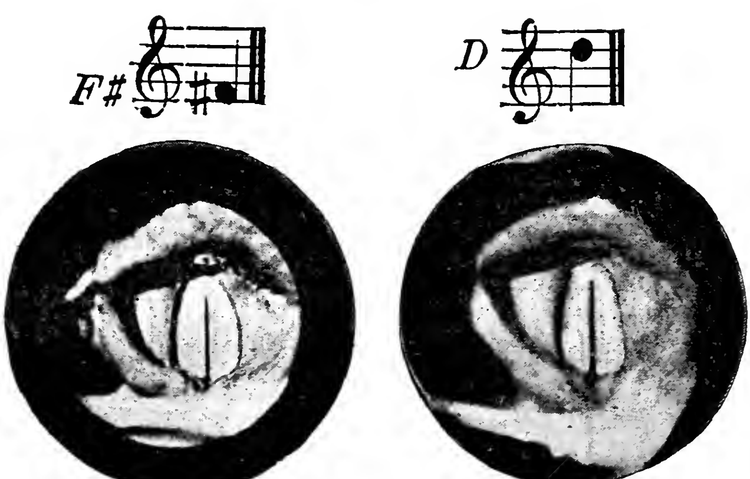


"The changes which occur at this point are extremely interesting and instructive in the transition from the lower to the middle register, from E to F sharp. In the voice of this subject the vibratory portions of the vocal cords are shortened about the $\frac{1}{16}$th of an inch. The anterior insertions of the cords can be seen in both photographs, therefore the actual difference in the length of the bands can be appreciated. The vocal cords have not only become shorter, but they appear to be subject to a much higher degree of tension. The cartilaginous glottis is closed, and the aperture in the ligamentous portion has been much reduced in size. The laws which govern the pitch in both string and reed instruments will aid us in explaining this change. Though the tone is higher, and the degree of stretching less than in the note below, the tension is increased, and the aperture through which the air passes is much narrower."

"The anterior, posterior, and lateral dimensions of the larynx are shown to have been considerably decreased when the voice broke into the register above. The voice acquired a very different quality, which continued in gradual elevation of pitch throughout the register." As marked a change as this in the mechanism of the vocal cords in females is, Dr. French believes, only found in the larynges of contralto singers.

"As the singer ascends the scale above the break at F sharp the vocal cords are increased in length, and the chink gradually enlarges, as shown in the next pair. The first photograph is of the larynx

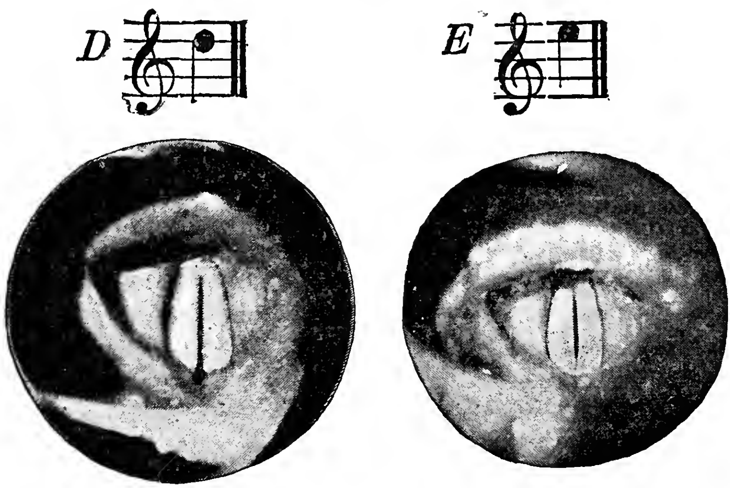
Fig. 3.



while singing F sharp, treble clef, first space, the note on which the lower break occurred, and the second while singing D, treble clef, fourth line, which is the highest note in the middle register of the voice of this singer. The difference in the length of the vocal cords and width of the chink of the glottis as the voice mounts from the lowest to the highest note of the middle register is clearly shown. Again, as the vocal bands increase in length in this register, their tension is apparently decreased."

"Now the voice mounts one note higher—that is, to E, treble clef, fourth space—and as it does so a distinct change in the quality of the voice is heard, and the second change in the mechanism of the vocal cords occurs. The changes which take place in the larynx at the upper break in the voice of this singer are shown in the next pair. The first photo represents the larynx while singing D, treble clef, fourth line, the note immediately preceding the break, and the

Fig 4.



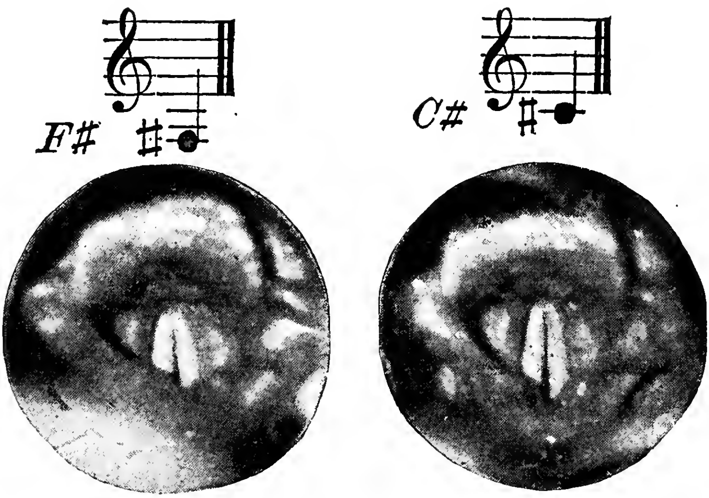
second shows the change which occurred while singing E, the next note above. A very decided change in the mechanism of the vocal cords is apparent. These ligaments have grown higher and narrower, and the chink which in the note before the break can be seen to be linear in shape and quite wide, after the break becomes considerably reduced in both length and width. Not only is the cartilaginous portion of the glottis closed in the note after the break, but also a small portion of the ligamentous glottis immediately adjoining it. The chink appears to be closed to the same extent in front as it was while producing the note immediately preceding it. There is, therefore, stop-closure in front and behind, which leaves a slit in the middle of the glottis measuring a little more than half the length of the vocal cords. In addition to these changes, it may be observed that the epiglottis is depressed and the arytenoid cartilages have again receded. As this is the highest note which this subject is capable of singing with ease, we cannot study the action of the vocal cords in the production of tones in the upper register."

"It may be remembered that in this larynx the vocal cords increase in length from the low F sharp to the E above. At the next note higher they began to increase in length again until D above was reached, and at E, the note next above, they were again suddenly shortened. It will be instructive to determine the degree to which the vocal cords were lengthened, and at what point in the scale they were longest. We saw that in the lower register the vocal cords were longest in the production of the highest note, and in the middle register they were also longest while the highest note was being sung. By comparing the photographs representing these notes it can be seen that the vocal cords were as long, if not the longest, while the highest note of the lower register was being sung. In this subject the vocal cords increase in length in each register, but they had as great a length in the lower as in either register above, if not greater. It is generally thought that the pitch is raised by the vocal cords increasing progressively in tension and length. In regard to length this is true in some cases, while in others it is only true as applied to a register, not to the whole voice."

In the second case, photographs of which were shown by Dr. French at Berlin, but of which he unfortunately could not send me copies, because the photographs of the larynx of this subject, though clear and strong enough for satisfactory exhibition upon the screen, were too weak for a direct reproduction by the photo-engraving process, the action of the larynx was in many respects the *reverse* of that just examined. In it the cartilaginous glottis did not appear to begin to open until the highest notes were reached. In the lower register the chink of the glottis decreased instead of increasing in size as the voice ascended. At the lower break the vocal cords were increased instead of decreased in length, and the chink of the glottis increased instead of decreasing. Again, the vocal cords attained their greatest length at the highest note in the voice of this subject, which corresponded to about the highest note of the middle register, whilst in the larynx before examined the chink of the glottis increased in size, and the vocal cords increased in length, as the voice ascended in each register. I should the more have liked to show the photographs illustrating this condition, inasmuch as the subject was also a contralto singer, and as the demonstration would have materially aided in strengthening the position that the action of the glottis in singing, even in voices belonging to the same class, varies very considerably.

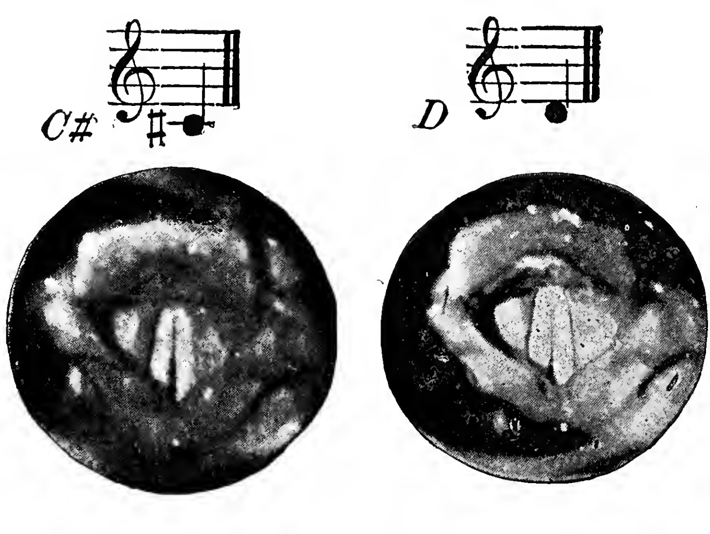
The next series of photographs, I am selecting from Dr. French's collection, illustrates the action of the glottis in singing, of a well-trained soprano singer, who possesses the extraordinary range of four octaves, the voice being of excellent quality. The first pair of photographs represent one of the lowest and the highest notes of the lower register of this singer's voice. As the voice mounts the scale the vocal cords increase in length and the cartilaginous portion of the glottis increases in size; the arytenoid cartilages recede from the anterior wall of the larynx. In the neighbourhood of C sharp a change in the quality of the voice was heard. Dr. French lays particular stress upon the fact that the change could be heard in the *neighbourhood* of C sharp, for the note at which the break occurred varied considerably in this subject. In some of the runs it occurred at C sharp; in others at D or E. Not knowing exactly where it would occur, it was difficult to get a satisfactory idea of the nature of the change in the laryngoscopic mirror. He therefore took photographs while the subject sang each note from A below to the A above. An examination of the negatives revealed the break at D, a

Fig. 5.



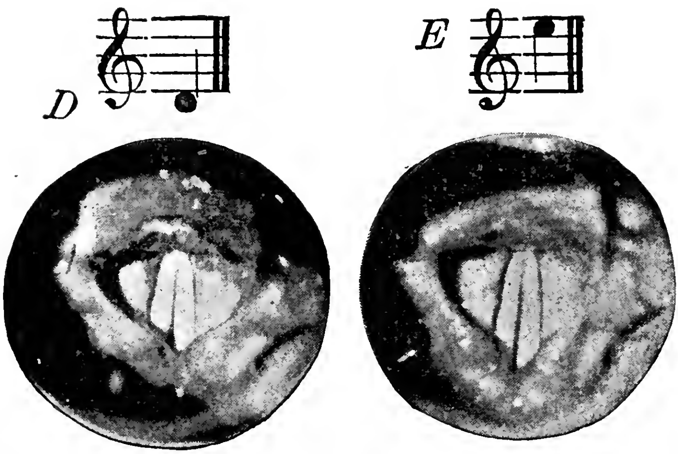
photograph of the larynx while singing which is shown in the next pair, together with one while singing the note immediately preceding it.

Fig. 6.



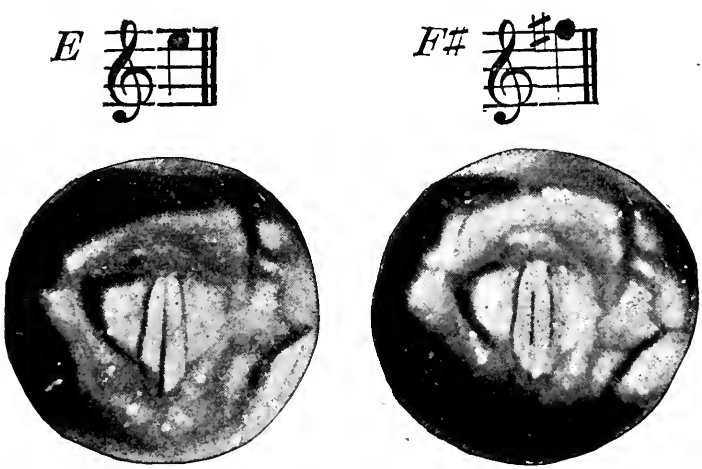
From this point the vocal cords are gradually increased in length and decreased in width as the voice mounts the scale in the middle register, as is seen in the following pair. This pair represents the lowest and highest notes of the middle register of this subject.

Fig. 7.



At the next note higher, F sharp, treble clef, top line, another change in the quality of the voice occurred, and with it a change in the laryngeal mechanism, which is displayed in the next pair of photographs. The voice has broken into the upper or head register

Fig. 8.



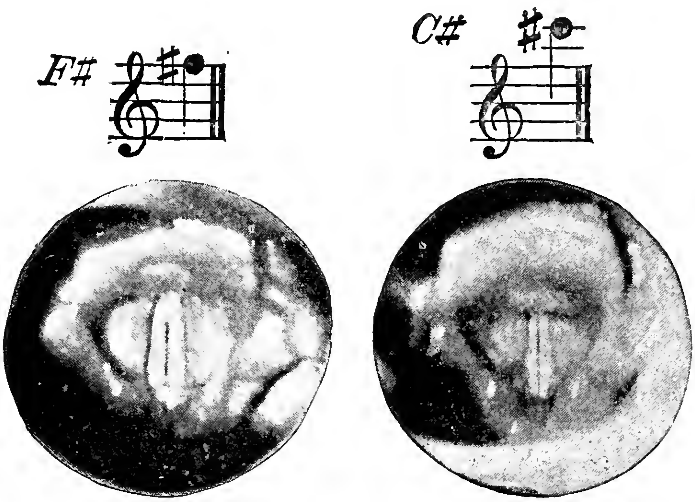
and the change in the mechanism is decided. The vocal cords are reduced in length and appear to be narrower. The edges of the cords are closer together, only a narrow linear slit being left between them; the capitula Santorini are tilted backward and the cartilaginous portion of the glottis is nearly or quite closed. The position of the epiglottis is about the same as when producing the note before the break.

The opinion prevails that in the production of tones in the upper register some portion of the edges of the vocal cords are in contact or pressed tightly together; in other words, that stop-closure occurs.

Here the anterior fourth of the glottic chink is closed, but the same amount of closure in the same position may be seen in the larynx singing the note before the break.

Now the voice mounts to high C sharp. The next pair shows the

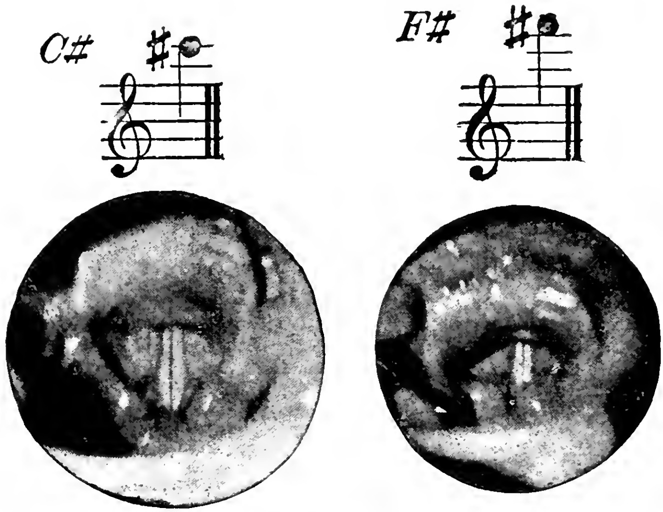
FIG. 9.



larynx while singing that note, and also the note on which the voice broke into the head register. In that representing C sharp it can be seen that the whole of the cavity of the larynx is smaller, and that the vocal cords and the chink of the glottis are narrower. The vocal cords appear to be much shorter, but as the anterior ends are covered by the cushion of the epiglottis, it is impossible to say how much shortened they really are. The arytenoid cartilages are closer together and are inclined further forward in the high than in the low notes of this register. The mucous membrane covering the lateral walls of the larynx is wrinkled, showing that during the production of this high note it is not capable of contracting to a sufficient extent to present a smooth surface. In the high note even the contact between the vocal cords, which can be seen in the lowest head note, and which we saw occur in the production of notes in the middle register, has disappeared, and there is a clear linear space between the vocal cords the entire length of the glottis.

The next pair represents high C sharp and a still higher note in the subject of this voice, F sharp. In that representing F sharp we may observe that the cavity of the larynx is greatly contracted, the epiglottis is not so high as when C sharp was sung, in fact the four walls of the larynx are crowded towards the centre and the epiglottis is curled inward, the arytenoid cartilages are almost if not quite in contact, the vocal cords are very short and look like threads. The most surprising revelation made in this picture is that there is no stop-closure. It is possible that there was slight contact between the edges of the vocal cords at the posterior portion of the glottis, but in

Fig. 10.



Dr. French's opinion air was passing between the edges of the cords the entire length of the glottis when this photograph was taken.

From the revelations made in the photographs of the glottis of different persons while head tones were being sung, Dr. French comes to the conclusion that contact of the vocal cords in the first 5 or 6 tones of the head register does not occur in half the number of cases.

Reluctantly I refrain from further following Dr. French in his interesting lecture. His argument of course gets the more convincing the more examples of the variety of ways in which the larynx acts in the cases of different singers are brought forward and illustrated by means of the camera. Time, however, will not allow me to do so, and I can only give the most important conclusions regarding the action of the glottis in female singers at which he finally arrives. They are as follows:—

"1. The larynx may act in a variety of ways in the production of the same tones or registers in different individuals."

"2. The rule, which, however, has many exceptions, is that the vocal cords are short and wide, and the ligamentous and cartilaginous portions of the glottis are open in the production of the lower tones; that as the voice ascends the scale the vocal cords increase in length and decrease in width. The aperture between the posterior portions of the vocal cords increases in size, the capitula Santorini are tilted more and more forward, and the epiglottis rises until a note in the neighbourhood of E, treble clef, first line, is reached. The cartilaginous glottis is then closed, the glottic chink becomes much narrower and linear in shape, the capitula Santorini are tilted backward and the epiglottis is depressed."

"When the vocal bands are shortened in the change at the lower break in the voice, it is mainly due to closure of the cartilaginous portion of the glottis, the ligamentous portion not usually being affected. If, therefore, the cartilaginous glottis is not closed there is usually no material change in the length of the vocal cords."

"As the voice ascends from the lower break the vocal cords increase in length and diminish in width, the posterior portion of the glottic chink opens more and more, the capitula Santorini are tilted forward and the epiglottis rises until, in the neighbourhood of E, treble clef, fourth space, another change occurs. The glottic chink is then reduced to a very narrow slit; in some subjects extending the whole length of the glottis; in others closing in front or behind in both. Not only is the cartilaginous glottis always closed, but the ligamentous glottis is, I believe, invariably shortened. The arytenoid cartilages are tilted backward, and the epiglottis is depressed. As the voice ascends in the head register the cavity of the larynx is reduced in size, the arytenoid cartilages are tilted forward and brought closer together, the epiglottis is depressed and the vocal cords decreased in length and breadth. If the posterior part of the ligamentous portion of the glottis is not closed in the lower, it is likely to be in the upper notes of the voice."

The series of photographs which were shown by Dr. French were not selected to prove any preconceived ideas; they simply represent the variations which will be met with in any four consecutive studies. It is, however, scarcely to be wondered at that the theories regarding the action of the glottis in singing differ so widely, especially those based upon the study of one subject or of a few.

Dr. French personally is of opinion that the female voice has three registers, and considers it quite probable that in voices with exceptional ranges there are four registers. At the same time, he says that sufficient evidence has not yet been obtained to make this demonstrable.

I am glad to have been able to show that this, the latest achievement of abstract science, so fully corroborates the views held by competent teachers of singing as to the enormous variety in producing the singing voice, and I can only, in conclusion of my discourse, express, together with my warmest thanks to Dr. French for having allowed me to illustrate my opinions by aid of the results of his perseverance and industry, the conviction that that teacher will be the most successful one who individualises in every single case confided to his care, remembering how delicate the mechanism is which is entrusted to him and how easily mischief may be wrought by wrong training, whilst that pupil will the most probably reap the best fruits of his studies who aims only at perfecting that which has been given to him by Nature, not at achieving what is impossible according to physiological laws.

[F. S.]

WEEKLY EVENING MEETING,

Friday, March 20, 1891.

Basil Woodd Smith, Esq. F.R.A.S. F.S.A. Vice-President, in the Chair.

Professor Victor Horsley, F.R.S. B.S. F.R.C.S. *M.R.I.* Fullerian Professor of Physiology, R.I.

Hydrophobia.

(No Abstract.)

GENERAL MONTHLY MEETING,

Monday, April 6, 1891.

Sir James Crichton Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

William Boyle Barbour, Esq. M.P.
The Right Hon. Lord Randolph Churchill, M.P.
C. E. H. Chadwyck-Healey, Esq. Q.C.
Mrs. C. E. H. Chadwyck-Healey,
William Frederick Hamilton, Esq. LL.D.
William Robert Lake, Esq.
The Rev. Edward G. C. Parr, M.A.
Thomas Slingsby Tanner, Esq.
Charles Humphrey Wingfield, Esq.
Latham Augustus Withall, Esq.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned for the following Donations to the Fund for the Promotion of Experimental Research:—

		£	s.
Ludwig Mond, Esq.		100	0
Lachlan M. Rate, Esq.		50	0
Charles Hawksley, Esq. (for new Optical Lantern)		50	0
Alfred Bray Kempe, Esq.	(do.)	5	5
David Edward Hughes, Esq.	(do.)	2	2
George Berkley, Esq.	(do.)	5	5
Basil Woodd Smith, Esq.	(do.)	5	5
Edward Pollock, Esq.	(do.)	2	2
Sir Frederick Bramwell, Bart.	(do.)	10	10
Sir Frederick Abel	(do.)	5	0
Professor Dewar	(do.)	10	10
Sir James Crichton Browne	(do.)	5	5
Warren W. de la Rue, Esq.	(do.)	10	10
Wm. Chandler Roberts-Austen, Esq.	(do.)	5	5

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta: Rendiconti. 1° Semestre, Vol. VII. Fasc. 1–6. 8vo. 1891.
Atti, Anno 43, Sess. 4ª, 5ª, 6ª. 4to. 1891.
Atti, Serie Quarta, Anno CCLXXXIII.-CCLXXXV. 4to. 1886–8.
Academy of Natural Sciences, Philadelphia—Proceedings, 1890, Part 2. 8vo.
Antiquaries, Society of—Archæologia, 2nd Series, Vol. II. Part 1. 4to. 1890.
Proceedings, Vol. XIII. No. 2. 8vo. 1890.
Aristotelian Society—Proceedings, Vol. I. No. 4, Part 1. 8vo. 1891.
Asiatic Society of Bengal—Journal, Vol. LVIII. Part 1, No. 3; Part 2, No. 5; Vol. LIX. Part 2, Nos. 2, 3. 8vo. 1889–90.
Proceedings, Nos. 4–10. 8vo. 1890.
Astronomical Society, Royal—Monthly Notices, Vol. LI. No. 4. 8vo. 1891.
British Architects, Royal Institute of—Proceedings, 1891, Nos. 10, 11. 4to.
Brymner, Douglas, Esq. (the Archivist)—Report on Canadian Archives, 1890. 8vo.
Cambridge Philosophical Society—Transactions, Vol. XV. Part 1. 4to. 1891.
Proceedings, Vol. VII. Part 3. 8vo. 1891.
Canadian Institute—Transactions, Vol. I. Part 1, No. 1. 8vo. 1890.
Chemical Industry, Society of—Journal, Vol. X. No. 2. 8vo. 1891.
Chemical Society—Journal for March, 1891. 8vo.
Civil Engineers' Institution—Minutes of Proceedings, Vol. CIII. 8vo. 1891.
Cracovie, l'Academie des Sciences—Bulletin, 1891, No. 2. 8vo.
Dawson, G. M. Esq. LL.D. F.G.S. (the Author)—Note on the Geological Structure of the Selkirk Range. 8vo. 1891.
Dax, Société de Borda—Bulletin, Quinzième Année. 3me et 4me Trimestre 8vo. 1890.
Editors—American Journal of Science for March, 1891. 8vo.
Analyst for March, 1891. 8vo.
Athenæum for March, 1891. 4to.
Brewers' Journal for March, 1891. 4to.
Chemical News for March, 1891. 4to.
Chemist and Druggist for March, 1891. 8vo.
Electrical Engineer for March, 1891. fol.
Engineer for March, 1891. fol.
Engineering for March, 1891. fol.
Horological Journal for March, 1891. 8vo.
Industries for March, 1891. fol.
Iron for March, 1891. 4to.
Ironmongery for March, 1891. 4to.
Murray's Magazine for March, 1891. 8vo.
Nature for March, 1891. 4to.
Open Court for March, 1891. 4to.
Photographic News for March, 1891. 8vo.
Public Health for March, 1891. 8vo.
Revue Scientifique for March, 1891. 4to.
Telegraphic Journal for March, 1891. fol.
Zoophilist for March, 1891. 4to.
Florence Biblioteca Nazionale Centrale—Bolletino, Nos. 124–126. 8vo. 1891.
Indice Cataloghi, Codici Palatini IV. Vol. II. Fasc. 3. 8vo. 1891.
Franklin Institute—Journal, No. 783. 8vo. 1891.
Geographical Society, Royal—Proceedings, New Series, Vol. XIII. Nos. 3, 4. 8vo. 1891.
Georgofili, Reale Accademia—Atti, Quarta Serie, Vol. XIII. Disp. 3ª. 8vo. 1890.
Harlem, Société Hollandaise des Sciences—Œuvres Complètes de Christian Huygens, Tome 3. Correspondance, 1660–1661. 4to. 1890.

Harris, John, Esq. (the Author)—The Laws of Force and Motion. 4to. 1890.
Johns Hopkins University—University Circulars, No. 86. 4to. 1891.
Liverpool Polytechnic Society—Proceedings for 53rd Session. 8vo. 1890.
McClean, Frank, Esq. M.A. M.R.I. (the Author)—Comparative Photographs of the High Sun and Low Sun Visible Spectra, with notes on the method of Photographing the Red End of the Spectrum. fol. 1890.
Manchester Geological Society—Transactions, Vol. XXI Parts 2-5. 8vo. 1890-1.
Ministry of Public Works, Rome—Giornale del Genio Civile, 1891, Fasc. 1°. And Designi. fol. 1891.
Morris, David K. Esq. (the Author)—Notes of a Thousand Men. 8vo. 1891.
New York Academy of Sciences—Transactions, Vol. IX. Nos. 3-8. 8vo. 1889-90.
Annals, Vol. IV. (Index); Vol. V. Nos. 4-8. 8vo. 1890.
North of England Institute of Mining and Mechanical Engineers—Report of the French Commission on the Use of Explosives in the presence of Fire-damp in Mines. Part 3. 8vo. 1891.
Odontological Society of Great Britain—Transactions, Vol. XXIII. Nos. 4, 5. New Series. 8vo. 1891.
Pennsylvania Geological Survey—Dictionary of Fossils, Vols. II. III. 8vo. 1889.
Atlases to Report, 1889. 8vo.
Report on Oil and Gas Fields. 8vo. 1890.
Pharmaceutical Society of Great Britain—Journal, March, 1891. 8vo.
Prince, C. Leeson, Esq. F.R.A.S. F.R. Met. Soc.—Summary of a Meteorological Journal for 1890.
Rathbone, E. P. Esq. (the Editor)—The Witwatersrand Mining and Metallurgical Review, Nos. 13, 14. 8vo. 1891.
Richards, Admiral Sir G. H. K.C.B. F.R.S. (the Conservator)—Report on the Navigation of the River Mersey, 1890. 8vo. 1891.
Rochester Academy of Science—Proceedings, Vol. I. Part 1. 8vo. 1890.
Rothschild, F. C. von, Esq.—Die Einrichtung und Verwaltung der F. C. von Rothschild'schen offentlichen Bibliotek (1887-90), von Dr. Christ Wilh. Berghœffer. 8vo. 1891.
Royal Institution of Cornwall—Journal, Vol. X. Part 2. 8vo. 1891.
Royal Society of London—Proceedings, No. 297. 8vo. 1891.
Royal Society of New South Wales—Journal and Proceedings, Vol. XXIV. Part 1. 8vo. 1890.
Saxon Society of Sciences, Royal—Mathematisch-physischen Classe: Abhandlungen, Band XVI. No. 3; Band XVII. Nos. 1, 2. 8vo. 1891.
Berichte, 1890, Nos. 2-4. 8vo. 1891.
Scottish Society of Arts, Royal—Transactions, Vol. XII. Part 4. 8vo. 1891.
Selborne Society—Nature Notes, Vol. II. No. 15. 8vo. 1891.
Smithsonian Institution—Annual Report, 1888. 8vo. 1890.
National Museum Report, 1888. 8vo. 1890.
Société Archæologique du Midi de la France—Bulletin, No. 5. 8vo. 1890.
Society of Architects—Proceedings, Vol. III. Nos. 7, 8, 9. 8vo. 1891.
Society of Arts—Journal for March, 1891. 8vo.
United Service Institution, Royal—Journal, No. 157. 8vo. 1891.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1891: Heft 2, 3. 4to.
Wells, Sir T. Spencer, Bart. F.R.C.S. M.R.I. (the Author)—Modern Abdominal Surgery. (The Bradshaw Lecture.) 8vo. 1891.
Wild, Dr. H. (the Director)—Annalen des Physikalischen Central-Observatoriums, Theil II. January, 1889. 4to.
Wright & Co. Messrs. J. (the Publishers)—Lectures on Diabetes. By Robert Saundby, M.D. 8vo. 1891.
Medical Annual, 1891. 8vo.

WEEKLY EVENING MEETING,

Friday, April 10, 1891.

WILLIAM CROOKES, Esq. F.R.S. Vice-President, in the Chair.

SIR WILLIAM THOMSON, D.C.L. LL.D. Pres. R.S. *M.R.I.*

Electric and Magnetic Screening.

THERE are five kinds of screening against electric and magnetic influences, which are quite distinct in our primary knowledge of them, but which must all be seen in connected relation with one another when we know more of electricity than we know at present: —I. Electrostatic screening; II. Magnetostatic screening; III. Variational screening against electromotive force; IV. Variational screening against magnetomotive force; V. Fire-screens and window-blinds or shutters.

I.

Electrostatic screening is of fundamental significance throughout electric theory. It has also an important place in the history of Natural Philosophy, inasmuch as consideration of it led Faraday from Snow Harris's crudely approximate but most interestingly suggestive doctrine of non-influence of unopposed parts and action in parallel straight lines between the mutually visible parts of mutually attracting conductors, to his own splendid theory of inductive attraction transmitted along curved lines of force by specific action in and of the medium intervening between the conductors.

A continuous metallic surface completely separating enclosed air from the air surrounding it acts as a perfect screen against all electrostatic influence between electrified bodies in the portions of air so separated. This proposition, which had been established as a theorem of the mathematical theory of electricity by Green, in the ninth article of his now celebrated essay,* was admirably illustrated by Faraday, by the observations which he made inside the wooden cube covered all around with wire netting and bands of tinfoil, which he insulated within this lecture-room:† "I went into the cube and lived in it; and, using lighted candles, electrometers, and all other tests of electrical states, I could not find the least influence upon them, or indication of anything particular given by them, though all the time the outside of the cube was powerfully charged, and large sparks and brushes were darting off from every point of its outer surface."

* See pp. 14 and 48 of the reprint edited by Ferrers.

† 'Experimental Researches,' 1173-1174.

The doctrine of electric images is slightly alluded to, and an illustrative experiment performed, showing the fixing of an electric image. The electroscope used for the experiments is an electrified pith ball, suspended by a varnished double-silk fibre of about 9 or 10 feet long. Figs. 1–4 represent experimental illustrations, in which the pith ball, positively electrified, experiences a force due to electrified bodies, optically screened from it by a thin sheet of tin-plate. In Figs. 1 and 2 the pith ball is attracted round a corner by a stick of rubbed sealing-wax, and in Figs. 3 and 4 repelled round a corner by a stick of rubbed glass. In Fig. 2 the sealing-wax *seems* to repel the pith ball, and in Fig. 4 rubbed glass *seems* to attract it.

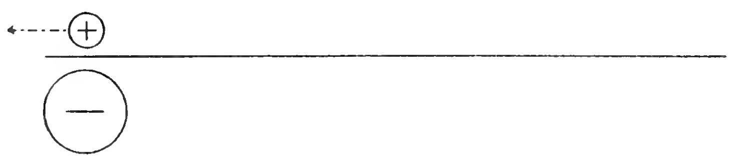


FIG. 1.

FIG. 2.

FIG. 3.

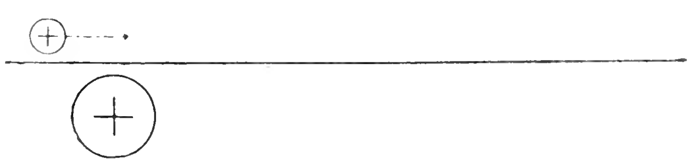


FIG. 4.

This experiment constituted a very palpable illustration of Faraday's induction in curved lines of force.

In the present lecture some experimental illustrations were given of electrostatic screening by incomplete plane sheets and curved surfaces of continuous metal, and of imperfectly conducting material, such as paper, slate, wood, and a sheet of vulcanite, moist or dry, window glass at ordinary temperatures in air of ordinary moisture, and by perforated metal screens and screens of network, or gratings of parallel bars.

The fixing of an electric image is shown in two experiments: (1) the image of a stick of sealing-wax in a thin plane sheet of vulcanite, moistened, warmed, and dried under the electric influence by the application and removal of a spirit-lamp flame; (2) the glass jar of a quadrant electrometer with a rubbed stick of sealing-wax held projecting into it, while the outer surface is moistened, warmed, and dried by the application and removal of a ring of flame produced by cotton wick wrapped on an iron ring and moistened in alcohol.

Fig. 5 is copied from a diagram of Clerk Maxwell's to illustrate screening by a plane grating of parallel bars of approximately circular

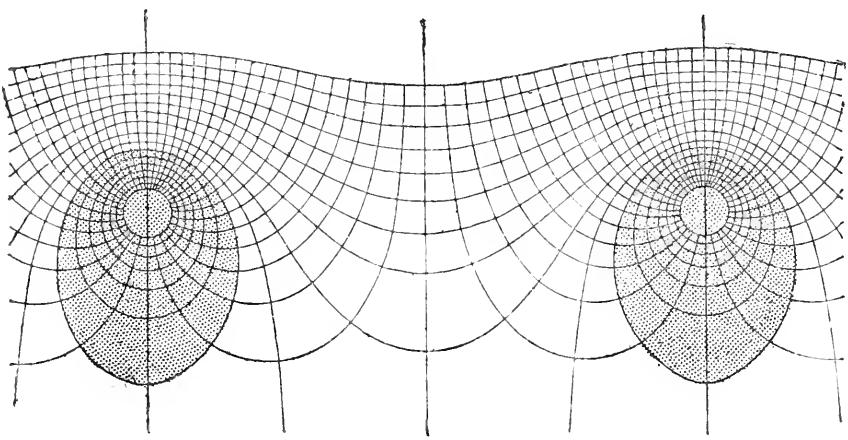


Fig. 5.

cross section, with distance from centre to centre twelve times the diameter of each bar.* It represents the lines of force due to equal quantities of opposite electricities on the grating itself, and a parallel plane of continuous metal (not shown in the diagram) at a distance from the grating of not less than one and a half times the distance from bar to bar. The shading shows the lines of force for the same circumstances, but with oval bars instead of the small circular bars of Maxwell's grating. It is interesting to see how every line of force ends in a bar of the grating, none straying to an infinite distance beyond it, which is necessarily the case when the quantities of electricity on the grating and on the continuous plane are equal and opposite. If an insulated electrified body, with electricity of the same name as that of the grating, for example, is brought up from below, it experiences no electric force differing sensibly from that which would be produced by its own inductive effect on the grating, till it is within a less distance from the grating than the distance from bar to bar, when it experiences repulsion or attraction, according as it is under a bar of the grating or under the middle of a space between two bars. If there be a parallel metal plane below the grating, kept

* 'Electricity and Magnetism,' vol. i. art. 203, fig. xiii.

at the same potential as the grating, it takes no sensible proportion of the electricity from the grating, and experiences no sensible force when its distance from the grating exceeds a limit depending on the ratio of the diameter of each bar to the distance from bar to bar. The mathematical theory of this action was partially given by Maxwell,* and yesterday I communicated an extension of it to the Royal Society.

II.

Magnetostatic screening by soft iron would follow the same law as electrostatic screening, if the magnetic susceptibility of the iron were infinitely great. It is not great enough to even approximately

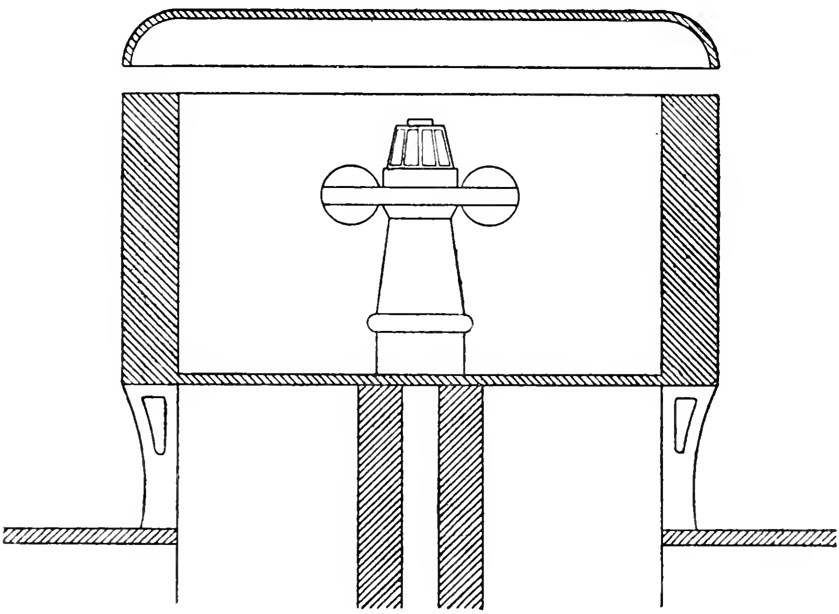


Fig. 6.

fulfil this condition in any practical case. The nearest approach to fulfilment is presented when we have a thick iron shell completely enclosing a hollow space, but the thickness must be a considerable proportion of the smallest diameter, not less than $\frac{1}{10}$, perhaps, for iron of ordinary magnetic susceptibility to produce so much of screening effect that the magnetic force in the interior should be anything less than 5 per cent. of the force at a distance outside, when the shell is placed in a uniform magnetic field. The accompanying diagram, Fig. 6, representing the conning-tower of H.M.S. '*Orlando*,' and the position of the compass within it, has been kindly sent to me by Captain Creak, R.N., for this lecture, by permission of the Controller

* Arts. 203–205.

of the Navy. It gives an interesting illustration of magnetic screening effect by the case of a belt of iron, 1 foot thick, 5 feet high, and 10 feet in internal diameter, with roof and floor of comparatively thin iron. Captain Creak informs me that the average horizontal component of the magnetic directing force on the compass in the centre of this conning-tower is only about one-fifth of that of the undisturbed terrestrial magnetism.

An evil practice, against which careful theoretical and practical warnings were published two or three years ago,* and which is now nearly, though, I believe, not at this moment quite thoroughly, stopped, of what is called single wiring in the electric lighting of ships, has been fallaciously defended by various bad reasons, among them an erroneous argument that the ship's iron produced a sufficient screening effect against disturbance of the ship's compasses, by the electric light currents, when that plan of wiring is adopted. The argument would be good for a ship 50 feet broad and 30 feet deep, if the deck and hull were of iron 3 feet thick. As it is, mathematical calculation shows that the screening effect is quite small in comparison with what the disturbance of the compass would be if the ship and her decks were all of wood. Actual observation, on ships electrically lighted on the single wire system by some of the best electrical engineers in the world, has shown, in many cases, disturbance of the compass of from 3 degrees to 7 degrees, produced by throwing off and on the groups of lights in various parts of the ship, which are thrown on and off habitually in the evenings and nights, in ordinary and necessary practice of sea-going passenger ships. When the facts become known to shipowners, single wiring will never again be admitted at sea unless the alternating current system of electric lighting is again adopted. But, although this system was largely used when electric lighting was first introduced into ships, the economy and other advantages of the direct-current system are so great that no one would think of using the alternate system for the trivial economy, *if any economy there is*, in the single wire, as compared with the double insulated wire system.

An interesting illustration of a case in which iron, of any thickness, however great, produces *no screening effect* on an electric current, steady or alternating, is shown by the accompanying diagram, Fig. 7, which represents in section an electric current along the axis of a circular iron tube, completely surrounding it. Whether the tube be long or short, it exercises no screening effect whatever. A single circular iron ring, supported in the air, with its plane perpendicular to the length of a straight conductor conveying an electric current, produces absolutely no disturbance of the circular endless lines of magnetic force which surround the wire; neither does any piece of

---

* See 'The Electrician,' vol. xxiii. p. 87. Paper read before the Institution of Electrical Engineers, by Sir William Thomson, "On the Security against Disturbance of Ships' Compasses by Electric Lighting Appliances."

iron, wholly bounded by a surface of revolution, with a straight conductor conveying electricity along its axis.

A screen of imperfectly conducting material is as thorough in its action, when time enough is allowed it, as is a similar screen of metal. But if it be tried against rapidly varying electrostatic force,

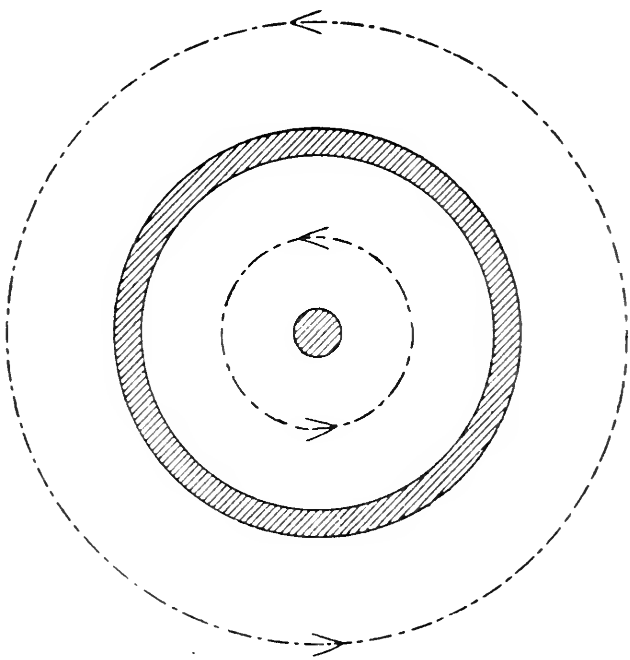


FIG. 7.

its action lags. On account of this lagging, it is easily seen that the screening effect against periodic variations of electrostatic force will be less and less, the greater the frequency of the variation. This is readily illustrated by means of various forms of idiostatic electrometers. Thus, for example, a piece of paper supported on metal in metallic communication with the movable disc of an attracted disc electrometer annuls the attraction (or renders it quite insensible) a few seconds of time after a difference of potential is established and kept constant between the attracted disc and the opposed metal plate, if the paper and the air surrounding it are in the ordinary hygrometric conditions of our climate. But if the instrument is applied to measure a rapidly alternating difference of potential, with equal differences on the two sides of zero, it gives very little less than the same average force as that found when the paper is removed and all other circumstances kept the same. Probably, with ordinary clean white paper in ordinary hygrometric conditions, a frequency of alternation of from 50 to 100 per second will more than suffice to render the screening influence of the paper insensible. And a much less frequency will suffice if the atmosphere surrounding the paper is artificially dried. Up to a frequency of millions per second, we may safely say that, the greater the frequency, the more perfect is the annulment of screening by the paper; and this statement holds also if the paper be thoroughly blackened on both sides with ink, although possibly in this condition a greater frequency than 50 to 100 per second might be required for practical annulment of the screening.

Now, suppose, instead of attractive force between the two bodies separated by the screen, as our test of electrification, that we have as test a faint spark, after the manner of Hertz. Let two well insulated metal balls, A, B, be placed very nearly in contact, and two much larger balls, E, F, placed beside them, with the shortest distance between E, F sufficient to prevent sparking, and with the lines joining the centres of the two pairs parallel. Let a rapidly alternating difference of potential be produced between E and F, varying, not abruptly, but according, we may suppose, to the simple harmonic law. Two sparks in every period will be observed between A and B. The interposition of a large paper screen between E, F, on one side, and A, B, on the other, in ordinary hygrometric conditions, will absolutely stop these sparks, if the frequency be less than, perhaps, 4 or 5 per second. With a frequency of 50 or more, a clean white paper screen will make no perceptible difference. If the paper be thoroughly blackened with ink on both sides, a frequency of something more than 50 per second may be necessary; but some moderate frequency of a few hundreds per second will, no doubt, suffice to practically annul the effect of the interposition of the screen. With frequencies up to 1000 million per second, as in some of Hertz's experiments, screens such as our blackened paper are still perfectly transparent, but if we raise the frequency to 500 million million, the influence to be transmitted is light, and the blackened paper becomes an almost perfect screen.

Screening against a varying magnetic force follows an opposite law to screening against varying electrostatic force. For the present I pass over the case of iron and other bodies possessing magnetic susceptibility, and consider only materials devoid of magnetic susceptibility, but possessing more or less of electric conductivity. However perfect the electric conductivity of the screen may be, it has no screening efficiency against a steady magnetic force. But if the magnetic force varies, currents are induced in the material of the screen which tend to diminish the magnetic force in the air on the remote side from the varying magnet. For simplicity, we shall suppose the variations to follow the simple harmonic law. The greater the electric conductivity of the material, the greater is the screening effect for the same frequency of alternation; and, the greater the frequency, the greater is the screening effect for the same material. If the screen be of copper, of specific resistance 1640 sq. cm. per second (or electric diffusivity 130 sq. cm. per second), and with frequency 80 per second, what I have called the "mhoic effective thickness"* is 0·71 of a cm.; and the range of current intensity at depth $n \times 0 \cdot 71$ cm. from the surface of the screen next the exciting magnet is ϵ^{-n} of its value at the surface.

Thus (as $\epsilon^3 = 20 \cdot 09$) the range of current intensity at depth 2·13 cm. is $\frac{1}{20}$ of its surface value. Hence we may expect that a sufficiently large plate of copper of $2\frac{1}{4}$ cm. thick will be a little less than perfect in its screening action against an alternating magnetic force of frequency 80 per second.

Lord Rayleigh, in his "Acoustical Observations,"† after referring to Maxwell's statement, that a perfectly conducting sheet acts as a barrier to magnetic force,‡ describes an experiment in which the interposition of a large and stout plate of copper between two coils renders inaudible a sound which, without the copper screen, is heard by a telephone in circuit with one of the coils excited by electromagnetic induction from the other coil, in which an intermittent current, with sudden, sharp variations of strength, is produced by a "microphone clock" and a voltaic battery. Larmor, in his paper on "Electromagnetic Induction in Conducting Sheets and Solid Bodies"§ makes the following very interesting statement:—"If we have a sheet of conducting matter in the neighbourhood of a magnetic system, the effect of a disturbance of that system will be to induce currents in the sheet of such kind as will tend to prevent any change in the conformation of the tubes [lines] of force cutting through the sheet. This follows from Lenz's law, which itself has been shown by Helmholtz and Thomson to be a direct consequence of the conservation of energy. But if the arrangement of the tubes [lines of force] *in* the conductor is unaltered, the field on the other side of the conductor into which they pass (supposed isolated from the outside spaces by the conductor) will be unaltered. Hence, if the disturbance is of an alternating character, with a period small enough to make it go through a cycle of changes before the currents decay sensibly, we shall have the conductor acting as a screen.

"Further, we shall also find, on the same principle, that a rapidly rotating conducting sheet screens the space inside it from all magnetic action which is not symmetrical round the axis of rotation."

Mr. Willoughby Smith's experiments on "Volta-electric induction," which he described in his inaugural address to the Society of Telegraph Engineers of November 1883, afforded good illustration of this kind of action with copper, zinc, tin, and lead, screens, and with different degrees of frequency of alternation. His results with iron are also very interesting: they showed, as might be expected, comparatively little augmentation of screening effect with augmentation of frequency. This is just what is to be expected from the fact

---

* 'Collected Papers,' vol. 3, art. cii. § 35.

† Phil. Mag. 1882, first half-year.

‡ 'Electricity and Magnetism,' § 665.

§ Phil. Mag. 1884, first half-year.

that a broad enough and long enough iron plate exercises a large magneto-static screening influence; which with a thick enough plate, will be so nearly complete that comparatively little is left for augmentation of the screening influence by alternations of greater and greater frequency.

A copper shell closed around an alternating magnet produces a screening effect which on the principle stated above we may reckon to be little short of perfection if the thickness be $2\frac{1}{4}$ cm. or more, and the frequency of alternation 80 per second.

Suppose now the alternation of the magnetic force to be produced by the rotation of a magnet M about any axis. First, to find the effect of the rotation, imagine the magnet to be represented by ideal magnetic matter. Let (after the manner of Gauss in his treatment of the secular perturbations of the solar system) the ideal magnetic matter be uniformly distributed over the circles described by its different points. For brevity call I the ideal magnet symmetrical round the axis, which is thus constituted. The magnetic force throughout the space around the rotating magnet will be the same as that due to I, compounded with an alternating force of which the component at any point in the direction of any fixed line varies from zero in the two opposite directions in each period of the rotation. If the copper shell is thick enough, and the angular velocity of the rotation great enough, the alternating component is almost annulled for external space, and only the steady force due to I is allowed to act in the space outside the copper shell.

Consider now, in the space outside the copper shell, a point P rotating with the magnet M. It will experience a force simply equal to that due to M when there is no rotation, and, when M and P rotate together, P will experience a force gradually altering as the speed of rotation increases, until, when the speed becomes sufficiently great, it becomes sensibly the same as the force due to the symmetrical magnet I. Now superimpose upon the whole system of the magnet, and the point P, and the copper shell, a rotation equal and opposite to that of M and P. The statement just made with reference to the magnetic force at P remains unaltered, and we have now a fixed magnet M and a point P at rest, with reference to it, while the copper shell rotates round the axis around which we first supposed M to rotate.

A little piece of apparatus, constructed to illustrate the result experimentally, was submitted to the Royal Institution and shown in action. The copper shell is a cylindric drum, 1·25 cm. thick, closed at its two ends with circular discs 1 cm. thick. The magnet is supported on the inner end of a stiff wire passing through the centre of a perforated fixed shaft which passes through a hole in one end of the drum, and serves as one of the bearings; the other bearing is a rotating pivot fixed to the outside of the other end of the drum. The accompanying sections, drawn to a scale of three-fourths full size, explain the arrangement sufficiently. A magnetic needle outside, deflected by the fixed magnet when the drum is at rest, shows a great diminution of the deflection when the drum is set to rotate. If the

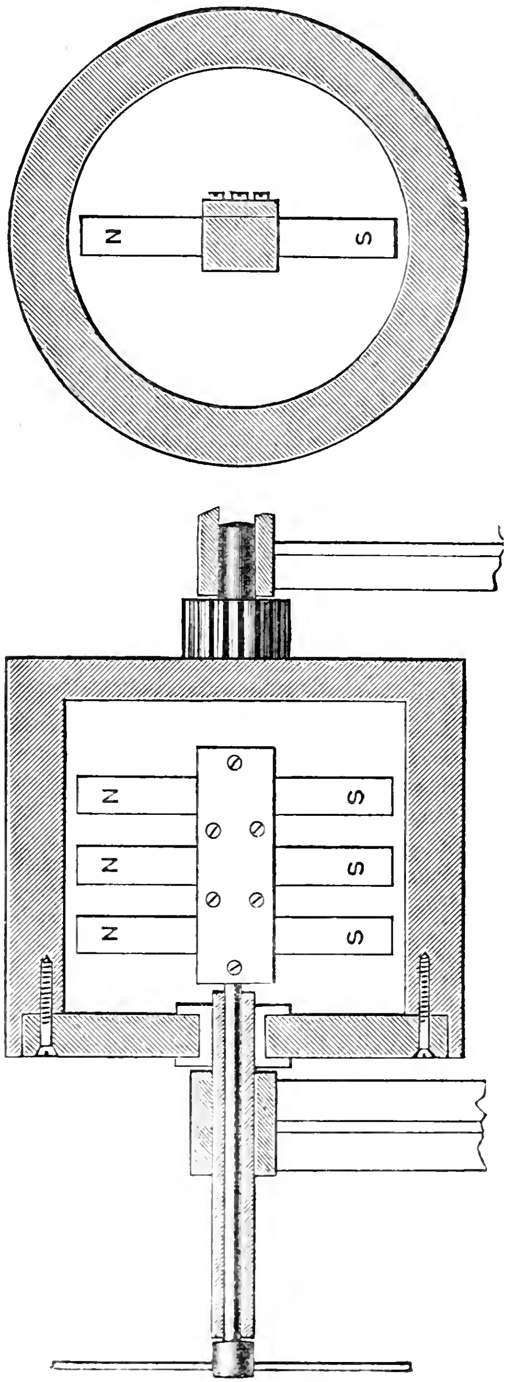


FIG. 8.

(triple compound) magnet inside is reversed, by means of the central wire and cross bar outside, shown in the diagram, the magnetometer outside is greatly affected while the copper shell is at rest; but scarcely affected perceptibly while the copper shell is rotating rapidly.

When the copper shell is a figure of revolution, the magnetic force at any point of the space outside or inside is steady, whatever be the speed of rotation; but if the shell be not a figure of revolution, the steady force in the external space observable when the shell is at rest becomes the resultant of the force due to a fixed magnet intermediate between M and I compounded with an alternating force with amplitude of alternation increasing to a maximum, and ultimately diminishing to zero, as the angular velocity is increased without limit.

If M be symmetrical, with reference to its northern and southern polarity, on the two sides of a plane through the axis of rotation, I becomes a null magnet, the ideal magnetic matter in every circle of which it is constituted being annulled by equal quantities of positive and negative magnetic matter being laid on it. Thus, when the rotation is sufficiently rapid, the magnetic force is annulled throughout the space external to the shell. The transition from the steady force of M to the final annulment of force, when the copper shell is symmetrical round its axis of rotation, is, through a steadily diminishing force, without alternations. When the shell is not symmetrical round its axis of rotation, the transition to zero is accompanied with alternations as described above.

When M is not symmetrical on the two sides of a plane through the axis of rotation, I is not null; and the condition approximated to through external space with increasing speed of rotation is the force due to I, which is an ideal magnet symmetrical round the axis of rotation.

A very interesting simple experimental illustration of screening against magnetic force may be shown by a rotating disc with a fixed magnet held close to it on one side. A bar magnet held with its magnetic axis bisected perpendicularly by a plane through the axis of rotation would, by sufficiently rapid rotation, have its magnetic force almost perfectly annulled at points in the air as near as may be to it, on the other side of the disc, if the diameter of the disc exceeds considerably the length of the magnet. The magnetic force in the air close to the disc, on the side next to the magnet, will be everywhere parallel to the surface of the disc.

[W. T.]

ANNUAL MEETING,

Friday, May 1, 1891.

Sir James Crichton Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

The Annual Report of the Committee of Visitors for the year 1890, testifying to the continued prosperity and efficient management of the Institution, was read and adopted. The Real and Funded Property now amounts to above 83,000*l.* entirely derived from the Contributions and Donations of the Members.

Fifty-six new Members were elected in 1890.

Sixty-three Lectures and Nineteen Evening Discourses were delivered in 1890.

The Books and Pamphlets presented in 1890 amounted to about 285 volumes, making, with 561 volumes (including Periodicals bound) purchased by the Managers, a total of 846 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer, and the Honorary Secretary, to the Committees of Managers and Visitors, and to the Professors, for their valuable services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:

President—The Duke of Northumberland, K.G. D.C.L. LL.D.
Treasurer—Sir James Crichton Browne, M.D. LL.D. F.R.S.
Secretary—Sir Frederick Bramwell, Bart. D.C.L. F.R.S. M. Inst. C.E.

Managers.

Sir Frederick Abel, K.C.B. D.C.L. F.R.S.
George Berkley, Esq. M. Inst. C.E.
Colonel Sir Archibald C. Campbell, Bart. M.P.
Sir James N. Douglass, F.R.S. M. Inst. C.E.
Sir Dyce Duckworth, M.D. LL.D. F.R.C.P.
Sir Douglas Galton, K.C.B. D.C.L. LL.D. F.R.S.
William Huggins, Esq. D.C.L. LL.D. F.R.S.
David Edward Hughes, Esq. F.R.S.
Ludwig Mond, Esq. F.C.S.
Edward Pollock, Esq.
John Rae, M.D. LL.D. F.R.S.
William Chandler Roberts-Austen, Esq. C.B. F.R.S.
Hon. Rollo Russell, F.M.S.
Basil Woodd Smith, Esq. F.R.S. F.S.A.
C. Meymott Tidy, Esq. M.B. A.C.S.

Visitors.

Alfred Carpmael, Esq.
Michael Carteighe, Esq. F.C.S.
Andrew Ainslie Common, Esq. F.R.S. F.R.A.S.
James Farmer, Esq. J.P.
George Herbert, Esq.
Frederick John Horniman, Esq. F.L.S.
Thomas John Maclagan, M.D.
James Mansergh, Esq. M. Inst. C.E.
John W. Miers, Esq.
Lachlan Mackintosh Rate, Esq. M.A.
Benjamin Ward Richardson, M.D. LL.D. F.R.S.
George John Romanes, Esq. M.A. LL.D. F.R.S.
Arthur William Rücker, Esq. M.A. F.R.S.
Joseph Wilson Swan, Esq.
Thomas Edward Thorpe, Esq. Ph.D. F.R.S.

WEEKLY EVENING MEETING,

Friday, May 1, 1891.

Sir James Crichton Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

James Edmund Harting, Esq. F.L.S. F.Z.S.

Hawks and Hawking.

The result of many years' experience has been to convince me that the art of Falconry (as all the old writers term it), that is, the art of taming and training birds of prey for the chase, and teaching them to exercise their natural instinct for our amusement and benefit, is really a noble art; and that the power which has been given to man to exert "dominion over the fowls of the air," when properly exercised, is the greatest and most wonderful form of control which can be exerted by man over the lower animals.

On looking into the literature of the subject, and it is pretty extensive, comprising more than 300 volumes in fourteen or fifteen languages, two points are particularly striking:—First, the great antiquity of Falconry; and, secondly, its wide-spread practice.

In the East, Falconry has been traced back to a period long anterior to the Christian era, and we may form some idea of its antiquity from Sir Henry Layard's discovery of a bas-relief amongst the ruins of Khorsabad, in which a falconer is represented carrying a hawk upon his fist. From this it is to be inferred that hawking was practised there some 1700 years B.C.

In China it was known even at an earlier date than this, for in an old Japanese work, of which a French translation appeared at the beginning of the present century, it is stated that falcons were amongst the presents made to princes in the time of the Hia dynasty, which commenced in the year 2205 B.C.

It would occupy too much time on the present occasion to discuss the origin of Falconry, on which a very great deal might be said. Suffice it to remark that from the East it was introduced into Europe, and from Europe, long afterwards, into England.

On looking into the history of Falconry in Europe, one figure of a great falconer in the middle ages stands out prominently, namely, the Emperor Frederick II. of Germany, who died in 1250. He had seen something of hawking in the East, and in 1239, on his return from a Crusade which he had undertaken the year before, when he was crowned King of Jerusalem and Sicily, he brought with him, from Syria and Arabia, several expert falconers with their hawks, and spent much of his leisure time in learning from them the secret of their art, which he considered the noblest and most worthy of all the arts. The excellent treatise which he composed in Latin, 'De arte venandi cum Avibus,' was the first which appeared in the West, and is still one of the best which exists.

In the Middle Ages the Germans were great falconers; so also were the French, and the natives of Brabant, of whom a celebrated Spanish falconer in 1383 wrote that they were the best falconers in the world. To a less extent the art was practised in Spain and Italy during many centuries, and books were written in all these countries, by those who had become proficient in the art and were fired with the enthusiasm of their success. The kings of Norway and Denmark preferred hunting to hawking, but rendered good service to the sister sport by procuring, from various parts of Scandinavia, the celebrated gerfalcons of Northern Europe, which were held in the highest esteem by those to whom they were sent as presents.

Although the precise date of the introduction of hawking into England cannot now be ascertained, we know, from several sources, that it was practised by our ancestors in early Saxon times. In a letter addressed by King Ethelbert (A.D. 748–760) to Boniface, Archbishop of Mayence, who died in 755, the monarch asked him to send over two falcons that would do to fly at the crane, for, said he "there are very few birds of use for that flight in this country," i.e. Kent. Asser, in his Life of Alfred the Great, particularly refers to the king's love of hawking; and William of Malmesbury records much the same of Athelstan, who procured his hawks from Wales. The same historian says of Edward the Confessor, that his chief delight was to follow a pack of swift hounds and cheer them with his voice, or to attend the flight of hawks taught to pursue and catch their kindred birds.

So general, indeed, was the pastime of hawking in Saxon times, that the monks of Abingdon found it necessary to procure a charter from King Kenulph to restrain the practice in harvest time, in order to prevent their lands from being trampled upon.

One of the most interesting pieces of documentary evidence on this part of the subject is preserved in the MS. department of the British Museum. I refer to the "Colloquy" of Archbishop Ælfric, a composition of the 10th century. The object of this and similar colloquies and vocabularies compiled about the same period was to interpret Latin to the Anglo-Saxon student, and furnish him with the Latin words for the common objects of life. In this MS. we find a dialogue between a scholar and a falconer, in which the latter imparts some interesting details on the subject of his art.*

Hawking was pursued by most of our early English kings with the greatest enthusiasm, and a long account might be furnished of

---

* This dialogue will be found printed in my 'Introduction' to 'A perfecte Booke for kepinge of Sparhawkes or Goshawkes,' written about 1575. Sm. 4to, 1886.

their doings in the hawking field, but a few examples only must suffice.

In October 1172, Henry II. was at Pembroke *en route* for Ireland, and there amused himself with hawking. On one occasion (says Giraldus Cambrensis) he saw a wild falcon perched upon a crag, and had a mind to try a flight at it with a large Norway hawk which he carried. The wild falcon, however, having mounted above the king's bird, stooped at it, and struck it down, to the king's great vexation. He, however, recognised the excellence of the Pembrokeshire Peregrines, and from that time, according to the chronicler, he used to send every year, at the proper season, for young falcons from the cliffs of South Wales.

Richard Cœur-de-Lion, while in the Holy Land, used to amuse himself with hawking at Jaffa, in the plain of Sharon.

King John used to send to Ireland for his hawks; amongst other places to Carrickfergus, co. Antrim, and was especially fond of a flight at the crane with gerfalcons. It appears, by entries in the Court Rolls of payments of the expenses of the journeys, that he took cranes with his hawks in Cambridgeshire, Lincolnshire, Dorset, and Somerset.

In the wardrobe accounts of Edward I., preserved in the British Museum (Add. MSS., No. 7965, Ed. I., 1297–8), is an entry of a payment of a reward to the king's falconer, for presenting three cranes taken with gerfalcons in Cambridgeshire.

These entries serve not only to illustrate the history of hawking in England, but are interesting as proving the former existence of the crane in this country in sufficient numbers to be flown at when required.

Henry VIII.'s love of hawking is well known from the anecdote related of him in Hall's Chronicle, to the effect that being one day out hawking at Hitchin, in Hertfordshire, he was leaping a dyke with a hawking pole, when it suddenly broke, and the king was immersed in mud and water, and might have lost his life had not Edmund Moody, one of the falconers, immediately come to his assistance, and dragged him out.

[A portrait of Robert Cheseman, Falconer to Henry VIII., from the painting by Holbein at the Hague, hangs upon the screen.]

During the reign of Elizabeth hawking was much in vogue, and we have here a portrait of her Grand Falconer, Sir Ralph Sadler, reproduced from an old panel portrait by Gerhardt, which hangs in the Manor House at Everley, Wilts, the former residence of Henry Sadler, the third son of Sir Ralph. This Sir Ralph Sadler was an important personage. He was Chief Secretary of State to Henry VIII., and afterwards to Queen Elizabeth, who made him her Grand Falconer, and gave him the manor, park, and warren of Everley, Wilts, on the attainder of the previous owner, the Duke of Somerset. He had charge of Mary Queen of Scots, when imprisoned in the Castle of Tutbury (1584–5), and got into trouble for taking her out hawking, and allowing her to roam too far from the castle. He died at the age of 80, in 1587, and was buried at Standon, where a noble monument is erected to his memory. [His portrait, reproduced in facsimile from the original by Gerhardt in possession of Sir John Astley, Bart., is here exhibited.]

James I., as is well known, was an enthusiastic sportsman, and especially delighted in hawking, on which amusement he spent considerable sums annually, as may be seen by the entries of payments made during this reign, printed in Devon's "Issues of the Exchequer." [His portrait, after Vandyke, is here exhibited.]

It was in this reign that Sir Thomas Monson, who succeeded Sir Ralph Sadler as Royal Falconer, was said to have given 1000*l.* for a cast of falcons—a story which has been repeatedly told in print, but which is altogether based upon a misapprehension of the facts, which are correctly stated by Sir Antony Weldon in his "Court and Character of King James," 1650; the truth being that Sir Thomas Monson spent 1000*l.* before he succeeded in getting a cast of falcons that were perfect for flying at the kite; and this he might very well have done, seeing that he would have to send to Norway or Iceland for gerfalcons. [Picture exhibited of kite-hawking with gerfalcons, from the original by Joseph Wolf in the possession of Lord Lilford.]

These were the palmy days of Falconry, when the sovereigns on both sides of the Channel were enthusiastic falconers, and when the best books on the subject were written by English and French masters of the craft. [Turbervile, Latham, and Bert; Jean de Franchières and Charles d'Arcussia.]

All the Stuarts were fond of hawking, but after the Restoration the sport ceased to be popular. The causes which led to its decline were many and various. The disastrous state of the country during the period of the civil wars naturally put an end for the time being to the general indulgence in field sports. The inclosure of waste lands, the drainage and cultivation of marshes, the great improvement in fire-arms, and particularly the introduction of shot, all contributed to lessen the interest once so universally taken in this sport. Fashion also, no doubt, had a good deal to do with the decline of hawking, for so soon as the reigning sovereign ceased to take an interest in the sport, the courtiers and their friends followed suit. Nevertheless, it never really died out, and from that time to the present it has never ceased to be practised by a few admirers of the old sport in various parts of the country, and during the last few years signs have not been wanting of its increasing popularity.

The birds used by falconers belong to two classes, the *long-winged dark-eyed falcons*, such as the Peregrine, Jerfalcon, Hobby, and Merlin, and the *short-winged yellow-eyed hawks*, such as the Goshawk and Sparrowhawk. [Stuffed specimens of all these, with their hoods, jesses, leashes, bells, &c., exhibited on a perch.]

The mode of flight of the birds belonging to these two classes, and their method of taking their prey, is quite different.

[The lecturer then described the manner of capturing, taming, and training the different kinds of hawks, and flying them at the different kinds of quarry suited to their strength and capacity, and compared the methods of the English and Dutch schools. He concluded by expressing the hope that Falconry would not be regarded as an obsolete *field sport*, but one which might still be pursued with pleasure and profit by any one minded to take it up at the present day.]

[J. E. H.]

GENERAL MONTHLY MEETING

Monday, May 4, 1891.

Sir James Crichton Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

The following Vice-Presidents for the ensuing year were announced:—

Sir Frederick Abel, K.C.B. D.C.L. F.R.S.
Sir Dyce Duckworth, M.D. LL.D.
William Huggins, Esq. D.C.L. LL.D. F.R.S.
David Edward Hughes, Esq. F.R.S.
Hon. Rollo Russell, F.M.S.
Basil Woodd Smith, Esq. F.R.A.S. F.S.A.
Sir James Crichton Browne, M.D. LL.D. F.R.S. Treasurer.
Sir Frederick Bramwell, Bart. D.C.L. F.R.S. Hon. Secretary.

Professor Edmond Becquerel, F.R.S. (of Paris),
Professor Marcellin Berthelot, F.R.S. (of Paris),
Professor Alfred Cornu, F.R.S. (of Paris),
Professor E. Mascart (of Paris),
Professor Louis Pasteur, F.R.S. (of Paris),
Professor Robert Wilhelm Bunsen, F.R.S. (of Heidelberg),
Professor H. L. F. von Helmholtz, F.R.S. (of Berlin),
Professor Rudolph Virchow, F.R.S. (of Berlin),
Professor August Wilhelm von Hofmann, Ph.D. F.R.S (of Berlin),
Professor Josiah Parsons Cooke (of Cambridge, U.S.),
Professor James Dwight Dana, LL.D. F.R.S. (of Newhaven, U.S.),
Professor J. Willard Gibbs (of Newhaven, U.S.),
Professor Simon Newcomb, F.R.S. (of Washington, U.S.),
Professor S. Cannizzaro, F.R.S. (of Rome),
Professor P. Tacchini (of Rome),
Professor Julius Thomsen, Ph.D. (of Copenhagen),
Professor Tobias Robert Thalen (of Upsal),
Professor Demetri Mendeleef, Ph.D. (of St. Petersburg),
Professor Jean C. G. de Marignac, F.R.S. (of Geneva),
Professor J. D. Van der Waals (of Amsterdam),
Professor Jean Servais Stas, F.R.S. (of Brussels),

were unanimously elected Honorary Members of the Royal Institution, in commemoration of the Centenary of the birth of Michael Faraday (born 22nd September, 1791).

Charles Davis, Esq.
John Douglas Fletcher, Esq.
Felix Semon, M.D. F.R.C.P.
Frederick Anthony White, Esq.

were elected Members of the Royal Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Great Trigonometrical Survey of India, Vols. XI. XII. XIII. 4to. 1890.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta: Rendiconti. 1° Semestre, Vol. VII. Fasc. 7. 8vo. 1891.
Agricultural Society of England, Royal—Journal, Third Series, Vol. II. Part 1. 8vo. 1891.
Asiatic Society of Bengal—Proceedings, No. 1. 8vo. 1891.
Astronomical Society, Royal—Monthly Notices, Vol. LI. No. 5. 8vo. 1891.
Bankers, Institute of—Journal, Vol. XII Parts 3, 4. 8vo. 1891.
Batavia Observatory—Rainfall in East Indian Archipelago, 1889. 8vo. 1890.
Magnetical and Meteorological Observations, Vol. XII. fol. 1890.
Bavarian Academy of Sciences—Annalen der Münchener Sternwarte, Band 1. 4to. 1890.
British Architects, Royal Institute of—Proceedings, 1890–1, No. 12. 4to.
British Museum (Natural History)—Catalogue of Fossil Fishes, Part 2. 8vo. 1891.
Catalogue of Fossil Cephalopoda, Part 2. 8vo. 1891.
Carmichael, C. H. E. Esq. (Foreign Secretary R.S.L.)—Report of Royal Society of Literature, 1889–1890. 8vo.
Chemical Industry, Society of—Journal, Vol. X. No. 3. 8vo. 1891.
Chemical Society—Journal for April, 1891. 8vo.
Cracovie, l'Academie des Sciences—Bulletin, 1891, No. 3. 8vo.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R I.—Journal of the Royal Microscopical Society, 1891, Part 2. 8vo.
East India Association—Journal, Vol. XXIII. No. 1. 8vo. 1891.
Editors—American Journal of Science for April, 1891. 8vo.
Analyst for April, 1891. 8vo.
Athenæum for April, 1891. 4to.
Brewers' Journal for April, 1891. 4to.
Chemical News for April, 1891. 4to.
Chemist and Druggist for April, 1891. 8vo.
Electrical Engineer for April, 1891. fol.
Engineer for April, 1891. fol.
Engineering for April, 1891. fol.
Horological Journal for April, 1891. 8vo.
Industries for April, 1891. fol.
Iron for April, 1891. 4to.
Ironmongery for April, 1891. 4to.
Monist for April, 1891. 8vo.
Murray's Magazine for April, 1891. 8vo.
Nature for April, 1891. 4to.
Open Court for April, 1891. 4to.
Photographic News for April, 1891. 8vo.
Public Health for April, 1891. 8vo.
Revue Scientifique for April, 1891. 4to.
Telegraphic Journal for April, 1891. fol.
Zoophilist for April, 1891. 4to.

Electrical Engineers' Institution—Journal, No. 92. 8vo. 1891.
Florence Biblioteca Nazionale Centrale—Bolletino, Nos. 127, 128. 8vo. 1891.
Franklin Institute—Journal, No. 784. 8vo. 1891.
General Steam Navigation Company—Handbook of Information. 8vo. 1891.
Geological Institute, Imperial, Vienna—Jahrbuch, Band XL. Heft 1, 2. 8vo. 1890.
Abhandlungen, Band XIV. 4to. 1890.
Verhandlungen, 1891, Nos. 2–4. 8vo.
Geological Society—Quarterly Journal, No. 186. 8vo. 1891.
Georgofili, Reale Accademie—Atti, Quarta Serie, Vol. XIV. Disp. 1ª. 8vo. 1891.
Horticultural Society, Royal—Journal, Vol. XIII. Part 1. 8vo. 1891.
Institute of Brewing—Transactions, Vol. IV. Nos. 3, 4, 5, 6. 8vo. 1891.
Iron and Steel Institute—Journal for 1890, Part 2. 8vo.
Kovalevsky, M. Edouard de (the Author)—L'Enseignement de l'Agriculture dans les écoles normales et primaires en France. 4to. 1891.
Linnean Society—Journal, Nos 187, 188. 8vo. 1891.
Manchester Geological Society—Transactions, Vol. XXI. Part 6. 8vo. 1891.
Manchester Literary and Philosophical Society—Memoirs and Proceedings, Vol. IV. No. 3. 8vo. 1890–91.
Massaroli, Guiseppe, Esq. (the Author)—Grande Inscription de Nabuchodonosor. 8vo. 1890.
Odontological Society of Great Britain—Transactions, Vol. XXIII. No. 6. New Series. 8vo. 1891.
Percival, Messrs. and Co. (the Publishers)—The Economic Review, Vol. I. No. 1. 8vo. 1891.
Pharmaceutical Society of Great Britain—Journal, April, 1891. 8vo.
Rathbone, E. P. Esq. (the Editor)—The Witwatersrand Mining and Metallurgical Review, Nos. 15, 16. 8vo. 1891.
Rio de Janeiro, Museu Nacional—Archivos do Museu Nacional, Vol. VII. 4to. 1887.
Le Museu Nacional de Rio de Janeiro par Ladislau Netto. 8vo. 1889.
Rio de Janeiro, Observatoire Imperiale de—Revista, No. 2. 8vo. 1891.
Royal Irish Academy—Transactions, Vol. XXIX. Part 15. 4to. 1891.
Royal Society of London—Proceedings, No. 298. 8vo. 1891.
Selborne Society—Nature Notes, Vol. II. No. 16. 8vo. 1891.
Society of Architects—Proceedings, Vol. III. No. 10. 8vo. 1891.
Society of Arts—Journal for April, 1891. 8vo.
St. Petersburg Academie Imperiale des Sciences—Bulletin (Nouvelle Serie), Tome II. No. 1. 4to. 1891.
Statistical Society, Royal—Journal, Vol. LIV. Part 1. 8vo. 1891.
Teyler Museum—Archives, Serie II. Vol. III. 5ᵉ Partie. 8vo. 1890.
United Service Institution, Royal—Journal, No. 158. 8vo. 1891.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1891: Heft 4. 4to.
Victoria Institute—Transactions, No. 95. 8vo. 1891.
Willing and Co. Messrs. (the Publishers)—Willing's British and Irish Press Guide 8vo. 1891.
Yorkshire Archæological and Topographical Association—Journal, Parts 43, 44. 8vo. 1891.
Zoological Society of London—Proceedings, 1890, Part 4. 8vo. 1891.

WEEKLY EVENING MEETING,

Friday, May 8, 1891.

Sir Frederick Bramwell, Bart. D.C.L. F.R.S. Honorary Secretary and Vice-President, in the Chair.

Professor W. Ramsay, Ph.D. F.R.S. *M.R.I.*

Liquids and Gases.

Almost exactly twenty years ago, on June 2nd, 1871, Dr. Andrews, of Belfast, delivered a lecture to the Members of the Royal Institution in this Hall, on "The Continuity of the Gaseous and the Liquid states of Matter." He showed in that lecture an experiment which I had best describe in his own words:—

"Take, for example, a given volume of carbonic acid at 50° Centigrade, or at a higher temperature, and expose it to increasing pressure till 150 atmospheres have been reached. In the process its volume will steadily diminish as the pressure augments; and no sudden diminution of volume, without the application of external pressure, will occur at any stage of it. When the full pressure has been applied, let the temperature be allowed to fall until the carbonic acid has reached the ordinary temperature of the atmosphere. During the whole of this operation no break of continuity has occurred. It begins with a gas, and by a series of gradual changes, presenting nowhere any abrupt alteration of volume or sudden evolution of heat, it ends with a liquid.

"For convenience, the process has been divided into two stages, the compression of the carbonic acid and its subsequent cooling. But these operations might have been performed simultaneously if care were taken so to arrange the application of the pressure and the rate of cooling that the pressure should not be less than seventy-six atmospheres when the carbonic acid has cooled to 31°."

I am able, through the kindness of Dr. Letts, Dr. Andrews' successor at Belfast, to show you this experiment, with the identical piece of apparatus used on the occasion of the lecture twenty years ago.

I must ask you to spend some time to-night in considering this remarkable behaviour; and in order to obtain a correct idea of what occurs, it is well to begin with a study of gases, not, as in the case you have just seen, exposed to high pressures, but under pressures not differing greatly from that of the atmosphere, and at temperatures which can be exactly regulated and measured. To many here to-night such a study is unnecessary, owing to its familiarity, but I will ask such of my audience to excuse me, in order that I may tell my story from the beginning.

Generally speaking, a gas, when compressed, decreases in volume to an amount equal to that by which its pressure is raised, provided its temperature be kept constant. This was discovered by Robert Boyle in 1660. (In 1661 he presented to the Royal Society a Latin translation of his book 'Touching the Spring of the Air and its Effects.') His words are:—

"'Tis evident that as common air, when reduced to half its natural extent, obtained a spring about twice as forcible as it had before; so the air, being thus compressed, being further crowded into half this narrow room, obtained a spring as strong again as that it last had, and consequently four times as strong again as that of common air."

To illustrate this, and to show how such relations may be expressed by a curve, I will ask your attention to this model. We have a piston, fitting a long glass tube. It confines air under the pressure of the atmosphere, that is, some 15 lb. on each square inch of area of the piston. The pressure is supposed to be registered by the height of the liquid in the vertical tube. On increasing the volume of the air, so as to double it, the pressure is decreased to half its original amount. On decreasing the volume to half its original amount the pressure is doubled. On again halving, the pressure is again doubled. Thus, you see, a curve may be traced, in which the relation of volume to pressure is exhibited. Such a curve, it may be remarked incidentally, is termed a hyperbola.

We can repeat Boyle's experiment by pouring mercury into the open limb of this tube containing a measured amount of air. On causing the level of the mercury in the open limb to stand 30 inches (that is the height of the barometer) higher in the open limb than the closed limb, the pressure of the atmosphere is doubled, and the volume is halved. And on trebling the pressure of the atmosphere the volume is reduced to one-third of its original amount, and on adding other 30 inches of mercury the volume of the air is now one quarter of that which it originally occupied.

It must be remembered that here the temperature is kept constant; that it is the temperature of the surrounding atmosphere.

Let us next examine the behaviour of a gas when its temperature is altered; when it becomes hotter. This tube contains a gas, air, confined by mercury, in a tube surrounded by a jacket or mantle of glass, and the vapour of boiling water can be blown into the space between the mantle and the tube containing the air, so as to heat the tube to 100°, the temperature of the steam. The temperature of the room is 17° C., and the gas occupies 290 divisions of the scale. On blowing in steam the gas expands, and on again equalising pressure it stands at 373 divisions of the scale. The gas has thus expanded from 290 to 373 divisions, i. e. its volume has increased by 83 divisions, and the temperature has risen from 17° to 100°, i. e. through 83 degrees. This law of the expansion of gases was discovered almost simultaneously by Dalton and Gay-Lussac in 1801. It usually goes by the name of Gay-Lussac's law. Now, if we do not allow the volume of the gas to increase, we shall find that the pressure will increase in the same proportion that the volume would have increased had the gas been allowed to expand, the pressure having been kept constant. To decrease the volume of the gas, then, according to Boyle's law, will require a higher initial pressure, and if we were to represent the results by a curve we should get a hyperbola, as before, but one lying higher as regards pressures. And so we should get a set of hyperbolas for higher and higher temperatures.

We have experimented up to the present with air, a mixture of two gases, oxygen and nitrogen; and the boiling-point of both of these elements lies at very low temperatures, $-184°$ and $-193 \cdot 1°$ respectively. The ordinary atmospheric temperature lies a long way above the boiling-points of liquid oxygen and liquid nitrogen at the ordinary atmospheric pressure. But it is open to us to study a gas, which, at the ordinary atmospheric temperature exists in the liquid state; and for this purpose I shall choose water-gas; in order that it may be a gas at ordinary atmospheric pressure, however, we must heat it to a temperature above 100° C., its boiling-point. This tube contains water-gas at a temperature of 105° C.; it is under ordinary pressure, for the mercury columns are at the same level in both the tube and in this reservoir, which communicates with the lower end of the tube by means of the india-rubber tubing. The temperature, 105°, is maintained by the vapour of chlorobenzene, boiling in the bulb sealed to the jacket, at a pressure lower than that of the atmosphere.

Let us now examine the effect of increasing pressure. On raising the reservoir, the volume of the gas is diminished, as usual, and nearly in the ratio given by Boyle's law; that is, the volume decreases in the same proportion as the pressure increases. But a change is soon observed; the pressure soon ceases to rise; the distance between the mercury in the reservoir and that in the tube remains constant, and the gas is now condensing to liquid. The pressure continues constant during this change, and it is only when all the water-gas has condensed to liquid water that the pressure again rises. After all gas is condensed, an enormous increase of pressure is necessary to cause any measurable decrease in volume, for liquid water scarcely yields to pressure, and in such a tube as this no measurements could be attempted with success.

Representing this diagrammatically, the right-hand part of the curve represents the compression of the gas, and the curve is, as before, nearly a hyperbola. Then comes a break, and great increase in volume occurs without rise of pressure, represented by a horizontal line. The substance in the tube here consists of water-gas in presence of water; the vertical, or nearly vertical, line represents the sudden and great rise of pressure, where liquid water is being slightly compressed. The pressure registered by the horizontal line is termed the "vapour-pressure" of water. If now the temperature were raised to 110° we should have a greater initial volume for the water-gas. It is compressible, by rise of the mercury, as before, the relation of pressure to volume being, as before, represented on the

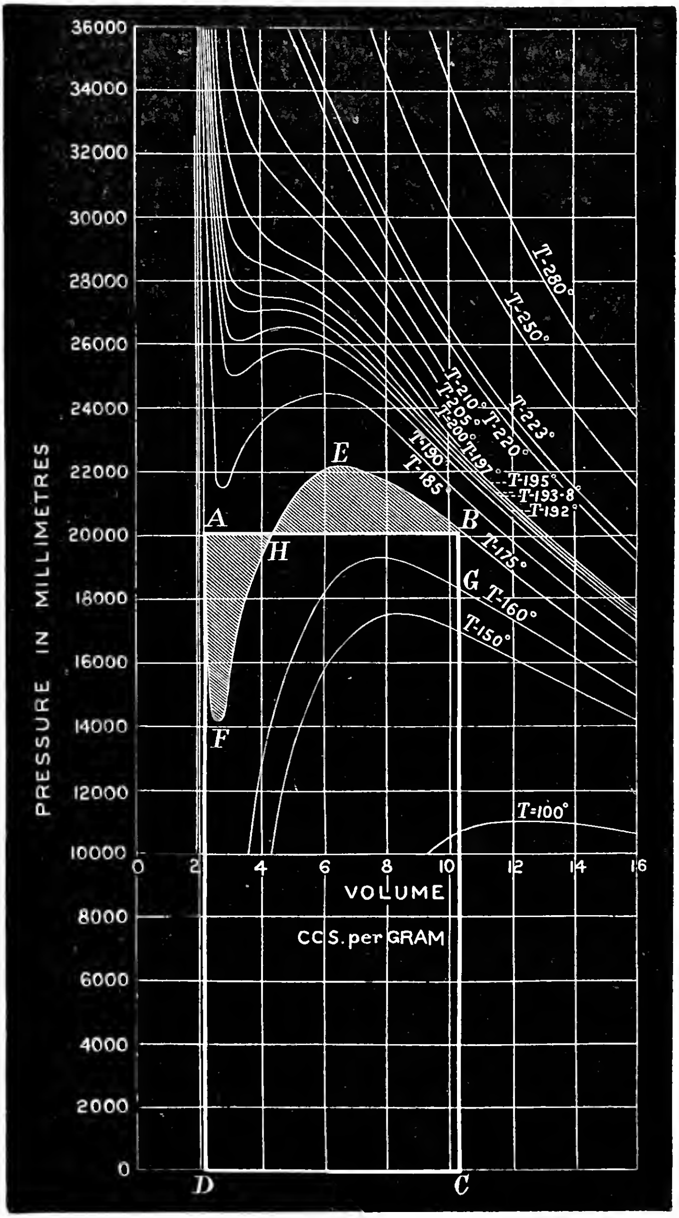


diagram as an approximate hyperbola; and, as before, condensation occurs when volume is sufficiently reduced; but this time at a higher pressure. We have again a horizontal portion, representing the pressure of water-gas at 110° in contact with liquid water; again a sharp angle, where all gaseous water is condensed; and again a very steep curve, almost a straight line, representing the slight decrease of volume of water, produced by a great increase of pressure. And should we have similar lines for 120°, 130°, 140°, 150°, and for all temperatures—such lines are called isothermal lines, or shortly, "isothermals," or lines of equal temperature, and represent the relations of pressure to volume for different temperatures. Dr. Andrews made similar measurements of the relations between the pressure and volumes of carbon dioxide, at pressures much higher than those I have shown you for water. But I prefer to speak to you about similar results obtained by Professor Sydney Young and myself with ether, because Dr. Andrews was unable to work with carbon dioxide free from air, and that influenced his results. For example, you see that the meeting-points of his hyperbolic curves with the straight lines of vapour-pressures are curves and not angles. That is caused by the presence of about 1 part of air in 500 parts of carbon dioxide; also, the condensation of gas was not perfect, for he obtained curves at the points of change from a mixture of liquid and gas to liquid. We, however, were more easily able to fill a tube with ether, free from air, and you will notice that the points I have referred to are angles, not curves.

Let me first direct your attention to the shapes of the curves in the figure, which represents such relations of volume, temperature, and pressure in the case of ether. As the temperature rises, the vapour-pressure lines lie at higher and higher pressures, and the lines themselves become shorter and shorter. And finally, at the temperature 31° for carbon dioxide, and at 125° for ether, there ceases to be a horizontal portion at all; or, rather, the curve touches the horizontal at one point in its course. That point corresponds to a definite temperature, 195° for ether; to a definite pressure, 27 metres of mercury, or 35·6 atmospheres; and to a definite volume, 4·06 cubic centimetres per gram of ether. At that point the ether is not liquid, and it is not gas; it is a homogeneous substance. At that temperature ether has the appearance of a blue mist. The striæ mentioned by Dr. Andrews and by other observers are the result of unequal heating, one portion of the substance being liquid and another gas. You see the appearance of this state on the screen.

When a gas is compressed, it is heated. Work is done on the gas, and its temperature rises. If I compress the air in this syringe forcibly, its temperature rises so high that I can set a piece of tinder on fire, and by its help explode a little gunpowder. If the ether at its critical point be compressed, by screwing in the screw, it is somewhat warmed, and the blue cloud disappears. Conversely, if it is expanded a little by unscrewing the screw, and increasing its volume, it is cooled, and a dense mist is seen accompanied by a shower of ether rain. This is seen as a black fog on the screen.

I wish also to direct your attention to what happens if the volume given to the ether is greater than the critical volume. On increasing the volume, you see that it boils away, and evaporates completely; and also what happens if the volume be somewhat less than the critical volume; it then expands as liquid, and completely fills the tube. It is only at a critical volume and temperature that the ether exists in the state of blue cloud, and has its critical pressure. If the volume be too great, the pressure is below the critical pressure; if too small, the pressure is higher than the critical pressure.

Still one more point before we dismiss this experiment. At a temperature some degrees below the critical temperature, the meniscus, i. e. the surface of the liquid is curved. It has a skin on its surface; its molecules, as Lord Rayleigh has recently explained in this room, attract one another, and it exhibits surface tension. Raise the temperature, and the meniscus grows flatter; raise it further, and it is nearly flat, and almost invisible; at the critical temperature it disappears, having first become quite flat. Surface-tension therefore disappears at the critical point. A liquid would no longer rise in a narrow capillary tube; it would stand at the same level outside and inside.

It was suggested by Professor James Thomson and by Professor Clausius, about the same time, that if the ideal state of things were to exist, the passage from the liquid to the gaseous state should be a continuous one, not merely at and above the critical point, but below that temperature. And it was suggested that the curves shown in the figure, instead of breaking into the straight line of vapour-pressure, should continue sinuously. Let us see what this conception would involve.

On decreasing the volume of a gas, it should not liquefy at the point marked B on the diagram, but should still decrease in volume on increase of pressure. This decrease should continue until the point E is reached. The anomalous state of matters should then occur, that a decrease in volume should be accompanied by a decrease of pressure. In order to lessen volume, the gas must be exposed to a continually diminishing pressure. But such a condition of matter is of its nature unstable, and has never been realised. After volume has been decreased to a certain point F, decrease of volume is again attended by increase of pressure, and the last part of the curve is continuous with the realisable curve representing the compression of the liquid above D.

Dr. Sydney Young and I succeeded by a method which I shall briefly describe in calculating the actual position of the unrealisable portions of the curve. They have the form pictured in the figure (shaded portion). The rise from the gaseous state is a gradual one; but the fall from the liquid state is abrupt.

Consider the volume 14 cubic centimetres per gram on the figure. The vertical equivolume line cuts the isothermal lines for the temperatures 175°, 180°, 185°, 190°, and so on, at certain definite pressures, which may be read from a properly constructed diagram. We can map the course of lines of equal volume, of which the instance given is one, using temperatures as ordinates and pressures as abscissæ. We can thus find the relations of temperature to pressure for certain definite volumes, which we may select to suit our convenience; say, 2 c.c. per gram, 3, 4, 5, 6, and so on. Now all such lines are straight. That is, the relation of pressure to temperature, at constant volume, is one of the simplest; pressure is a linear function of temperature measured on the absolute scale. Expressed mathematically,

$$p = b\,t - a,$$

where b and a are constants, depending on the volume chosen, and varying with each volume. But a straight line may be extrapolated without error, and so having found values for a and b for such a volume as 6 c.c. per gram, by help of experiments at temperatures higher than 195°, it is possible by extrapolation to obtain the pressures corresponding to temperatures below the critical point 195°, in a simple manner. But below that temperature the substance at volume 6 is in practice partly liquid and partly gas. Yet it is possible by such means to ascertain the relations of pressure to temperature for the *unrealisable portion* of the state of a liquid, that is, we can deduce the pressure and temperature corresponding to a continuous change from liquid to gas. And in this manner the sinuous lines on the figure have been constructed.

It is possible to realise experimentally certain portions of such continuous curves. If we condense all gaseous ether, and, when the tube is completely filled with liquid, carefully reduce pressure, the pressure may be lowered considerably below the vapour pressure corresponding to the temperature of ebullition without any change, further than the slight expansion of the liquid resulting from the reduction of pressure—an expansion too small to be seen with this apparatus. But on still further reducing pressure sudden ebullition occurs, and a portion of the liquid suddenly changes into gas, while the pressure rises quickly to the vapour-pressure corresponding to the temperature. If we are successful in expelling all air or gas from the ether in filling the tube, a considerable portion of this curve can be experimentally realised.

The first notice of this appearance, or rather, of one owing its existence to a precisely similar cause, is due to Mr. Hooke, the celebrated contemporary of Boyle. It is noted in the account of the Proceedings of the Royal Society, on November 6th, 1672, that "Mr. Hooke read a discourse of his, containing his thoughts of the experiment of the quicksilver's standing top-full, and far above the 29 inches; together with some experiments made by him, in order to determine the cause of this strange phenomenon. He was ordered to prepare those experiments for the view of the Society." And on

November 13th, "The experiment for the high suspension of quicksilver being called for, it was found that it had failed. It was ordered that thicker glasses should be provided for the next meeting."

There can be no doubt that this behaviour is caused by the attraction of the molecules of the liquid for each other. And if the temperature be sufficiently low, the pressure may be so reduced that it becomes negative—that is, until the liquid is exposed to a strain or pull, as is the mercury. This has been experimentally realised by M. Berthelot, and by Mr. Worthington, the latter of whom has succeeded in straining alcohol at the ordinary temperature with a pull equivalent to a negative pressure of 25 atmospheres, by completely filling a bulb with alcohol and then cooling it. The alcohol in contracting strains the bulb inwards, and finally, when the tension becomes very great, parts from the glass with a sharp "click."

To realise a portion of the other bend of the curve, an experiment has been devised by Mr. John Aitken. It is as follows: If air (that is space, for the air plays a secondary part) saturated with moisture be cooled, the moisture will not deposit unless there are dust-particles on which condensation can take place. It is not at first evident how this corresponds to the compressing of a gas without condensation. But a glance at the figure will render the matter plain. Consider the isothermal (175′) 75° for ether at the point marked B. If it were possible to lower the temperature to 160° without condensation, keeping volume constant, pressure would fall, and the gas would then be in the state represented on the isothermal line 160° at G; that is, it would be in the same condition as if it had been compressed without condensation.

You saw that a gas, or a liquid, is heated by compression; a piece of tinder was set on fire by the heat evolved on compressing air. You saw that condensation of ether was brought about by diminution of pressure; that is, it was cooled. Now if air be suddenly expanded it will do work against atmospheric pressure, and will cool itself. This globe contains air; but the air has been filtered carefully through cotton-wool, with the object of excluding dust-particles. It is saturated with moisture. On taking a stroke of the pump, so as to exhaust the air in the globe, no change is evident; no condensation has occurred, although the air has been so cooled that the moisture should condense, were it possible. On repeating the operation with the same globe after admitting dusty air—ordinary air from the room —a slight fog is produced, and owing to the light behind, a circular rainbow is seen; a slight shower of rain has taken place. There are comparatively few dust-particles, because only a little dusty air has been admitted. On again repeating, the fog is denser; there are more particles on which moisture may condense.

One point more and I have done. Work is measured by the distance or height through which a weight can be raised against the force of gravity. The British unit of work is a foot-pound, that is, a pound raised through one foot; that of the metric system is one gramme raised through one centimetre. If a pound be raised through two feet, twice as much work is done as that of raising a pound through one foot, and an amount equal to that of raising two pounds through one foot. The measure of work is then the weight, multiplied by the distance through which it is raised. When a gas expands against pressure it does work. The gas may be supposed to be confined in a vertical tube, and to propel a piston upwards against the pressure of the atmosphere. If such a tube has a sectional area of one square centimetre, the gas in expanding a centimetre up the tube lifts a weight of nearly 1000 grams through one centimetre (for the pressure of the atmosphere on a square centimetre of surface is nearly 1000 grams); that is, it does 1000 units of work, or ergs. So the work done by a gas in expanding is measured by the change of volume multiplied by the pressure. On the figure the change of volume is measured horizontally, the change of pressure vertically. Hence the work done is equivalent to the area on the diagram A B C D.

If liquid as it exists at A change to gas as it exists at B, the substance changes its volume, and may be made to do work. This is familiar in the steam-engine, where work is done by water expanding to steam, and so increasing its volume. The pressure does nor alter during this change of volume if sufficient heat be supplied, hence the work done during such a change is given by the rectangulat area.

Suppose that a man is conveying a trunk up to the first storey of a house, he may do it in two (or perhaps a greater number of) ways. He may put a ladder up to the drawing-room window, shoulder his trunk, and deposit it directly on the first floor. Or he may go down the area stairs, pass through the kitchen, up the kitchen stairs, up the first flight, up the second flight, and down again to the first storey. The end result is the same; and he does the same amount of work in both cases, so far as conveying the weight to a given height is concerned; because in going downstairs he has actually allowed work to be done on him by the descent of the weight.

Now the liquid in expanding to gas begins at a definite volume; it evaporates gradually to gas without altering pressure, heat being of course communicated to it during the change, else it would cool itself; and it finally ends as gas. It increases its volume by a definite amount at a definite pressure, and so does a definite amount of work; this work might be utilised in driving an engine.

But if it pass continuously from liquid to gas, the starting-point and the end point are both the same as before. An equal amount of work has been done. But it has been done by going down the area-stair, as it were, and over the round I described before.

It is clear that a less amount of work has been done on the left-hand side of the figure than was done before; and a greater amount on the right-hand side; and if I have made my meaning clear, you will see that as much less has been done on the one side, as more has been done on the other; that is, that the area of the figure B E H must be equal to that of the figure A F H. Dr. Young and I have tried this experimentally, that is, by measuring the calculated areas; and we found them to be equal. This can be shown to you easily by a simple device, namely, taking them out and weighing them. As this diagram is an exact representation of the results of our experiments with ether, the device can be put in practice. We can detach these areas which are cut out in tin, and place one in each of this pair of scales, and they balance. The fact that a number of areas thus measured gave the theoretical results, of itself furnishes a strong support of the justice of the conclusions we drew as regards the forms of these curves.

To attempt to explain the reasons of this behaviour would take more time than can be given to-night; moreover, to tell the truth, we do not know them. But we have at least partial knowledge; and we may hope that investigations at present being carried out by Professor Tait may give us a clear idea of the nature of the matter, and of the forces which act on it, and with which it acts, during the continuous change from gas to liquid.

[W. R.]

WEEKLY EVENING MEETING,

Friday, May 15, 1891.

Sir James Crichton Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

Professor G. D. Liveing, M.A. F.R.S.

Crystallisation.

There is something very fascinating about crystals. It is not merely the intrinsic beauty of their forms, their picturesque grouping, and the play of light upon their faces, but there is a feeling of wonder at the power of nature which causes substances in passing from the fluid to the solid state to assume regular shapes bounded by plane faces, each substance with its own set of forms, with faces arranged in characteristic symmetry; some, like alum, in perfect octahedra, and others, like blue vitriol, in shapes which are regularly oblique.

It is this power of nature which will be the subject of this discourse. I hope to show that crystalline forms with all their regularity and symmetry are the outcome of accepted mechanical principles. I shall invoke no peculiar force, but only such as we are already familiar with in other natural phenomena. In fact, I shall call in only the same force that produces the rise of a liquid in a capillary tube, and the surface tension at the boundary of two substances which do not mix.

Whether this force is different from gravity I shall not stop to inquire. Any attractive force which for small masses, such as we suppose the molecules of matter to be, is only sensible at insensible distances, is sufficient for my purpose.

We know that the external form of a crystal is intimately connected with its internal structure, with the chemical nature, the arrangement and the motions, of the molecules. This internal structure betrays itself in the cleavages with which every one is familiar in mica and selenite, which extend to the minutest parts, so that when calc-spar is crushed, even the dust consists of tiny rhombs. It is still better seen in the optical characters. The regular crystals, like common salt, give no double refraction, while those less regular refract doubly, and indicate different degrees of symmetry by their action on polarised light. These familiar facts suggest that it is the internal structure which determines the external form.

As a starting-point for considering that structure I assume that crystals are made up of molecules, and that in the solid state the molecules have little freedom; that they are always within the range of each other's influence, and cannot change their relative places. Nevertheless, these molecules must be in constant and very rapid motion. Not only will they communicate heat to colder bodies which touch them, but they are always radiating, which means that they are always producing waves in the ether at the rate of many billions a second. We are sure that they have a great deal of energy, and if they cannot move far they must have very rapid vibratory motion. It is reasonable to suppose that the parts of each molecule swing backwards and forwards through, or about, the centre of mass of the molecule. The average distance to which the parts swing will give the average dimensions of the molecule.

Dalton fancied that he had proved that the atoms of the chemical elements must be spherical, because there was no assignable cause why they should be longer in one dimension than another. I rather invert the argument. I see no reason why the excursions of the parts of a molecule from the centre of mass should be equal in every direction. I assume, as the most general case, that these excursions are unequal in different directions, and since the movements must be symmetrical with reference to the centre of mass, they will in general be included within an ellipsoid, of which the centre is the centre of mass.

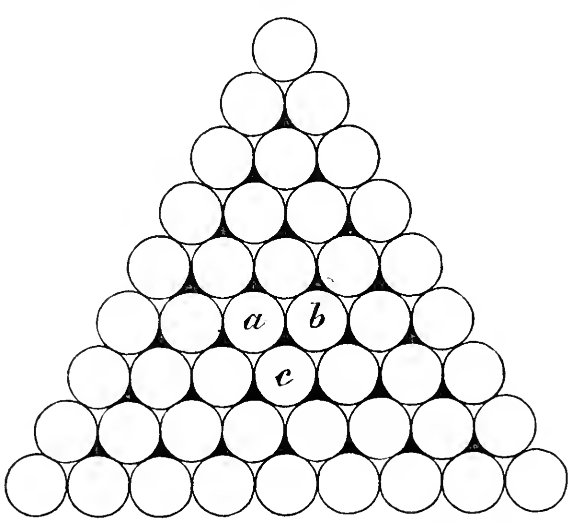
Here I may, perhaps, guard against a misconception. Chemists are familiar with the notion of complex molecules, and most of us figure to ourselves a molecule of common salt as consisting of an atom of sodium and an atom of chlorine held together by some sort of force, and it may be imagined that these atoms are the parts of the molecules which I have in mind. That, however, is not my notion. I am paradoxical enough to disbelieve altogether in the existence of either sodium or chlorine in common salt. Were my audience a less philosophical one, I could imagine the retort on many a lip—"Why, you can get sodium and chlorine out of it, and you can make it out of sodium and chlorine." But, no; you cannot get either sodium or chlorine out of it without first adding something which seems to me of the essence of the matter. You can get neither sodium nor chlorine from it without adding energy. Nor can you make salt out of those elements without subtracting energy. My point is that the energy is of the essence of the molecule. Each kind of molecule has its own kind of motion; and in this I think most physicists will agree with me. The chemists will agree with me in thinking that all the molecules of the same element or compound are alike in mass, and in the space they occupy at a given temperature and pressure. The only further assumption I have to make is that the form of the ellipsoid, the relative length of its axes, is on the average the same for all the molecules of the same substance. This implies that the distances of the excursions of the parts of the molecule depend upon its constitution, and are, on the average, the same in similarly constituted molecules under similar circumstances.

I have now come to the end of my postulates. I hope they are such as you will readily concede. I want you to conceive of each molecule that its parts are in extremely rapid vibration, so that it occupies a larger space than it would occupy if its parts were all at rest; and that the excursions of the parts about the centre of mass are, on the average, at a given temperature and pressure, comprised within a certain ellipsoid: that the dimensions of this ellipsoid are the same for all molecules of the same chemical constitution, but different for different kinds of molecules.

We have next to consider how these molecules will pack themselves in passing from the fluid state, in which they can and do move about amongst themselves, into the solid state, in which they have no sensible freedom. If they attract one another according to any law, and for my purpose gravity will suffice, then the laws of energy require that for stable equilibrium the potential energy of the system shall be a minimum. This is the same, in the case we are considering, as saying that the molecules shall be packed in such a way that the distance between their centres of mass shall be the least possible, or as many of them as possible be packed into a given space.

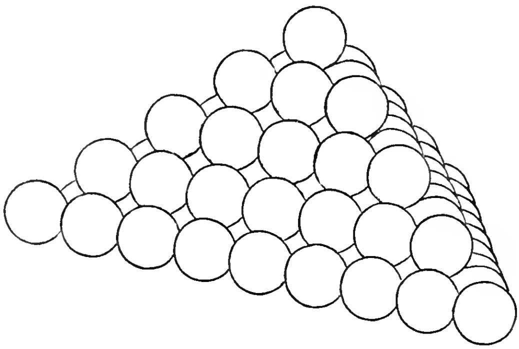
In order to see how this packing will take place, it will be easiest to consider the case in which the axes of the ellipsoids are all equal—that is, when the ellipsoids happen to be spheres. The problem is then reduced to finding how to pack the greatest number of equal spherical balls into a given space. It is easy to reduce this problem to that of finding how the spheres can be arranged so that each sphere shall be touched by as many as possible of its neighbours. In this way the cornered spaces between the spheres, the spaces not occupied, are reduced to a minimum. Now, you can arrange balls so that each is touched by twelve others, but not by more than twelve. This, then, will be the arrangement which the molecules will naturally assume.

FIG. 1.



We may do this apparently in two ways. We may begin with arranging balls on a flat surface so that each is touched by six others, as in Fig. 1. We may then place a ball so that it rests on three, *a*, *b*, *c*, in the figure, and may place six others, touching it, and resting in the six adjacent triangular spaces which are black in the figure. Above these we can again place three more so as to touch the first. If we complete the pile we get a triangular pyramid, as in Fig. 2. Or we

FIG. 2.



may begin by arranging balls on the flat, as in Fig. 3, so that each is touched by four others. We may then place one ball so as to rest on four, such as *a*, *b*, *c*, *d*, in the figure. Then place four others, touching it, in the four adjacent square-shaped openings which are shaded in the figure. Above these, in places corresponding to *a*, *b*, *c*, *d*, four more may be placed so as to touch the first. If the pile be completed it will form a four-sided pyramid, as in Fig. 4.

FIG. 3.

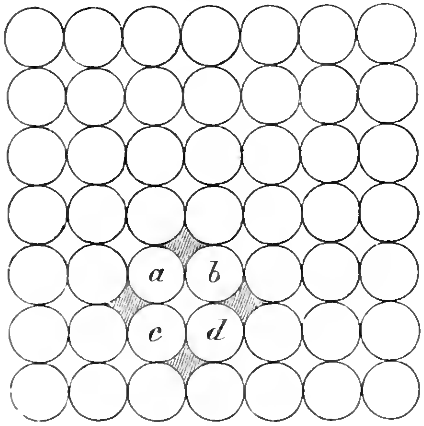
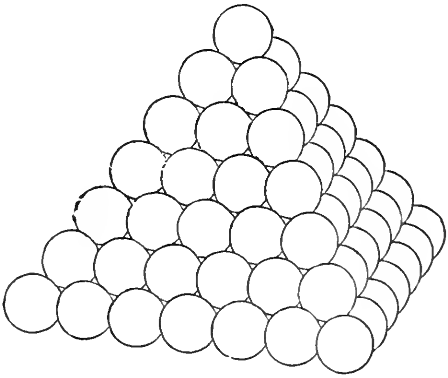


FIG. 4.



Although this arrangement seems at first sight different from that in Fig. 2, it is not so; for it will be seen that in the faces of the pyramid of Fig. 2 the arrangement is that of Fig. 3, while in the faces of the pyramid in Fig. 4 the arrangement is that of Fig. 1. Fig. 2 is really part of Fig. 4 turned over on its side.

Before proceeding to the packing of ellipsoids, let us consider how this packing of the spheres will affect the external form. And here I must bring in the surface tension. We are familiar with the effects of this force in the case of liquids, and if we adopt the usual theory of it we must have a surface tension at the boundary of a solid as well as at the surface of a liquid. I know of no actual measures of the surface tension of solids. But Quincke has given us the surface tensions of a number of substances at temperatures near their points of solidification. The surface tension of most of the solids are probably greater than these, since surface tension usually diminishes with increase of temperature.

TABLE OF SURFACE TENSIONS OF SUBSTANCES near their Temperatures of Solidification, in dynes per lineal centimetre, after Quincke.

Platinum	1,658	Antimony	244
Gold	983	Borax	212
Zinc	860	Sodium carbonate	206
Tin	587	Sodium chloride	114
Mercury	577	Water	86·2
Lead	448	Selenium	70·4
Silver	419	Sulphur	41·3
Bismuth	382	Phosphorus	41·1
Potassium	364	Wax	33·4
Sodium	253		

We have evidently to do here with an agency which we cannot neglect. In all these cases the measured tension is at a surface bounded by air, and is such as tends to contract the surface. We have then at the boundary between a crystallising solid and a fluid, gas or liquid, out of which it is solidifying, a certian amount of potential energy; and by the laws of energy the condition of equilibrium is that this potential energy shall be a minimum. The accepted theory of surface tension is that it arises from the mutual attractions of the molecules. The energy will therefore be a minimum for a surface in which the molecules are as closely set as possible.

Now if you draw any surface through a heap of spherical balls arranged so that each is touched by twelve others, you will find that the surfaces which have the greatest number of centres of the balls in unit area are all plane surfaces; and those for which the concentration is greatest are the faces of a regular octahedron, next those of a cube, next those of a rhombic dodecahedron, and so on for the other planes which follow the crystallographic law of indices. Taking the concentration in the faces of the cube as unity, those of other forms will be

Octahedron	1·1547	Eikositessarahedron	·4083
Dodecahedron	·7071	Triakisoctahedron	·3333
Tetrakishexahedron	·4472		

It must not be supposed that these figures give the surface energies. We have at present no means of determining the exact magnitudes of the surface energies. What we can assert is that greater concentration means in general less surface energy.

Hence when the molecules are spherical the bounding surface *tends* to be that of a regular octahedron.

But we have another point to consider. Since the solid must be a closed figure, there will be edges where the bounding planes meet each other. At these edges the surface tensions will have a resultant tending to compress the crystal, and there must be a corresponding resultant pressure on the opposite side. It follows from this that if one pair of faces are developed on one side of a crystal a parallel pair must in general be developed on the opposite side, and if one face of a form, be it cube, octahedron, or other form, be developed, all the faces of that form will, as a rule, be developed.

But there is yet another point to be taken into account. The surface energy may become less in two ways; one by reducing the tension per unit of surface, and the other by reducing the total surface for the same quantity of matter. When a liquid separates from another liquid, as chloroform from a solution of chloral hydrate by adding an alkali, or a cloud from moist air, the liquid assumes the form which for a given mass has the least surface, that is the drops are spherical. If you cut off the projecting angles, and plane away the projecting edges of a cube or octahedron, you bring it nearer to a sphere, and diminish the surface per unit volume. And if diminution of the total surface is not compensated by the increase of the surface energy on the truncations, there will be a tendency for the crystal to grow with such truncations. The like will be true in more complicated combinations. There will be a tendency for such combinations to form provided the surface energy of the new faces is not too great as compared with that of the first formed faces.

But it does not always happen that an octahedron of alum developes truncated angles. This leads to another point. To produce a surface in a homogeneous mass requires a supply of energy, and to produce a surface in the interior of any fluid is not easy. Air may be supersaturated with aqueous vapour, or a solution supersaturated with a salt, and no cloud or crystals be formed in the interior, unless there is some discontinuity in the mass, specks of dust or something of the kind.

When solid matter separates from a solution, a certain amount of energy is available from the change of state, and supplies the surface energy of the new solid. The amount of this available energy is proportional to the mass of solid separated. But since the surface varies as the square of the diameter, while the mass varies as the cube of the diameter, the amount of energy available when the mass is very minute may be insufficient. In fact, a very small mass of solid might be squeezed into liquid again by its own surface energy. It will be easier to add to a surface already formed, even if that surface be one of less energy than that of the new solid, than it is to break the continuity of the fluid. Hence we find that crystals often form on the side of the vessel, or at the top where the liquid meets the air. But it is easiest of all to add to a surface of the same energy as that of the crystal. The additional energy required will then be only for the extension of the surface. This explains why dropping a crystal into a supersaturated solution starts crystallisation. Large crystals grow more readily than small ones because the extension of surface, that is the addition of energy, for a given addition of mass is less in the former. Also it is easier to add to the faces already formed than to develop new faces.

While speaking of the difficulty of creating a new surface in the interior of a mass, the question of cleavage suggests itself. It is plain that in dividing a crystal we create a new surface on each of the two parts, each with its own surface energy. The division must therefore take place most readily where that surface energy is a minimum. Hence I infer that the principal cleavage of a crystal made up of molecules for which the vibrations are comprised within spherical spaces will be octahedral. As a fact, we find that the greater part of substances which crystallise in what is called the regular system, have an octahedral cleavage. But not all; there are some which have a cubical cleavage such as rock salt and galena, and a very few like blende have the principal cleavage dodecahedral. These I have to explain.

I may, however, first observe that some substances, like fluor spar, which have a very distinct octahedral cleavage, are rarely met with in octahedral crystals, but usually in the cubic form. In regard to this we must remember that the surface energy depends upon the nature of both the substances which meet at the common surface, their electrical state, their temperature, and other circumstances. It is a well known fact that the form assumed by a salt on crystallising is affected by the character of the solution. Thus, alum, which from a solution in pure water takes the octahedral form, from a solution neutralised with potash takes the cubic form. It is therefore quite possible that, under the circumstances in which the natural crystals of fluor spar were formed, the surface energy of the cubical faces was less than that of the octahedral, although when we experiment upon them in the air it is the other way. The closeness of the molecules in the surface of the solid will determine the surface energy so far as the solid alone is concerned; but though this may be the most important factor of the result, the molecules of the fluid in contact with the crystal have their effect too.

But to return to cubic and dodecahedral cleavages. If we suppose the excursions of the parts of the molecule to be greater in some one direction than in the others, the figure within which the molecule vibrates will be a prolate spheroid; if it be less, an oblate spheroid. Now if such spheroids be packed as close as possible, each can be touched by twelve others, and they can be packed just as the spheres were, provided their axes be all parallel. It matters not what the orientation of the axes may be so far as the closeness of packing goes, so long as their parallelism is maintained; but the orientation will affect very much the symmetry of the crystal.

If we suppose the spheroids to be oblate, and arrange them as in Figure 1, with their axes perpendicular to the plane of that figure, and place the next layer in those triangular openings which are white in the figure, and complete the pyramid, the magnitude of the three angles at the apex of the pyramid will depend on the relative flatness of the spheroids. In case the length of the axis of the spheroids is half their greatest diameter, these three angles will be right angles, and the whole heap of molecules will have a cubic symmetry, and in the faces of the cubes the concentration will be a maximum, and therefore the surface energy a minimum, and the easiest cleavage will be cubic. If the concentration in the cubic faces be 1·0000, that in the octahedral faces will be 0·5774, and that in the dodecahedral 0·7071. We have here the case of crystals like rock salt and galena. Suppose, however, we start with the arrangement of Figure 3, and keep the axes perpendicular to the plane of that figure; and suppose, further, that the biggest diameter of the spheroids is greater than the length of the axis in the ratio of the diagonal to the side of a square, we shall again get a heap with a cubic symmetry; but in this case the maximum concentration will be in the faces of the dodecahedron, and we have the case of blende in which the easiest cleavage is dodecahedral.

In order to see what the symmetry will be in other cases, we may look at the problem from another point of view. Suppose a cube made up of spherical molecules to be subject to a uniform stress perpendicular to one face of the cube, so that all the spheres are strained, either by extension or compression, into spheroids, we should get that diagonal of the octahedron which was parallel to the stress either lengthened or shortened, but the symmetry about that diagonal would remain as before. We should get a crystal of the pyramidal system. If the spheroids were prolate and sufficiently elongated, the easiest cleavage would be perpendicular to the axis as in potassium ferrocyanide and apophyllite. If the spheroids were oblate the fundamental octahedron would be more obtuse, and if obtuse enough the easiest cleavage would be in faces parallel to the axis of symmetry.

Again, if the stress, instead of being perpendicular to one face of the cube, were parallel to a diagonal of the cube, the cube would become a rhombohedron, and the spheres would become spheroids with their axes parallel to the axis of the rhombohedron. If the spheroids were prolate the rhombohedron would be acute, and the easiest cleavage perpendicular to the axis as we find it in beryl and many other crystals. If the original cube were formed of spheroids with their axes half the length of their greatest diameters, and the stress parallel to the axes were such as to alter the length of the axes only a little, we should get crystals with a rhombohedral cleavage like calcite. The crystals like beryl almost always exhibit hexagonal forms, six-sided prisms and pyramids. To explain this I would observe that if we start with spheroids arranged as in Figure 1, with their axes perpendicular to the plane of that figure, and place three others touching that marked a; there are two ways in which we can do this. We may place the three either in the white or in the black triangles. The two positions differ in such a way that you pass from one to the other by turning the three spheroids as a whole through 180°. The relation is that of twin crystals. If a crystal were growing by addition to the face which we suppose represented in Figure 1, it would be as likely that one arrangement should be taken as the other so far as the middle part of the face is concerned. But a crystal built up of such alternate layers of twins would have ridged and furrowed faces, that is faces of extra surface-tension, except in the case of hexagonal forms. For hexagonal forms are no way altered by being turned through two right angles. There will therefore be a tendency for such forms to grow unless the rhombohedral faces have a much less surface energy. Hexagonal forms have also less surface per unit of volume than rhombohedrons, and lend themselves to the formation of nearly globular crystals, with a minimum of total surface, as is often seen in pyromorphite.

Recurring to the cube of spheres, if it be subject to a stress in a direction not parallel to an edge or diagonal, we shall get an arrangement of spheroids which will give forms of less symmetry. Also if it be subject to two uniform stresses at right angles to one another the spheres will become ellipsoids and may be taken to represent the molecules in the most general case. The degrees of symmetry, and the directions of most easy cleavage, may be worked out on the lines already indicated, and will be found to correspond with those observed in nature.

Bravais long ago suggested arrangements of the molecules corresponding to the symmetry, and Sohncke has extended his suggestions, but neither has assigned any mechanical reason why the molecules should so arrange themselves. They also supposed different arrangements for different kinds of symmetry. I have endeavoured to give a sufficient reason for the positions taken by the molecules and to show that out of the one arrangement by which the molecules are packed as closely as is possible all the varieties of symmetry will arise.

M. Curie also has, before me, pointed out that differences of surface tension will determine the relative sizes of different faces; but he has not pointed out that the same principle determines that the faces shall be planes, and that similar edges and angles shall be similarly modified, or that the law of indices in the relations of different forms is a direct consequence of it.

We are able now, I think, to understand the interesting facts brought forward by Prof. Judd in a discourse which he delivered at the Royal Institution in the early part of this year.

It does not matter how long a crystal has been out of the solution or vapour in which it was formed, the surface tension remains the same, and it must grow on its old faces if replaced in the same medium.

Also if it have any part broken off, the tension of the broken surface will, if it be not a cleavage face, be greater than on a face of the crystal, and in growing, the laws of energy necessarily cause it to grow in such a way as to reduce the potential energy to a minimum, i. e. to replace the broken surface by the regular planes of less surface energy.

The formation of what have been called "negative crystals" by fusion in the interior of a mass, is due to the same principle. If the mass is crystalline in structure the surfaces of least energy will be most easily produced in the inside as well as on the outside.

We see a very similar result in the development of crystalline form by the action of solvents, as of acids on metals. The substance acted on must be crystalline in its molecular arrangement internally, though its external figure may have been derived from the shape of the vessel or other cause. If this is the case, and if the acid is not so strong as to dissolve the metal rapidly, there must be a tendency for those parts of the surface for which the energy is greatest to be most easily removed. The result is to leave a crystalline form with surfaces of minimum energy, as we see in Widmanstatt figures, a tin plate acted on by dilute aqua regia, and many such cases.

In fact, the solution of solids in liquids is very closely and directly connected with the surface tension. One of many facts connected with crystallisation is that the same substance in one crystalline form may be soluble in a liquid in which it is not soluble when it has another crystalline form. It is probably the low surface energy of one form of crystals of sulphur which makes them insoluble in carbon disulphide, and this low surface energy may be an electric effect. It is not difficult to understand that the same molecules may give rise to crystals of different degrees of symmetry, according to the orientation of the axes, and the orientation of the axes may very well depend on the distribution of the mass within the molecule, or the molecules may in one case contain a greater number of chemical atoms than in the other. With different crystalline forms of the same kind of substance we shall in general have different surface energies, and a surface of great energy will be attacked by a solvent when one of less energy will resist it.

I pointed out that the law of symmetry, the development of all the faces of any form, and the similar modification of all corresponding edges and angles, is in general necessary in order to give equilibrium under the action of the surface tensions. But we often find crystals with only half the modifications required for symmetry. In such cases the surface tensions must produce a stress in the interior tending to deform the crystal. When the crystal was in process of formation there was necessarily equilibrium, and there must have been a pressure equal and opposite to this effect of the surface tension. There are various ways in which we may suppose that such a force would arise; for instance, the electric field might produce a stress in opposition to the aggregation of the molecules in the closest possible way, and then the crystal would develop such faces as would give rise to an equal and opposite stress. The presence of molecules of some other material, different from those forming the bulk of the crystal, might cause a similar effect, so might inequalities of temperature.

In the case of an electric stress, or one due to inequalities of temperature, when the electric stress, or the inequality of temperature was removed, the crystal would be left with an internal strain, because the stress due to the want of symmetry must be met by an equal pressure.

Crystals of this sort generally betray the internal strain, either by developing electricity of opposite kinds at the two ends when they are heated, or cooled; or they affect polarised light, rotating the plane of polarisation. That these effects are really due to the state of internal strain is proved because tourmalines, and other crystals, which are pyro-electric when unsymmetrical, show no such property when symmetrically grown; and sodium chlorate when in solution, quartz when fused, and so on, lose their rotatory power. On the other hand there are many substances which in solution show a rotatory power, and as a rule such substances produce unsymmetrically developed crystals. This is well seen in the tartrates. The constitution of the molecules must naturally be such that they will not, without some strain, form crystals, and equilibrium in the crystal is attained by the opposing stress arising from want of symmetry in the surface tensions. In all such crystals the rotatory power disappears either in whole or in part when the substance crystallises. It is impossible however, in biaxal crystals to tell whether there is rotation or not. According to Des Cloizeaux the only crystal formed from a liquid having rotatory power, which shows rotation in the solid state, is strychnine sulphate. This substance forms crystals like prussiate of potash, double square pyramids with the two apices truncated. Its rotatory power in the crystalline form is much stronger than in the liquid form. The crystals are not hemihedral, and the rotatory power is not due to any stress arising from want of symmetry in surface tensions. Effects more or less analogous to those due to the stress arising from unsymmetric development may be produced in crystals by external pressure. Thus a piece of rock salt, which in its natural state has no action on polarised light, when compressed in a vice will change the plane of polarisation. Also a cleavage slice of potassium ferrocyanide which is uniaxal, may, by compression, be made to give in convergent polarised light the two eyes and elliptic rings of a biaxal crystal.

Explanation of the Plate.

Fig. 4 shows half of a regular octahedron formed of a pile of spherical balls, and Fig. 5 shows part of a face of a dodecahedron produced by truncating one edge of Fig. 4. In this it is seen that in the plane of the dodecahedral face each ball is touched by only two others.

Fig. 6 shows the triangular pyramid formed of oblate spheroids, which becomes one corner of a cube when the ratio of the diameters of the spheroids is 2 : 1, but one corner of a rhombohedron if the ratio is greater or less.

Figs. 7, 8. and 9 represent halves of octahedra formed of prolate spheroids. In Fig. 7 the axes are perpendicular to the base, and the octahedron has pyramidal symmetry. In Fig. 8 the axes are parallel to one edge of the base, and the octahedron has right prismatic symmetry. In Fig. 9 the axes are in planes parallel to one edge of the base, but inclined to that edge, and the octahedron is oblique.

[G. D. L.]

Fig 4.

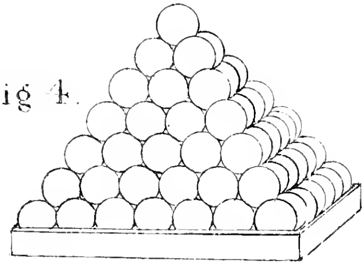


Fig 5.

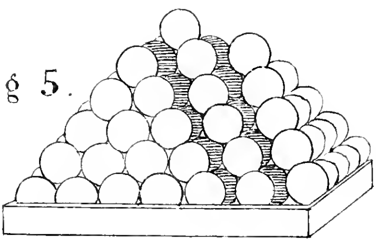


Fig 2.

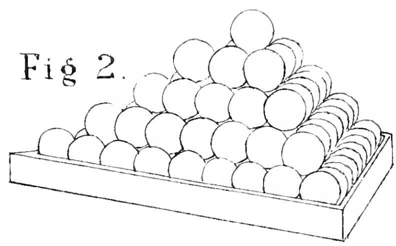


Fig 9.

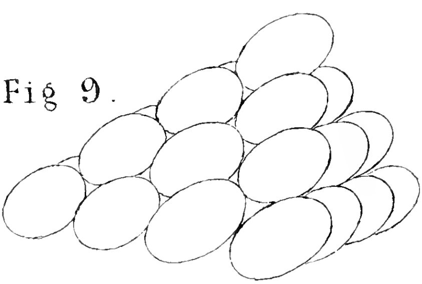


Fig 6.

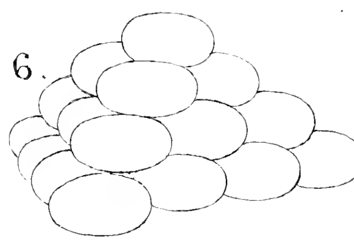


Fig 7.

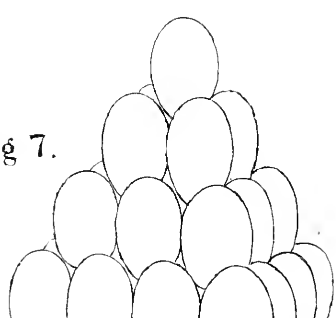
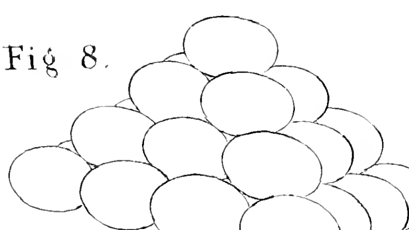


Fig 8.



WEEKLY EVENING MEETING,

Friday, May 22, 1891.

David Edward Hughes, Esq. F.R.S. Vice-President, in the Chair.

Professor J. A. Ewing, M.A. B.Sc. F.R.S.

PROFESSOR OF APPLIED MECHANICS AND MECHANISM IN THE UNIVERSITY OF CAMBRIDGE.

The Molecular Process in Magnetic Induction.

Magnetic induction is the name given by Faraday to the act of becoming magnetised, which certain substances perform when they are placed in a magnetic field. A magnetic field is the region near a magnet, or near a conductor conveying an electric current. Throughout such a region there is what is called magnetic force, and when certain substances are placed in the magnetic field the magnetic force causes them to become magnetised by magnetic induction. An effective way of producing a magnetic field is to wind a conducting wire into a coil, and pass a current through the wire. Within the coil we have a region of comparatively strong magnetic force, and when a piece of iron is placed there it may be strongly magnetised. Not all substances possess this property. Put a piece of wood or stone or copper or silver into the field, and nothing noteworthy happens; but put a piece of iron or nickel or cobalt and at once you find that the piece has become a magnet. These three metals, with some of their alloys and compounds, stand out from all other substances in this respect. Not only are they capable of magnetic induction—of becoming magnets while exposed to the action of the magnetic field—but when withdrawn from the field they are found to retain a part of the magnetism they acquired. They all show this property of retentiveness, more or less. In some of them this residual magnetism is feebly held, and may be shaken out or otherwise removed without difficulty. In others, notably in some steels, it is very persistent, and the fact is taken advantage of in the manufacture of permanent magnets, which are simply bars of steel, of proper quality, which have been subjected to the action of a strong magnetic field. Of all substances, soft iron is the most susceptible to the action of the field. It can also, under favourable conditions, retain, when taken out of the field, a very large fraction of the magnetism that has been induced—more than nine-tenths—more, indeed, than is retained by steel; but its hold of this residual magnetism is not firm, and for that reason it will not serve as a material for permanent magnets. My purpose to-night is to give some account of the molecular process through which we may conceive magnetic induction to take place, and of the structure which makes residual magnetism possible.

When a piece of iron or nickel or cobalt is magnetised by induction, the magnetic state permeates the whole piece. It is not a superficial change of state. Break the piece into as many fragments as you please, and you will find that every one of these is a magnet. In seeking an explanation of magnetic quality we must penetrate the innermost framework of the substance — we must go to the molecules.

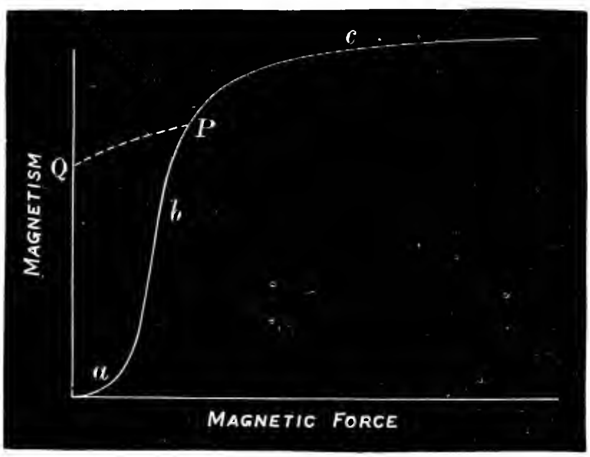
Now, in a molecular theory of magnetism there are two possible beginnings. We might suppose, with Poisson, that each molecule becomes magnetised when the field begins to act. Or we may adopt the theory of Weber, which says that the molecules of iron are always magnets, and that what the field does is to turn them so that they face more or less one way. According to this view, a virgin piece of iron shows no magnetic polarity, not because its molecules are not magnets, but because they lie so thoroughly higgledy-piggledy as regards direction that no greater number point one way than another. But when the magnetic force of the field begins to act, the molecules turn in response to it, and so a preponderating number come to face in the direction in which the magnetic force is applied, the result of which is that the piece as a whole shows magnetic polarity. All the facts go to confirm Weber's view. One fact in particular I may mention at once—it is almost conclusive in itself. When the molecular magnets are all turned to face one way, the piece has clearly received as much magnetisation as it is capable of. Accordingly, if Weber's theory be true, we must expect to find that in a very strong magnetic field a piece of iron, or other magnetisable metal, becomes *saturated*, so that it cannot take up any more magnetism, however much the field be strengthened. This is just what happens: experiments were published a few years ago which put the fact of saturation beyond a doubt, and gave values of the limit to which the intensity of magnetisation may be forced.

When a piece of iron is put in a magnetic field, we do not find that it becomes saturated unless the field is exceedingly strong. A weak field induces but little magnetism ; and if the field be strengthened, more and more magnetism is acquired. This shows that the molecules do not turn with perfect readiness in response to the deflecting magnetic force of the field. Their turning is in some way resisted, and this resistance is overcome as the field is strengthened, so that the magnetism of the piece increases step by step. What is the directing force which prevents the molecules from at once yielding to the deflecting influence of the field, and to what is that force due? And again, how comes it that after they have been deflected they return partially, but by no means wholly, to their original places when the field ceases to act?

I think these questions receive a complete and satisfactory answer when we take account of the forces which the molecules necessarily exert on one another in consequence of the fact that they are magnets. We shall study the matter by examining the behaviour of groups of little magnets, pivoted like compass needles, so that each is free to turn except for the constraint which it suffers on account of the presence of its neighbours.

But first let us see more particularly what happens when a piece of iron or steel or nickel or cobalt is magnetised by means of a field the strength of which is gradually augmented from nothing. We may make the experiment by placing a piece of iron in a coil, and making a current flow in the coil with gradually increased strength, noting at each stage the relation of the induced magnetism to the strength of the field. This relation is observed to be by no means a simple one: it may be represented by a curve (Fig. 1), and an

FIG. 1.



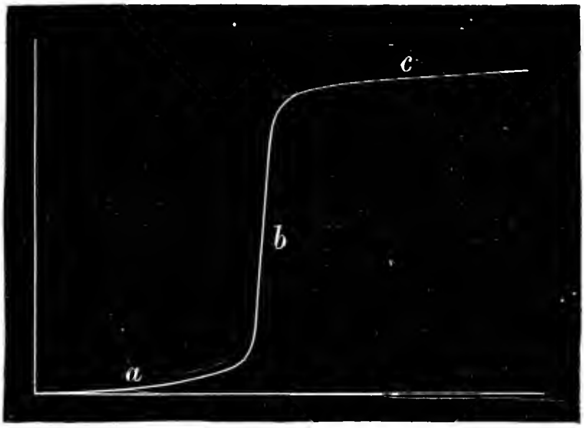
inspection of the curve will show that the process is divisible, broadly, into three tolerably distinct stages. In the first stage (*a*) the magnetism is being acquired but slowly: the molecules, if we accept Weber's theory, are not responding readily—they are rather hard to turn. In the second stage (*b*) their resistance to turning has to a great extent broken down, and the piece is gaining magnetism fast. In the third stage (*c*) the rate of increment of magnetism falls off: we are there approaching the condition of saturation, though the process is still a good way from being completed.

Further, if we stop at any point of the process, such as *P*, and gradually reduce the current in the coil until there is no current, and therefore no magnetic field, we shall get a curve like the dotted line *PQ*, the height of *Q* showing the amount of the residual magnetism.

If we make this experiment at a point in the first stage (*a*), we shall find, as Lord Rayleigh has shown, little or no residual magnetism; if we make it at any point in the second stage (*b*), we shall find very much residual magnetism; and if we make it at any point in the third stage (*c*), we shall find only a little more residual magnetism than we should have found by making the experiment at the end of stage *b*. That part of the turning of the molecules which goes on in stage *a* contributes nothing to the residual magnetism. That part which goes on in stage *c* contributes little. But that part of the turning which goes on in stage *b* contributes very much.

In some specimens of magnetic metal we find a much sharper separation of the three stages than in others. By applying strain in certain ways it is possible to get the stages very clearly separated. Fig. 2, a beautiful instance of that, is taken from a paper by Mr.

Fig. 2.



Nagaoka—one of an able band of Japanese workers who are bidding fair to repay the debt that Japan owes for its learning to the West. It shows how a piece of nickel which is under the joint action of pull and twist becomes magnetised in a growing magnetic field. There the first stage is exceptionally prolonged, and the second stage is extraordinarily abrupt.

The bearing of all this on the molecular theory will be evident when we turn to these models, consisting of an assemblage of little pivoted magnets, which may be taken to represent, no doubt in a very crude way, the molecular structure of a magnetisable metal. I have here some large models, where the pivoted magnets are pieces of sheet steel, some cut into short flat bars, others into diamond shapes with pointed ends, others into shapes resembling mushrooms or umbrellas, and in these the magnetic field is produced by means of a coil of insulated wire wound on a large wooden frame below the magnets. Some of these are arranged with the pivots on a gridiron or lazy-tongs of jointed wooden bars, so that we may readily distort them, and vary the distances of the pivots from one another, to imitate some of the effects of strain in the actual solid. But to display the experiments to a large audience a lantern model will serve best. In this one the magnets are got by taking to pieces numbers of little pocket compasses. The pivots are cemented to a glass plate, through which the light passes in such a way as to project the shadows of the magnets on the screen. The magnetic force is applied by means of two coils, one on either side of the assemblage of magnets and out of the way of the light, which together produce a nearly uniform magnetic field throughout the whole group. You see this when I make manifest the field in a well-known fashion, by dropping iron filings on the plate.

We shall first put a single pivoted magnet on the plate. So long as no field acts it is free to point anyhow—there is no direction it prefers to any other. As soon as I apply even a very weak field it responds, turning at once into the exact direction of the applied force, for there was nothing (beyond a trifling friction at the pivot) to prevent it from turning.

Now try two magnets. I have cut off the current, so that there is at present no field, but you see at once that the pair has, so to speak, a will of its own. I may shake or disturb them as I please, but they insist on taking up a position (Fig. 3) with the north end of one as close as possible to the south end of the other. If disturbed they return to it: this configuration is highly stable. Watch what happens when the magnetic field acts with gradually growing strength. At first, so long as the field is weak (Fig. 4), there is but little deflection;

FIG. 3.

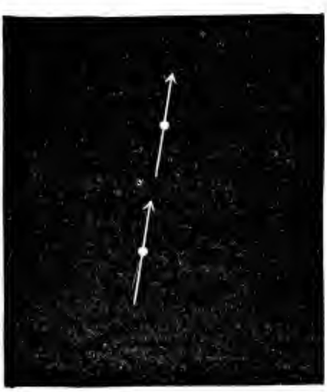
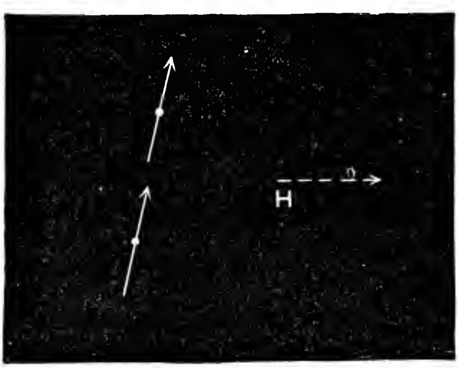


FIG 4.



but as the deflection increases it is evident that the stability is being lost, the state is getting more and more critical, until (Fig. 5) the tie that holds them together seems to break, and they suddenly turn, with violent swinging, into almost perfect alignment with the magnetic force H. Now I gradually remove the force, and you see that they are slow to return, but a stage comes when they swing back, and a complete removal of the force brings them into the condition with which we began (Fig. 3).

If we were to picture a piece of iron as formed of a vast number of such pairs of molecular magnets, each pair far enough from its neighbours to be practically out of reach of their magnetic influence, we might deduce many of the observed magnetic properties, but not all. In particular, we should not be able to account for so much residual magnetism as is actually found. To get that, the molecules must

FIG. 5.

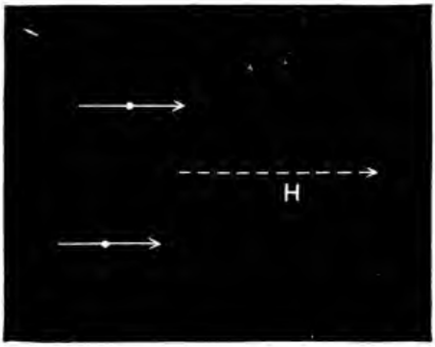
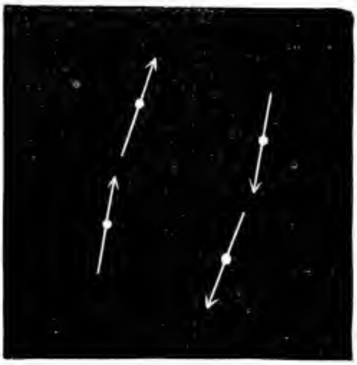


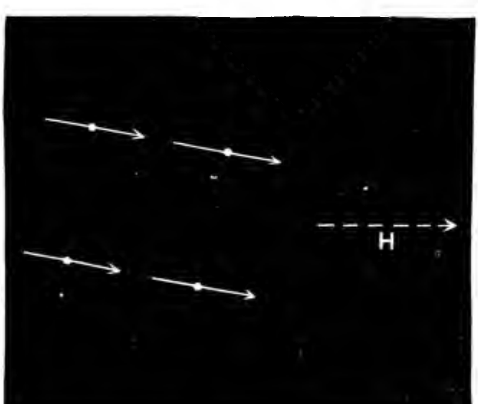
FIG. 6.



make new connections when the old ones are broken; their relations are of a kind more complex than the quasi-matrimonial one which this experiment exhibits. Each molecule is a member of a larger community, and has probably many neighbours close enough to affect its conduct.

We get a better idea of what happens by considering four magnets (Fig. 6). At first, in the absence of deflecting magnetic force, they group themselves in stable pairs—in one of a number of possible

FIG. 7.



combinations. Then—as in the former case—when magnetic force is applied, they are at first slightly deflected, in a manner that exactly tallies with what I have called the stage *a* of the magnetising process. Next comes instability. The original ties break up, and the magnets swing violently round; but finding a new possibility of combining (Fig. 7), they take to that. Finally, as the field is further strengthened

FIG. 8.

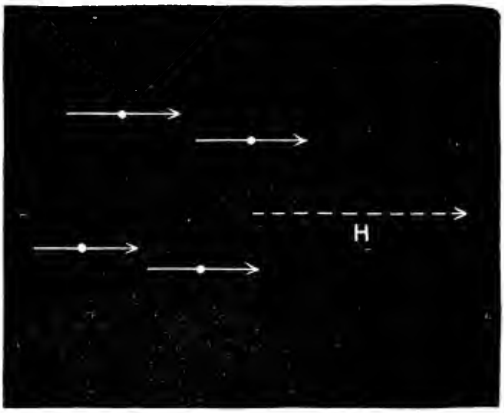
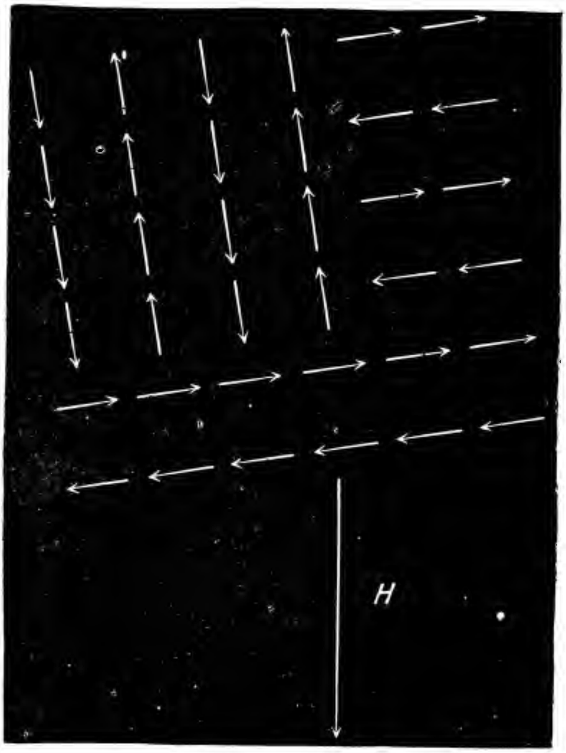


FIG. 9.



they are drawn into perfect alignment with the applied magnetic force (Fig. 8).

We see the same three stages in a multiform group (Figs, 9, 10, 11).

At first, the group, if it has been shuffled by any casual disturbance, arranges itself at random in lines that give no resultant polarity. A weak force produces no more than slight quasi-elastic deflections; a stronger force breaks up the old lines, and forms new ones more favourably inclined to the direction of the force (Fig. 10). A very strong force brings about saturation (Fig. 11).

In an actual piece of iron there are multitudes of groups lying variously directed to begin with—perhaps also different as regards the spacing of their members. Some enter the second stage while

Fig. 10.

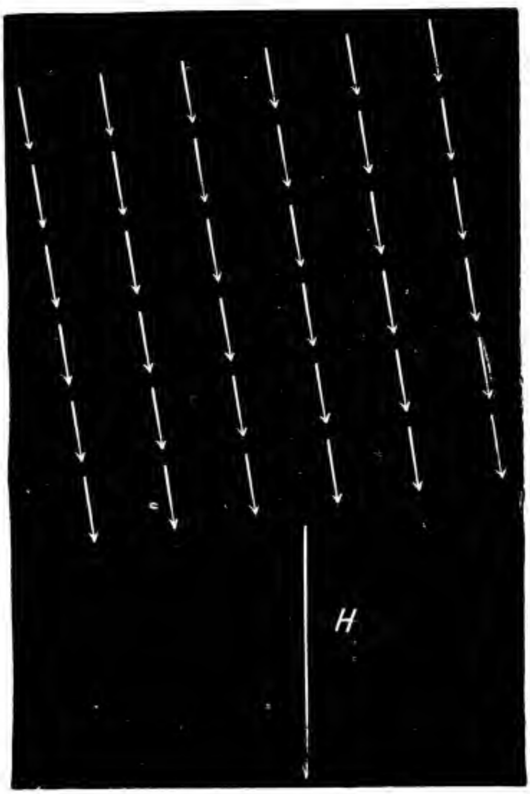
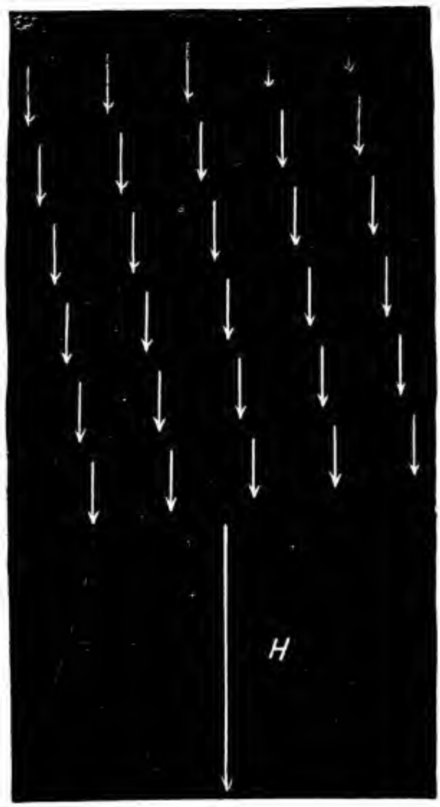


Fig. 11.

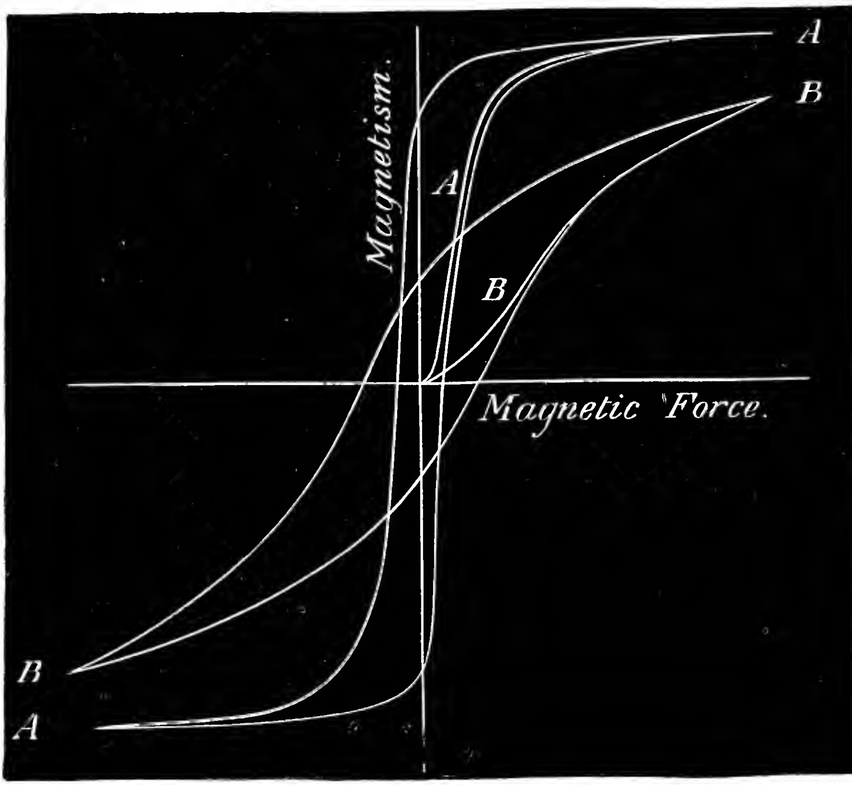


others are still in the first, and so on. Hence, the curve of magnetisation does not consist of perfectly sharp steps, but has the rounded outlines of Fig. 1.

Notice, again, how the behaviour of these assemblages of elementary magnets agrees with what I have said about residual magnetism. If we stop strengthening the field before the first stage is passed—before any of the magnets have become unstable and have tumbled round into new places—the small deflection simply disappears, and there is no residual effect on the configuration of the group. But if we carry the process far enough to have unstable deflections, the effects of these persist when the force is removed, for the magnets then retain the new grouping into which they have fallen (Fig. 10). And again, the quasi-elastic deflections which go on during the third stage do not add to the residual magnetism.

Notice, further, what happens to the group if after applying a magnetic force in one direction and removing it, I begin to apply force in the opposite direction. At first there is little reduction of the residual polarity till a stage is reached when instability begins,

FIG. 12.



Cyclic reversal of magnetisation in (A A) annealed iron wire, (B B) the same piece hardened by stretching.

and then reversal occurs with a rush. We thus find a close imitation of all the features that are actually observed when iron or any of the other magnetic metals is carried through a cyclic magnetising process (Fig. 12). The effect of any such process is to form a *loop* in the curve which expresses the relation of the magnetism to the magnetising force. The changes of magnetism always lag behind the changes of magnetising force. This tendency to lag behind is called magnetic *hysteresis.*

We have a manifestation of hysteresis whenever a magnetic metal has its magnetism changed in any manner through changes in the magnetising force, unless, indeed, the changes are so minute as to be confined to what I have called the first stage (*a*, Fig. 1). Residual magnetism is only a particular case of hysteresis.

Hysteresis comes in whatever be the character or cause of the magnetic change, provided it involves such deflections on the part of the molecules as make them become unstable. The unstable movements are not reversible with respect to the agent which produces them—that is to say, they are not simply undone step by step as the agent is removed.

We know, on quite independent grounds, that when the magnetism of a piece of iron or steel is reversed, or indeed cyclically altered in any way, some work is spent in performing the operation—energy is being given to the iron at one stage, and is being recovered from it at another; but when the cycle is taken as a whole, there is a net loss, or rather a waste of energy. It may be shown that this waste is proportional to the area of the loop in our diagrams. This energy is dissipated; that is to say, it is scattered and rendered useless: it takes the form of heat. The iron core of a transformer, for instance, which is having its magnetism reversed with every pulsation of the alternating current, tends to become hot for this very reason; indeed, the loss of energy which happens in it, in consequence of magnetic hysteresis, is a serious drawback to the efficiency of alternating-current systems of distributing electricity. It is the chief reason why they require much more coal to be burnt, for every unit of electricity sold, than direct-current systems require.

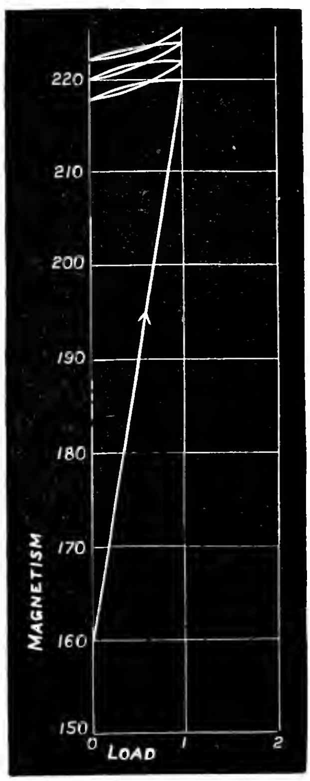
The molecular theory shows how this waste of energy occurs. When the molecule becomes unstable and tumbles violently over, it oscillates and sets its neighbours oscillating, until the oscillations are damped out by the eddy currents of electricity which they generate in the surrounding conducting mass. The useful work that can be got from the molecule as it falls over is less than the work that is done in replacing it during the return portion of the cycle. This is a simple mechanical deduction from the fact that the movement has unstable phases.

I cannot attempt, in a single lecture, to do more than glance at several places where the molecular theory seems to throw a flood of light on obscure and complicated facts, as soon as we recognise that the constraint of the molecules is due to their mutual action as magnets.

It has been known since the time of Gilbert that vibration greatly facilitates the process of magnetic induction. Let a piece of iron be briskly tapped while it lies in the magnetic field, and it is found to take up a large addition to its induced magnetism. Indeed, if we examine the successive stages of the process while the iron is kept vibrating by being tapped, we find that the first stage (*a*) has practically disappeared, and there is a steady and rapid growth of magnetism almost from the very first. This is intelligible enough. Vibration sets the molecular magnets oscillating, and allows them to break their primitive mutual ties and to respond to weak deflecting forces. For a similar reason vibration should tend to reduce the residue of magnetism which is left when the magnetising force is removed, and this, too, agrees with the results of observation.

Perhaps the most effective way to show the influence of vibration is to apply a weak magnetising force first, before tapping. If the force is adjusted so that it nearly but not quite reaches the limit of stage (*a*), a great number of the molecular magnets are, so to speak, hovering on the verge of instability, and when the piece is tapped they go over like a house of cards, and magnetism is acquired with a rush. Tapping always has some effect of the same kind, even though there has been no special adjustment of the field.

FIG. 13.



Effects of loading, unloading, and reloading a soft iron wire in a weak magnetic field.

And other things besides vibration will act in a similar way, precipitating the break-up of molecular groups when the ties are already strained. Change of temperature will sometimes do it, or the application or change of mechanical strain. Suppose, for instance, that we apply pull to an iron wire while it hangs in a weak magnetic field, by making it carry a weight. The first time that we put on the weight, the magnetism of the wire at once increases, often very greatly, in consequence of the action I have just described (Fig. 13). The molecules have been on the verge of turning, and the slight strain caused by the weight is enough to make them go. Remove the weight, and there is only a comparatively small change in the magnetism, for the greater part of the molecular turning that was done when the weight was put on is not undone when it is taken off. Reapply the weight, and you find again but little change, though there are still traces of the kind of action which the first application brought about. That is to say, there are some groups of molecules which, though they were not broken up in the first application of the weight, yield now, because they have lost the support they then obtained from neighbours that have now entered into new combinations. Indeed, this kind of action may often be traced, always diminishing in amount, during several successive applications and removals of the load (see Fig. 13), and it is only when the process of loading has been many times repeated that the magnetic change brought about by loading is just opposite to the magnetic change brought about by unloading.

Whenever, indeed, we are observing the effects of an alteration of physical condition on the magnetism of iron, we have to distinguish between the primitive effect, which is often very great and is not reversible, and the ultimate effect, which is seen only after the molecular structure has become somewhat settled through many repetitions of the process. Experiments on the effects of temperature, of strain, and so forth, have long ago shown this distinction to be exceedingly important: the molecular theory makes it perfectly intelligible.

Further, the theory makes plain another curious result of experiment. When we have loaded and unloaded the iron wire many times over, so that the effect is no longer complicated by the primitive action I have just described, we still find that the magnetic changes which occur while the load is being put on are not simply undone, step by step, while the load is being taken off. Let the whole load be divided into several parts, and you will see that the magnetism has two different values, in going up and in coming down, for one and the same intermediate value of the load. The changes of magnetism lag behind the changes of load: in other words, there is hysteresis in the relation of the magnetism to the load (Fig. 14). This is because some of the molecular groups are every time being broken up during the loading, and re-established during the unloading, and that, as we saw already, involves hysteresis. Consequently, too, each loading and unloading requires the expenditure of a small quantity of energy, which goes to heat the metal.

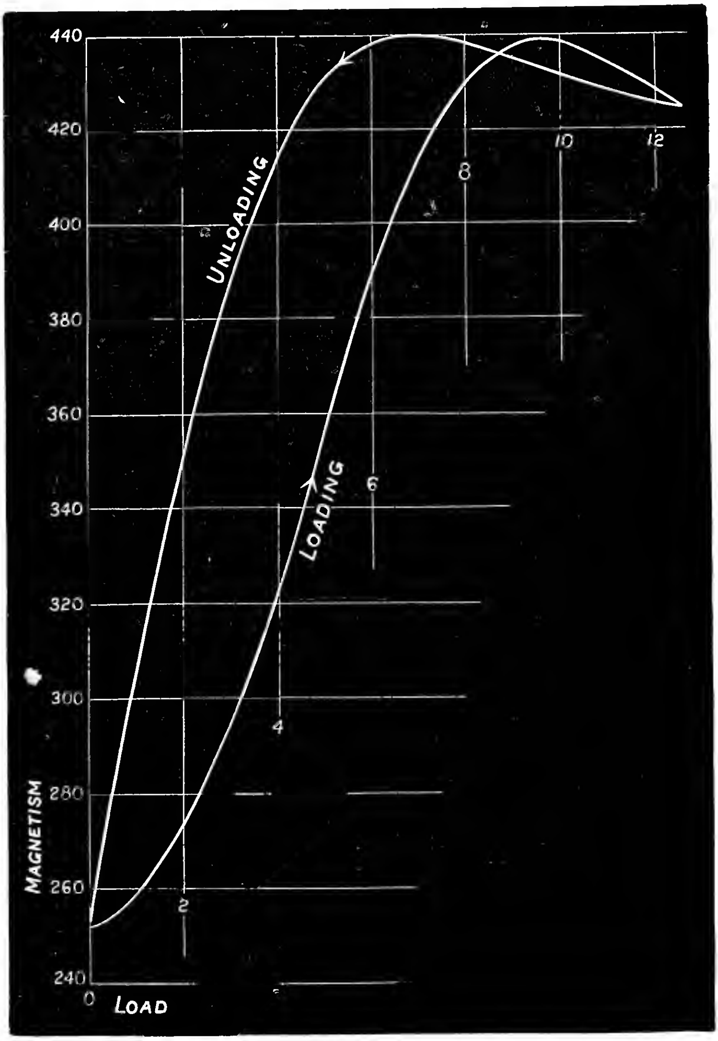
Moreover, a remarkably interesting conclusion follows. This hysteresis, and consequent dissipation of energy, will also happen though there be no magnetisation of the piece as a whole: it depends on the fact that the molecules are magnets. Accordingly, we should expect to find—and experiment confirms this*—that if the wire is loaded and unloaded, even when no magnetic field acts and there is no magnetism, its physical qualities which are changed by the load will change in a manner involving hysteresis. In particular, the length must be less for the same load during loading than during unloading so that work may be wasted in every cycle of loads. There can be no such thing as perfect elasticity in a magnetisable metal, unless, indeed the range of the strain is so very narrow that none of the molecules tumble through unstable states. This may have something to do with the fact, well known to engineers, that numerous repetitions of a straining action, so slight as to be safe enough in itself, have a dangerous effect on the structure of iron or steel.

---

* *See* Phil. Trans. 1885, p. 614.

Another thing on which the theory throws light is the phenomenon of time-lag in magnetisation. When a piece of iron is put into a steady magnetic field, it does not take instantly all the magnetism

Fig. 14.

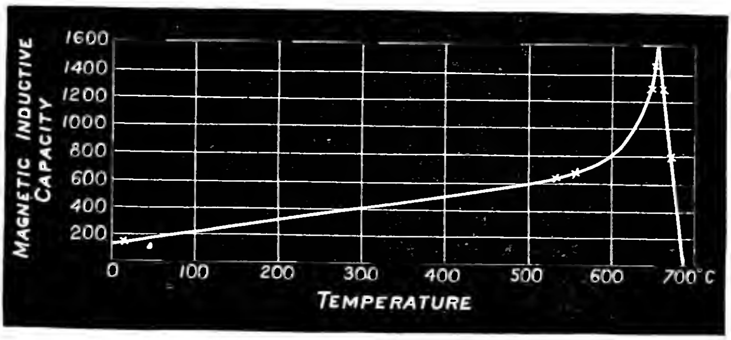


Hysteresis in the changes of magnetism produced by applying and removing load.

that it will take if time be allowed. There is a gradual creeping up of the magnetism, which is most noticeable when the field is weak and when the iron is thick. If you will watch the manner in which a group of these little magnets breaks up when a magnetic force is applied to it, you will see that the process is one that takes time. The first molecule to yield is some outlying one which is comparatively unattached—as we may take the surface molecules in the piece of iron to be. It falls over, and then its neighbours, weakened by the loss of its support, follow suit, and gradually the disturbance propagates itself from molecule to molecule throughout the group. In a very thin piece of iron—a fine wire, for instance—there are so many surface molecules, in comparison with the whole number, and consequently so many points which may become origins of disturbance, that the breaking up of the molecular communities is too soon over to allow much of this kind of lagging to be noticed.

Effects of temperature, again, may be interpreted by help of the molecular theory. When iron or steel or nickel is heated in a weak magnetic field, its susceptibility to magnetic induction is observed to increase, until a stage is reached, at a rather high temperature, when the magnetic quality vanishes almost suddenly and almost completely. Fig. 15, from one of Hopkinson's papers, shows what is observed as the temperature of a piece of steel is gradually raised. The sudden loss of magnetic quality occurs when the metal has become red-hot; the

FIG. 15.



Effects of rising temperature on the magnetic inductive capacity of steel (Hopkinson).

magnetic quality is recovered when it cools again sufficiently to cease to glow. Now, as regards the first effect—the increase of susceptibility with increase of temperature—I think that is a consequence of two independent effects of heating. The structure is expanded, so that the molecular centres lie further apart. But the freedom with which the molecules obey the direction of any applied magnetic force is increased not by that only, but perhaps even more by their being thrown into vibration. When the magnetic field is weak heating consequently assists magnetisation, sometimes very greatly, by hastening the passage from stage *a* to stage *b* of the magnetising process. And it is at least a conjecture worth consideration whether the sudden loss of magnetic quality at a higher temperature is not due to the vibrations becoming so violent as to set the molecules spinning, when, of course, their polarity would be of no avail to produce magnetisation. We know, at all events, that when the change from the magnetic to the non-magnetic state occurs, there is a profound molecular change, and heat is absorbed which is given out again when the reverse change takes place. In cooling from a red heat, the iron actually extends at the moment when this change takes place (as was shown by Gore), and so much heat is given out that (as Barrett observed) it reglows, becoming brightly red, though just before the change it had cooled so far as to be quite dull. [Experiment, exhibiting retraction and reglow in cooling, shown by means of a long steel wire, heated to redness by the electric current.] The changes which occur in iron and steel about the temperature of redness are very complex, and I refer to this as only one possible direction in which a key to them may be sought.

An interesting illustration of the use of these models has reached me, only this morning, from New York. In a paper just published in the *Electrical World*,* Mr. Arthur Hoopes supports the theory I have laid before you by giving curves which show the connection, determined experimentally, between the resultant polarity of a group of little pivoted magnets and the strength of the magnetic field, when the field is applied, removed, reversed, and so on. I shall throw these curves upon the screen, and, rough as they are, in consequence of the limited number of the magnets, you see that they succeed remarkably well in reproducing the features which we know the curves for solid iron to possess.

It may, perhaps, be fairly claimed that the models whose behaviour we have been considering have a wider application in physics than merely to elucidate magnetic processes. The molecules of bodies may have polarity which is not magnetic at all—polarity, for instance due to static electrification—under which they group themselves in stable forms, so that energy is dissipated whenever these are broken up and rearranged. When we strain a solid body beyond its limit of elasticity, we expend work irrecoverably in overcoming, as it were, internal friction. What is this internal friction due to but the breaking and making of molecular ties? And if internal friction is to be ascribed to that, why not also the surface friction which causes work to be spent when one body rubs upon another? In a highly suggestive passage of one of his writings, † Clerk Maxwell threw out the hint that many of the irreversible processes of physics are due to the breaking up and reconstruction of molecular groups. These models help us to realise Maxwell's notion, and, in studying them to-night, I think we may claim to have been going a step or two forward where that great leader pointed the way. [J. A. E.]

---

* Reprinted in the *Electrician* of May 29th, 1891.

† 'Encyc. Brit.' Art. "Constitution of Bodies."

WEEKLY EVENING MEETING,

Friday, May 29, 1891.

William Huggins, Esq. D.C.L. LL.D. F.R.S. Vice-President, in the Chair.

David Gill, Esq. LL.D. F.R.S. Her Majesty's Astronomer at the Cape of Good Hope.

An Astronomer's Work in a Modern Observatory.

The work of Astronomical Observatories has been divided into two classes, viz. Astrometry and Astrophysics. The first of these relates to Astronomy of precision, that is to the determination of the positions of celestial objects; the second relates to the study of their physical features and chemical constitution.

Some years ago the aims and objects of these two classes of observatories might have been considered perfectly distinct, and, in fact, were so considered. But I hope to show that in more recent years their objects and their processes have become so interlaced that they cannot with advantage be divided, and a fully equipped modern observatory must be understood to include the work both of *Astrometry* and *Astrophysics.*

In any such observatory the principal and the fundamental instrument is the transit circle. It is upon the position in the heavens of celestial objects, as determined with this instrument or with kindred instruments, that the whole fair superstructure of exact astronomy rests; that is to say, all that we find of information and prediction in our nautical almanacs, all that we know of the past and can predict of the future motions of the celestial bodies.

Here is a very small and imperfect model, but it will serve to render intelligible the photograph of the actual instrument which will be subsequently projected on the screen. [Here the lecturer described the adjustments and mode of using a transit circle.]

We are now in a position to understand photographs of the instrument itself. But first of all as to the house in which it dwells. Here, now on the screen, is the outside of the main building of the Royal Observatory, Cape of Good Hope. I select it simply because being the observatory which it is my privilege to direct, it is the one of which I can most easily procure a series of photographs. It was built during the years 1824–28, and like all the observatories built about that time, and like too many built since, it is a very fair type of most of the things which an observatory should not be. It is, as you see, an admirably solid and substantial structure, innocent of any architectural charm, and so far as it affords an excellent dwelling-place, good library accommodation, and good rooms for computers, no fault can be found with it. But these very qualities render it undesirable as an observatory. An essential matter for a perfect observatory should be the possibility to equalise the internal and the external temperature. The site of an instrument should also be free from the immediate surroundings of chimneys or other origin of ascending currents of heated air. Both these conditions are incompatible with thick walls of masonry and the chimneys of attached dwelling houses, and therefore, as far as possible, I have removed the instruments to small detached houses of their own. But the transit circle still remains in the main building, for, as will be evident to you, it is no easy matter to transport such an instrument.

The two first photographs show the instrument, in one case pointed nearly horizontally to the north, the other pointed nearly vertical. Neither can show all parts of the instrument, but you can see the massive stone piers, weighing many tons each, which, resting on the solid blocks 10 feet below, support the pivots. Here are the counterweights which remove a great part of the weight of the instrument from the pivots, leaving only a residual pressure sufficient to enable the pivots to preserve the motion of the instrument in its proper plane. Here are the microscopes by which the circle is read. Here the opening through which the instrument views the meridian sky. The observer's chair is shown in this diagram. His work appears to be very simple, and so it is, but it requires special natural gifts—patience and devotion, and a high sense of the importance of his work—to make a first-rate meridian observer. Nothing apparently more monotonous can be well imagined if a man is "not to the manner born."

Having directed this instrument by means of the setting circle to the required altitude, he clamps it there and waits for the star which he is about to observe to enter the field. This is what he sees. [Artificial transit of a star by lantern.]

As the star enters the field it passes wire after wire, and as it passes each wire he presses the key of his chronograph and records the instant automatically. As the star passes the middle wire he bisects it with the horizontal web, and again similarly records on his chronograph the transit of the star over the remaining webs. Then he reads off the microscopes by which the circle is read, and also the barometer and thermometer, in order afterwards to be able to calculate accurately the effect of atmospheric refraction on the observed altitude of the star; and then his observation is finished. Thus the work of the meridian observer goes on, star after star, hour after hour, and night after night; and, as you see, it differs very widely from the popular notion of an astronomer's occupation. It presents no dreamy contemplation, no watching for new stars, no unexpected or startling phenomena. On the contrary, there is beside him the carefully prepared observing-list for the night, the previously calculated circle setting for each star, allowing just sufficient time for the new setting for the next star after the readings of the circle for the previous observation.

After four or five hours of this work the observers have had enough of it; they have, perhaps, observed fifty or sixty stars, they determine certain instrumental errors, and betake themselves to bed, tired, but (if they are of the right stuff) happy and contented men. At the Cape we employ two observers, one to read the circle and one to record the transit. Four observers are employed, and they are thus on duty each alternate night. Such is the work that an outsider would see were he to enter a working meridian observatory at night, but he would find out if he came next morning that the work was by no means over. By far the largest part has yet to follow. An observation that requires only two or three minutes to make at night, requires at least half an hour for its reduction by day. Each observation is affected by a number of errors, and these have to be determined and allowed for. Although solidly founded on massive piers resting on the solid rock, the constancy of the instrument's position cannot be relied upon. It goes through small periodic changes in Level in Collimation and in Azimuth, which have to be determined by proper means, and the corresponding corrections have to be computed and applied; and also there are other corrections for refraction, &c., which involve computation and have to be applied. But these matters would fall more properly under the head of a special lecture upon the transit instrument. I mention them now merely to explain why so great a part of an astronomer's work comes in the daytime, and to dispel the notion that his work belongs only to the night.

One might very well occupy a special lecture in an account of the peculiarities of what is called personal equation—that is to say, the different time which elapses for different observers between the time when the observer believes the star to be upon the wire and the time when the finger responds to the message which the eye has conveyed to the brain. Some observers always press the key too soon, some always too late. Some years ago I discovered, from observations to which I will subsequently refer, that *all* observers press the chronograph key either too soon for bright stars or too late for faint ones.

Other errors may, and I am sure do, arise both at Greenwich and the Cape from the impossibility of securing uniformity of outside and inside temperature in a building of strong masonry. The ideal observatory should be solid as possible as to its foundations, but light as possible as to its roof and walls—say, a light framework of iron covered with canvas. But it would be undesirable to cover a valuable and permanent instrument in this way.

But here is a form of observatory which realises all that is required, and which is eminently suited for permanent use. The walls are of sheet iron, which readily acquire the temperature of the outer air. The iron walls are protected from direct sunshine by wooden louvres, and small doors in the iron walls admit a free circulation of air. The revolving roof is a light framework of iron covered with well-painted *papier maché*.

The photograph now on the screen shows the interior of the observatory, and this brings me to the description of observations of an entirely different class. In this observatory the roof turns round on wheels, so that any part of the sky can be viewed from the telescope. This is so because the instrument in this observatory is intended for purposes which are entirely different from those of a transit circle. The transit circle, as we have seen, is used to determine the *absolute* positions of the heavenly bodies; the heliometer to determine with greater precision than is possible by the absolute method the *relative* positions of celestial objects.

To explain my meaning as to absolute and relative positions:— It would, for example, be a matter of very little importance if the absolute latitude of a point on the Royal Exchange or the Bank of England were one-tenth of a second of arc (or ten feet) wrong in the maps of the Ordnance Survey of England—that would constitute a small *absolute* error common to all the buildings on the same map of a part of the city, and common to all the adjoining maps also. Such an error, regarded as an *absolute* error, would evidently be of no importance if every point on the map had the same absolute error. There is no one who can say at the present moment whether the absolute latitude of the Royal Exchange—nay, even of the Royal Observatory, Greenwich—is known to ten feet. But it would be a very serious thing indeed if the relative positions on the same map were ten feet wrong *here* and *there*. For example, if of two points marking a frontage boundary on Cornhill one were correct, the other ten feet in error—what a nice fuss there would be! what food for lawyers! what a bad time for the Ordnance Survey Office! Well, it is just the same in astronomy.

We do not know, we probably never shall know with certainty, the absolute places of even the principal stars to $\frac{1}{10}$th of a second of arc. But $\frac{1}{10}$th of a second of arc in the measure of some relative position would be fatal. For example, in the measurement of the sun's parallax an error of $\frac{1}{10}$th of a second of arc means an error of 1,000,000 miles, in round numbers, in the sun's distance; and it is only when we can be quite certain of our measures of much smaller quantities than $\frac{1}{10}$th of a second of arc, that we are in a position to begin seriously the determination of such a problem as that of the distances of the fixed stars. For these problems we must use differential measures, that is measures of the relative positions of two objects. The most perfect instrument for such purposes is the heliometer.

Lord McLaren has kindly sent from Edinburgh, for the purposes of this lecture, the parts of his heliometer which are necessary to illustrate the principles of the instrument.

This instrument is the same which I used on Lord Crawford's expedition to Mauritius, in 1874. It was also kindly lent to me by Lord Crawford for an expedition to the Island of Ascension to observe the opposition of Mars, in 1877. In 1879, when I went to the Cape, I acquired the instrument from Lord Crawford, and carried out certain researches with it on the distances of the fixed stars.

In 1887, when the Admiralty provided the new heliometer for the Cape Observatory, this instrument again changed hands. It became the property of Lord McLaren. I felt rather disloyal in parting with so old a friend. We had spent so many happy hours together, we had shared a good many anxieties together, *and we knew each other's weaknesses so well.* But my old friend has fallen into good hands, and has found another sphere of work.

The principle of the instrument is as follows. [The instrument was here explained.]

There is now on the screen a picture of the new heliometer of the Cape Observatory, which was mounted in 1887, and has been in constant use ever since. It is an instrument of the most refined modern construction, and is probably the finest apparatus for refined measurement of celestial angles in the world.

[Here were explained the various parts of the instrument in relation to the model, and the actual processes of observation were illustrated by the images of artificial stars projected on a screen.]

Here, again, there is little that conforms to the popular idea of an astronomer's work; there is no searching for objects, no contemplative watching, nothing sensational of any kind. On the contrary, every detail of his work has been previously arranged and calculated beforehand, and the prospect that lies before him in his night's work is simply more or less of a struggle with the difficulties which are created by the agitation of the star images, caused by irregularities in the atmospheric refraction. It is not upon one night in a hundred that the images of stars are perfectly tranquil. You have the same effect in an exaggerated way when looking across a bog on a hot day. Thus, generally, as the images are approached, they appear to cross and recross each other, and the observer must either seize a moment of comparative tranquillity to make his definitive bisection, or he may arrive at it by gradual approximations till he finds that the vibrating images of the two stars seem to pass each other as often to one side as to the other. So soon as such a bisection has been made the time is recorded on the chronograph, then the scales are pointed on and printed off, and so the work goes on, varied only by reversals of the segments and of the position circle. Generally, I now arrange for 32 such bisections, and these occupy about an hour and a half. By that time one has had about enough of it, the nerves are somewhat tired, so are the muscles of the back of the neck, and, if the observer is wise, and wishes to do his best work, he goes to bed early and gets up again at two or three o'clock in the morning, and goes through a similar piece of work. In fact this must be his regular routine night after night, whenever the weather is clear, if he is engaged, as I have been, on a large programme of work on the parallaxes of the fixed stars, or on observations to determine the distance of the sun by observations of minor planets.

I will not speak now of these researches, because they are still in progress of execution or of reduction. I would rather, in the first place, endeavour to complete the picture of a night's work in a modern observatory.

We pass on to celestial photography, where astrometry and astrophysics join hands. Here on the screen is the interior of one of the new photographic observatories, that at Paris. [Brief description.]

Here is the exterior of our new photographic observatory at the Cape. Here is the interior of it, and the instrument. [Brief description.]

The observer's work during the exposure is simply to direct the telescope to the required part of the sky, and then the clockwork *nearly* does the rest—but not quite so. The observer holds in his hand a little electrical switch with two keys; by pressing one key he can accelerate the velocity of the driving screw by about 1 per cent., and by pressing the other he can retard it 1 per cent. In this way he keeps one of the stars in the field always perfectly bisected by the cross wires of his guiding telescope, and thus corrects the small errors produced partly by changes of refraction, partly by small unavoidable errors in cutting the teeth of the arc into which the screw of the driving shaft of the clockwork gears.

The work is monotonous rather than fatiguing, and the companionship of a pipe or cigar is very helpful during long exposures. A man can go on for a watch of four or five hours very well, taking plate after plate, exposing each, it may be, forty minutes or an hour. If the night is fine a second observer follows the first, and so the work goes on the greater part of the night. Next day he develops his plate and gets something like this. [Star cluster.]

Working just in this way, but with the more humble apparatus which you see imperfectly in the picture now on the screen, we have with a rapid rectilinear lens by Dallmeyer of 6 inches aperture photographed at the Cape during the past six years the whole of the southern hemisphere from 20° of south declination to the south pole.

The plates are being measured by Professor Kapteyn, of Groningen, and I expect that in the course of a year the whole work containing all the stars to $9\frac{1}{2}$ magnitude (between 200,000 and 300,000 stars) in that region will be ready for publication. This work is essential as a preliminary step for the execution in the southern hemisphere of the great work inaugurated by the Astrophotographic Congress at Paris in 1887, the last details of which were settled at our meeting at Paris in April last. What we shall do with the new apparatus perhaps I may have the honour to describe to you some years hence, after the work has been done.

We now come to an important class of astronomical work more purely astrophysical, for the illustration of which I can no longer appeal to the Cape, because I regret to say that we are not yet provided with the means for its prosecution. I refer to the use of the spectroscope in astronomy, and especially to the latest developments of its use for the accurate measurement of the velocity of the motions of stars in the line of sight.*

It is beyond the province of this lecture to enter into history, but it is impossible not to refer to the fact that the chief impulse to astronomical work in this direction was given by Dr. Huggins, our chairman to-night—nay, more, except for the early contributions of Fraunhofer to the subject, Dr. Huggins certainly is the father of sidereal spectroscopy, and that not in one but in every branch of it. He has devised the means, pointed the way, and, whilst in many branches of the work he still continues to lead the way, he has of necessity left the development of other branches to other hands.

From an astrometer's point of view the most important advance that has been made in spectroscopy of recent years is the sudden development of precision in the measures of star motion in the line of sight. The method remained for fifteen or sixteen years quite undeveloped from the condition in which it left the hands of Dr. Huggins, and certainly no progress in the accuracy attained by Dr. Huggins was made till the matter was taken up by Dr. Vogel at Potsdam. At a single step Dr. Vogel has raised the precision of the work from that of observations in the days of Ptolemy to that of the days of Bradley—from the days of the old sights and pinnules to the days of telescopes. Therefore I take a Potsdam observation as the best type of a modern spectroscopic observation for description, especially as I have recently visited Dr. Vogel at Potsdam, and he has kindly given me a photograph of his spectroscope, as well as of some of the work done with it.

A photograph of the Potsdam spectroscope attached to the equatorial is now on the screen. [Description.]

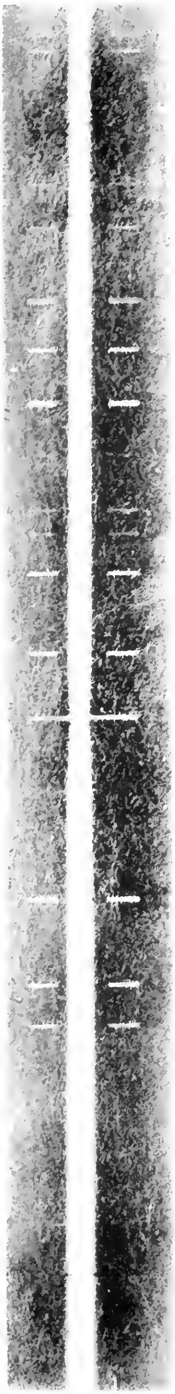
The method of observation consists simply in inserting a small photographic plate in the dark slide, directing the telescope to the star, and keeping the image of the star continuously on the slit during an exposure of about an hour ; and this is what is obtained on development of the picture.

If the star remained perfectly at rest between the jaws of the slit the spectrum would be represented by a single thread of light, and of course no lines would be visible upon such a thread ; but the observer intentionally causes the star image to travel a little along the slit during the time of exposure, and so a spectrum of sensible width is obtained. (Fig. 1.)

You will remark how beautifully sharp are the faint lines in this spectrum. Those who have tried to observe the spectrum of Sirius in the ordinary way know that many of these fine lines cannot be seen or measured with certainty. The reason is that on account of irregularities in atmospheric refraction, the image of a star in the

* The older methods enabled us to measure motions at right angles to the line of sight, but till the spectroscope came we could not measure motions in the line of sight.

Fig. 1.

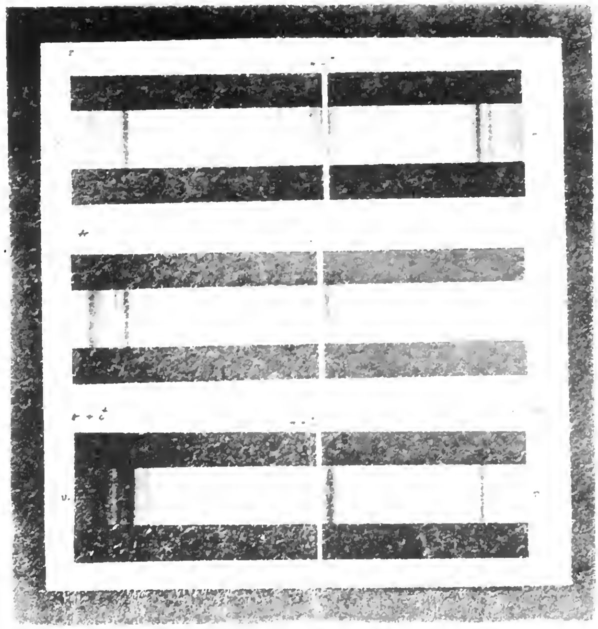


$H\gamma$

Spectrum of Sirius with comparison Spectrum of Iron.

Fig. 2.

α *Aurigæ.*

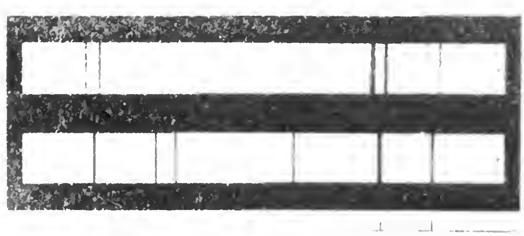


October.

December.

March.

Fig. 3.



telescope is rarely tranquil, sometimes it shines brightly in the centre of the slit, sometimes barely in the slit at all, and the eye becomes puzzled and confused. But the photographic eye is not in the least disturbed; when the star image is in the slit, the plate goes on recording what it sees, and when the star is not in the slit the plate does nothing, and it is of no consequence whatever how rapidly these alternate appearances and disappearances recur. The only difference is that when the air is very steady and the star's image, therefore, always in the slit, the exposure takes less time than when the star is unsteady.

That is one reason why the Potsdam results are so accurate. And there are many other reasons besides, into which I cannot now enter. What, however, it is very important to note is this, that we have here a method which is to a great extent independent of the atmospheric disturbances which in all other departments of astronomical observation have imposed a limit to their precision. Accurate astro-spectroscopy, therefore, may be pushed to a degree of perfection which is limited only by the optical aid at our disposal and by the sensibility of our photographic plates.

And now I think we have sufficiently considered the ordinary processes of astronomical observation to illustrate the character of the work of an astronomer at night; the picture should be completed by an account of his work by day. But to go into that matter in detail would certainly not be within the limits of this lecture. It is better that I should in conclusion touch upon some recent remarkable results of these day and night labours. It is these after all that most appeal to you, it is for these that the astronomer labours, it is the prospect of them that lightens the long watches of the night and gives life to the otherwise dead bones of mechanical routine.

Let us take first some spectroscopic results. To explain their meaning let me remind you for a moment of the familiar analogy between light and sound.

The pitch of a musical note depends on the rapidity of the vibrations communicated to the air by the reed or string of the musical instrument that produces the note, a low note being given by slow vibrations and a high one by quick vibrations.

Just in the same way red light depends on relatively slow vibrations of ether, and blue or violet light on relatively quick vibrations. Well, if there is a railway train rapidly approaching one, and the engine sounds its whistle, more waves of sound from that whistle will reach the ear in a second of time, than would reach the ear were the train at rest. On the other hand, if the train is travelling at the same rate *away* from the observer, fewer waves of sound will reach his ears in a second of time. Therefore an observer beside the line should observe a distinct change of pitch in the note of the engine whistle as the train passes him, and as a matter of fact such a change of pitch can be and has been observed.

Just in the same way, if a source of light could be moved rapidly enough towards an observer it would become bluer, or if away from him it would become more red in colour. Only it would require a change of velocity in the moving light of some thousands of miles per second in order to render the difference of colour sensible to the eye. The experiment is, therefore, not likely to be frequently shown at this lecture table!

But the spectroscope enables such changes of colour to be measured with extreme precision. Here on the screen is the most splendid illustration of this that exists at present, viz. copies of three negatives of the spectrum of α Aurigæ, taken at Potsdam in October and December of 1888, and in March 1889. (Fig. 2.)

The white line (the picture being a positive) represents the bright line $H\gamma$ given by the artificial light of hydrogen, the strong black line in the picture of the star spectrum corresponds to the black absorption line which is due to hydrogen in the atmosphere of the star.

Why is it that the artificial hydrogen line does not correspond with the stellar line in these three pictures? The answer is, either the star is moving towards or from the earth in the line of sight, or the earth is moving from or towards the star. But in December the earth in its motion round the sun is moving at right angles to the direction of α Aurigæ, why then does not the stellar hydrogen line agree in position with the terrestrial hydrogen line; the simple explanation is that α Aurigæ is moving with respect to the sun.

In what way is it moving? Well, that also is clear: the stellar line is displaced towards the red end of the spectrum, that is to say the star light is redder than it should be in consequence of a motion of recession; this proves that the star is moving away from us, and measures of the photograph show the rate of this motion to be $15\frac{1}{2}$ miles per second. We also know that in October the earth in its motion round the sun is moving towards α Aurigæ nearly at the same rate as we have just seen that α Aurigæ is running away from the sun. Consequently, at that time, their relative motions are nearly insensible, because both are going at the same rate in the same direction, and we find accordingly in October, that the positions of the stellar and artificial hydrogen lines perfectly correspond. Finally, in March, the earth in its motion round the sun is moving away from α Aurigæ, and as α Aurigæ is also running away from the sun the star-light becomes so much redder than normal that the stellar hydrogen line is shifted completely to one side of the hydrogen and artificial line.

The accuracy of these results may be proved as follows:—

If we measure all the photographs of α Aurigæ which Dr. Vogel has obtained we can derive from each a determination of the relative velocity of the motion of the star with respect to our earth.

Of course these velocities are made up of the velocity of motion of α Aurigæ with respect to the sun (which we may reasonably assume to be a uniform velocity) and the velocity of the earth due to α Aurigæ—Potsdam.

Date.	Observed Relative Motion of Earth and Star. Miles per sec.	Motion of Earth.	Concluded Motion, Star Relative to the Sun.
1888.			
October 22nd	+ 2·5	− 13·0	+ 15·5
„ 24th	+ 3·1	− 12·4	+ 15·5
„ 25th	+ 3·1	− 12·4	+ 15·5
„ 28th	+ 2·5	− 11·8	+ 14·3
November 9th	+ 6·8	− 8·7	+ 15·5
December 1st	+ 11·8	− 3·1	+ 14·9
„ 13th	+ 14·9	+ 0·6	+ 14·3
1889.			
January 2nd	+ 20·5	+ 6·8	+ 13·7
February 5th	+ 32·9	+ 14·3	+ 18·6
March 6th	+ 34·2	+ 16·8	+ 17·4

α Aurigæ—Greenwich.

Date.	Observed Relative Motion of Earth and Star. Miles per sec.	Motion of Earth.	Concluded Motion, Star Relative to the Sun.
1887.			
January 26th	+ 16·4	+ 12·6	+ 3·8
February 16th	+ 34·4	+ 15·9	+ 18·5
October 22nd	+ 39·8	− 13·5	+ 52·3
„ 25th	+ 25·4	− 13·0	+ 38·4
„ 29th	+ 40·6	− 12·1	+ 52·7
1888.			
December 7th	+ 29·0	− 1·2	+ 36·2
1889.			
February 15th	+ 23·8	+ 16·0	+ 7·8
March 5th	+ 20·3	+ 17·1	+ 3·2
September 17th	+ 18·6	− 13·3	+ 33·3
„ 19th	+ 21·8	− 16·7	+ 38·5
„ 25th	+ 24·8	− 16·5	+ 41·3
November 25th	+ 24·5	− 4·9	+ 29·4

its motion round the sun. But the velocity of the earth's motion in its orbit is known with an accuracy of about one five-hundredth part of its amount, and therefore, within that accuracy, we can allow precisely for its effect on the relative velocity of the earth and α Aurigæ. When we have done so we get the annexed results for the velocity of the motion of α Aurigæ with respect to the sun. You see by the annexed table how beautifully they agree in the Potsdam results, and how comparatively rough and unreliable are the results obtained by the older method at Greenwich.

I believe that in a few years, at least in a period of time that one may hope to see, we shall not be content merely to correct our results for the motion of the earth in its orbit only, and so test our observations of motion in the line of sight, but that we shall have arrived at a certainty and precision of working which will *permit the process to be reversed*, and that we shall be employing the spectroscope to determine the velocity of the earth's motion in its orbit, or in other words to determine the fundamental unit of astronomy, the distance of the sun from the earth.

I will take as another example one recent remarkable spectroscopic discovery.

Miss Maury, in examining a number of photographs of steller spectra taken at Harvard College, discovered that in the spectrum of β Aurigæ certain lines doubled themselves every two days, becoming single in the intermediate days. Accurate Potsdam observations confirmed the conclusion.

The picture on the screen (Fig. 3) shows the spectrum of β Aurigæ photographed on November 22 and 25 of last year. In the first the lines are single, in the other every line is doubled. Measures and discussion of a number of these photographs have shown that the doubling of the lines is perfectly accounted for by the supposition of two suns revolving round each other in a period of four days, each moving at a velocity of about 70 miles a second in its orbit.

When one star is approaching us and the other receding, the lines in the spectrum formed by the light of the first star will be moved towards the blue end of the spectrum, those in the spectrum of the second star towards the red end of the spectrum. Then, as the two stars come into the same line with us, their motions become at right angles to the line of sight, and their two spectra, not being affected by motion, will perfectly coincide; but then, after the stars cross, their spectra again separate in the opposite direction, and so they go on.

Thus by means of their spectra we are in a position to watch and to measure the relative motions of two objects that we can never see apart; nay more, we can determine not only their period of revolution but also the velocity of their motions in their orbits. Now, if we know the time that a body takes to complete its revolution, and the velocity at which it moves, clearly we know the dimensions of its orbit, and if we know the dimensions of an orbit we know what attractive force is necessary to compel the body to keep in that orbit, and thus we are able to weigh these bodies. The components of β Aurigæ are two suns, which revolve about each other in four days; they are only between 7 and 8 millions of miles (or one-twelfth of our distance from the sun) apart, and if they are of equal weight they each weigh rather over double the weight of our sun.

I have little doubt that these facts do not represent a permanent condition, but simply a stage of evolution in the life-history of the system, an earlier stage of which may have been a nebular one.

Other similar double stars have been discovered both at Potsdam and at Cambridge, U.S., stars that we shall never see separately with the eye aided by the most powerful telescope; but time does not permit me to enter into any account of them.

I pass now to another recent result that is of great cosmical interest.

The Cape photographic star charting of the southern hemisphere has been already referred to. In comparing the existing eye estimates of magnitude by Dr. Gould with the photographic determinations of these magnitudes, both Professor Kapteyn and myself have been greatly struck with a very considerable systematic discordance between the two. In the rich parts of the sky, that is in the Milky Way, the stars are systematically photographically brighter by comparison with the eye observations than they are in the poorer part of the sky, and that not by any doubtful amount but by half or three-fourths of a magnitude. One of two things was certain, either that the eye observations were wrong, or that the stars of the Milky Way are bluer or whiter than other stars. But Professor Pickering, of Cambridge, America, has lately been making a complete photographic review of the heavens, and by placing a prism in front of the telescope he has made pictures of the whole sky like this. [Here two examples of the plates of Pickering's spectroscopic Durchmusterung were exhibited on the screen.] He has discussed the various types of the spectra of the brighter stars, as thus revealed, according to their distribution in the sky. He finds thus that the stars of the Sirius type occur chiefly in the Milky Way, whilst stars of other types are fairly divided over the sky.

Now stars of the Sirius type are very white stars, very rich relative to other stars in the rays which act most strongly on a photographic plate. Here then is the explanation of the results of our photographic star-charting, and of the discordance between the photographic and visual magnitudes in the Milky Way.

The results of the Cape charting further show that it is not alone to the brighter stars that this discordance extends, but it extends also, though in a rather less degree, to the fainter stars of the Milky Way. Therefore we may come to the very remarkable conclusion that the Milky Way is a thing apart, and that it has been developed perhaps in a different manner, or more probably at a different and probably later epoch from the rest of the sidereal universe.

Here is another interesting cosmical revelation which we owe to photography.

You all know the beautiful constellation Orion, and many in this theatre have before seen the photograph of the nebula which is now on the screen, taken by Mr. Roberts.

Here is another photograph of the same object taken with a much longer exposure. You see how over-exposed, in fact, burnt out, the brightest part of the picture is, and yet what a wonderful development of faint additional nebulous matter is revealed.

But I do not think that many persons in this room have seen *this* picture, and probably very few have any idea what it represents. It is from the original negative taken by Professor Pickering, with a small photographic lens of short focus, after six hours' exposure in the clear air of the Andes, 10,000 feet above sea-level.

The field embraces the three well-known stars in the belt of Orion on the one hand, and β Orionis (Rigel) on the other. You can hardly recognise these great white patches as stars; their ill-defined character is simply the result of excessive over-exposure. But mark the wonders which this long exposure with a lens of high intrinsic brilliancy of image has revealed. Here is the great nebula, of course terribly over-exposed, but note its wonderful fainter ramifications. See how the whole area is more or less nebulous, and surrounded as it were with a ring fence of nebulous matter. This nebulosity shows a special concentration about β Orionis.

Well, when Professor Pickering got this wonderful picture, knowing that I was occupied with investigations on the distances of the fixed stars, he wrote to ask whether I had made any observations to determine the distance of β Orionis, as it would be of great interest to know from independent evidence whether this very bright star was really near to us or not. It so happens that the observations were made, and their definitive reduction has shown that β Orionis is really at the same distance from us as are the faint comparison stars. β Orionis is, therefore, probably part and parcel of an enormous system in an advanced but incomplete state of stellar evolution, and that what we have seen in this wonderful picture is all a part of that system.

I should explain what I mean by an elementary or by an advanced state of stellar evolution. There is but one theory of celestial evolution which has so far survived the test of time and comparison with observed facts, viz. the nebular hypothesis of Laplace. Laplace supposed that the sun was originally a huge gaseous or nebulous mass of a diameter far greater than the orbit of Neptune. I say *originally*, do not misunderstand me. We have finite minds; we can imagine a condition of things which might be supposed to occur at any particular instant of time however remote, and at any particular distance of space however great, and we may frame a theory beginning at another time still more remote, and so on. But we can never imagine a theory beginning at an infinite distance of time or at an infinitely distant point in space. Thus, in any theory which man with his finite mind can devise, when we talk of *originally* we simply mean at or during the time considered in our theory.

Now, Laplace's theory begins at a time, millions on millions of years ago, when the sun had so far disentangled itself from chaos, and its component gaseous particles had by mutual attraction so far coalesced as to form an enormous gaseous ball, far greater in diameter than the orbit of the remotest planet of our present system. The central part of this ball was certainly much more condensed than the rest, and the whole ball revolved. There is nothing improbable in this hypothesis. If gaseous matter came together from different parts of space such coalition would unquestionably occur, and as in the meeting of opposite streams of water or of opposite currents of wind, vortices would be created and revolution about an axis set up such as we are familiar with in the case of whirlpools or cyclones. The resultant would be rotation of the whole globular gaseous mass about an axis.

Now this gaseous globe begins to cool, and as it cools it necessarily contracts. Then follows a necessary result of contraction, viz. the rotation becomes more rapid. This is a well-known fact in dynamics, about which there is no doubt. Thus, the cooling and the contracting go on, and simultaneously the velocity of rotation becomes greater and greater. At last the time arrives when, for the outside particles, the velocity of rotation becomes such that the centrifugal force is greater than the attractive force, and so the outside particles break off and form a ring. Then, as the process of cooling and contraction proceed still further, another ring is formed, and so on, till we have finally a succession of rings and a condensed central ball. If from any cause the cooling of any of these rings does not go on uniformly, or if some of the gaseous matter of the ring is more easily liquefied than others, then probably a single nucleus of liquid matter will be formed in that ring, and this nucleus will finally by attraction absorb the whole of the matter of which the ring is composed—at first as a gaseous ball with a condensed nucleus, and this will finally solidify into a planet. Or, meanwhile, this yet unformed planet may repeat the history of its parent sun. By contraction, and consequent acceleration of its rotation, it may throw off one or more rings, which in like manner condense into satellites like our moon, or those of Jupiter, Saturn, Uranus, or Neptune. Such, very briefly outlined, is the celebrated nebular hypothesis of Laplace. No one can positively say that the hypothesis is true, still less can any one say that it is untrue. Time does not permit me to enter into the very strong proofs which Laplace urged in favour of its acceptance.

But I beg you for one moment to cast your imaginations back to a period of time long antecedent to that when our sun had begun to disentangle itself from chaos, and when the fleecy clouds of cosmic stuff had but commenced to rush together. What should we see in such a case were there a true basis for the theory of Laplace?

Certainly, in the first place, we should have a huge whirlpool or cyclone of cosmic gaseous stuff, the formation of rings, and the condensation of these rings into gaseous globes.

Remembering this, look now on this wonderful photograph of the nebula in Andromeda, made by Mr. Roberts. In the largest telescopes this nebula appears simply as an oval patch of nearly uniform light, with a few dark canals through it, but no idea of its true form can be obtained, no trace can be found of the significant story which this photograph tells. It is a picture that no human eye unaided by photography has ever seen. It is a true picture drawn without the intervention of the hand of fallible man, and uninfluenced by his bias or imagination. Have we not here, so at least it seems to me, a picture of a very early stage in the evolution of a star cluster or sun-system—a phase in the history of another star-system similar to that which once occurred in our own—millions and millions of years ago—when our earth, nay, even our sun itself, "was without form and void," and "darkness was on the face of the deep."

During this lecture I have been able to trace but very imperfectly the bare outlines of an astronomer's work in a modern observatory, and to give you a very few of its latest results—results which do not come by chance, but by hard labour, and to men who have patience to face dull daily routine for the love of science—to men who realise the imperfections of their methods and are constantly on the alert to improve them.

The mills of the astronomer grind slowly, and he must be infinitely careful and watchful if he would have them like the mills of God, to grind exceeding small.

I think he may well take for his motto these beautiful lines:—

"Like the star
Which shines afar,
Without haste,
Without rest,
Let each man wheel
With steady sway,
Round the task
Which rules the day,
And do his best."

[D. G.]

WEEKLY EVENING MEETING,

Friday, April 17, 1891.

Edward Frankland, Esq. D.C.L. LL.D. F.R.S. Vice-President, in the Chair.

Professor A. W. Rücker, M.A. F.R.S. *M.R.I.*

Magnetic Rocks.

The cause of terrestrial magnetism is still unknown, and the problem of attempting to discover it is not rendered more easy by the fact that a solution may be looked for in either of two different directions.

On the one hand the earth is partly composed of magnetic material, and if vast masses of this were permanently magnetised, the principal phenomena observed upon the surface might be produced. On the other hand, we know that different points on the earth's crust are at different electrical potentials, and it is conceivable that the directive forces exerted on the magnet might be due to a world-wide system of earth currents. Both theories are beset with difficulties, and at present we are accumulating facts, in the hope that a clue to an explanation may hereafter be found.

A mere dry record of observations is, however, hardly a subject for a lecture, and I should not have mooted the question if there had not been another problem, related to, though differing from that of terrestrial magnetism, with regard to which it is perhaps possible to form an opinion as to the direction in which the balance of evidence inclines.

If the magnetic declination be determined at a number of stations scattered all over the surface of the globe, lines can be drawn through those places at which the deviation from true north is the same. If the scale of the map on which they are depicted is small, and if the distances between the stations are measured in scores or in hundreds of miles, these isogonal lines are smooth curves; but if the number of stations be multiplied, and the scale on which the results are represented increased, the curves are found to be irregular, and to be complicated by unexpected bends and twists.

These irregularities must be due to disturbing magnetic forces, produced by local causes, which deflect the needle from its normal direction; and if the number of stations be sufficiently great there is no difficulty in sifting out the disturbing from the normal forces and determining with approximate accuracy the directions in which they act.

If this is done, the question may be asked whether these local peculiarities are due to rock magnetism or to earth currents. There is no reason why both should not in some cases coexist, but as there are, as I think, weighty reasons for believing that rock magnetism is often the principal cause, I propose to discuss them.

Permanent magnetisation of the rocks may perhaps be discarded as the cause of disturbances which extend over large areas. Basaltic columns are often strongly magnetised, but then magnetisation is irregular. At a short distance the opposite poles would neutralise each other's effects, and widespreading effects are most likely due to the inductive influence of the earth acting on widespread masses.

The evidence for this may be summed up as follows:—In some cases, as in that of the cliffs on the Hudson River and at Snake Hill (New Jersey), the mass is apparently polarised at the upper and lower extremities, as it would be if magnetised by the earth. Clear indications of this fact are, however, often difficult to obtain, as they are masked by local permanent magnetism.

A more certain test can be applied in the case of less strongly magnetic rocks if the instruments can be placed in their neighbourhood, but on non-magnetic soil. These conditions are satisfied by the Malvern Hills, and they are found to attract the north pole of a magnet, which is consistent with the view that they are magnetised inductively. Again, over large districts, the centres of which are marked by the outcrop of basaltic rocks, the magnetic forces tend towards the centre, which is again what would occur if the rocks which appear on the surface are the uppermost portions of a much larger mass magnetised by the inductive influence of the earth. This state of things is observed in the south and west of Scotland and in Antrim.

Lastly, Captain Creak, F.R.S., has shown that islands in the northern and southern hemispheres attract the north and south poles of the magnet respectively.

The only question that remains is whether the presence of rocks similar to those which exist on the surface would suffice to account for the observed surface disturbances. To test this the magnetic permeabilities of a number of specimens of basalt kindly supplied by Professor Judd have been determined. Assuming (1) that magnetite becomes non-magnetic at the same temperature as iron, (2) that its magnetic properties are not affected by great pressures, (3) that the temperature at which iron ceases to be magnetic is reached at a depth of 12 miles, (4) that large sheets of magnetic rock exist between the surface and this depth, the areas of which are of the same dimensions as those of the regions of high vertical force which exist in the United Kingdom, (5) that the magnetic susceptibilities of these rocks vary between the mean of those of 13 specimens from the Island of Mull (0·00163), and of 34 specimens from the west of Scotland (0·00271), it is found that a fair agreement exists between the results of calculation and of observation, and that there is no doubt that the calculated and observed disturbances are of the same order of magnitude.

All these facts then accord well with the theory that local and regional magnetic disturbances are due to the inductive action exerted by the earth's magnetic field on rocks. It is, however, necessary to discuss the arguments which can be brought forward on the other side of the question.

The neighbourhood of Melton Mowbray is the source of a considerable magnetic disturbance. Mr. Preece, F.R.S., was good enough to cause an earth-current survey to be made between the post offices in that district. The earth currents appeared in all cases to run out from Melton. This might raise a doubt as to whether the currents were not largely due to small differences between the earth plates causing them when connected to act as a battery. If this is so the differences of potential to which the earth currents are due must be less than those due to the plates.

If, however, we assume that real earth currents were measured the directions were not in all cases such as would produce the observed deviations of the magnet. The potential differences were also much less than those which at Greenwich produce or are at least connected with similar deflections of the needle. The difference is not small. If in both cases the earth currents are the cause, equal potential differences must produce at Greenwich magnetic effects a hundred times less powerful than those produced at Melton Mowbray.

Perhaps, however, the strongest argument against the earth-current theory is based on Captain Creak's generalisation as to the magnetic properties of islands. If opposite poles are attracted in the two hemispheres, disturbance currents must circulate round the island in opposite directions. No adequate physical cause has been suggested why current eddies of contrary directions of circulation should be produced in the two hemispheres.

If then we accept the view that the balance of evidence at present inclines towards the rock theory, it is evident that in a survey of magnetic disturbances the lines towards which the magnet is attracted are in general loci of nearest approach of the magnetic rocks to the surface, or of centres of highest magnetic susceptibility, or of both of these combined.

It is thus possible that from such observations we may learn something as to the distribution of basic rocks at depths far below those which ordinary geological methods can reach. It is therefore interesting to note that the results obtained in the United Kingdom have received a remarkable confirmation from France. Corresponding to a ridge (or locus of attraction) which runs south from Reading and enters the channel near Chichester, is another which emerges from the channel almost exactly opposite to it and passes to the south of Paris. The southern termination is not yet known, but the magnetic disturbance increases as the latitude diminishes. There can be little doubt that a well-marked locus of attraction for the north pole of the needle runs from Reading to the south of Paris.

[A. W. R.]

WEEKLY EVENING MEETING,

Friday, April 24, 1891.

The Right Hon. Earl Percy, F.S.A. Vice-President, in the Chair.

The Rev. Canon Ainger, M.A. LL.D.

Euphuism—Past and Present.

[No Abstract.]

GENERAL MONTHLY MEETING,

Monday, June 1, 1891.

Sir James Crichton Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

R. W. Peregrine Birch, Esq.
William Edmonds, Esq.
Nicholas Eumorfopoulos, Esq.
Mrs. Charles Hoare,
Mrs. Edward Singleton,
Harold Swithinbank, Esq.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned for the following Donation to the Fund for the Promotion of Experimental Research:—

Mrs. Priestley .. £21

The following Letter, recommended by the Managers to be sent to the Members, was read and approved:—

"Royal Institution, Albemarle Street, W.,
"1st June, 1891.

"*Centenary of the Birth of Michael Faraday.*

"Sir,

"I am directed by the Managers to inform you that His Royal Highness the "Prince of Wales has fixed 4 o'clock, on Wednesday, the 17th June, for the "delivery by Lord Rayleigh of the first of the two Lectures in connection with "this Centenary, and that Friday evening, the 26th June, at 9 o'clock, has been "appointed for the second of these Lectures, which will be given by Professor "Dewar.

"With the view of insuring the comfort of those Members attending, the Managers find it will be necessary to confine the invitations to 'friends' to one person for each Member attending.

"To facilitate the arrangements, I am directed to request that you will be good enough to let me know, not later than Thursday morning, the 11th June, whether you propose to be present at one, or at both, of these Lectures, and whether you wish a card for a friend (to be introduced personally by you) for the Lecture of the 17th inst., or for the Lecture of the 26th, or for both.

"I am, Sir,
"Your obedient Servant,
"FREDERICK BRAMWELL,
"*Honorary Secretary.*

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Catalogue of Birds in the Museum of Hon. East India Company, Vols. I. II. 8vo. 1854–58.
Catalogue of Mammalia (ditto). 8vo. 1851.
Catalogue of Lepidopterous Insects (ditto), Vols. I. II. 8vo. 1857–59.
Abel, Sir Frederick, K.C.B. F.R.S. D.C.L. M.R.I. (the Author)—Presidential Address delivered at the Iron and Steel Institute, May 6th, 1891. 8vo.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta: Rendiconti. 1° Semestre, Vol. VII. Fasc. 8. 8vo. 1891.
Atti, Serie Quarta, Anno CCLXXV.-CCLXXVI. 4to. 1888–89.
Academy of Natural Sciences, Philadelphia—Proceedings, 1890, Part 3. 8vo.
American Philosophical Society—Proceedings, No. 134. 8vo. 1890.
Astronomical Society, Royal—Monthly Notices, Vol. LI. No. 6. 8vo. 1891.
Bankers, Institute of—Journal, Vol. XII. Part 5. 8vo. 1891.
Bavarian Academy of Sciences—Abhandlungen, Band XVII. Abth. 2. 4to. 1891.
Almanach für 1890.
British Architects, Royal Institute of—Proceedings, 1891, Nos. 14, 15. 4to.
British Association for Advancement of Science—Report of Meeting at Leeds, 1890. 8vo.
Canada, Geological and Natural History Survey of—Contributions to Canadian Palæontology, Vol. III. 4to. 1891.
Canadian Institute—Transactions, Vol. I. Part 2, No. 2. 8vo. 1891.
Fourth Annual Report, 1890–91. 8vo.
Time Reckoning for the 20th Century. By S. Fleming, LL.D. C.E. 8vo. 1889.
Carey, Colonel William, C.B. R.A. (the Author)—The Crustacean Diving Dress. 12mo. 1891.
Chemical Industry, Society of—Journal, Vol. X. No. 4. 8vo. 1891.
Chemical Society—Journal for May, 1891. 8vo.
Cracovie, l'Academie des Sciences—Bulletin, 1891, No. 4. 8vo.
Editors—American Journal of Science for May, 1891. 8vo.
Analyst for May, 1891. 8vo.
Athenæum for May, 1891. 4to.
Brewers' Journal for May, 1891. 4to.
Chemical News for May, 1891. 4to.
Chemist and Druggist for May, 1891. 8vo.
Electrical Engineer for May, 1891. fol.
Engineer for May, 1891. fol.
Engineering for May, 1891. fol.
Horological Journal for May, 1891. 8vo.
Industries for May, 1891. fol.
Iron for May, 1891. 4to.
Ironmongery for May, 1891. 4to.

Murray's Magazine for May, 1891. 8vo.
Nature for May, 1891. 4to.
Open Court for May, 1891. 4to.
Photographic News for May, 1891. 8vo.
Public Health for May, 1891. 8vo.
Revue Scientifique for May, 1891. 4to.
Telegraphic Journal for May, 1891. fol.
Zoophilist for May, 1891. 4to.

Electrical Engineers' Institution—Journal, No. 93. 8vo. 1891.
Florence Biblioteca Nazionale Centrale—Bolletino, Nos. 129–130. 8vo. 1891.
Franklin Institute—Journal, No. 785. 8vo. 1891.
Geographical Society, Royal—Proceedings, New Series, Vol. XIII. No. 5. 8vo. 1891.
Geological Institute, Imperial, Vienna—Verhandlungen, 1891, Nos. 5–7. 8vo.
Harlem, Société Hollandaise des Sciences—Archives Néerlandaises, Tome XXV. 1er Livraison. 8vo. 1891.
Harris, John, Esq. (the Author)—"The Way out of the Wood." 8vo. 1891.
Institute of Brewing—Transactions, Vol. IV. No. 7. 8vo. 1891.
Johns Hopkins University—University Circulars, Nos. 87, 88. 4to. 1891.
Studies in Historical and Political Science, Eighth Series, Nos. 5–12. 8vo. 1890.
American Chemical Journal, Vol. XII. Nos. 6–8; Vol. XIII. No. 1. 8vo. 1890–91.
American Journal of Philology, Vol. XI. Nos. 2, 3. 8vo. 1890.
Kansas Academy of Sciences—Transactions, Vol. XII. Part 1. 8vo. 1890.
Linnean Society—Journal, No. 193. 8vo. 1891.
Mechanical Engineers' Institution—Proceedings, 1891. No. 1. 8vo.
Meriden Scientific Association—Transactions, Vol. IV. 8vo. 1889–90.
Murray, John, Esq. (the Publisher)—Dictionary of Greek and Roman Antiquities. Edited by Dr. W. Smith and others. Vol. II. 8vo. 1891.
National Life-boat Institution, Royal—Annual Report, 1891. 8vo.
Newberry Library (Chicago), Trustees of the—Proceedings of the Trustees for year ending January 5, 1891. 8vo.
North of England Institute of Mining and Mechanical Engineers—Transactions, Vol. XXXIX. Parts 1, 2; Vol. XL. Part 1. 8vo. 1891.
Odontological Society of Great Britain—Transactions, Vol. XXIII. No. 7. New Series. 8vo. 1891.
Pharmaceutical Society of Great Britain—Journal, May, 1891. 8vo.
Physical Society of London—Proceedings, Vol. XI. Part 1. 8vo. 1891.
Rio de Janeiro, Observatoire Imperiale de—Revista, Nos. 3, 4. 8vo. 1891.
Royal Botanic Society of London—Quarterly Record, 1880–1890. 8vo.
Royal Society of Antiquaries of Ireland—Journal, Vol. I. Fifth Series, No. 5. 8vo. 1891.
St. Petersburg Academie Imperiale des Sciences—Memoires, Tome XXXVIII. Nos. 2, 3. 8vo. 1891.
Sanitary Institute—Transactions, Vol. XI. 8vo. 1891.
Selborne Society—Nature Notes, Vol. II. No. 17. 8vo. 1891.
Smithsonian Institution—Smithsonian Miscellaneous Collections, Vol. XXXIV. Nos. 1–3. 8vo. 1890.
Society of Architects—Proceedings, Vol. III. No. 11. 8vo. 1891.
Society of Arts—Journal for May, 1891. 8vo.
Turpin, Eugène, Esq. (the Author)—The Truth on Melinite. 8vo. 1890.
United Service Institution, Royal—Journal, No. 159. 8vo. 1891.
University of London—Calendar, 1891–92. 8vo.
Upsal University—Bulletin de l'Observatoire Météorologique, Vol. XXII. 4to. 1890–91.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1891: Heft 5. 4to.

EXTRA EVENING MEETING,

Tuesday, June 2, 1891.

David Edward Hughes, Esq. F.R.S. Vice-President, in the Chair.

Charles Waldstein, Esq. Ph.D. Litt.D. L.H.D.

The Discovery of the " Tomb of Aristotle."

Dr. Charles Waldstein said that during the excavations carried on at Eretria, in Eubœa, by the American Archæological School of Athens under his direction, the discovery of a tomb stands foremost. This tomb is of great magnitude and splendour, and, by a process of inference, from data which, taken singly, might appear minute or insignificant but assume a new aspect when they are found to be interdependent and to converge to a common centre, he is led to believe to be that of the great philosopher Aristotle.

Dr. Waldstein contributed a short account of his discoveries to the 'Nineteenth Century' for May, which may be said to be only of a preliminary character, and in which he confined himself mainly to a narrative of the excavations and to the negative aspects of the question and the objections which might fairly be urged against the hypothesis that on this spot were interred the philosopher's remains. In the meantime Dr. Waldstein has been engaged in literary and epigraphical researches to enable him to arrive at a final conclusion on the subject. These investigations are not yet completed, and he hopes to ransack all the principal libraries in Europe in search of literary or other *indicia* which may go to support or destroy the theory.

It should be premised that Dr. Waldstein went to Eretria with no thought of such a discovery. He knew that it was a place of great historical importance and antiquity, and he knew also that there were dispersed among the clandestine dealers in antiquities at Athens many objects which could be traced to that ancient city, so familiar to students of Herodotus and Thucydides. Even if it be found that the explorer is mistaken, there can be no doubt of the great value and interest of the ancient remains which have been disinterred, and of the light which they reflect on an interesting period of Hellenic history and culture.

It would be remembered, he said, that Eretria and Chalcis were the two great commercial cities of Eubœa, and every reader of Thucydides was familar with the rivalry which so long subsisted between Eretria and Athens. Its position was on the Euripus, with a beautiful hilly landscape behind, and the mountains of Attica opposite on the other side of the channel.

Especial attention was drawn to Eretria by the discovery at Chalcis, in 1869, of a long inscription referring to the former city, the date of which lay between the years 340 and 278 B.C. This document embodied a formal contract for the execution of a work resembling that which in our own times has been done by the Bedford Level Commissioners. It recited that an engineer, Chaerephanes by name, contracted with the Eretrians to drain their marshes. He was himself to bear the cost of the work on condition that he was to be allowed to cultivate the reclaimed land for ten years at an annual rental of 30 talents, or about 7000*l.* The work was to be completed in four years. In case of war the ten years' lease was to be prolonged by a like period. There were also provisions for the compensation of persons whose land might be taken for the making of reservoirs or sluices, and the concession was to continue in the heirs of Chaerephanes, and the latter was to find sureties for the due execution of the works. This was one of the many indications of the richness of Eubœa as a field for archæological research, and would be found to have an incidental bearing upon the question at issue.

At the beginning of the present year Dr. Waldstein, having obtained a concession from the Greek authorities, proceeded from Athens to Eretria for the purpose of excavating the theatre and of digging out tombs, and in particular of discovering if he could, the temple of Artemis Amarysia. As is well known, the Greeks were in the habit of burying their dead outside the city walls, and at Eretria there was a continuous succession of graves running in different directions from the ancient city. These graves were of different periods, some as late as the Roman period, and many of the persons buried were foreigners. Out of 26 inscriptions he found that no fewer than eight referred to strangers and sojourners in the land. In the course of his excavations he came upon the most beautiful of all the family tombs which has yet been discovered.

The lecturer had described the difficulties which he encountered in the labour of excavation in the article above referred to, and, in fact he and his associates had three times to give up the attempt. In the course of his narrative he gave an interesting account of Greek writing materials—*μέλαν γραφικόν*, for ink, *καλαμός γραφικός*, a pen—being the materials used for permanent records on papyrus; whilst the *στῦλος* or *γραφὶς* was the stylus used for writing notes of transient importance on waxed tablets. He had already in the article referred to described the statuettes and ornaments and other things, including the only extant metal pen, so far as he knew, which had been found in Greece.

As before mentioned, Dr. Waldstein, in his contribution to the 'Nineteenth Century,' had dealt in a sceptical spirit with his own discovery. He now argued the affirmative side of the question, and indicated the considerations which induced him to believe that in the family tomb which he had discovered once reposed the Stagirite's remains.

According to the best authorities, Aristotle died at Chalcis in 332 B.C., of disease in the stomach, at the age of 63 years. The stories that he committed suicide by drinking hemlock and that he drowned himself in the Euripus, in consequence of disappointment at not being able to discover the cause of the ebb and flow of the tide, were both discredited by Zeller and the best authorities.

But it would be asked, as he died at Chalcis, how came he to be buried at Eretria, which was some 12 miles distant from the city of Chalcis? One answer to this objection was that in the Macedonian period the name Chalcis was sometimes used for the whole island of Eubœa, so completely had it eclipsed its former rival Eretria. Strabo described Chalcis as τὰ πρωτεῖα καὶ μητρόπολις of Eubœa. He then said, δευτερεύει δ' ἡ Ἐρέτρια. Thus the statement that he died at Chalcis was not inconsistent with his having been buried at Eretria.

Further, from the will of Aristotle himself, as published in Diogenes Laertius, it was to be inferred that the philosopher's house was not in the city, but in the country. By that will, which was a most interesting document, he gives his second wife, Herpyllis, a choice of residence; ἐὰν μὲν ἐν Χαλκίδι βούληται οἰκεῖν τὸν ξενῶνα τὸν πρὸς τῷ κήπῳ, ἐὰν δὲ ἐν Σταγείροις τὴν πατρῴαν οἴκιαν. Now, it was well ascertained, first, that the term Ξενών, or guests' quarters, was at this date applied not to a part of the principal residence, but to a separate house on a gentleman's estate. Thus in this instance, if the widow elected to live in Eubœa the Ξενών would correspond to the dower house. Next, it was not customary in Aristotle's time to have gardens in a city, and it was Epicurus who first, in the year 308, established gardens within the city. Thus the words πρὸς τῷ κήπῳ indicated that the house was in the country.

It was noticeable, also, that the contract to which he had referred, though it was to be performed at Eretria, was found at Chalcis, and there were other similar inscriptions dealing with Eretrian affairs which were discovered at Chalcis and not at Eretria. Again, it was known that Eretria was a philosophic centre, and Menedemus, the philosopher, lived there, and the place was also visited by Phædon. It might, in fact, be regarded as a literary suburb of Chalcis. Then the will contained instructions for the philosopher's burial. In effect, he said, "Bury me where you like. But take up the bones of my first wife and put them in the grave with me." Now, it was clear from the excavations that the tomb was a family grave; and from the will it was apparent that Aristotle would be the first occupant. There was architectural evidence that the particular part of the mausoleum in which the head of the family reposed was built towards the close of the fourth century.

Of course the name Aristotle was not unique, and the inscription deciphered on the slab, Βιότη Ἀριστοτέλου, was not conclusive. But it was by no means so common as other Greek patronymics. There were about 20 Aristotles whose names were recorded in literature. But none of these was Eubœan, save one whom he found to be a Chalcidian. We were, moreover, in possession of details of Aristotle's family history. He was twice married, his first wife having been called Pythias and his second Herpyllis. He had two children, a son Nichomachus, and a daughter Pythias. Nicomachus died without having been married, and Pythias was married three times. By Nicanor, her first husband, she had no children. To Procles, her second husband, who was a descendant of the Lacedæmonian King Demaratus, she bore two sons, Procles and Demaratus. By Metrodorus, who was a physician, she had a son who also bore the name of Aristotle. The name was also found in inscriptions in Sicily where there was a Chalcidian settlement, but it did not appear in Eretrian inscriptions earlier than the second century. In an Eretrian inscription of the second century there were about 1600 names, among which were found a Nicomachus and a Procles and three Aristotles. Now, it was an admissible hypothesis that the family of Pythias, one of whose sons was Aristotle, lived at Eretria or one of the cities of Eubœa, because we had also the name of Procles in this inscription. This Aristotle, the son of Pythias, was mentioned in the will of Theophrastus, who was the successor in the Peripatetic school of Aristotle.

A curious point arose in connection with this inscription *Βιότη Ἀριστοτέλου*. The ordinary genitive of the word was *Ἀριστοτέλους*. The latter form was invariably found in inscriptions before 353 B C. But from 350 to 300 the former began to prevail, and the German scholar Meisterhans had discoved 39 instances of *ου* when *ους* might have been expected. After 300 the latter form was found exclusively on inscriptions. The inscription was assigned by the best epigraphical authorities to the third century B.C. Now, it was clear from an examination of the remains that the principal grave, which was shown by the strigil to be that of a male, belonged to an earlier period than the adjoining graves. This Biote might, from the genitive which follows, have been either the wife or the daughter of Aristotle, the philosopher's grandson.

In addition to the inferences which might be drawn from the circumstances which he had mentioned, there were others to be derived from the study of iconography. There were a number of terra-cotta statuettes in the grave. But one, in particular, was of a singular and striking character. These statuettes in tombs were known to have relation to and to be frequently descriptive of the persons interred; and this was immediately recognisable as a type of the statues of the fourth century B.C., known as those of philosophers and orators. The figure was draped and the hands folded at the side. The grave was clearly that of a person of great distinction. There was a gold diadem and a band of pure gold about $1\frac{1}{2}$ inch wide, with *repoussé* patterns fastened round the brow, and then six were drawn out one after the other. Then at the head, where a portion of the skull remained, there was still another diadem with eaves of conventional ivy attached to it, and there was also, as he had mentioned, a metal pen. Here, therefore, he had discovered a tomb belonging to a great family, the burying-place of an eminent man, as was shown by the profusion of gold ornaments, and this man was a man of letters, as evidenced by the pen and styluses, and a philosopher, as indicated by the statuette. When to this was added the startling inscription which was disclosed in the adjoining space, the chain of historical and circumstantial evidence appeared to be almost irresistible.

The lecturer concluded his discourse with an enunciation of the principles by which in researches of this character, according to the doctrine termed by the late Henry Bradshaw, "prince of librarians and bibliographers," the doctrine of equivalents, a date might be assigned to a book or a work of art by the concurrence of notes or indications which were independently known to have been prevalent at a particular period.

The lecture was admirably illustrated from photographs taken for Dr. Waldstein by Mr. and Mrs. Gordon Oswald.

WEEKLY EVENING MEETING,

Friday, June 5, 1891.

Sir Dyce Duckworth, M.D. LL.D. F.R.C.P. Vice-President, in the Chair.

St. George J. Mivart, Esq. Ph.D. M.D. F.R.S. V.P.L.S. V.P.Z.S. *M.R.I.*

The Implications of Science.

After a brief introduction, the lecturer said:—By "the implications of science" I mean nothing to which any section of my hearers can object, whatever their notions about creed or conduct may be. I desire carefully to eliminate all question of either religion or morals, and I shall confine myself purely and simply to the consideration of certain propositions which appear to me to be latent within, and to give force to, what we regard as well-ascertained scientific truths. They are propositions which must, I believe, be assented to by every consistent follower of science who is convinced that science has brought to our knowledge some truths on which we can with entire confidence rely.

My appeal then is to the pure intellect of my hearers, and to nothing else. And, indeed, I desire to take this opportunity plainly to declare, that not only here and now, but everywhere and always, I unhesitatingly affirm that no system can or should stand which is unable to justify itself to reason. I possess no faculty myself, nor do I believe that any human faculty exists, superior to the intellect, or which has any claim to limit or dominate the intellect's activity. Feelings and sentiments have their undoubted charm and due place in human life, but that place is a subordinate one, and should be under the control of right reason.

Yet it is by no means only, or mainly, against those who would undervalue reason in the interest of sentiment, that I have this evening to protest. My object is to uphold what I believe to be the just claims of our rational nature against all who, from whatever side, or in the name of whatsoever authority, would impugn its sovereign claims upon our reverence or unduly restrict the area of its sway.

As I have already intimated, I propose to fulfil this task by calling attention to some half-dozen far-reaching truths implicitly contained in scientific doctrines universally admitted; so that those doctrines cannot logically be maintained if such implied truths are really and seriously doubted, and still less if they are really disbelieved and denied. These truths, then, are what I mean by "the implications of science." But what is science?

The word "science" is now very commonly taken as being synonymous with "physical science." There is much to be said against giving the word so narrow a meaning; nevertheless that meaning will sufficiently serve my purpose this evening. Science then, thus understood, is merely ordinary knowledge pursued with extreme care—most careful observation, measuring, weighing, &c., together with most careful reasoning as to the results of observations and experiments, and also painstaking verification of any anticipations which may have been hazarded. In this way our thoughts are made to conform as accurately as may be with what we regard as the realities they represent.

The value and the progress of science are unquestioned. Many foolish discussions are carried on in the world about us. But certainly no one disputes or doubts the value of science or the fact of its progress. The value of carefully-ascertained scientific truths will not at any rate be disputed in this theatre, which has witnessed the triumphs of the immortal Faraday, and which may justly claim to be a very temple of science. And certainly I have no disposition to undervalue it, who have loved it from my earliest years and devoted such small powers as I possess to its service. I am profoundly convinced that, since I can recollect, biological science has made great progress, and I see grounds for absolute certainty now, about many propositions in zoology which were doubtful or undreamed of when I was a lad.

We all then agree that science does advance. Nevertheless it is obvious that such advance would be impossible if we could not by observations, experiments, and inferences, become so certain with respect to some facts, as to be able to make them the starting points for fresh observations and inferences as to other facts. Thus, with respect to the world we live in, most educated men are now certain as to its daily and annual revolutions, as also that its crust is largely composed of sedimentary rocks, containing remains, or indications, of animals and plants more or less different from those which now live. No one can reasonably deny that we may repose with absolute confidence and entire certainty upon a variety of such assertions.

But our scientific certainties have been acquired more or less laboriously, and a questioning attitude of mind is emphatically the scientific attitude. We ought never to rest satisfied about any scientific inquiry the truth of which has not been demonstrated, unless we find that it is one which we have no probable power to answer—it would obviously be idle to occupy ourselves about the shape or number of the mountains on that side of the moon which is constantly turned away from us.

Yet although doubt and inquiry are necessary in science, nevertheless doubt has its legitimate limits. Blind disbelief is scientifically fatal as well as blind belief. We all know how apt men are, when seeking to avoid one extreme, to fall into the opposite one, and it is possible to get into an unhealthy condition of mind so as to be unable to give a vigorous assent to anything. It is necessary distinctly to recognise there is such a thing as legitimate certainty, not to perceive the force of which is illegitimate doubt. Such doubt would necessarily discredit all physical science. Universal doubt, for example, is an absurdity—it is scepticism run mad. If any one affirms that "nothing is certain," he obviously contradicts himself, since he thereby affirms the certainty of uncertainty. He says that which if true absolutely contradicts what he has declared to be true. But a man who affirms what the system he professes to adopt forbids him to affirm, and who declares that he believes what he also declares to be unbelievable, should hardly complain if he is called foolish. No system can be true, and no reasoning can be valid, which inevitably ends in absurdity. Such scepticism, then, cannot be the mark of an exceptionally intellectural mind, but of an exceptionally foolish one, and every position which necessarily leads to scepticism of this sort must be an untenable position.

A very little reflection suffices to show how self-refuting such modes of thought are: Thus, if a man were to say—"I cannot know anything, because I cannot be sure that my faculties are not always fallacious," or "I cannot be sure of anything because, for all I know, I may be the plaything of a demon who amuses himself by constantly deceiving me"—in both these cases he contradicts himself. He contradicts himself because he obviously grounds his assertion upon his perception of the truth that "we cannot arrive at conclusions which are certain, by means of principles which are uncertain or false." But if he knows that truth he must know that his faculties are not always fallacious and that his demon cannot deceive him in everything.

My object in making these remarks is to enable us to get clear of mere idle, irrational doubts which have no place in science, and can have none, so that we may recognise the fact that we all of us have certainty as to some facts according to our degrees of knowledge. Obviously we can only judge of truth by our mental faculties, and if a man denies their validity we must pass him by, contenting ourselves with calling his attention to the fact that he refutes himself. If a man professes to doubt his faculties, or to doubt whether language can be trusted to convey thought, then plainly we cannot profitably argue with him. But if, on account of his absurdity, we cannot refute him, it is no less plain that he cannot defend his scepticism. Were he to attempt to do so, then he would show, by that very attempt, that he really had confidence in reason and in language, however he might verbally deny it. Confident, then, that there are some scientific statements on which we may rely with certainty, let us consider a few truths implicitly contained in them.

In the first place, science makes use not only of observations and experiments, but also of reasoning as to the results of such experiments. It needs that we should draw valid inferences; but this implies that we may, and must, place confidence in the principle of deduction—in that perception of the mind which we express by the word "therefore."

When we use that word, we mean to express by it that there is a truth, the certainty of which is shown through the help of different facts or principles, which themselves are known to be true.

It is sometimes objected to deductive reasoning—to the syllogism—that it really teaches us nothing new, all that is contained in the conclusion being contained already in the premises. But this objection is due to a want of perception of the great difference which exists between implicit and explicit knowledge. Let us suppose a person to be looking at some very flexible and soft kind of fish. He may perhaps say to himself, "This creature can have no spinal column!" Then it may strike him that naturalists have classed fishes, together with other animals, in a great group, one character of which is the possession of a spinal column, and so he may explicitly recognise a truth implied in what he knew before. So great indeed is the difference between explicit and implicit knowledge, that the latter may not deserve to be called real knowledge at all. No one will affirm that a student who has merely learned the axioms and definitions of Euclid, has thereby obtained such a real knowledge of all the geometrical truths the work contains, that he will fully understand all its propositions and theorems without having to study them. Yet all the propositions, &c., of Euclid are implicitly contained in the definitions and axioms. Nevertheless the student will have to go through many processes of inference by which these implicit truths may be explicitly recognised by him, before he can be said to have any real knowledge of them.

The validity of inference is then one of the truths implied by physical science, and we shall presently see the intellectual penalty which must be paid for any real doubt about it.

In the second place, physical science is emphatically experimental science. But every experiment, carefully performed, implies a most important latent truth. For when an experiment has shown us that anything is certain—as for example, that a newt's leg may grow again after amputation, because one has actually grown again—we shall find that such certainty implies a prior truth. It implies the truth that if the newt has come to have four legs once more, it cannot at the very same time have only three legs. This may seem too trivial a remark to some of my hearers, but there is nothing like a concrete example for making an abstract truth plain. Anything we are certain about because it has been proved to us by experiment, is certain only if we know, and because we know, that a thing which has been actually proved cannot at the same time remain unproven. If we reflect again on this proposition we shall see that it depends on a still more fundamental truth which our reason recognises—the truth, namely, that "nothing can at the same time both be and not be"—the truth known as "the law of contradiction"; and this I bring forward as a second truth implied by physical science.

If we reflect upon this law we shall see that our intellect recognises it as an absolute and necessary truth, which carries with it its own evidence. It is but the summing up, in one general expression, of all the concrete separate cases, such as that of the newt's leg, of the fact that if a man possesses two eyes he cannot at the same time have only one—and so on.

But an objection has been made as follows: "It is very true that I cannot imagine having two eyes and only one eye at the same time, and so I must practically acquiesce in the statement, but I am only compelled to do so by the impotence of my imagination." Thus instead of the "law of contradiction" Mr. Herbert Spencer has put forward as an ultimate truth his "universal postulate"—the assertion that "we must accept as true propositions we cannot help thinking, because we cannot imagine the contrary." But if any of my hearers will reflect over what his mind tells him when it pronounces that he cannot at the same time have both two eyes and only one eye, he will I think, see that his perception is (as mine is) a perception of real incompatibility and consequent positive impossibility. He will not find his mind a mere blank passively unable to imagine something. He will find that his mind actively asserts its power to judge of the matter, as well as what its judgment is, and that the truth is one which positively applies to things and not merely to his own imaginings.

Moreover, this objection ignores the difference between intellect and imagination. Yet there are very many things we can conceive of but cannot imagine, as for example, our "act of sight" or our own annihilation. But it appears to me evident that Mr. Herbert Spencer's "universal postulate" can never be itself an ultimate truth, but must depend upon the law of contradiction. For, supposing we had tried to imagine a thing and failed, how could we, from that, ever be sure we might not at the same time have tried and succeeded, if we could not rely upon the law of contradiction? The consequences resulting from any real doubt as to this law we will see later on.

In the pursuit of science, observation is anterior to experiment, but in every observation in which we place confidence, and, still more, in every experiment, a third fundamental truth is necessarily implied: this implied truth is the validity of our faculty of memory.

It is plain that it would be impossible for us to be certain about any careful observation or any experiment, if we could not feel confidence in our memory being able to vouch for the fact that we had observed certain phenomena and what they were. But what is memory? Evidently we cannot be said to remember anything unless we are conscious that the thing we so remember has been present to our mind on some previous occasion. A mental image might present itself to our imagination a hundred times; but if at each recurrence it seemed to us something altogether new and unconnected with the past, we could not be said to remember it. It would rather be an example of extreme forgetfulness than of memory.

By asserting the trustworthiness of our faculty of memory, I do not, of course, mean that we may not occasionally make mistakes about the past. It is quite certain we may and do make such mistakes. But nevertheless we are all of us certain as to some past events. Probably there is no single person now in this room, who is not certain that he was somewhere else before he entered it. Memory informs us—certainly it informs me—as surely concerning some portions of the past, as consciousness does concerning some portions of the present.

If we could not trust our faculty of memory, the whole of physical science would be, for us, a mere present dream. But there can be no such thing as proof of the trustworthiness of memory, since no argument is possible without trusting to the veracity of memory. It is therefore a fundamental fact which must be taken on its own evidence and from a consideration of the results of any real doubts about it: results I will refer to presently.

Yet it has been strangely declared, by a leading agnostic, that we may trust our memory because we learn its trustworthiness by experience. Surely never was fallacy more obvious! How could we ever gain experience if we did not trust memory in gaining it? Particular acts of memory may, of course, be confirmed by experience if the faculty of memory be already trusted, but in every such instance it must be confided in. The agnostic referred to has told us in effect that we may place confidence in our present memory, because in past instances its truth has been experimentally confirmed, while we can only know it has been so confirmed, by trusting our present memory!

But if we admit the trustworthiness of memory at all, a most important consequence follows—one relating to the distinction between what is subjective and what is objective. Every feeling or state of consciousness present to the mind of the subject who possesses it is subjective, and the whole of such experiences taken together constitute the sphere of subjectivity. Whatever is external to our present consciousness or feeling is for us objective, and all that is thus external is the region of objectivity. Now memory, inasmuch as it reveals to us part of our own past, reveals to us what is objective, and so introduces us into the realm of objectivity, shows us more or less of objective truth, and carries us into a real world which is beyond the range of our own present feelings. This progress, then—this knowledge of objectivity—is, through memory, implied in every scientific experiment the facts of which we regard as certain.

But our scientific observations and experiments carry with them yet another implication more important still: this is the certainty of our knowledge of our own continuous existence. Unless we can be sure that we actually made those observations and experiments on our having made which we rely for our conclusions, how can those conclusions be confidently relied on by us?

This implication is so important—in my opinion so fundamentally important—that I must crave your permission to notice it, later on, at some length. But before considering it, I desire to call your attention to the fact that the propositions thus implied by physical science, run directly counter to a system of thought which is widely current to-day, and which has now and again found expression in this theatre. The popular views I refer to may be conveniently summed up as follow:

1. All our knowledge is merely relative.
2. We can know nothing but phenomena.
3. We have no supremely certain knowledge but that of our own feelings, and therefore we have none such of our continuous existence.
4. We cannot emerge from subjectivity or attain to real knowledge of anything objective.

Therefore, either I am very much mistaken, or those who uphold the views I have just summed up are much mistaken.

It may seem presumptuous on my part to come forward here to-night to controvert a system upheld by men of such undoubted ability and so unquestionably competent in science, as are men who uphold the system I oppose. I feel, therefore, that a few words of personal apology and explanation are due from me.

For full five and thirty years I have been greatly interested in such questions. But when my intellectual life began, it was as a student and disciple of that school with which the names of John Stuart Mill, Alexander Bain, G. H. Lewes, Herbert Spencer, and Professor Huxley have been successively associated—more or less closely. The works of writers of that school I studied to the best of my ability, and I had the advantage of personal acquaintance with some of the more distinguished of them. Thus, by conversation, I was much better enabled to learn what their system was, than I could have learned by reading only.

However, by degrees, I became sceptical about the validity of the system I had, at first, ingenuously adopted; but it took me not a few years to clearly see my way through all the philosophical fallacies—as I now regard them—in which I found myself entangled. I say "see my way through," for I did not free myself from them by drawing back, but by pushing forwards—slowly working my way through them and out on the other side. These circumstances constitute my apology for appearing before you as I do. I have been a dweller in the country which I am willing to aid any one to explore who may wish to explore it.

I might now at once return to further consider those implications of science to which I have called your attention, but I think it will be better to first briefly pass two important matters in review. The first concerns our means of investigation as to such fundamental questions; the second relates to our ultimate grounds for forming judgments about them. We have to consider how fundamental truth can be acquired and tested. Evidently the only means of which we can make use are our thoughts—our reason—our intellectual activity. "Thoughts" may be, and should be, carefully examined and criticised, but however much we may do so and whatever the results we arrive at, such results can only be reached by thoughts, and must be expressed by the aid of our thoughts. This will probably seem such a manifest truism that I shall be thought to have committed an absurdity in enunciating it. To suppose that by any reasoning we can come to understand what we can never think, may seem an utterly incredible folly; yet at a meeting of a metaphysical society in London a speaker not long ago expressly declared "thought" to be a misleading term, the use of which should be avoided.

Now I am far from denying that unconscious activities, of various different orders, take place in our being; yet whatever influence such activities may have, they cannot affect our judgments save by and in thoughts.

If a man is convinced that thoughts are worthless tools, he can only have arrived at that conclusion by using the very tools he declares to be worthless. What, then, ought his conclusion to be worth even in his own eyes? It is simply impossible by reason to get behind or beyond conscious thought, and our thoughts are and must be our only means of investigating problems however fundamental. Even in investigating the properties of material bodies, it is to self-conscious reflective thought that our final appeal must be made. It is to our thoughts and not to our senses only, that our ultimate appeal must be made, even with respect to the most material physical science matters. Some persons may imagine that with respect to such investigations about the properties of material bodies, it is to our sensations alone that we must ultimately appeal. But it is not so. Any one would be mad to question the extreme importance, the absolute necessity, of our sensations in such a case; nevertheless, after we have made all the observations and experiments we can, how can we know we have obtained such results as we may have obtained, save by our self-conscious thought? By what other means are we to judge between what may seem to be the conflicting indications of different sense impressions? Our senses are truly tests of certainty but not *the* test. Certainty belongs to thought, and self-conscious reflective thought is our last and absolute criterion.

As to the ultimate grounds on which our judgments respecting such problems must repose, as Mr. Arthur Balfour has forcibly pointed out, that is a question altogether distinct from all questions as to the origin of our judgments, or reasonings about their truth. Such matters are very interesting, but they are not here in point, since it is plain that no proposition capable of proof can be one the certainty of which is fundamental. For in order to prove anything by reasoning, we must show that it necessarily follows as a consequence from other truths which therefore must be deemed more indisputable. But the process must stop somewhere. We cannot prove everything. However long our arguments may be, we must at last come to ultimate statements which must be taken for granted like the validity of the process of reasoning itself, which is one of the implications of science. If we had to prove either the validity of that process or such ultimate statements, then either we must argue in a circle, or our process of proof must go on for ever without coming to a conclusion, which means there could be no such thing as "proof" at all.

Therefore the grounds of certainty which any fundamental proposition may possess, cannot be anything external to it—which would imply this impossible proof. The only ground of certainty which an ultimate judgment can possess is its own self-evidence—its own manifest certainty in and by itself. All proof, all reasoning must ultimately rest upon truths which carry with them their own evidence and do not therefore need proof.

It is possible that some of my hearers may be startled at the suggestion of believing anything whatever on its own evidence, fancying it is equivalent to a suggestion that they should believe anything blindly. This, I think, is due to the following fact of mental association. The immensely greater part of our knowledge is gained by us indirectly—by inference or testimony of some kind. We commonly ask for a proof, with regard to any new and remarkable statement, and no truths are brought more forcibly home to our minds than are those demonstrated by Euclid. Thus it is that many persons have acquired a feeling that to believe anything which cannot be proved, is to believe blindly. Hence arises the tendency to distrust what is above and beyond proof. We are apt to forget what on reflection is manifest, namely that if it is not blind credulity to believe what is evident to us by means of something else, it must be still less blind to believe that which is directly evident in and by itself.

And self-conscious reflective thought tells me clearly, that the law of contradiction is not only implied by all science, and necessary to the validity of all science, but that it is, as I said, an absolutely necessary truth which carries with it its own evidence. It must be a truth then applicable both to the deepest abyss of past time and the most distant region of space. But here again I think it possible that one or two of my hearers may be startled, and perhaps doubting how things in this respect may be in the Dog star now or how they were before the origin of the solar system. I fancy I hear some one asking, "How is it possible that we, mere insects, as it were, of a day, inhabiting an obscure corner of the universe, can know that anything is and must be true for all ages and every possible region of space?"

In the first place I think the difficulty which may be thus felt is due to the abstract form of the law of contradiction. And yet, as I said before, it is but the summing up of all the particular instances as to each one of which no difficulty at all is felt, but each is clearly seen to be true. Any man who really doubted whether if his legs were cut off they might not at the same time remain on, would have a mind in a diseased condition. There is, however, another reason which indisposes some persons to see the necessary force of this law. It is due, I think, to a second fact of mental association.

Things which are very distant or which happened a long time ago, are known to us only in roundabout ways, and we often feel more or less want of certainty about them. On the other hand we have a practical certainty concerning the things which are about us at any given moment. Thus we have come to associate a feeling of uncertainty with statements about things very remote. But nothing can well be more remote from us than the more distant regions of space or before the origin of the solar system. It is not surprising then that this mental association should call forth a feeling of uncertainty with respect to any statement about universal truth.

It is no doubt wonderful that we should be able to know any necessary and universal truths; but it is less exceptionally wonderful, when we come to think the matter all round, than it may at first sight appear to be. It is wonderful, but so, deeply considered, is all our knowledge. It is wonderful that through molecular vibrations, or other occult powers of bodies, we have sensations—such as of musical tones, sweetness, blueness, or what not. It is wonderful that through sensations, actual and remembered, we have perceptions. It is wonderful that on the occurrence of certain perceptions, we recognise our own existence past and present. So also it is wonderful that we recognise that what we know *is*, cannot at the same time not be. The fact is so, and we perceive it to be so; we know things and we know that we know them. How we know them is a mystery indeed, but one about which it is, I think, perfectly idle to speculate. It is precisely parallel to the mystery of sensation. We feel things savoury or odorous, or brilliant, or melodious, as the case may be; and with the aid of the scalpel and the microscope we may investigate the material conditions of such sensations. But how such conditions can give rise to the feelings themselves, is a mystery which defies our utmost efforts to penetrate. I make no pretension to be able to throw any light upon the problem How is knowledge possible? any more than on the problem How is sensation possible? or on the questions How is life possible? or How is extension possible? but "Ignorantia modi non tollit certitudinem facti"; and we know that we are living, that we feel, and that we do know something—if only that we know we doubt about the certainty of our knowledge.

And, with respect to such doubt, let me here put before you the intellectual penalties which have to be paid for any real and serious doubt with respect to the implications of science. I think we shall see that nothing less than intellectual suicide or mental paralysis must be the result. And such a result must also be logically fatal to every branch of science.

The first implication I put before you was the validity of inference. Now no one who argues, or who listens to or reads, with any serious intention, the arguments of others, can, without stultifying himself, profess to think that no process of reasoning is valid. If the truth of no mode of reasoning is certain, if we can make no certain inferences at all, then all arguments must be useless, and to proffer, or to consider, them must be alike vain. But not only must all reasoning addressed to others be thus vain, the silent reasoning of solitary discursive thought must be vain also. Yet what does this amount to, save an utter paralysis of the intellect. It is scepticism run mad.

But the implication I regard as one of the most important of all, is the implication of our knowledge of our own continuous existence, concerning which I said I must crave your permission to speak at some length. It was the mention of this implication which led me to refer to that system of thought it is my object here to controvert.

I have heard it proclaimed in this theatre by Professor Huxley, that we cannot have supreme certainty as to our own continuous existence, and that such knowledge is but secondary and subordinate to our knowledge of our present feelings or "states of consciousness."

Of course I am not thus accusing him of originating any such erroneous views. In that matter he is but a follower of that daring and playful philosopher Hume. I say "playful," because I cannot myself think that he really believed his own negations. He seems to me too acute a man to have been himself their dupe. But, however this may be, I here venture directly to contradict Hume's and Professor Huxley's affirmation, which is also adopted by Mr. Herbert Spencer, and to affirm that we have the highest certainty as to our own continuous existence.

It is, of course, quite true that we have complete certainty about our present feelings, as also that we cannot know ourselves apart from our feelings; but it is no less true that we cannot be conscious of feelings apart from the "self" which has those feelings. Now it is assumed by those I oppose that we can know nothing with complete certainty, unless we know it by itself or unmodified, or as existing absolutely. But in fact nothing, so far as we know, exists apart from every other entity and unmodified, or "absolutely," as it is, in my opinion, absurdly called. No wonder then if we do not know things in a way in which they never do, and probably never can, exist. We really know nothing by itself, because nothing exists by itself. It is not wonderful, then, if we only know ourselves as related to our simultaneously known feelings, or *vice versâ*. It is quite true that we never know our own substantial essential being alone and unmodified; but then we have never for an instant so existed. Our knowledge of ourselves in this respect is like our knowledge of anybody and everybody else. Most persons here present doubtless know Professor Tyndall; yet they never knew him (no one ever knew him) except in some state—either at home or away from home, either sitting or not sitting, either in motion or at rest, either with his head covered or uncovered—and this for the very good and obvious reason that he never did or could exist for a moment save in some state. But this does not prevent your knowing him very well, and the same consideration applies to our knowledge of ourselves. When I consider what is my primary, direct consciousness at any moment, I find it to be neither a consciousness of a state of feeling, nor of my continuous existence, but a consciousness of doing something or having something done to me—action or reaction. I have always indeed some feeling and also some sense of my self-existence, but what I perceive primarily, directly and immediately, is neither the feeling nor the self-existence, but some concrete actual doing, being, or suffering, then experienced. We can, indeed, become distinctly and explicitly aware of either the feeling or the self-existence, by turning back the mind upon itself. But to know that one has a feeling, or is in a state, or even that a feeling exists, is plainly an act by which no one begins to think. It is evidently a secondary act—an act of reflection. No one begins by perceiving his perception, any more than he begins by expressly adverting to the fact that it is he himself who perceives it.

Let us suppose two men to be engaged in a fencing match. Each man while he is parrying, lunging, &c., has his feelings or states, and knows that it is he who is carrying on the struggle; yet it is neither his mental states nor the persistence of his being which he directly regards, but his concrete activity—what he is doing and what is being done to him. He may, of course, if he chooses, direct his attention either to the feelings he is experiencing, or to his underlying continuous personality. Should he do so, however, a hit from his adversary's foil will be a probable result.

But to become aware that one has any definite feeling, is a reflex act at least as secondary and posterior as it is to become aware of the self which has the feeling. I say "at least," but I believe that of the two perceptions (1) of feelings and (2) of self, it is the self which is the more prominently given in our primary direct cognitions. I believe that a more laborious act of mental digging is requisite to bring explicitly to light the implicit mental state, than to bring forward explicitly the implicit self-existence. Men continually and promptly advert to the fact that actions and sufferings are their own, but do not by any means so continually and promptly advert to the fact that the feelings they experience are existing feelings.

Therefore I am convinced that one of the greatest and most fundamental errors of our day is the mistake of supposing that we can know our mental states or feelings more certainly and directly than we can know the continuously existing self which has those feelings.

Our perception of our continuous existence also involves the validity of our faculty of memory which is implied in this way, as well as in every scientific experiment we may perform. For we cannot obviously have a reflex perception, either of our feelings or our self-existence, without trusting our memory as to the past; since, however rapid our mental processes may be, no mental act takes place without occupying some period of time—and, indeed, nervous action is not extremely rapid. In knowing therefore such facts by a reflex act, we know by memory what is already past. Thus our certainty as to our own continuous existence, necessarily carries with it a certainty as to our faculty of memory. Therefore the mental idiocy of absolute scepticism is the penalty that has to be paid for any real doubt about our own existence or the trustworthiness of the faculty of memory, for all our power of reposing confidence in our observations, experiments, or reasonings would, in that case, be logically at an end. On the other hand the validity of our faculty of memory establishes once for all (as we have seen) the fact that we can transcend our present consciousness, and know real objective truth.

Let us now see the consequences of the denial, or real doubt, of the second implication of science—the law of contradiction. Without it we can be certain of nothing, and it therefore lands us in absolute scepticism. And if we would rise from such intellectual paralysis, we must accept that dictum as it presents itself to our minds; and the dictum presents itself to my mind, not as a law of thought only, but a law of things. It affirms, for example, that no creature anywhere or anywhen can at the same time be both bisected and entire.

An amusing instance of the way in which very distinguished men may be misled as to the question of our power of perceiving necessary truth, is offered by an imaginary case which has been put forward by Professor Clifford and Professor Helmholz. Their object in advancing it was to show, by an example, how truths which appear necessary to us are not objectively necessary. But the result appears to me to show the direct contradictory of what they intended. Their intention evidently was to support the proposition that we can know no truths to be absolutely necessary, and the result is to show that even according to them, some truths are absolutely necessary. The necessary truths they propose to controvert are that a straight line is the shortest line between two points, and that two straight lines cannot enclose a space.

For this purpose curious creatures, possessing length and breadth but no thickness, were supposed, by them, to be living on a sphere, with the surface of which their bodies would coincide. They were imagined to have experience of length and breadth in curves, but none of heights and depth or of any straight lines. To such creatures, it was said, our geometrical necessary truths would not appear truths at all. A straight line for them could not be the shortest line, while two parallel lines prolonged would enclose a space.

To this imaginary objection I reply as follows: Beings so extraordinarily defective might, likely enough, be unable to perceive geometrical truths which to less defective creatures—such as ourselves—are perfectly clear. Nevertheless if they could conceive of such things at all, as those we denote by the terms "straight lines" and "parallel lines," then there is nothing to show that they could not also perceive those same necessary truths concerning them which are evident to us.

It is strange that the very men who make this fanciful objection actually show, by the way they make it, that they themselves perceive the necessary truth of those geometrical relations, the necessity of which they verbally deny. For how, otherwise, could they affirm what would or would not be the necessary results attending such imaginary conditions? How could they confidently declare what perceptions such conditions would certainly produce, unless they were themselves convinced of the validity of the laws regulating the experiences of such beings? If they affirm, as they do, that they perceive what must be the truth in their supposed case, they thereby implicitly assert the existence of some absolutely necessary truths, or else their own argument itself falls to the ground.

But this same implication of science respecting the objective, absolute validity of the law of contradiction, also refutes that popular system of philosophy which declares that all our knowledge is merely relative and that we can know nothing as it really exists independently of our knowledge of it—the system which proclaims the relativity of knowledge.

Of course anything which is known to us cannot at the same time be unknown to us, and so far as this, our knowledge may be said to affect the things we know. But this is trivial. Our knowing or not knowing any object is—apart from some act of ours which results from our knowledge—a mere accident of that body's existence which is not otherwise affected thereby.

Again, as I before remarked, nothing, so far as we know, exists by itself, and unrelated to any other thing. To say, therefore, that all our knowledge is relative, might only mean that knowledge concords with objective reality. But this is by no means what the upholders of the relativity of knowledge intend to signify: they deny the objective validity, the actual correspondence with reality, of any of our perceptions or cognitions—even, as Mr. Herbert Spencer tells us, our cognition of difference.

Every system of knowledge, however, must start with the assumption, implied or expressed, that something is true. By the teachers of the doctrine of the relativity of knowledge it is evidently taught that this doctrine of the relativity of knowledge is true. But if we cannot know that anything corresponds with external reality, if nothing we can assert has more than a relative or phenomenal value, then this character must also appertain to the doctrine of the relativity of knowledge. Either this system of philosophy is merely relative or phenomenal, and cannot be known to be true, or else it is absolutely true and can be known so to be. But it must be merely relative and phenomenal, if everything known by man is such. Its value, then, can be only relative and phenomenal; therefore it cannot be known to correspond with external reality, and cannot be asserted to be true; and anybody who asserts that we can know it to be true, thereby asserts that it is false to say that our knowledge is only relative. In that case some of our knowledge must be absolute; but this upsets the foundation of the whole system. Any one who upholds such a system as this may be compared to a man seated high up on the branch of a tree which he is engaged in sawing across where it springs from the tree's trunk. The position taken up by such a man would hardly be deemed the expression of an exceptional amount of wisdom. My time has expired, and I may say no more.

The considerations I have put before you this evening, should they commend themselves to your judgment, will, I think, lead you to admit that if we feel confidence and certainty in any part of any branch of physical science, we thereby implicitly affirm that the human mind can by consciousness and memory know more than phenomena—can know some objective reality—can know its own continuous existence, the validity of inference, and the certainty of universal and necessary truth, as exemplified in the law of contradiction. In other words, the system of the relativity of knowledge is untrue. Thus, the dignity of that noble, wonderful power, the human intellect, is fully established, and the whole of our reason, from turret to foundation stone, stands firmly and secure. If I have succeeded in bringing this great truth home to one or two of my hearers who before doubted it, I am abundantly repaid for the task I have undertaken.

[St. G. M.]

WEEKLY EVENING MEETING,

Friday, June 12, 1891.

SIR FREDERICK BRAMWELL, Bart. D.C.L. F.R.S. Honorary Secretary and Vice-President, in the Chair.

HAROLD B. DIXON, Esq. M.A. F.R.S. Professor of Chemistry in the Owens College, Manchester.

The Rate of Explosions in Gases.

THE rapid act of chemical change, which follows the kindling of an explosive mixture of gases, has of late years attracted the interest both of practical engineers and of theoretical chemists. To utilise for motive power the expansive force of ignited gases; to minimise the chance of disastrous conflagrations of fire-damp in coal mines; to follow the progress of chemical changes under the simplest conditions, are some among the problems presented to us in industry or science, demanding for their solution a knowledge of the phenomena of the explosions of gases.

To understand the nature of explosions in gases it is necessary to know certain fundamental properties of the explosive mixture. With this object in view experimenters have sought to determine for various mixture of gases:—the heat of chemical combination; the temperature of inflammation; the pressure developed; and lastly, the rate at which the explosion is propagated under different conditions.

It is on the last of these problems—the determination of the velocity with which the flame travels through the gas—that I have been asked to speak.

Twenty-four years ago Bunsen described a method of measuring the rapidity of the flame in gas explosions. Passing a mixture of explosive gases through an orifice at the end of a tube and igniting the gases as they issued into the air, he determined the rate at which the gases must be driven through the tube to prevent the flame passing back through the opening, and exploding inside the tube. By this method he found that the rate of propagation of the ignition of hydrogen and oxygen was 34 metres per second, while the rate of ignition of carbonic oxide and oxygen was less than 1 metre per second. Bunsen applied these results to the rate of explosion of gases in closed vessels, and his results were accepted without cavil for fourteen years.

By 1880 facts began to accumulate which seemed inconsistent with Bunsen's conclusions. For instance, between 1876–80 I had several times observed that the flame of carbonic oxide and oxygen travelled in a long eudiometer too quickly to be followed by the eye. Mr. A. V. Harcourt, in his investigation of an explosion which happened in a large gas main near the Tottenham Court Road in 1880, was led to the conclusion that the flame travelled at a rate exceeding 100 yards per second. In the winter of 1880–1 I noticed the rapid increase of velocity as a flame of carbon bisulphide with nitric oxide travelled down a long glass vessel; and shortly afterwards I attempted to measure the rate of explosion of carbonic oxide and oxygen by photographing on a moving plate the flash at the beginning and end of a long tube. The two flashes appeared to be simultaneous to the eye, but no record of the rate was obtained, for the apparatus was broken to pieces by the violence of the explosion.

In July 1881 two papers appeared in the 'Comptes Rendus,' one by M. Berthelot, the other by MM. Mallard and Le Chatelier. Both papers announced the discovery of the enormous velocity of explosion of gaseous mixtures. Other papers quickly followed by the same authors. M. Berthelot made the important discovery that the rate of explosion rapidly increases from its point of origin until it reaches a maximum which remains constant, however long the column of gases may be. This maximum M. Berthelot states to be independent of the pressure of the gases, of the material of the tube, and of its diameter above a small limit. The rate of explosion thus forms a new physico-chemical constant, having important theoretical and practical bearings. The name "L'Onde Explosive" is given by Berthelot to the flame when propagated through an explosive mixture of gases at the maximum velocity.

While Berthelot, associated with Vieille, was measuring the rate of the "explosion-wave" for various mixtures of gases, Mallard and Le Chatelier continued the study of the preliminary phenomena of explosion which precede the formation of the "wave." They showed by photographing on a revolving cylinder:—(1) that when a mixture such as nitric oxide and carbon bisulphide is ignited at the open end of a tube, the flame travels a certain distance (depending on the diameter and length of the tube) at a uniform velocity; (2) that at a certain point in the tube, vibrations are set up which alter the character of the flame, and that these vibrations become more intense, the flame swinging backwards and forwards, with oscillations of increasing amplitude; and (3) that the flame either goes out altogether, or that the rest of the gas detonates with extreme velocity. Again, when a mixture of gases was fired near the closed end of the tube, they found the velocity of the flame regularly increased, as far as their instruments were able to record the rapidly increasing pace.

Mixtures of coal-gas with air, and of fire-damp with air, show phenomena of the first and second kind. Ignited at the open end of a tube these mixtures burn at a uniform rate for a certain distance, and then the flame begins to vibrate.

The vibrations acquire greater or less velocity according to the nature of the mixture and the conditions of the experiment; but the third régime of uniform maximum velocity is not set up. In narrow tubes the explosion soon dies out.

The phenomena studied by Mallard and Le Chatelier have been observed on a large scale in explosions in coal mines. It has been noticed that little damage was caused at the source of an explosion, and for a distance varying from 50 to 80 yards from the origin of the flame, while beyond that distance falls of roof, broken tubs, and blown-out stoppings have testified to the violence exerted by the explosion. Great as the destruction is which an explosion of fire-damp and air causes in a mine, it is fortunate that these mixtures do not *detonate.*

Passing on to Berthelot's researches on the régime of detonation, I will briefly summarise the results he has arrived at.

The actual velocities of explosion are compared by Berthelot with the mean velocity of translation of the gaseous products of combustion, supposing these products to contain all the heat that is developed in the reaction.

For instance, we know the total heat given out when hydrogen and oxygen combine. If this heat is contained in the steam produced, we can calculate what its temperature must be if we know its heat capacity. And if we know the temperature of the steam, we can calculate the mean velocity with which the molecules must be moving. Now Berthelot supposes that the heat is all contained in the steam produced. He assumes that the heat capacity of steam is the same as the sum of those of its constituents; and he supposes, moreover, that the steam is heated at constant pressure. Making these assumptions, he calculates out the theoretical mean velocity of the products of combustion of various mixtures, and finds a close accordance between these numbers and the explosion rates of the same mixtures. He concludes that the explosive wave is propagated by the impact of the products of combustion of one layer upon the unburnt gases in the next layer, and so on to the end of the tube at the rate of movement of the products of combustion themselves. If his theory is true, it accounts not only for the extreme rapidity of explosion of gaseous mixtures, and gives us the means of calculating the maximum velocity obtainable with any mixture of gases, but it also affords us information on the specific heats of gases at very high temperatures, and it explains the phenomena of detonation whether of gases or of solid or liquid explosives.

Table I. shows the explosion rates found by Berthelot, compared with the theoretical velocity of the products of combustion :—

TABLE I.

BERTHELOT'S EXPERIMETNS.

Gaseous Mixture.	Velocity in Metres per Second.	
	Theoretical.	Found.
H_2 + O Hydrogen and oxygen.	2830	2810
H_2 + N_2O Hydrogen and nitrous oxide.	2250	2284
CO + O Carbonic oxide and oxygen.	1940	1090
CO + N_2O Carbonic oxide and nitrous oxide.	1897	1106
CH_4 + O_4 Marsh gas and oxygen.	2427	2287
C_2H_4 + O_6 Ethylene and oxygen.	2517	2210
C_2N_2 + O_4 Cyanogen and oxygen.	2490	2195
C_2H_2 + O_5 Acetylene and oxygen.	2660	2482
CO + H_2 + O_2 Carbonic oxide, hydrogen, and oxygen.	2236	2008

Two points in Table I. favoured the view that Berthelot might have here given the true theory of explosions: first, the close coincidence between the rates of explosion of hydrogen both with oxygen and nitrous oxide with the calculated mean velocities of the products of combustion; and secondly, the great discordance between the found and calculated rates for carbonic oxide with both oxygen and nitrous oxide. I had previously discovered that pure carbonic oxide cannot be exploded either with pure oxygen or pure nitrous oxide. The discordance found by Berthelot was what I should have expected from my own experiments.

A consideration of Berthelot's results, published in full in the 'Annales de Chimie,' led me to think it would be useful to repeat and extend these experiments. My objects were chiefly: (1) to determine as accurately as possible the rate of the explosion-wave for some well-known mixtures; (2) to measure the rate of the explosion wave in carbonic oxide with different quantities of steam; and (3) to determine the influence of inert gases on the propagation of the wave.

1. The results obtained with hydrogen and oxygen, with hydrogen and nitrous oxide, and with marsh gas and oxygen in exact proportions for complete combustion, were in close accordance with the mean results of Berthelot; for ethylene, acetylene, and cyanogen my numbers differed appreciably, but in no case differed by more than 7 per cent. from the rates observed by Berthelot:—

TABLE II.

VELOCITY OF EXPLOSION IN METRES PER SECOND.

		Berthelot.	Dixon.
Hydrogen and oxygen	$H_2 + O$	2810	2821
Hydrogen and nitrous oxide	$H_2 + N_2O$	2284	2305
Marsh gas and oxygen	$CH_4 + O_4$	2287	2322
Ethylene and oxygen	$C_2H_4 + O_6$	2210	2364
Acetylene and oxygen	$C_2H_2 + O_5$	2482	2391
Cyanogen and oxygen	$C_2N_2 + O_4$	2195	2321

The general agreement between these measurements left no room for doubt about the substantial accuracy of Berthelot's experiments. The formula he gives does, therefore, express with a close degree of approximation the rates of explosion of many gaseous mixtures.

2. The formula fails for the explosion of carbonic oxide with oxygen or nitrous oxide. This was to be expected if, in the detonation of carbonic oxide in a long tube, the oxidation is effected indirectly by means of steam, as it is in the ordinary combustion of the gas. Measurements of the rate of explosion of carbonic oxide and oxygen in a long tube showed that the rate increased as steam was added to the dry mixture, until a maximum velocity was attained when between 5 and 6 per cent. of steam was present.

3. When electrolytic gas was mixed with an excess of either hydrogen or oxygen the rate of explosion was found to be altered; the addition of hydrogen increasing the velocity, the addition of oxygen diminishing it. The addition of an inert gas nitrogen, incapable of taking part in the chemical change, produced the same effect as the addition of oxygen—one of the reacting substances—only the retarding effect of nitrogen was less marked than that of an equal volume of oxygen. The retardation of the explosion-wave

caused by the addition of an inert gas to electrolytic gas evidently, therefore, depends upon the volume and the density of the gas added. In the following table the retarding effect of oxygen and nitrogen, on the explosion of electrolytic gas, is compared:—

TABLE III.

RATE OF EXPLOSION OF ELECTROLYTIC GAS WITH EXCESS OF OXYGEN AND NITROGEN.

Volume of oxygen added to $H_2 + O$	O_1	O_3	O_5	O_7
Rate.	2328	1927	1690	1281
Volume of nitrogen added to $H_2 + O$	N_1	N_3	N_5	N_7
Rate.	2426	2055	1822	—

I think it a fair inference from these facts to conclude, when the addition of a gas to an explosive mixture retards the rate of explosion by an amount proportional to its volume and density, that such added gas is inert as far as the propagation of the wave is concerned, and that any change which it may undergo takes place after the wave-front has passed by—in other words, is a *secondary* change.

This principle has been applied to determine whether, in the combustion of gaseous carbon, the oxidation to carbonic acid is effected in one or two stages—an important question, on which there is little experimental evidence. If, for instance, in the combustion of a hydrocarbon, or of cyanogen, the carbon is first burnt to carbonic oxide, which subsequently is burnt to carbonic acid, the rate of the explosion-wave should correspond with the carbonic oxide reaction, in this case the primary reaction; whereas, if the carbon of these gases burns to carbonic acid directly, in one stage, then the rate of the explosion-wave should correspond with the complete reaction.

Now, if we adopt Berthelot's formula as a working hypothesis, we can calculate the theoretical rates of explosion of marsh gas, ethylene, or cyanogen: (1) on the supposition that the carbon burns directly to CO_2, and (2) on the supposition that the carbon burns first to CO, and the further oxidation is a subsequent or secondary reaction. On the first supposition, if 100 represents the rate of explosion of these three gases burning to carbonic oxide, the addition of the oxygen required to burn the gases to carbonic acid should *increase* the rate of explosion:—

	Marsh Gas.	Ethylene.	Cyanogen.
Calculated rate of explosion when burnt to CO_2	104	103	107

Whereas if these gases really burn first to carbonic oxide, and the extra oxygen is inert in propagating the explosion-wave, then the addition of this inert oxygen would diminish the rate of explosion:—

	Marsh Gas.	Ethylene.	Cyanogen.
Calculated rate of explosion when burnt to CO with inert oxygen present ..	92	88	87

The experiments show that if 100 be taken as the rate of explosion when the oxygen is only sufficient to burn the carbon to carbonic oxide, the following are the rates found when oxygen is added sufficient to burn the carbon to carbonic acid:—

	Marsh Gas.	Ethylene.	Cyanogen.
Rates found	94	92	84

The results are, therefore, in favour of the view that, in the explosion of these gases, the carbon is first burnt to carbonic oxide.

But stronger evidence on this point is obtained by comparing the explosion rate of these gases (1) when fired with oxygen sufficient to burn the carbon in them to carbonic acid, and (2) when nitrogen is substituted for the oxygen in excess of that required to burn the carbon to carbonic oxide. We have seen that oxygen added to electrolytic gas hinders the explosion more than nitrogen. In precisely the same way oxygen added to a mixture of equal volumes of cyanogen and oxygen hinders the explosion more than the same volume of nitrogen. The conclusion we must come to is that the oxygen added to the mixture expressed by the formula $C_2N_2 + O_2$ is as inert (so far as the propagation of the explosion-wave is concerned) as oxygen added to the mixture expressed by the formula $H_2 + O$. The same phenomena occur in the explosion of marsh gas, ethylene, and acetylene. In all these cases the substitution of nitrogen for the oxygen required to burn the carbon from carbonic oxide to carbonic acid *increases* the velocity of the explosion. These facts seem only consistent with the view that the carbon burns directly to carbonic oxide, and the formation of carbonic acid is an after-occurrence.

Finally, the rates of explosion of cyanogen and the hydrocarbons, when their carbon is burnt to carbonic oxide, have been found greater than the velocities calculated from Berthelot's formula. This accords with the observation previously made that the rate of explosion of electrolytic gas with excess either of hydrogen or oxygen is far higher than the calculated rate. It would seem probable that the theoretical rates as calculated by Berthelot should be modified, in spite of the close agreement which his numbers show. I think the low rates found, when hydrogen, marsh gas, cyanogen, &c., are exploded with equivalent proportions of oxygen, depend partly on the carbon burning to carbonic oxide and partly on the dissociation of the steam at the high temperature. If the formula is modified in these respects, velocities can be calculated which agree with the experimental results where dissociation does not occur.

I suggest the following modifications: (1) the specific heats should be taken at constant volume instead of at constant pressure; (2) the density of the gas should be taken as the mean of the burnt and unburnt molecules, instead of that of the burnt molecules alone; and (3) a correction should be made for the alteration of volume by the chemical reaction, which in some cases increases, in others diminishes, the volume.

The rates so calculated agree with the explosion rates of cyanogen when burnt to carbonic oxide either by oxygen, nitrous oxide, or nitric oxide; with the explosion rates of hydrogen and oxygen with a large excess either of hydrogen, oxygen, or nitrogen; with the explosion rates of ethylene and acetylene with oxygen and a large excess of nitrogen; and, lastly, with the explosion rates of hydrogen and chlorine with an excess of hydrogen.

In conclusion, I would say that these experiments have amply confirmed the truth of Berthelot's statement that the explosion-wave is a "specific constant" for every gaseous mixture; that it has been shown that the rate of explosion depends upon the primary reaction occurring, and that the determination of the rate may throw some light on what is now so obscure—the mode in which chemical changes are brought about; and, finally, that it does not seem impossible that a connection between the rate of the molecules and the rate of the explosion may be worked out, which will give us some definite information on points of high interest in the theory of gases.

[H. B. D.]

GENERAL MONTHLY MEETING,

Monday, July 6, 1891.

SIR JAMES CRICHTON BROWNE, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

Henry Claudius Ash, Esq.
Henry T. C. Knox, Esq.
John George Mair-Rumley, Esq. M. Inst. C.E.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to Miss Jane Barnard, Dr. J. H. Gladstone, the Rev. A. R. Abbott, Mr. T. F. Deacon, Mr. A. Blaikley, and others, for the loan of the valuable and interesting Collection of Faraday Memorials shown in the Library on the occasion of the two Lectures on June 17th and 26th given in commemoration of the Faraday Centenary.

The Special Thanks of the Members were returned to Sir Frederick Abel, K.C.B. for his valuable present of an Œrtling Balance, and to Mr. Ludwig Mond, for his donation of £100 towards expenses connected with the Faraday Centenary Commemoration.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Academy of Natural Sciences, Philadelphia—Proceedings, 1891, Part 1. 8vo.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta: Rendiconti. 1° Semestre, Vol. VII. Fasc. 9. 8vo. 1891.
American Academy of Arts and Sciences—Proceedings, New Series, Vol. XVII. 8vo. 1890.
Astronomical Society, Royal—Monthly Notices, Vol. LI. No. 7. 8vo. 1891.
Bankers, Institute of—Journal, Vol. XII. Part 6. 8vo. 1891.
British Architects, Royal Institute of—Proceedings, 1890–1, Nos. 16, 17. 4to.
Buckton, George B. Esq. F.R.S. M.R.I. (the Author)—Monograph of the British Cicadæ or Tettigidæ, Part 6. 8vo. 1891.
California State Mining Bureau—Tenth Annual Report for 1890. 8vo. With Maps.
Chief Signal Officer, U.S. Army—Annual Report for 1890. 8vo. 1890.
Chemical Industry, Society of—Journal, Vol. X. No. 5. 8vo. 1891.
Chemical Society—Journal for June, 1891. 8vo.
Civil Engineers' Institution—Proceedings, Vol. CIV. 8vo. 1891.
Cornwall Polytechnic Society, Royal—Annual Report for 1890. 8vo.
Cortie, the Rev. A. L. (the Author)—Note on the Spectrum of the Sun-spot of June, 1889. 8vo.
Further Note, with a correction. 8vo.
Cracovie, l'Academie des Sciences—Bulletin, 1891, No. 5. 8vo.

Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I.—Journal of the Royal Microscopical Society, 1891, Part 3. 8vo.
Cutter, Drs. E. and J. A. (the Authors)—Heartrest Sanatory. 4to. 1891.
East India Association—Journal, Vol. XXIII. No. 2. 8vo. 1891.
Editors—American Journal of Science for June, 1891. 8vo.
Analyst for June, 1891. 8vo
Athenæum for June, 1891. 4to.
Brewers' Journal for June, 1891. 4to.
Chemical News for June, 1891. 4to.
Chemist and Druggist for June, 1891. 8vo.
Electrical Engineer for June, 1891. fol.
Engineer for June, 1891. fol.
Engineering for June, 1891. fol.
Horological Journal for June, 1891. 8vo.
Industries for June, 1891. fol.
Iron for June, 1891. 4to.
Ironmongery for June, 1891. 4to.
Murray's Magazine for June, 1891. 8vo.
Nature for June, 1891. 4to.
Open Court for June, 1891. 4to.
Photographic News for June, 1891. 8vo.
Public Health for June, 1891. 8vo.
Revue Scientifique for June, 1891. 4to.
Telegraphic Journal for June, 1891. fol.
Zoophilist for June, 1891. 4to.
Florence Biblioteca Nazionale Centrale—Bolletino, Nos. 131, 132. 8vo. 1891.
Franklin Institute—Journal, No. 786. 8vo. 1891.
Geneva, Société de Physique et d'Histoire Naturelle—Mémoires, Tome XXXI. Partie 1. 4to. 1890–1.
Geographical Society, Royal—Proceedings, Vol. III. Nos. 6, 7. 8vo. 1891.
Great Eastern Railway Company—The Rivers and Broads of Norfolk and Suffolk. 8vo. 1891.
New Holidays in Essex. 8vo. 1891.
The Log of the Lalage. 8vo. 1889.
Johns Hopkins University—University Circulars, No. 89. 4to. 1891.
American Chemical Journal, Vol. XIII. No. 5. 8vo. 1891.
Mensbrugghe, G. Van der, Esq. (the Author)—Sur un particularité curieuse des Cours d'Eau. 8vo. 1891.
La surface Commune à deux Liquides. 3rd Part. 8vo. 1891.
Meteorological Society, Royal—Quarterly Journal, No. 78. 8vo. 1891.
Meteorological Record, No. 39. 8vo. 1890.
Ministry of Public Works, Rome—Giornale del Genio Civile, 1891, Fasc. 2–4. 8vo. And Designi. fol. 1891.
Odontological Society of Great Britain—Transactions, Vol. XXIII. No. 8. New Series. 8vo. 1891.
Parker, R. W. Esq. (the Author)—Diphtheria; its nature and treatment. 8vo. 1891.
Pharmaceutical Society of Great Britain—Journal, June, 1891. 8vo.
Rathbone, E. P. Esq. (the Editor)—The Witwatersrand Mining and Metallurgical Review, No. 17. 8vo. 1891.
Royal Irish Academy—Transactions, Vol. XXIX. Part 16. 4to. 1891.
Proceedings, 3rd Series, Vol. I. No. 5. 8vo. 1891.
Royal Society of London—Proceedings, No. 299. 8vo. 1891.
Saxon Society of Sciences, Royal—Mathematische-Physischen Classe:
Abhandlungen, Band XVII. Nos. 3, 4. 4to. 1891.
Berichte, 1891, No. 1. 8vo. 1891.
Science and Education Library, South Kensington Museum—Catalogue of the Science Library. 8vo. 1891.

Sidgreaves, the Rev. W. (the Author)—Results of Meteorological and Magnetical Observations, 1889–90. 8vo. 1890.
Society of Architects—Proceedings, Vol. III. No, 12. 8vo. 1891.
Society of Arts—Journal for June, 1891. 8vo.
Thompson, Sir Henry, F.R.C.S. M.R.I. &c. (the Author)—Modern Cremation. 2nd Edition. 8vo. 1891.
United Service Institution, Royal—Journal, No. 160. 8vo. 1891.
Wright & Co. Messrs. John (the Publishers)—Porthcawl as a Health Resort. By Robert C. Hunter. 8vo. 1891.
Zoological Society of London—Proceedings, 1891. Part 1. 8vo. 1891.
Transactions—Vol. XIII. Parts 1, 2. 4to. 1891.

GENERAL MONTHLY MEETING,

Monday, November 2, 1891.

SIR JAMES CRICHTON BROWNE, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

George Frederick Deacon, Esq. M. Inst. C.E.
Miss Henriette Hertz,
Arthur Walter Mills, Esq.
Robert Mond, Esq. B.A. F.R.S.E.
Joseph Shaw, Esq.
D. Hack Tuke, M.D. F.R.C.P.
Lieut.-Col. H. S. S. Watkin, C.B. R.A.
William Henry White, Esq. C.B. F.R.S. M. Inst. C.E.

were elected Members of the Royal Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Great Trigonometrical Survey of India, Vol. XIV. 4to. 1890.
The Governor-General of India—Geological Survey of India: Memoirs, Vol. XXIV. Part 3. 8vo. 1891.
Records, Vol. XXIV. Parts 1–3. 8vo. 1891.
Index to first 20 vols of the Records, 1868–1887. 8vo. 1891.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta: Rendiconti. 1° Semestre, Vol. VII. Fasc. 10–12. 2° Semestre, Vol. VII. Fasc. 1–6. 8vo. 1891.
Memorie, Vol. VIII. 4to. 1891.
Academy of Natural Sciences, Philadelphia—The Priority in demonstrating the Toxic Effect of Matter accompanying the Tubercle Bacillus and its Nidus. 8vo. 1891.
Agricultural Society of England, Royal—Journal, Vol. II. Part 3. 8vo. 1891.
American Philosophical Society—Proceedings, No. 135. 8vo. 1891.
Asiatic Society of Bengal—Proceedings, Nos. II.–VI. 8vo. 1891.
Journal, Vol. LIX. Part 1, Nos. III.–IV. Part 2, No. IV. Part 2, Supplement, No. 2. Vol. LX. Part I. No 1. Part II. No. 1. 8vo. 1891.
Astronomical Society, Royal—Monthly Notices, Vol. LI. Nos. 8, 9. 8vo. 1891.
Bankers, Institute of—Journal, Vol. XII. Part 7. 8vo. 1891.
Bavarian Academy of Sciences—Sitzungsberichte, 1891. Heft 1, 2. 8vo.
Belgium, Royal Academy of Sciences—Mémoires Couronnés, Tomes L.–LI. 4to. 1889, 90.
Mémoires Couronnés. Collection in 8vo. Tomes XLIII.–XLV. 8vo. 1889–91.
Bulletins, 3me Série. Tomes XVIII.–XXI. 8vo. 1889–91.
Annuaire. 1890–91. 8vo. 1890–91.
Catalogue de la Bibliothèque, Partie II. 8vo. 1890.
Boston Society of Natural History—Proceedings, Vol. XXV. Part 1. 8vo. 1891.
Bowman, Sir William, Bart. LL.D. F.R.S. M.R.I. (the Author)—Frans Cornelis Donders, 1819–1889. 8vo. 1891. (Proceedings of Royal Society.)
British Architects, Royal Institute of—Proceedings, 1891, Nos. 18–20; 1891–2, No. 1. 4to.
British Museum Trustees—Ancient Greek Inscriptions, Parts 2, 3. fol. 1883-90.
Cuneiform Inscriptions of Western Asia, Vol. IV. fol. 1891.
Egyptian Texts from the Coffin of Amamu. fol. 1886.

British Museum Trustees—continued.
Facsimiles of Epistles of Clement of Rome. fol. 1856.
Photographs of the Papyrus of Nebseni. fol. 1886.
Catalogue of English Coins, Vol. I. 8vo. 1887.
Catalogue of Greek Coins: Peloponnesus, Attica, Corinth, Pontus, &c. 4 vols. 8vo. 1887–9.
Catalogue of Persian Coins. 8vo. 1887.
Catalogue of Additions to MSS. 1876–87. 2 vols. 8vo. 1882–9.
Catalogue of Ancient MSS. Part I. (Greek). fol. 1881.
Catalogue of Seals in MSS. Department, Vol. I. 8vo. 1887.
Catalogue of Persian MSS. Vol. III. 4to. 1883.
Catalogue of Printed Maps, Plans, and Charts. 2 vols. 4to. 1885.
Catalogue of Prints and Drawings, Vol IV. 1761–1770. 8vo. 1883.
Catalogue of Early Prints (German and Flemish). Vol. II. 8vo. 1883.
British Museum (Natural History)—Catalogue of Birds, Vol. XIX. 8vo. 1891.
Catalogue of Fossil Birds. 8vo. 1891.
Illustrations of Lepidoptera Heterocera, Part VIII. 4to. 1891.
List of British Oligocene and Eocene Mollusca. 8vo 1891.
Buckton, George B. Esq. F.R.S. M.R.I. (the Author)—Monograph of the British Cicadæ or Tettigidæ, Part 7. 8vo. 1891.
California, University of—Publications, 1890–91. 8vo.
Canada. Geological and Natural History Survey of—Contributions to Canadian Palæontology, Vol. I. Part 3. 8vo. 1891.
Annual Report, Vol. IV. New Series. 8vo. 1890.
Cassell & Co. Messrs. (the Publishers)—Howard's Art of Reckoning. 8vo. 1891.
Chemical Industry Society of—Journal, Vol. X. Nos. 6, 7. 8vo. 1891.
Chemical Society—Journal for July–Oct. 1891. 8vo.
City of London College—Calendar, 1891–92. 8vo. 1891.
Civil Engineers' Institution—Proceedings, Vol. CVI. 8vo. 1891.
Subject Index, LIX.–CVI. 1879–91. 8vo. 1891.
Engineering Education. 8vo. 1891.
Clarke, William Harrison, Esq. (the Author)—The Civil Service Law. 8vo. 1891.
Clinical Society—Transactions, Vol. XXIV. 8vo. 1891.
Colonial Institute, Royal—Proceedings, Vol. XXII. 8vo. 1890–91.
Cooper Union, Trustees of the (New York, U.S.A.)—Annual Report. 8vo. 1891.
Cracovie, l'Academie des Sciences—Bulletin, 1891, No. 6. 8vo.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I.—Journal of the Royal Microscopical Society, 1891, Part 4. 8vo.
East India Association—Journal, Vol. XXIII. No. 4. 8vo. 1891.
Editors—American Journal of Science for July–Oct. 1891. 8vo.
Analyst for July–Oct. 1891. 8vo.
Athenæum for July–Oct. 1891. 4to.
Brewers' Journal for July–Oct. 1891. 4to.
Chemical News for July–Oct. 1891. 4to.
Chemist and Druggist for July–Oct. 1891. 8vo.
Electrical Engineer for July–Oct. 1891. fol.
Engineer for July–Oct. 1891. fol.
Engineering for July–Oct. 1891. fol.
Horological Journal for July–Oct. 1891. 8vo.
Industries for July–Oct. 1891. fol.
Iron for July–Oct. 1891. 4to.
Ironmongery for July–Oct. 1891. 4to.
Monist for July–Oct. 1891. 8vo.
Murray's Magazine for July–Oct. 1891. 8vo.
Nature for July–Oct. 1891. 4to.
Open Court for July–Oct. 1891. 4to.
Photographic News for July–Oct. 1891. 8vo.
Revue Scientifique for July–Oct. 1891. 4to.
Telegraphic Journal for July–Oct. 1891. fol.
Zoophilist for July–Oct. 1890. 4to.

Electrical Engineers, Institution of—Journal, No. 94. 8vo. 1891.
Florence Biblioteca Nazionale Centrale—Bolletino, Nos. 133-140. 8vo. 1891.
Franklin Institute—Journal, Nos. 788-790. 8vo. 1891.
Fraser, Colonel A. T. R.E. M.R.I. (the Author)—Darkness and Light in the Land of Egypt. 8vo. 1891.
Letters on Indian Finance and Thorough Cultivation. By Sir A. Cotton. 8vo. 1887-91.
Geographical Society, Royal—Proceedings, Vol. III. Nos. 8-11. 8vo. 1891.
Geological Institute, Imperial, Vienna—Verhandlungen, 1891, Nos. 8-13. 8vo.
Abhandlungen, Band XV. Heft 3. 4to. 1891.
Geological Society—Quarterly Journal, Nos. 187, 188. 8vo. 1891.
Georgofili, Reale Accademia—Atti, Quarta Serie, Vol. XIV. Disp. 2, 3. 8vo. 1891.
Harlem, Société Hollandaise des Sciences—Archives Néerlandaises, Tome XXV. 2me Livraison. 8vo. 1891.
Hazell, Watson, & Viney (the Publishers)—Hazell's Annual for 1891. 8vo.
Hicks, E. L. Esq. (the Author)—The Collection of Ancient Marbles at Leeds. 8vo. 1890. (Journal of Hellenic Studies, Vol. XI.)
Horticultural Society, Royal—Journal, Vol. XIII. Part 2. 8vo. 1891.
Huggins, William, Esq. D.C.L. LL.D. F.R.S. M.R.I. (the Author)—Presidential Address to British Association, Cardiff Meeting, 1891. 8vo.
Institute of Brewing—Transactions, Vol. IV. No. 8. 8vo. 1891.
Iron and Steel Institute—Journal for 1891. 8vo.
Jablonowski'sche Gesellschaft, Leipzig—Preisschriften, XVIII. 4to. 1891.
Jenkins, Rev. Canon R. C. M.A. (the Author)—Pre-Tridentine Doctrine. 8vo. 1891.
Johns Hopkins University—University Circulars, No. 91. 4to. 1891.
American Chemical Journal, Vol. XIII. No. 6. 8vo. 1891.
Langley, S. P. Esq. (the Author)—Recherches expérimentales aerodynamiques. 4to. 1891. (Comptes Rendus.)
Linnean Society—Journal, Nos. 148, 176, 194, 197. 8vo. 1891.
Transactions: Botany, Vol. III. Parts 2, 3. 4to. 1891. Zoology, Vol. V. Parts 4-7. 4to. 1890-91.
Proceedings, August, 1891. 8vo.
Madras Government Central Museum—Report, 1890-91. fol. 1891.
Major, Frederick, Esq. (the Author)—Spacial and Atomic Energy, Part III. 'Heat.' 8vo. 1891.
Manchester Geological Society—Transactions, Vol. XXI. Parts 7-10. 8vo. 1891.
Manchester Literary and Philosophical Society—Memoirs and Proceedings, Vol. IV. Nos. 4, 5. 8vo. 1890-91.
Mechanical Engineers' Institution—Proceedings, 1891, Nos. 2-3. 8vo.
Meteorological Office—Quarterly Weather Report, 1880, Parts 3, 4. 8vo. 1891.
Monthly Weather Report, 1887, May-December. 8vo. 1891.
Hourly Means, 1887. 8vo. 1891.
Meteorological Observations at Stations of the Second Order, for 1887. 8vo. 1891.
Cyclone Tracts in the South Indian Ocean. fol. 1891.
Meteorological Society, Royal—Quarterly Journal, No. 79. 8vo. 1891.
Meteorological Record, No. 40. 8vo. 1891.
Ministry of Public Works, Rome—Giornale del Genio Civile, 1891, Fasc. 5-8. 8vo. And Designi. fol. 1891.
Montt, Pedro, Esq. (the Author)—Exposition of the Illegal Acts of Ex-President Balmaceda. 8vo. 1891.
Morize, H. Esq. (the Author)—Ébauche d'une Climatologie du Brésil. 8vo. 1891.
Musical Association—Proceedings, 17th Session, 1890-91. 8vo. 1891.
North of England Institute of Mining and Mechanical Engineers—Transactions, Vol. XXXVIII. Part 6; Vol. XL. Parts 2, 3. 8vo. 1891.
Nova Scotian Institute of Science—Proceedings and Transactions, Vol VII. Part 4. 8vo. 1890.
Messrs. Partridge, S. W. & Co. (the Publishers)—Henry M. Stanley. By Arthur Montefiore. 8vo. 1890.

Pharmaceutical Society of Great Britain—Journal, July–Oct. 1891. 8vo.
Preussische Akademie der Wissenschaften—Sitzungsberichte, Nos. I.–XL. 8vo. 1891.
Rathbone, E. P. Esq. (the Editor)—The Witwatersrand Mining and Metallurgical Review, Nos. 18–21. 8vo. 1891.
Richardson, B. W. M.D. F.R.S. M.R.I. (the Author)—The Asclepiad, Vol. VIII. No. 31. 8vo. 1891.
Rio de Janeiro, Observatorire Imperiale de—Revista, Nos. 5–8. 8vo. 1891.
Robinson, Henry, Esq. M. Inst. C.E. (the Author)—Electric Lighting by Municipal Authorities. 8vo. 1891.
Royal Botanic Society of London—Quarterly Record, 1891. Nos. 46, 47. 8vo.
Royal College of Surgeons, England—Calendar, 1891. 8vo.
Royal Dublin Society—Transactions, Vol IV. (Sec. II.) Parts 6–8. 8vo. 1890–91.
Proceedings, Vol. VI. N.S. Part 10. Vol. VII. N.S. Parts 1, 2. 8vo. 1890–91.
Royal Irish Academy—Proceedings, 3rd Series, Vol. II. No. 1. 8vo. 1891.
Royal Society of Antiquaries of Ireland—Journal, Vol. I. Fifth Series, Nos. 6, 7. 8vo. 1891.
Royal Society of Canada—Proceedings and Transactions, Vol. VIII. 4to. 1891.
Royal Society of Edinburgh—Transactions, Vol. XXXIV. Vol. XXXVI. Part 1. 4to. 1889–90.
Proceedings, Vol. XVII. 8vo. 1889–90.
Royal Society of Literature—Annual Report, 1891. 8vo.
Royal Society of London—Proceedings, Nos. 300–302. 8vo. 1891.
Royal Society of New South Wales—Journal and Proceedings, Vol. XXIV. Part 2. 8vo. 1890.
Russell, The Hon. Rollo, M.R.I. (the Author)—The Spread of Influenza; its supposed relations to Atmospheric Conditions. 8vo. 1891.
Saxon Society of Sciences, Royal—Mathematische-Physischen Classe:
Abhandlungen, Band XVII. No. 5. 4to. 1891.
Berichte, 1891, No. 2. 8vo. 1891.
Philologisch-Historischen Classe:
Abhandlungen, Band XII. No. 3. 8vo. 1891.
Berichte, 1891, No. 1. 8vo. 1891.
Selborne Society—Nature Notes, Vol. II. Nos. 19, 20. 8vo. 1891.
Smithsonian Institution—Smithsonian Miscellaneous Collections, Vol. XXXIV. Nos. 4, 5. 8vo. 1891.
Contributions to Knowledge, Vol. XXVII. fol. 1891.
Annual Report, 1889, Part I. 8vo. 1890.
Society of Arts—Journal for July–Oct. 1891. 8vo.
St. Bartholomew's Hospital—Statistical Tables for 1890. 8vo.
Statistical Society, Royal—Journal, Vol. LIV. Parts 2, 3. 8vo. 1891.
Tacchini, Professor P. Hon. Mem. R.I. (the Author)—Memorie della Societá degli Spettroscopisti Italiani, Vol. XX. Disp. 7A. 4to. 1891.
Tasmania, Royal Society—Proceedings for 1890. 8vo. 1891.
Teyler Museum—Archives, Série II. Vol. III. Part 6. 8vo. 1891.
Toronto University—Papers. 1890–91. 8vo.
United Service Institution, Royal—Journal, Nos. 161–164. 8vo. 1891.
United States Navy—General Information Series No. X. 8vo. 1891.
University Correspondence College—Calendar, 1891–92. 8vo.
Upsal, Royal Society of Sciences—Nova Acta, Series 3, Vol, XIV. Fasc. 2. 4to. 1891.
Vereins zur Beförderung des Gewerbfleises in Preussen—Verhandlungen, 1891: Heft 6–8. 4to.
Victoria Institute—Transactions, No. 96. 8vo. 1891.
Wild, Dr. H. (the Director)—Annalen der Physikalischen Central-Observatoriums, 1890. Theil I. 8vo. 1891.
Wright & Co. Messrs. John (the Publishers)—The Practice of Hypnotic Suggestion. By G. C. Kingsbury, M.D. 8vo. 1891.
Zoological Society of London—Proceedings, 1891, Part II. 8vo. 1891.

GENERAL MONTHLY MEETING,

Monday, December 7, 1891.

Sir James Crichton Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

Lorentz Albert Groth, Esq.
John Imray, Esq. M.A. M. Inst. C.E.
George Stillingfleet Johnson, Esq. F.C.S.
John List, Esq. M. Inst. C.E.
Mrs. Douglas Powell,
James Shand, Esq. M. Inst. C.E.
Mrs. Thomas Threlfall,

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned for the following Donations to the Fund for the Promotion of Experimental Research:—

Professor Dewar (For Structural Alterations)	..	£200
Mr. Ludwig Mond (For New Pressure Pump)	..	£120

The Honorary Secretary reported that the late Mr. J. P. Stocker, *M.R.I.* had bequeathed £100 to the Royal Institution.

The following Lecture Arrangements were announced:—

On Life in Motion, or the Animal Machine. By Professor John G. McKendrick, M.D. LL.D. F.R.S. Professor of Physiology in the University of Glasgow. Six Lectures (adapted to a Juvenile Auditory), on Dec. 29 (*Tuesday*), Dec. 31, 1891; Jan. 2, 5, 7, 9, 1892.

On The Structure and Functions of the Nervous System. The Brain. By Professor Victor Horsley, F.R.S. B.S. F.R.C.S. *M.R.I.* Fullerian Professor of Physiology, R.I. Twelve Lectures on *Tuesdays*, Jan. 19, 26, Feb. 2, 9, 16, 23, March 1, 8, 15, 22, 29, April 5.

On Some Aspects of Greek Sculpture in Relief. By A. S. Murray, Esq. LL.D. F.S.A. Keeper of Greek and Roman Antiquities at the British Museum. Three Lectures on *Thursdays*, Jan. 21, 28, Feb. 4.

On Some Recent Biological Discoveries. By Professor E. Ray Lankester, M.A. LL.D. F.R.S. Three Lectures on *Thursdays*, Feb. 11, 18, 25.

On The Progress of Romance in the Middle Ages. By Professor W. P. Ker, M.A. Professor of English Literature in University College, London. Three Lectures on *Thursdays*, March 3, 10, 17.

On Epidemic Waves. By B. Arthur Whitelegge, M.D. B.Sc. Three Lectures on *Thursdays*, March 24, 31, April 7.

On THE INDUCTION COIL AND ALTERNATE CURRENT TRANSFORMER. By PROFESSOR J. A. FLEMING, M.A. D.Sc. *M.R.I.* Three Lectures on *Saturdays*, Jan. 23, 30, Feb. 6.

On MATTER: AT REST AND IN MOTION. By THE RIGHT HON. LORD RAYLEIGH, M.A. D.C.L. F.R.S. *M.R.I.* Professor of Natural Philosophy, R.I. Six Lectures on *Saturdays*, Feb. 13, 20, 27, March 5, 12, 19.

On DRAMATIC MUSIC, FROM SHAKESPEARE TO DRYDEN. (The Play, the Masque, and the Opera.) (With Illustrations.) By PROFESSOR J. F. BRIDGE, Mus. Doc. Three Lectures on *Saturdays*, March 26, April 2, 9.

THE PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Governor-General of India—Geological Survey of India: Palæontologica Indica, Series XIII. Vol. IV. Part 2. fol. 1890.
Memoirs, Vol. XXIII. 4to. 1891.
The French Government—Documents Inédits sur l'Histoire de France:
Lettres du Cardinal Mazarin. Par A. Chéruel. Tome VI. 1653–1655. 4to. 1890.
Comptes des Bâtiments du Roi, sous le Règne de Louis XIV. Par J. Guiffrey. Tome 3e. 4to. 1891.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta: Rendiconti. 2o Semestre, Vol. VII. Fasc. 7, 8. 8vo. 1891.
Atti. Anno 43, Sess. 7a; Anno 44, Sess. 1. 4to. 1890–91.
Memorie, Vol. IX. Partie 2a. 4to. 1891.
Academy of Natural Sciences, Philadelphia—Proceedings, 1891, Part 2. 8vo.
American Association for the Advancement of Science—Proceedings, 39th Meeting, Indianapolis, 1890. 8vo. 1891.
Antiquaries, Society of—Proceedings, Vol. XIII. No. 3. 8vo. 1890–91.
Archæologia. Second Series. Vol. II. 4to. 1890.
Aristotelian Society—Proceedings, Vol. I. No. 4, Part 2. 8vo. 1891.
Asiatic Society of Great Britain, Royal—Journal for October, 1891. 8vo.
Bankers, Institute of—Journal, Vol. XII. Part 8. 8vo. 1891.
Bavarian Academy of Sciences—Neue Annalen, Band II. 4to. 1891.
Boston Society of Natural History—Proceedings, Vol. XXV. Part 2. 8vo. 1891.
British Architects, Royal Institute of—Proceedings, 1891–2, Nos. 2–4. 4to.
Calendar, 1891–2. 8vo.
Cambridge Philosophical Society—Proceedings, Vol. VII. Part 4. 8vo. 1891.
Canadian Institute—Transactions, Vol. II. Part 1, No. 3. 8vo. 1891.
Chemical Industry, Society of—Journal, Vol. X. No. 10. 8vo. 1891.
Chemical Society—Journal for November, 1891. 8vo.
Cracovie, l'Academie des Sciences—Bulletin, 1891, No. 7. 8vo.
Dax, Société de Borda—Bulletin, seizième année, 1er–3e, Trimestre. 8vo. 1891.
Devonshire Association for the Advancement of Science, Literature, and Art—Report and Transactions, Vol. XXIII. 8vo. 1891.
Devonshire Domesday, Part 8. 8vo. 1891.
Editors—American Journal of Science for November, 1891. 8vo.
Analyst for November, 1891. 8vo.
Athenæum for November, 1891. 4to.
Brewers' Journal for November, 1891. 4to.
Chemical News for November, 1891. 4to.
Chemist and Druggist for November, 1891. 8vo.
Educational Review, New Series, Vol. I. No. 1. 8vo. 1891.
Electrical Engineer for November, 1891. fol.
Engineer for November, 1891. fol.
Engineering for November, 1891. fol.

Editors—continued.
Horological Journal for November, 1891. 8vo.
Industries for November, 1891. fol.
Iron for November, 1891. 4to.
Ironmongery for November, 1891. 4to.
Monist for November, 1891. 8vo.
Murray's Magazine for November, 1891. 8vo.
Nature for November, 1891. 4to.
Open Court for November, 1891. 4to.
Photographic News for November, 1891. 8vo.
Revue Scientifique for November, 1891. 4to.
Telegraphic Journal for November, 1891. fol.
Zoophilist for November, 1891. 4to.
Ex-Libris Society—Journal for December, 1891. 4to.
Florence Biblioteca Nazionale Centrale—Bolletino, Nos. 141, 142. 8vo. 1891.
Indice e Cataloghi, Vol. I. Fasc. 3; Vol. II. Fasc. 4. 8vo. 1891.
Franklin Institute—Journal, No. 791. 8vo. 1891.
Geographical Society, Royal—Proceedings, Vol. XIII. No. 12. 8vo. 1891.
Geological Institute, Imperial, Vienna—Verhandlungen, 1891, No. 14. 8vo.
Jahrbuch, Band XL. Heft 3, 4; Band XLI. Heft 1. 8vo. 1891.
Greville, Edward, Esq. J.P. (the Editor)—The Year Book of Australia, 1886, 1889, and 1890. 8vo.
Historical Society, Royal—Transactions, New Series, Vol. V. 8vo. 1891.
Howard Association—Penological and Preventive Principles. By W. Tallack. 8vo. 1889.
Institute of Brewing—Transactions, Vol. V. No. 1. 8vo. 1891.
Johns Hopkins University—University Circulars, Nos. 92, 93. 4to. 1891.
American Chemical Journal, Vol. XIV. Nos. 2–4. 8vo. 1891.
American Journal of Philology, Vol. XI. No. 4; Vol. XII. No. 1. 8vo. 1891.
Studies in Historical and Political Science, Ninth Series, Nos. 1–8. 8vo. 1891.
Lawes, Sir John Bennet, Bart. LL.D. F.R.S. (the Author)—Field and other Experiments conducted at Rothamsted, Herts. fol. 1891.
Linnean Society—Journal, Nos. 149, 150, 195. 8vo. 1891.
Longmans & Co. Messrs. (the Publishers)—The Principles of Chemistry. By D. Mendeleef. Translated by G. Kamensky and A. J. Greenaway. 2 vols. 8vo. 1891.
Manchester Geological Society—Transactions, Vol. XXI. Part 11. 8vo. 1891.
Meteorological Office—Daily Weather Charts of the Arabian Sea (June 1885). 4to. 1891.
Charts of the Indian Ocean adjacent to Cape Gardafui. fol. 1891.
Ministry of Public Works, Rome—Giornale del Genio Civile, 1891, Fasc. 9. 8vo. And Designi. fol. 1891.
New York Academy of Sciences—Annals, Vol. V. Extra, Parts 1–3. 8vo. 1891.
Transactions, Vol. X. Parts 2–6. 8vo. 1890–91.
Odontological Society—Transactions, Vol. XXIV. No. 1. 8vo. 1891.
Pharmaceutical Society of Great Britain—Journal, November, 1891. 8vo.
Radcliffe Library—Catalogue of Books added to Library during 1890. 4to. 1891.
Ramsay, William, Esq. Ph.D. F.R.S. M.R.I. (the Author)—A System of Inorganic Chemistry. 8vo. 1891.
Rio de Janeiro, Observatoire Impérial de—Revista, No. 9. 8vo. 1891.
Saxon Society of Sciences, Royal—Mathematische-Physischen Classe:
Abhandlungen, Band XVII. No. 6. 4to. 1891.
Philologisch-Historischen Classe:
Abhandlungen, Band XIII. No. 2. 4to. 1891.
Société Archéologique du Midi de la France—Bulletin, Série in 8vo, Nos. 6–7. 1890–91.
Society of Architects—Proceedings, Vol. IV. Nos. 1, 2. 8vo. 1891.

Society of Arts—Journal for November, 1891. 8vo.
St. Pétersbourg Académie Impériale des Sciences—Bulletin, Tome XXXIV. No. 2. 4to. 1891.
Surgeon-General's Office, U.S. Army—Index Catalogue of the Library, Vol. XII. 4to. 1891.
United Service Institution, Royal—Journal, Nos. 165, 166. 8vo. 1891.
United States Department of Agriculture—North American Fauna, Part 5. 8vo. 1891.
Monthly Weather Review for July and August, 1891. 4to.
Special Report of Chief of Weather Bureau. 8vo. 1891.
Vereins zur Beförderung des Gewerbfleises in Preussen—Verhandlungen, 1891: Heft 9. 4to.
Wild, Henry, Esq. F.R.S. (the Author)—The Phenomena of Terrestrial Magnetism. 4to. 1891.
Zoological Society of London—Proceedings, 1891, Part 3. 8vo. 1891.
Transactions, Vol. XIII. Part 3. 4to. 1891.

THE FARADAY CENTENARY.*

Wednesday, June 17, 1891.

H.R.H. The Prince of Wales, K.G. F.R.S. Vice-Patron, in the Chair.

There were also present—The Duke of Northumberland (President), Lord Morris, Sir William Thomson (Pres. R.S.), Sir George Stokes, M.P. Sir William Grove, Count Tornielli (the Italian Ambassador), Sir Frederick Leighton, Sir James Crichton Browne, Sir Joseph Lister, Sir Frederick Abel, Sir William Bowman, Sir Archibald Geikie, Sir Henry Roscoe, M.P. Sir Somers Vine, Sir Frederick Bramwell, Professor Dewar, and Professor Horsley.

The Prince of Wales opened the proceedings with the following address:—

Ladies and Gentlemen,—I can well remember that two-and-twenty years ago I had the high privilege of presiding at a meeting here. That meeting was a very large one, and included many of the most eminent scientific men of the day. Among those present on that occasion, I remember, were the illustrious chemist, Professor Dumas, Sir Edward Sabine, Sir Roderick Murchison, Sir Henry Holland, a very old personal friend of mine, Dr. Bence Jones, Mr. Warren de la Rue, and many others, who I regret to say have now passed away. The object of our meeting on that occasion was to select a suitable memorial to the memory of the great Faraday, the eminent chemist and philosopher, who, I may say, was also the founder of modern electricity. As you are well aware, the fine statue by Foley, which is in the hall below, was, we thought, a suitable memorial to that great man. As for myself personally, I feel proud to think that in the days of my boyhood my brother and myself used to attend his chemical lectures here about Christmas time, and I shall ever remember the admirable and lucid way in which he delivered those lectures to us who were mere boys, and gave us a deep interest in chemistry, which we kept up for many years, and which I had the opportunity of practising at the University of Oxford. I can only regret that I have not since had the time to pursue that interesting science. To-day is a memorable day, for this year we celebrate the centenary of the birth of that great man; and we all of us have reason to feel grateful that two such eminent men as Lord Rayleigh and Professor Dewar should have consented to give lectures on the work of the great Faraday. I have only now to beg Lord Rayleigh to give us his address.

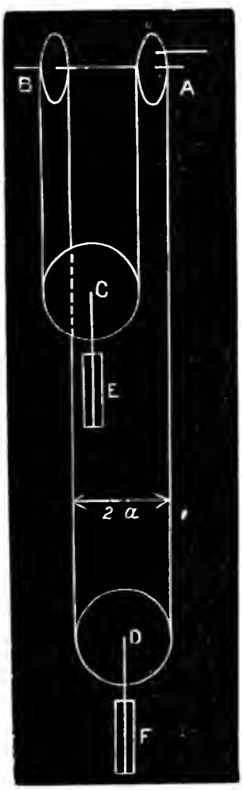
Lord Rayleigh said that the man whose name and work they were

* Michael Faraday, born 22nd September, 1791.

celebrating was identified in a remarkable degree with the history of this Institution. If they could not take credit for his birth, in other respects they could hardly claim too much. During a connection of fifty-four years, Faraday found there his opportunity, and for a large part of the time his home. The simple story of his life must be known to most who heard him. Fired by contact with the genius of Davy, he volunteered his services in the laboratory of the Institution. Davy, struck with the enthusiasm of the youth, gave him the desired opportunity, and, as had been said, secured in Faraday not the least of his discoveries. The early promise was indeed amply fulfilled, and for a long period of years by his discoveries in chemistry and electricity Faraday maintained the renown of the Royal Institution and the honour of England in the eye of the civilised world. He should not attempt in the time at his disposal to trace in any detail the steps of that wonderful career. The task had already been performed by able hands. In their own 'Proceedings' they had a vivid sketch from the pen of one whose absence that day was a matter of lively regret. Dr. Tyndall was a personal friend, had seen Faraday at work, had enjoyed opportunities of watching the action of his mind in face of a new idea. All that he could aim at was to recall, in a fragmentary manner, some of Faraday's great achievements, and, if possible, to estimate the position they held in contemporary science.

Whether they had regard to fundamental scientific import, or to practical results, the first place must undoubtedly be assigned to the great discovery of the induction of electrical currents. He proposed first to show the experiment in something like its original form, and then to pass on to some variations, with illustrations from the behaviour of a model, whose mechanical properties were analogous. He was afraid that these elementary experiments would tax the patience of many who heard him, but it was one of the difficulties of his task that Faraday's discoveries were so fundamental as to have become familiar to all serious students of physics.

The first experiment required them to establish in one coil of copper wire an electric current by completing the communication with a suitable battery; that was called the primary circuit, and Faraday's discovery was this: That at the moment of the starting or stopping of the primary current in a neighbouring circuit, in the ordinary sense of the words completely detached, there was a tendency to induce a current. He had said that those two circuits were perfectly distinct, and they were distinct in the sense that there was no conducting communication between them, but, of course, the importance of the experiment resided in this—that it proved that in some sense the circuits were not distinct; that an electric current circulating in one does produce an effect in the other, an effect which is propagated across a perfectly blank space occupied by air, and which might equally well have been occupied by vacuum. It might appear that that was a very simple and easy experiment, and of course it was so in a modern laboratory, but it was otherwise at the time when Faraday first made it. With all his skill, Faraday did not light upon the truth without delay and difficulty. One of Faraday's biographers thus wrote:—"In December 1824, he had attempted to obtain an electric current by means of a magnet, and on three occasions he had made elaborate and unsuccessful attempts to produce a current in one wire by means of a current in another wire, or by a magnet. He still persevered, and on August 29, 1831—that is to say, nearly seven years after his first attempts—he obtained the first evidence that an electric current induced another in a different circuit." On September 23rd, he writes to a friend, R. Phillips: "I am busy just now again with electro-magnetism, and think I have got hold of a good thing, but cannot say; it may be a weed instead of a fish that, after all my labour, I at last haul up." We now know that it was a very big fish indeed. Lord Rayleigh proceeded to say that he now proposed to illustrate the mechanics of the question of the induced current by means of a model (see figure), the first idea of which was due to Maxwell. The one actually employed was a combination known as Huygens's gear, invented by him in connection with the winding of clocks. Two similar pulleys, A B, turn upon a piece of round steel fixed horizontally. Over these is hung an endless cord, and the two bights carry similar pendant pulleys, C, D, from which again hang weights, E, F. The weight of the cord being negligible, the system is devoid of potential energy; that is, it will balance, whatever may be the vertical distance between C and D. Since either pulley, A, B, may turn independently of the other, the system is capable of two independent motions. If A, B turn in the same direction and with the same velocity one of the pendant pulleys, C, D rises, and the other falls. If, on the other hand, the motions of A, B are equal and opposite, the axes of the pendant pulleys and the attached weights remain at rest. In the electrical analogue the rotatory velocity of A corresponds to a current in a primary circuit, that of B to a current in a secondary. If, when all is at rest, the rotation of A be suddenly started, by force applied at the handle or otherwise, the inertia of the masses E, F opposes their sudden movement, and the consequence is that the pulley B turns *backwards*, i.e. in the opposite direction to the rotation imposed upon A. This is the current induced in a secondary circuit when an electromotive-force begins to act in the primary. In like manner, if A, having been for some time in uniform movement, suddenly stops, B enters into motion in the direction of the former movement of A. This is



the secondary current on the break of the current in the primary circuit. It might perhaps be supposed by some that the model was a kind of trick. Nothing could be further from the truth. The analogy of the two things was absolutely essential. So far was this the case that precisely the same argument and precisely the same mathematical equations proved that the model and the electric currents behaved in the way in which they had seen them behave in the experiment. That might be considered to be a considerable triumph of the modern dynamical method of including under the same head phenomena the details of which might be so different as in this case. If they had a current which alternately stopped and started, and so on, for any length of time, they, as it were, produced in a permanent manner some of the phenomena of electrical induction; and if it were done with sufficient rapidity it would be evident that something would be going on in the primary and in the secondary circuit. The particular apparatus by which he proposed to illustrate those effects of the alternating current was devised by a skilful American electrician, Prof. Elihu Thomson, and he had no doubt it would be new to many. The alternating current was led into the electro-magnet by a suitable lead; if another electric circuit, to be called the secondary circuit, was held in the neighbourhood of that, currents would be induced and might be made manifest by suitable means. Such a secondary circuit he held in his hand, and it was connected with a small electric glow-lamp. If a current of sufficient intensity were induced in that secondary circuit it would pass through the lamp, which would be rendered incandescent. [Illustrating.] It was perfectly clear there was no conjuring there; the incandescent lamp brightened up. One of the first questions which presented itself was, what would be the effect of putting something between? Experimenting with a glass plate, he showed there was no effect, but when they tried a copper plate the lamp went completely out, showing that the copper plate was an absolute screen to the effect, whatever it might be. Experiments of that kind, of course in a much less developed and striking form, were made by Faraday himself, and must be reckoned amongst some of his greatest discoveries.

Before going further, he might remark on what strong evidence they got in that way of the fact that the propagation of the electric energy which, having its source in the dynamo downstairs, eventually illuminated that little lamp, was not merely along the wires, but was capable of bridging over and passing across a space free from all conducting material, and which might be air, glass, or, equally well, vacuum. Another kindred effect of a striking nature, devised by Prof. Elihu Thomson, consisted in the repulsive action which occurred between the primary current circulating around a magnet and the current induced in a single hoop of aluminium wire. Illustrating this by experiment, he showed that the repulsion was so strong as to throw the wire up a considerable height. Those effects were commonly described as dependent upon the mutual induction between two distinct circuits, one being that primarily excited by a battery or other source of electricity, while the other occurred in a detached circuit. Many surprising effects, however, depended on the reactions which took place at different parts of the same circuit. One of these he illustrated by the decomposition of water under the influence of self-induction.

About the time the experiments of which he had been speaking were made, Faraday evidently felt uneasiness as to the soundness of the views about electricity held by his contemporaries, and to some extent shared by himself, and he made elaborate experiments to remove all doubt from his mind. He re-proved the complete identity of the electricity of lightning and of the electricity of the voltaic cell. He evidently was in terror of being misled by words which might convey a meaning beyond what facts justified. Much use was made of the term "poles" of the galvanic battery. Faraday was afraid of the meaning which might be attached to the word "pole," and he introduced a term since generally substituted, "electrode," which meant nothing more than the way or path by which the electricity was led in. "Electric fluid" was a term which Faraday considered dangerous, as meaning more than they really knew about the nature of electricity, and as was remarked by Maxwell, Faraday succeeded in banishing the term "electric fluid" to the region of newspaper paragraphs.

Diamagnetism was a subject upon which Faraday worked, but it would take him too long to go into that subject, though he must say a word or two. Faraday found that whereas a ball of iron or nickel or cobalt, when placed near a magnet or combination of magnets, would be attracted to the place where the magnetic force was the greatest, the contrary occurred if for the iron was substituted a corresponding mass of bismuth or of many other substances. The experiments in diamagnetism were of a microscopic character, but he would like to illustrate one position of Faraday's, developed years afterwards by Sir Wm. Thomson, and illustrated by him in many beautiful experiments, only one of which he now proposed to bring before them. Supposing they had two magnetic poles, a north pole and a south pole, with an iron ball between them, free to move along a line perpendicular to that joining the poles, then, according to the rule he had stated, the iron ball would seek an intermediate position, the place at which the magnetic force was the greatest. Consequently, if the iron ball be given such a position, they would find it tended with considerable force to a central position of equilibrium; but if, instead of using opposite poles, they used two north poles, they would find that the iron ball did not tend to the central position, because that was not the place in which the magnetic force was the greatest. At that place there was no magnetic force, for the one pole completely neutralised the action of the other. The greatest force would be a little way out, and that, according to Faraday's observations, systematised and expressed in the form of mathematical law by Sir Wm. Thomson, was where the ball would go. [This was illustrated by experiment.]

The next discovery of Faraday to which he proposed to call attention was one of immense significance from a scientific point of view, the consequences of which were not even yet fully understood or developed. He referred to the magnetisation of a ray of light, or what was called in more usual parlance the rotation of the plane of polarisation under the action of magnetic force. It would be hopeless to attempt to explain all the preliminaries of the experiment to those who had not given some attention to those subjects before, and he could only attempt it in general terms. It would be known to most of them that the vibrations which constituted light were executed in a direction perpendicular to that of the ray of light. By experiment he showed that the polarisation which was suitable to pass the first obstacle was not suitable to pass the second, but if by means of any mechanism they were able after the light had passed the first obstacle, to turn round the vibration, they would then give it an opportunity of passing the second obstacle. That was what was involved in Faraday's discovery. [Experiment.] As he had said, the full significance of the experiment was not yet realised. A large step towards realising it, however, was contained in the observation of Sir Wm. Thomson, that the rotation of the plane of polarisation proved that something in the nature of rotation must be going on within the medium when subjected to the magnetising force, but the precise nature of the rotation was a matter for further speculation, and perhaps might not be known for some time to come.

When first considering what to bring before them he thought, perhaps, he might include some of Faraday's acoustical experiments, which were of great interest, though they did not attract so much attention as his fundamental electrical discoveries. He would only allude to one point which, as far as he knew, had never been noticed, but which Faraday recorded in his acoustical papers. "If during a strong steady wind, a smooth flat sandy shore, with enough water on it, either from the receding tide or from the shingle above, to cover it thoroughly, but not to form waves, be observed in a place where the wind is not broken by pits or stones, stationary undulations will be seen over the whole of the wet surface. . . . . These are not waves of the ordinary kind, they are (and this is the remarkable point) accurately parallel to the course of the wind." When he first read that statement, many years ago, he was a little doubtful as to whether to accept the apparent meaning of Faraday's words. He knew of no suggestion of an explanation of the possibility of waves of that kind being generated under the action of the wind, and it was, therefore, with some curiosity that two or three years ago, at a French watering-place, he went out at low tide, on a suitable day when there was a good breeze blowing, to see if he could observe anything of the waves described by Faraday. For some time he failed absolutely to observe the phenomenon, but after a while he was perfectly well able to recognise it. He mentioned that as an example of Faraday's extraordinary powers of observation, and even now he doubted whether anybody but himself and Faraday had ever seen that phenomenon.

Many matters of minor theoretic interest were dealt with by Faraday, and reprinted by him in his collected works. He was reminded of one the other day by a lamentable accident which occurred owing to the breaking of a paraffin lamp. Faraday called attention to the fact, though he did not suppose he was the first to notice it, that by a preliminary preparation of the lungs by a number of deep inspirations and expirations, it was possible so to aërate the blood as to allow of holding the breath for a much longer period than without such a preparation would be possible. He remembered some years ago trying the experiment, and running up from the drawing-room to the nursery of a large house without drawing any breath. That was obviously of immense importance, as Faraday pointed out, in the case of danger from suffocation by fire, and he thought that possibly the accident to which he alluded might have been spared had the knowledge of the fact to which Faraday drew attention been more generally diffused.

The question had often been discussed as to what would have been the effect upon Faraday's career of discovery had he been subjected in early life to mathematical training. The first thing that occurred to him about that, after reading Faraday's works, was that one would not wish him to be anything different from what he was. If the question must be discussed, he supposed they would have to admit that he would have been saved much wasted labour, and would have been better *en rapport* with his scientific contemporaries if he had had elementary mathematical instruction. But mathematical training and mathematical capacity were two different things, and it did not at all follow that Faraday had not a mathematical mind. Indeed, some of the highest authorities had held (and there could be no higher authority on the subject than Maxwell) that his mind was essentially mathematical in its qualities, although they must admit it was not developed in a mathematical direction. With these words of Maxwell he would conclude:—"The way in which Faraday made use of his idea of lines of force in co-ordinating the phenomena of electric induction shows him to have been a mathematician of high order, and one from whom the mathematicians of the future may derive valuable and fertile methods."

Sir William Thomson, in moving a vote of thanks to Lord Rayleigh for his lecture, said that the Royal Institution was during the last part of Faraday's life, and during the whole of his scientific career, his home. The splendid results of Faraday's labours contributed in no small degree to the scientific glory of the 19th century, and helped to make it one of the most prolific periods in the world's history. Faraday was throughout animated solely by the love of knowledge. He freely gave his discoveries to mankind, and left it to others to turn them to practical and profitable account.

Sir George Stokes, in seconding the motion, said that he had had the honour of a personal acquaintance with Faraday, whose single-minded devotion to knowledge for its own sake was beyond all praise.

The vote of thanks was cordially passed.

Lord Rayleigh, in acknowledgment, said that it had been a great honour and a great responsibility which had been placed upon him. He remembered with gratitude the instruction which he had derived at Cambridge from Sir Gabriel Stokes, and felt deeply indebted to Sir William Thomson for all that he had learnt from his writings and his conversation.

Sir Frederick Bramwell read the following letter from Dr. Tyndall:—

Hind Head House, Haslemere,
June 16, 1891.

Dear Sir Frederick Bramwell,

As Faraday recedes from me in time, he becomes to me more and more beautiful. Anything, therefore, calculated to do honour to his memory must command my entire sympathy.

But the utmost liberty I can now allow myself is to be shifted from my bed to a couch and wheeled to a position near the window, from which I can see the bloom of the gorse and the brown of the heather.

Thus, considerations affecting the body only present an insuperable barrier to my going to London on Wednesday.

Yours very truly,
John Tyndall

Sir Frederick Bramwell read a list of the names of the honorary members elected on May 4, 1891, in commemoration of the Centenary of Faraday; and reported that the following letters had been received from them.

Paris, 18 Mai 1891.

Monsieur,

J'ai reçu l'invitation que vous voulez bien m'adresser à assister au Centenaire de Faraday et l'annonce de mon élection comme membre honoraire de l'Institution Royale. Je vous prie de remercier le Conseil de l'honneur qu'il me fait. Je serais très heureux d'entendre les lectures de Lord Rayleigh et du Professeur Dewar, et très désireux de concourir à rendre hommage à la mémoire de l'illustre Faraday. Je l'ai connu à Londres, lorsqu'il a bien voulu assister à une lecture que j'y ai donnée, il y a un quart de siècle, à "Royal Institution," et me témoigner sa sympathie. Mais je ne puis m'engager à venir cette année, avant que la date exacte de la réunion soit fixée, étant moi-même assujetti à de nombreux devoirs comme Sénateur et comme Secrétaire Perpétuel de l'Académie des Sciences.

Veuillez, monsieur, agréer l'assurance de ma considération la plus distinguée.

M. Berthelot.

9, Rue de Grenelle, Paris,
le 20 Mai 1891.

Monsieur le Secrétaire Honoraire,

Je vous prie de vouloir bien présenter mes vifs remercîments aux Membres du Conseil de l'Institution Royale pour l'honneur que m'a fait l'assemblée générale en m'élisant membre honoraire.

Je ferai mon possible pour me rendre à Londres afin d'assister aux séances où Lord Rayleigh et le Professeur Dewar rappelleront quelques-uns des admirables travaux de Faraday et d'être personnellement "admis" à l'Institution. Mais cela dépendra naturellement de la date de ces réunions.

Veuillez agréer, je vous prie, Monsieur le Secrétaire Honoraire, l'expression de mes sentiments respectueux et dévoués.

A. Cornu.

Bureau Central Météorologique, 176, Rue de l'Université,
Cabinet du Directeur,
Paris, le 16 Mai 1891.

Monsieur le Secrétaire Honoraire,

Je vous prie de transmettre au Conseil de l'Institution Royale l'expression de ma plus vive reconnaissance pour la haute distinction qu'il a bien voulu m'accorder par la nomination de "membre honoraire."

J'attache d'autant plus de prix à cette faveur qu'elle a pour occasion la célébration du centenaire de la naissance de Faraday, l'un des plus grands hommes de bien et l'une des plus pures illustrations du monde scientifique.

Je serais très heureux si mes obligations me permettent de recevoir personnellement le diploma qui m'est destiné et d'entendre célébrer la gloire de Faraday par les savants les plus autorisés.

Veuillez agréer, monsieur le Secrétaire honoraire, l'assurance de ma haute considération.

E. Mascart.

Institut Pasteur, 25, Rue Dutot,
Paris, le 20 Mai 1891.

Monsieur le Secrétaire,

J'ai reçu, avec des sentiments de vive satisfaction, la lettre par laquelle vous m'informiez que, à l'occasion du centenaire de la naissance de Michael Faraday, l'Institution Royale de la Grande Bretagne m'a élu membre honoraire de cette célèbre Institution.

Je suis touché profondément de cette haute marque d'estime donnée à mes travaux et je vous prie d'être auprès des membres de l'Institution l'interprète de mes sentiments de gratitude.

Malheureusement, l'état de ma santé ne me permettra pas de pouvoir assister aux leçons qui seront faites à l'Institution Royale pour honorer la mémoire de l'illustre Faraday, sous la présidence de S.A.R. le Prince de Galles, ni d'avoir l'honneur d'aller en personne recevoir le diplôme qui m'est destiné.

Veuillez recevoir, monsieur le Secrétaire, l'expression de ma haute considération.

L. Pasteur.

[Translation.]

Heidelberg, May 21, 1891.

Dear Sir,

You have been so kind as to inform me by a letter of May, 1891, that the Royal Institution of Great Britain has nominated me as honorary member at the last general meeting.

I am exceedingly grateful for such a mark of favour so kind and indulgent towards me. I beg you to thank the Royal Institution heartily for their sign of friendly sympathy. I feel how much I am esteemed by the distinction, especially as I do not conceal from myself how far from the object in view are my scientific efforts to obtain the end before me.

I am very sorry that I must renounce the pleasure of being present at the Faraday festivities, my age not allowing me to undertake a long journey so far away from home.

I beg to assure you of my highest respect, in which I remain, dear sir, your obedient humble servant,

R. Bunsen.

Charlottenburg, Marchstrasse,
June 4, 1891.

Dear Sir,

I beg to excuse that I shall not be able to come to London for the Centenary of the birth of Michael Faraday, in order to receive there the Diploma of Membership to the Royal Institution in so exceptionally honouring way, as you indicate, namely, by the hands of His Royal Highness the Prince of Wales. I was obliged to accept an election as honorary president of the Commission of Jurors at the Electric Exhibition at Frankfurt, and the first meeting of this commission for the constitution and organization shall take place in the same time, when I ought to come to London.

Begging that you will be so kind to excuse me also before His Royal Highness,

I am, Sir, your obedient servant,

Hermann v. Helmholtz.

Berlin, 10, Dorotheen Strasse,
May 24, 1891.

My Dear Sir Frederick,

When returning home from a short Whitsuntide excursion I found your kind letter conveying the welcome news of my having been elected an Honorary Member of the Royal Institution of Great Britain. I hasten to express my heartfelt thanks for the rare distinction bestowed upon me. The honour of becoming associated with so renowned a corporation, conspicuous at all times, is doubled by the auspicious occasion on which it is conferred.

In Faraday, I admired the incomparable experimental thinker; I loved the noble-minded, kind-hearted man. During the twenty years I have had the good fortune of living in dear old England I did not miss a single one of his lectures. It would be difficult to say how deeply I am indebted to these and to a great many other lectures I attended at the Royal Institution, and often in my own courses when showing a highly instructive experiment, I delight in telling my students where and by whom performed I saw it for the first time.

I am glad to hear that two lectures on Faraday's life work will be delivered by the eminent professors of the Institution in the theatre hallowed by his never-to-be-forgotten addresses. Unfortunately my duties in the University do not permit me to leave Berlin during the months of June and July, so I must forego the pleasure of hearing them. They will, however, most undoubtedly be printed.

I remain, my dear Sir Frederick, with reiterated thanks, yours very sincerely,

A. W. von Hofmann.

Tutzing, Bavaria, May 19, 1891.

Dear Sir,

I have received the announcement of my election as Honorary Member of the Royal Institution of Great Britain. Although I feel the great honour of this election, and although I would be very proud to accept the diploma in such a glorious assembly as will be present at the Centenary of the birth of Michael Faraday, I must beg excuse for me. Our University lectures shall be re-opened next week and then be continued until the end of July, and I am bound by my office and business to stay at Berlin during this whole time. So I must beg you, dear sir, to express my hearty thanks to the Members of the Royal Institution of Great Britain, and to forward to me that diploma.

Yours very sincerely,

Rud. Virchow, M.D. LL.D., F.R.S.
Professor in the University of Berlin.

12, Ware Street, Cambridge,
Mass., U.S.A.

SIR,

I feel highly honored by my election as an Honorary Member of the Royal Institution of Great Britain, and gratefully accept the privileges it implies. It would give me the greatest pleasure to be able to take part in the celebration of the Centenary of the Birth of Michael Faraday, but I am at present in such poor health that an ocean voyage would be impracticable. Another year I earnestly hope I may be able to visit the "old country" once more, and shall look forward with satisfaction to be received as a member where I have often been a guest. Indeed I have very tender associations with the Royal Institution; for it was there, as a young man on my first visit to England, that I made the acquaintance of Faraday through the introduction of a mutual friend. That acquaintance was to me an inspiration, and I look back to it as one of the most important influences in my education. I remember distinctly that after one of his lectures to a juvenile audience, when I could not restrain my enthusiasm, and expressed my admiration at his power of commanding attention, and my surprise at the simplicity of the means employed, the great master replied, "That is the whole secret of interesting these young people. I always use the simplest means, but I never leave a point not illustrated. If I mention the force of gravitation I take up a stone and let it drop." At this distance of time I cannot be sure that I quote his exact language, but the illustration and the lesson I could not forget; and to this lesson more than to any one thing I owe whatever success I have had as a teacher of physical science. You can then well understand how glad I should be to pay honor to the memory of Michael Faraday not only as the consummate investigator, but also as the great teacher and noble man.

I have the honour to be, Sir, your obedient servant,

JOSIAH P. COOKE.

New Haven, Conn., U.S.A.,
May 28, 1891.

SIR,

Your communication announcing the high honor conferred on me by the Royal Institution in electing me Honorary Member on the occasion of the Faraday Centenary was received early this week. It is a special pleasure to have my name associated with those of the Members of the Royal Institution in whose laboratory Faraday, one of the greatest of philosophers, carried on a large part of his work, and to have it thus honored in connection with the Centenary of the birth of the illustrious Faraday.

I regret to have to say that the state of my health will not permit of my visiting London to attend the proposed lectures of Lord Rayleigh and Professor Dewar.

I have the honour to be your obedient servant,

JAMES D. DANA.

New Haven, Conn., U.S.A.,
May 25, 1891.

My Dear Sir,

Your note has been received informing me that I have been elected an Honorary Member of the Royal Institution of Great Britain. Please express to the Managers of the Institution my high appreciation of the distinguished honor which has been conferred on me. I regret that my engagements are not likely to permit me to be present at the interesting occasion of the centennial which is to be celebrated.

I remain, Sir, with sincere respect, yours faithfully,

J. Willard Gibbs.

Washington, June 1, 1891.

Sir,

I have the honour to acknowledge receipt of the invitation with which you have honoured me, in the name of the Royal Institution of Great Britain, to be present at the Centenary of the birth of Faraday and receive the Diploma of honorary membership of the Institution.

No ordinary engagements would be allowed to prevent my attendance at a celebration of such interest by an Institution whose name and organization are so intimately associated with the greatest of experimental physicists. But physical disability renders it imprudent to undertake a journey abroad at the present time. I can therefore only assure you of my very high appreciation of the honour done me, and my extreme regret that I cannot be present in person to receive it.

I am, with high respect, your most obedient servant,

Simon Newcomb.

University of Rome, May 22, 1891.

Sir,

In reply to your communication informing me of the honor just conferred on me at the General Meeting of the Royal Institution of Great Britain, I can but express my earnest thanks and assure you of the deep sympathy I feel in all that has been, or may be done, to honor the memory of Michael Faraday.

With regard to my attending the lectures I fear it will be extremely difficult, as although the University courses finish about the middle of June, they are followed immediately by a long series of examinations which carry us into the middle of July. I must reserve, therefore, my acceptance, but it would give me great satisfaction to be present besides procuring me the pleasure of becoming personally acquainted with many colleagues whom I have not the advantage of knowing.

Allow me, meanwhile, to thank the Managers and yourself for the kind intention of communicating to me the date to be fixed upon for the lectures.

I have the honor to be, Sir, yours truly,

STANISLAS CANNIZZARO.

[Translation.]

R. Osservatorio del Collegio Romano,
Rome, May 26, 1891.

DEAR SIR,

I have received your esteemed letter of the present month informing me of the great honour that has been conferred upon me by naming me an honorary member of the Royal Institution of Great Britain, and inviting me to attend the scientific gathering to celebrate Faraday's Centenary.

I fully appreciate the distinction, and my wish would be to attend the said gathering, but I cannot come to a determination until June, and I must know the exact date of the meeting.

I must in the meanwhile ask you to kindly convey my best wishes for the Institution, and pray accept the respectful regards of your obedient servant

PIETRO TACCHINI.

University, Copenhagen,
May 22, 1891.

SIR,

Having by your letter been informed of my being elected an honorary member of the Royal Institution of Great Britain in occasion of the Centenary of the birth of Michael Faraday, I beg you, Sir, receive my sincere thanks for this acknowledgment of my labours and the honour thereby shown not only myself, but also my little hardly treated native country.

Of course I would have much gratification in attending on your meeting in London to receive the Diploma personally, but unfortunately my health for the moment is not quite to be relied upon for making the said long journey.

I have the honour to be, Sir, your obedient servant,

JULIUS THOMSEN.

Upsala, June 2, 1891.

DEAR SIR,

I have the honour to acknowledge the receipt of your estimated letter, by which you have announced to me that, in connection with the Centenary of the birth of Michael Faraday, the Royal Institution of Great Britain in London has conferred on me the grand honour to be elected an Honorary Member of the Institution.

Moreover, you have kindly invited me to attend to the lecture, which is to be delivered the 17th June at the very place where the celebrated explorer of physical truths has worked.

Unhappily for me I cannot see any possibility for my going to London this summer, and therefore I allow me to beg you to be my interpreter before the Royal Institution and express my most humble thanks for the great honour the Royal Institution has bestowed on me.

At last I permit me to express my warmest felicitations for the prosperity of the Royal Institution, and hope that the Institution in the future may be able to continue her noble work to support many such heros of science as the celebrated Michael Faraday, whose epoch-making discoveries, deep thoughts and ingenious presentiments now fructify the scientific labours everywhere and in the greatest manner contribute not only to the successful progress of science, but also to the happiness and welfare of the whole humanity.

I am, Sir, your most obedient servant,

ROB. THALEN,

Prof. at the University in Upsala.

St. Petersburg, May 14/26, 1891.

MOST HONOURED SIR,

In answer to your information of my being elected Member of the Royal Institution of Great Britain in connection with the Centenary of the birth of Faraday, I can only express my heartfelt thanks for the honour of being admitted to the circle of the highly esteemed, universally renowned English men of science. The engagements and duties, which I took on me, make it impossible for me to leave Russia and come personally amongst you at the end of June or in July. These are the reasons why I must beg you, most honoured Sir, to be so kind to transmit my sincere and profound gratitude to the Members of the Royal Institution, and my welcome to the memory of Michael Faraday as from one who is a devoted admirer of his glorious name.

I have the honour to be, Sir, your obedient servant,

D. MENDELEEFF.

Genève, 16 Mai 1891.

MONSIEUR,

J'ai l'honneur de vous accuser réception de la lettre par laquelle vous m'annoncez que l'Institution Royale de Londres a daigné me conférer le titre de membre honoraire et je vous prie de vouloir bien me servir d'interprète auprès de vos savants collègues pour leur exprimer ma reconnaissance pour une aussi honorable distinction.

Mais je regrette que mon âge et ma santé ne me permettent pas d'aller à Londres assister à l'assemblée où doit être célébré le centenaire de l'illustre physicien Michael Faraday.

Veuillez agréer, Monsieur, l'assurance de ma considération la plus distinguée.

CHARLES MARIGNAC,
Professeur honoraire à l'Université de Genève.

Amsterdam, May 16, 1891.

SIR,

I have the honour of accusing the receipt of your missive containing the communication that the Royal Institution of Great Britain has conferred upon me the honorary membership.

Let me express the high satisfaction I feel, that the Institution, which at all times has taken the lead in the domain of science, has done me the honour of associating my name with herself. That this takes place at a moment that you remember your august compatriot, makes it particularly agreeable to me.

It is with sentiments of profound gratitude that I accept the honour, and I take the liberty of asking you to be so kind as to bring my thanks into the Institution.

If my health, which lately has somewhat suffered, permits it, I will be present in London to be admitted to the Institution.

I have the honour to be, Sir, your obedient servant,

J. D. VAN DER WAALS.

Bruxelles, le 15 Juin 1891.

MONSIEUR LE SECRÉTAIRE HONORAIRE,

J'ai compté jusqu'au dernier moment de pouvoir me rendre à Londres pour assister à la Séance, Juin 17, de l'Institution Royale de la Grande Bretagne, remercier ses membres de l'honneur qu'ils m'ont fait, et de recevoir des mains de S.A. Royale le Prince de Galles le diplôme de membre honoraire de l'Institution ; mais l'état d'affaiblissement résultant de mon grand âge (78 ans) m'empêche absolument d'entreprendre le voyage. Je suis donc obligé de vous prier d'excuser mon absence, de remercier en mon nom les membres de l'Institution Royale de l'insigne honneur qu'ils m'ont fait et de me faire parvenir par la voie de l'ambassade de Belgique à Londres et au besoin par la poste, le diplôme de membre honoraire.

Veuillez agréer, Monsieur le Secrétaire honoraire, avec mes plus vifs remerciements, l'hommage de mes sentiments de haute et respectueuse considération.

J. S. STAS.

SIR FREDERICK BRAMWELL also reported that the following congratulations had been received from Russia :—

[Telegram.]

The Imperial University of St. Petersburg heartily congratulates the Royal Institution of Great Britain on the memorable centenary of the birthday of its illustrious member and president, the great natural philosopher, Michael Faraday.

Rector of University, NIKITIN.

[Telegram.]

Remembering the great discoveries of Michael Faraday, the Imperial Medical Academy of St. Petersburg begs to congratulate the Royal Institution of Great Britain at the celebration of the hundredth birthday of the eminent natural philosopher.

President PASHUTIN.

[Telegram.]

The Imperial Technical Society of Russia begs the Royal Institution to receive on this memorable anniversary the most cordial congratulations, and the expression of a sincere admiration and a profound gratefulness to the genius of Faraday the creator of electrical engineering.

President, GUERCEVANOV.
Secretary, SIEZNIVSKY.

[Printed.]

The most distinguished member of the Royal Institution of Great Britain, Michael Faraday, by his continued labours enriched the science of electricity with new ideas, the development of which has justly given our century the right to call itself the age of electricity.

The Russian Physico-Chemical Society at the Imperial University of St. Petersburg, begs to congratulate the Royal Institution of Great Britain on the hundredth anniversary of the birthday of Michael Faraday, the celebrated natural philosopher.

President, PROF. TH. PETRUCHEFSKY.
Secretary, N. KHAMONTOFF.

The following honorary members were introduced to H.R.H. The Prince of Wales, and received their diplomas at his hands :—

Professor A. Cornu (of Paris).
,, E. Mascart (of Paris).
,, Pietro Tacchini (of Rome).
,, J. D. Van der Waals (of Amsterdam).

The Duke of Northumberland then asked the meeting to express its sense of the kindness of his Royal Highness in presiding, and said he had permission to read two letters written many years ago, proving how keen was the interest taken by him in the lessons he had received from Faraday. The letters were as follows, the first being addressed to Mr. Faraday, and the second to Mrs. Faraday on the occasion of her husband's death:—

Windsor Castle, 16 January, 1856.

Dear Sir,

I am anxious to thank you for the advantage I have derived from attending your most interesting lectures. Their subject, I now feel, is of great importance. I hope to follow the advice you gave us of pursuing it beyond the lecture room, and I can assure you that I shall always cherish with great pleasure the recollection of having been assisted in my early studies in chemistry by so distinguished a man.

Believe me, dear sir, yours truly,

Albert Edward.

Wiesbaden, 10 September, 1867.

Dear Mrs. Faraday,

Although I have not the pleasure of knowing you, I cannot resist sending you a few lines to tell you how deeply grieved and distressed I am to hear of the death of your husband, Professor Faraday. Having had the great pleasure of knowing him for some years, and having heard his interesting lectures already when quite a boy, I can fully appreciate how great the loss must be, not only to you, but to the whole country at large, where his name was deeply venerated by all classes. His name will not only be remembered as a great and distinguished scientific man, but also as a good man, whose excellent and amiable qualities were so universally known. Pardon my trespassing so soon on your great grief, and

Believe me, dear Mrs. Faraday, yours very sincerely,

Albert Edward.

The Duke of Northumberland continued: He thought they would all agree that that was a touching letter of condolence. His Royal Highness had now long been a patron of the Institution, and had watched its progress with interest, which he hoped would be continued. He trusted that his Royal Highness would have the gratification of seeing the country prosper long under the rule of his august family, and of seeing the benefits of science resulting in the increased happiness of the people.

Sir W. Grove seconded the resolution, and said he was possibly the only one in the room who had known Faraday in his prime.

He wished they had been celebrating the centenary with Faraday alive.

The thanks of the meeting to his Royal Highness having been expressed by acclamation,

The Prince of Wales acknowledged it as follows:—Ladies and Gentlemen, I feel that I cannot, out of courtesy to yourselves, and of the noble duke who has so kindly proposed the usual thanks, and to Sir William Grove who has given us an interesting speech, pass it by without expressing to you my warmest thanks. It is a great honour and privilege to me to preside on this most interesting and memorable occasion. I have now known this room for thirty-six years, and I agree with Sir William Grove in wishing that we were celebrating the centenary of Faraday alive, and not dead—that he was alive to spend his hundredth birthday among us. I feel every time I come into this room as if I can see him standing there at that table, where he gave his interesting lectures and experiments when I was a boy. I again tender my thanks to you, as I do to Lord Rayleigh, for the most interesting lecture he has given.

There was an exhibition in the Library of memorials of Faraday kindly lent to the Institution by Miss Jane Barnard and others.

THE FARADAY CENTENARY.

Friday, June 26, 1891.

The Duke of Northumberland, K.G. D.C.L. LL.D. President, in the Chair.

There were also present—Lord Halsbury, Sir Lyon Playfair, Sir Richard Webster, Sir Edward Fry, Sir William Thomson, Sir Joseph Lister, Sir James Crichton Browne, Lord Rayleigh, Sir Joseph Fayrer, Sir William Bowman, Sir Frederick Abel, Dr. Frankland, Professor Odling, Mr. Ludwig Mond, and Sir Frederick Bramwell.

The Chemical Work of Faraday in relation to Modern Science.

By Professor Dewar, M.A. F.R.S.

Prof. Dewar commenced his lecture by saying that his eminent colleague had done such ample justice to the physical side of Faraday's work, that his own task would be limited to dealing with those early researches in which he developed that astounding manipulative power which enabled him to conduct his subsequent electrical investigations in so remarkable a manner. He proposed to give a brief sketch of the more important of the distinctive chemical labours of Faraday, and then to select one of the many veins of investigation he had opened up, and show what had resulted from its development.

Faraday's chemical work might be divided into the following groups or periods:—Period of Analytic Work. Organic Research. Study of Gaseous Properties. Investigations on Steel and Glass. Determination of Electro-chemical Equivalents. Regelation. Action of Metals on Light. Work on Chemical Manipulation. Published Lectures.

Having given a short résumé of Faraday's progress through these subjects, Prof. Dewar referred to his first great work in organic research, the production of two compounds of chlorine and carbon, the perchloride and the protochloride, and the determination of the composition of "Julian's chloride of carbon." The original specimens prepared by Faraday were exhibited, and it was pointed out that the discoverer's analyses of these bodies were absolutely accurate, notwithstanding the difficulties attending such work at that time. His discovery of "bicarburet of hydrogen" (now widely known and largely manufactured as benzol), and a "new hydrocarbon" (now known as butylene) was then described, it being pointed out that having regard to the methods of working which Faraday had to employ, the isolation and determination of the composition of such bodies was marvellous, and was to be explained only by his wonderful manipulative skill.

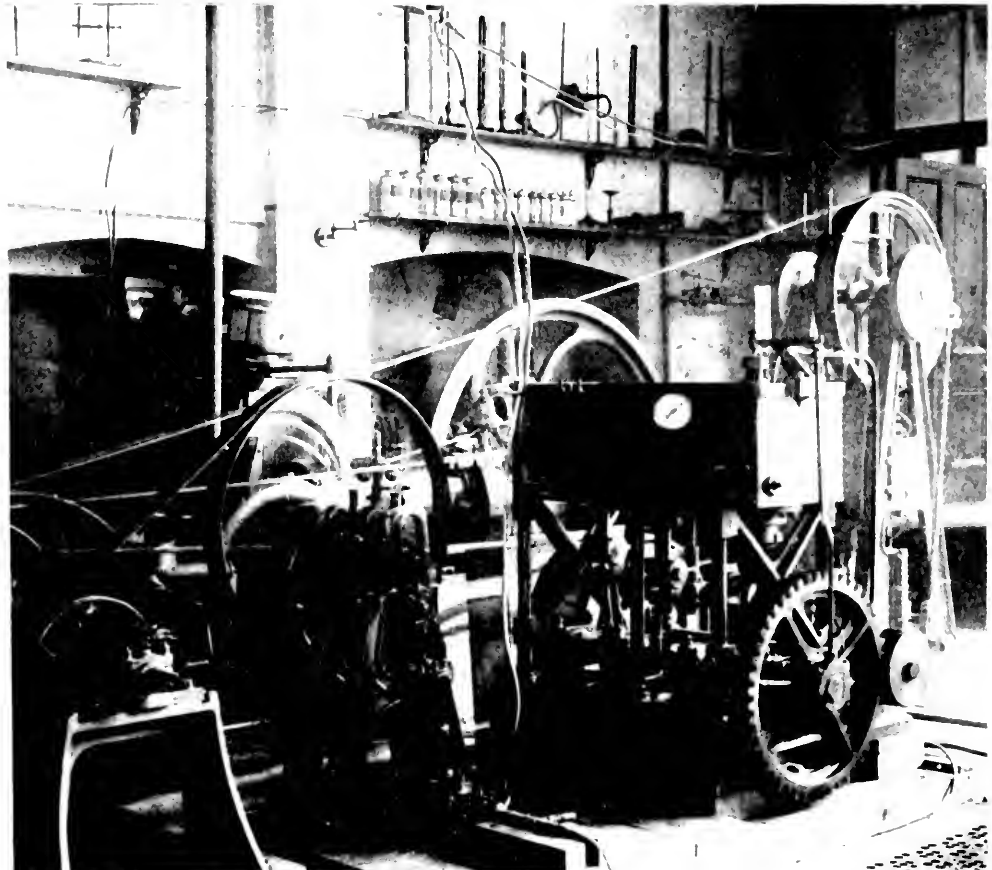
Probably Faraday's most remarkable discovery in organic chemistry was the fact that naphthalene could be dissolved by strong sulphuric acid, and that when thus dissolved the solution did not precipitate naphthalene on being treated with water. That enabled him to prove combination between sulphuric acid and a hydrocarbon. The body, which he called "sulpho-naphthalic acid," is probably the first of the sulpho-acids now so largely employed in the colour industry.

Faraday's next important work was an investigation into the properties of combinations of steel with other metals, in the course of which he demonstrated the now well-recognised fact that an admixture of such minute proportions as one-five-hundredth of such metals as silver, nickel, palladium, &c., will entirely alter the character of the metal. Concurrently with this, he worked on the improvement of optical glass; and it was observed that although the fruits of his labours in this direction lay dormant for some time, they ultimately resulted in one of his most important discoveries, namely, the rotation of the plane of polarisation in the magnetic field. The glass produced by Faraday by the fusion of oxide of lead with boracic acid was selected by him because of its superior fluidity combined with great density. (Experiments were given illustrating the peculiar physical and electrical properties of the Faraday glass.)

The next research was that on the liquefaction of gases, which, although carried out by Faraday, was nevertheless done at the instigation of Davy. Davy had discovered a substance which proved to be a hydrate of chlorine, and which he found could be kept either in ice or in sealed tubes. Faraday had produced a quantity of this substance during the cold weather, and had made an analysis of it. Davy then suggested that it should be heated in a sealed tube, and, without saying what he really expected to take place, indicated that one of three things would happen, namely, that it would either melt, act on water, or produce liquid chlorine. The latter event happened, and opened up vast possibilities, the prosecution of which Davy left to Faraday. (Experiment on the liquefaction of chlorine given.) The necessity of obtaining tubes strong enough to stand the pressure required for the liquefaction experiments led Faraday to make investigations at this time into the production of bottle and other glass.

Faraday next turned his attention to researches on the electro-chemical relations of bodies, crystallisation, and the action of metals on light. It was in connection with the research on crystallisation in 1856 that Faraday made his interesting discovery of the phenomenon of regelation, by virtue of which two portions of a piece of ice, after being severed, freeze together again on being brought into contact, even when the temperature of the surrounding medium is higher than the freezing point of water. Although discovered by Faraday, it was not until comparatively recent times that the explanation of the phenomenon was given, and its influence on glacial motion clearly established. (Experiment on regelation shown.)

FIG. I.



Specimens, arranged and tabulated by himself, of Faraday's last research on the optical properties of gold leaf in a highly attenuated form were exhibited and described.

Turning then to the special subject of the evening's discourse, the liquefaction of gases, Prof. Dewar stated that although Faraday made his first researches in this direction as early as 1823, the matter lay dormant for many years, until his interest in it was reawakened by Thilorier's discovery that solid carbonic acid could be produced in the form of a snow-like substance, boiling at − 80°C., and capable of being handled. Faraday was the first to introduce this discovery into England in a lecture given at the Royal Institution on the 18th May, 1838; and, thereafter, by its aid, he resumed his work on the liquefaction of the various gases which had resisted his former efforts. All through the summer of 1844 he was busily employed at this work, using the low temperatures, which Thilorier's new product enabled him to obtain, combined with great pressures. (Specimens of gases thus liquefied by Faraday shown.) This important work was the subject of a Friday evening lecture given at the Royal Institution early in 1845, a full abstract of which appeared in the *Times* of that date, the Institution itself not having then commenced the publication of its proceedings. In the course of that address Faraday produced a small quantity of ethylene; and he expressed the opinion that if a method could be found of producing liquid nitrous oxide in large quantities, that would be the material which would enable him to liquefy oxygen and the other gases which had hitherto resisted all his efforts. (Experiments showing the comparative boiling points of solid carbonic acid, nitrous oxide, and ethylene at ordinary pressure and under diminished pressure given.) Faraday hoped that the production of solid nitrous oxide would enable him to get temperatures as far below the boiling point of carbonic acid as the temperature of that body was below ordinary temperatures. As a matter of fact, it is impossible to reach such low temperatures by the agency of solid nitrous oxide, and such great depression of temperature was not attained until such time as liquid ethylene became available. The lecturer here showed and described a diagram of the machinery and apparatus now employed at the Royal Institution for the liquefaction and solidification of gases, see Fig. 1. The method of producing liquid ethylene, and of employing it over and over again in the apparatus was described.

The work done in connection with this subject since the time of Faraday, and especially the investigations of Andrews and Van der Waals, had enabled scientists of the present day to calculate exactly the temperature of the boiling point of hydrogen, the gaseous body which has in the liquid state the lowest boiling point of all the elementary substances, and which has up to the present time resisted liquefaction. The temperature of boiling hydrogen would be − 250° C. The lowest point attained by Faraday was 110° C., and the lowest temperature yet reached was − 210° C.

Prof. Dewar then performed the experiment of actually producing liquid oxygen, which was seen to boil quietly when collected in an open vessel at a temperature of $-180°$ C. The colour was slightly blue, only a few particles of solid matter being visible, which Prof. Dewar explained were traces of solid carbonic acid, the elimination of which had given him considerable trouble. The lecturer further proved by actual experiment on his own hand and on a glass vessel that the liquid oxygen was in the spheroidal condition; and also that alcohol when added to the liquid became instantly solidified. The usual test for oxygen by means of a glowing taper was also made on the vapour given off by the liquid. The form and arrangement of the apparatus employed on the lecture table is shown in Fig. 2.

Prof. Dewar stated that the prosecution of the researches inaugurated by Faraday was enabling scientists to approach nearer and nearer to the zero of absolute temperature; and the speculations of physicists were now directed to the probable characteristics of hydrogen and of matter in general when that condition should be attained. At such a temperature the properties of matter would in all probability be entirely changed; the old Lucretian law would be suspended, molecular motion would probably cease, and what might be called the death of matter would ensue—as in fact the death of chemical affinity and chemical action was known to take place at the low temperatures already attainable. (Experiment proving this by the immersion of phosphorus, sodium, and potassium in liquid oxygen.) On the other hand, it was found that even at such low temperatures oxygen retained its characteristic absorption spectrum.* Further experiments were given proving the liquefaction of ozone by means of liquid oxygen—a tube of the liquid thus produced showing the characteristic deep blue colour of that substance.

In conclusion, Prof. Dewar said that although great progress had been made since Faraday's time, chemists were still working distinctly on the lines of his early researches; and it seemed to him that no fitter method of celebrating the centenary of Faraday's birth could be chosen than the demonstration of the realisation of some of his own ideas.

---

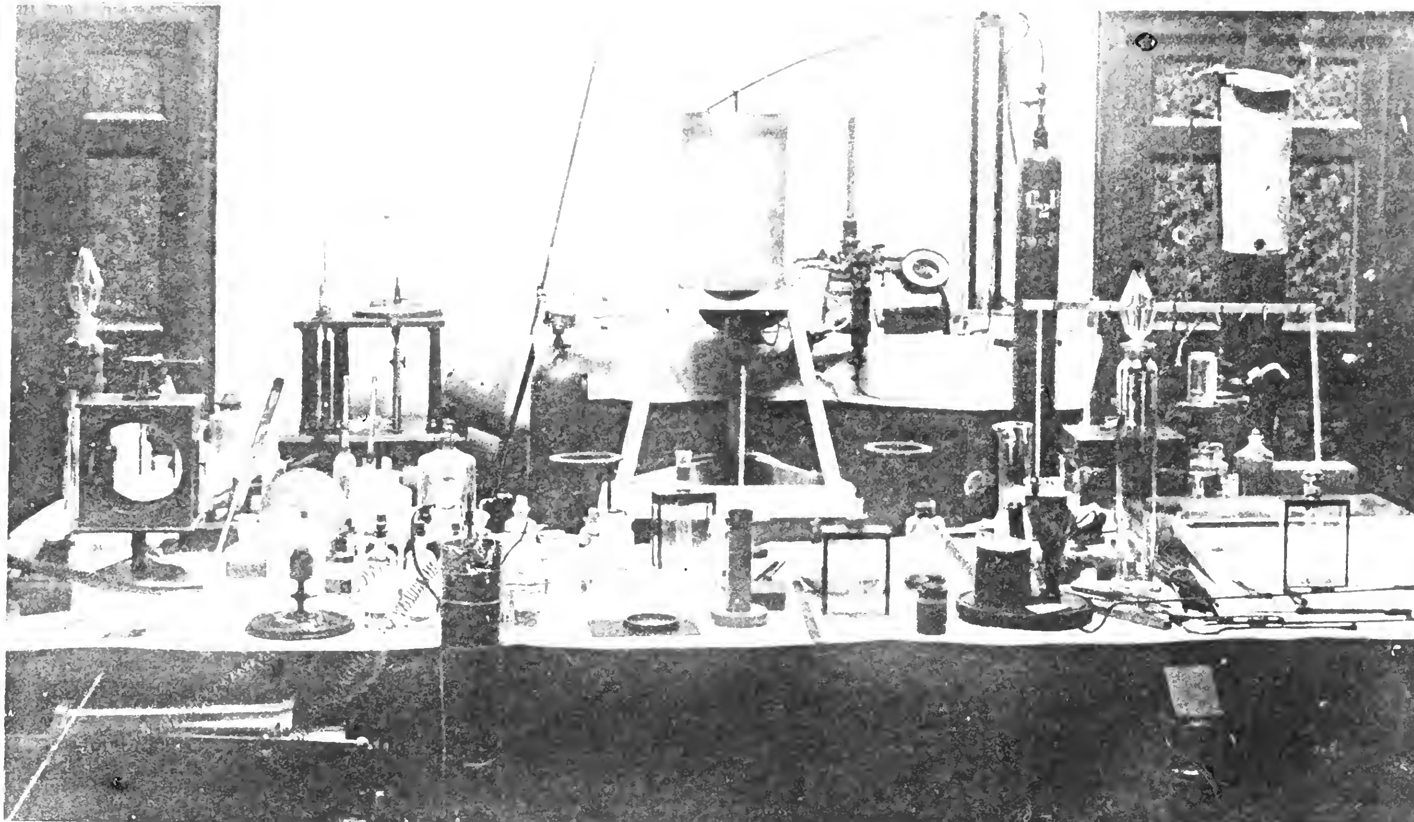
* The recently discovered magnetic property of the liquid adds a new interest to this substance.

"Royal Institution, 10th December, 1891.

"DEAR SIR WILLIAM THOMSON,—The following observation, which I have just made, may interest the members of the Royal Society, and if you think it of sufficient importance you may announce it at this day's meeting.

"At 3 p.m. this afternoon I placed a quantity of liquid oxygen in the state of rapid ebullition in air (and therefore at a temperature of $-181°$ C.) between the poles of the historic Faraday magnet, in a cup-shaped piece of rock salt (which I have found is not moistened by liquid oxygen, and therefore keeps it in the spheroidal state), and to my surprise I have witnessed the liquid oxygen, as soon as the magnet was stimulated, *suddenly leap up to the poles and remain there permanently attached until it evaporated.* To see liquid oxygen suddenly attracted by the magnet is a very beautiful confirmation of our knowledge of the properties of gaseous oxygen.—Yours faithfully, JAMES DEWAR."—[Proc. Royal Society, vol. l. p. 24.]

FIG. II.



Photograph taken by Miss Reynolds.

On the conclusion of the lecture, a vote of thanks to Prof. Dewar was moved by the Lord Chancellor, who said:—

My Lord Duke, my Lords, Ladies and Gentlemen,—I am very happy indeed to be made the instrument of conveying your thanks for the most interesting lecture we have listened to. I could not help thinking while our lecturer was giving us an account of all these wonderful things, that he was illustrating in his own person something which he had said. He pointed out how the torch of science was passed on from hand to hand, how, for instance, Davy had handed to Faraday some of the sources of those great discoveries which he afterwards disclosed to the world; and I thought that it required some such successor to give adequate expression to the history of Faraday's work. Faraday had many friends; many of us have listened to him in this theatre, as indeed I have had the privilege of doing myself; and I think I may say that no one came within the sphere of his kindly and gentle influence who did not become a hearty and attached friend. But I should think that very few of those friends would be able to give adequate expression to what he had done, the discoveries he had made, and the ever increasing effect which those discoveries had exercised upon the progress of modern science. We have listened to-night to a most able exposition of Faraday's work; and I think that Prof. Dewar has shown that he has in truth succeeded to that work, that he is worthy to receive that torch and carry it on and give a brighter illumination to science than it has ever yet received. I am sure that there is none here who will not heartily join with your Grace in thanking Prof. Dewar for the able, learned and lucid lecture in which he has explained to ignorant people like myself Faraday's wonderful discoveries in science.

Sir Lyon Playfair, in seconding the motion, said: It is indeed a great privilege to all of us to see the great progress which has been made in the discoveries of Faraday during the last fifty years. Those little tubes, containing the original liquefied gases which Faraday liquefied under pressure and low temperatures were very important and were considered at the time very remarkable productions. But you see how the subject has since grown; how carbonic acid, for instance, first liquefied, has since been solidified so that it can be handled like snow; and you have seen the remarkable way in which oxygen has been liquefied on the present occasion. An old Professor of chemistry like myself can appreciate the wonderful manipulative power which Prof. Dewar has displayed this evening. Even in the chemical laboratory, with everything quiet around you, it is difficult to make these experiments successfully, but in a theatre of this kind it is marvellous how everything goes wrong; and if we had not had a manipulator of great accuracy and knowledge, we could not have had the gratification which we have enjoyed this evening. What strikes me as being so excellent in my friend, and much more than friend—for he is the greatest chemist that I ever produced, and I am extremely glad to think that he looks up to his old teacher with affection—while I look to him with love and honour—what I wanted to say is that I think he has done quite rightly in giving you the scientific side of these wonderful discoveries, and showing you the way in which they are growing and giving us a better knowledge of the condition of matter. When Faraday first made experiments like these, some wiseacres said: What is the use of it? Faraday replied; "Will you tell me what is the use of a baby?" But Faraday's baby has centred around it all the hopes and desires of the parents that produced it, and the State also has shown much interest in its upbringing. The bodies that appear in those tubes have become important factors in the progress and industry of the world. The carbonic acid, which I recollect first seeing as a little globule of acid, is now carried in cylinders filling railway trucks, and is applied to many purposes, some important, others more useful than important. For instance the liquid carbonic acid enables barmaids to get beer up from the cellars below without pumping it; that nitrous oxide which we were so interested in as a condensed gas is now largely used by dentists as a means of extracting our teeth without pain; sulphurous acid will, I am certain, become most important in war, for if you took a brittle shell filled with liquid sulphurous acid and threw it between the decks of a ship it would produce such a stink that everybody would disappear in a moment. The time is coming when other gases will be used in this way. Their importance does not altogether consist in their applications to industry, though they are becoming very important in that way. But their importance is that they are teaching us more of the constitution and properties of matter; it is in that respect that they are becoming so interesting in the eyes of scientific men. I have been extremely interested in watching the production of that liquid oxygen. I looked upon it with great respect, and wondered to see it not covered with a cage as if likely to go off at any moment in a terrific explosion. But it is produced in such a manner that its own cold keeps it down, and so we saw it handled in the most marvellous way as an ordinary liquid. I have the utmost pleasure in seconding the vote of thanks to Prof. Dewar for the brilliant exposition which he has given us.

The Chairman then put the motion, and it was carried with acclamation.

Prof. Dewar in reply said: My Lord Duke, my Lords, Ladies and Gentlemen,—I am exceedingly indebted to you for the very kind way in which you have referred to the labours of the lecturer. I can assure you that it has been a source of great pleasure to me, and that in fact I have had the least part to do. This kind of illustration cannot possibly be given without means of various kinds, and there are several benefactors whom I should like to mention in connection with this lecture. First of all, Dr. Anderson gave the pumps which enabled me to compress and evaporate such volatile bodies; secondly, we require machinery to set those pumps in motion, and somebody to look after it, and that has been supplied by the kindness of Mr. Robert

Wilson, of the well-known firm of Messrs. Crossley, who is always ready and willing to help us; thirdly, as regards the cost of the material used —which has been by no means small—I am indebted to another member and great benefactor of the Institution, namely, Mr. Ludwig Mond, F.R.S. And lastly, but not least, I am indebted to my assistants, Mr. Lennox and Mr. Heath, for the assiduous and self-sacrificing way in which they have laboured in order to make these experiments go successfully. As Sir Lyon Playfair has said, it is comparatively easy to do these things in the quiet of the laboratory, but immensely difficult to get them to go on an occasion like this; and when we consider the long distances over which these highly condensed gases have to be conveyed, and the complex arrangements necessary to avoid all fear of danger, I think you will agree that the benefactors who rendered these arrangements possible are deserving of more credit than the lecturer.

Lord Justice Fry then proposed a vote of thanks to his Grace the Duke of Northumberland for his kindness in presiding over the meeting. In doing so, his Lordship said: While I ask you to tender his Grace your hearty thanks for attending to-night I cannot omit to ask you to thank him also for even greater services. He has presided over this Institution for many years, and has ever shown in its affairs a warm and intelligent interest, and he has been a most liberal benefactor of the Institution. At our ordinary meetings we have no opportunity of expressing our feelings to our benefactors; but on this extraordinary occasion we have that opportunity. I feel that I only express the sentiments of all here when I propose to proffer your warmest thanks to his Grace, not only for presiding this evening, but also for the great debt of gratitude which we owe him for his past services.

Sir Richard Webster said: I have the great privilege of being permitted to second the vote of thanks to his Grace. I most heartily endorse all that Lord Justice Fry has said with respect to the eminent services rendered to this Institution by his Grace the Chairman. I also heartily agree with what has been said by previous speakers with respect to the admirable lecture that we have heard to-night, some portion of which will, I hope, remain in my mind and memory, but the immediate effect of which has been to completely paralyse the power of ordinary speech. I feel it a great privilege to have been permitted to take some part in the proceedings, and have the greatest pleasure in seconding the vote proposed by Lord Justice Fry, which I venture to hope may be carried by acclamation.

The vote having been put and carried by acclamation,

The Duke of Northumberland said in response: My Lords, Ladies and Gentlemen, I feel somewhat embarrassed on the present occasion, because I had no expectation of, nor did I feel myself entitled to, the vote of thanks you have been so kind as to pass. I should have been wanting in duty if I had not been here to attend the Centenary of the illustrious man whose memory we have met to celebrate; and I must say I have been amply rewarded by the lectures I have heard, both from Lord Rayleigh and Prof. Dewar. They have almost persuaded me between them that I understand something of this science, which I confess but for them would have seemed impossible. Time is getting on, and I therefore will not detain you longer than to thank you most sincerely, and to ask you to accept this simple expression of my gratitude.

Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, January 22, 1892.

Sir Frederick Bramwell, Bart. D.C.L. F.R.S. Hon. Secretary and Vice-President, in the Chair.

The Right Hon. Lord Rayleigh, M.A. D.C.L. LL.D. F.R.S. *M.R.I.* Professor of Natural Philosophy, R.I.

The Composition of Water.

[No Abstract.]

WEEKLY EVENING MEETING,

Friday, January 29, 1892.

Sir James Crichton-Browne, M.D. LL.D. F.R.S. Vice-President and Treasurer, in the Chair.

Sir George Douglas, Bart. M.A.

Tales of the Scottish Peasantry.

It is only within comparatively recent years that the homely stories in the mouths of the common people have been constituted a branch of learning, and have had applied to them, as such, the methods and the terminology of science. A noteworthy gain to knowledge has, beyond a doubt, resulted from this treatment; but, side by side with this gain to knowledge, is there not involved in the said method of treatment a loss to the stories themselves? Classified, tabulated, scientifically named, they are no longer the wild free product of Nature that we knew:—no doubt they are still very interesting—the study of them is full of instruction; but their poetry, their brightness, the fragrance which clung about them in their native air, their native soil, is gone. So that,—with all due recognition of the value of the labours of the scientific folk-lorist, the comparative mythologist,—there yet remains room, I believe, to regard these stories from another point of view, namely, the *literary*, or *critical* one. I hope the time has not yet come when the old tales shall have entirely ceased to charm; and I believe that there are persons in existence who would regard it as a real and personal loss could they be made to believe that the ideal hero of their childhood, as he falls in a bloody battle wounded to the death, is in reality a myth, or figure, for the setting of the sun; and who would even feel themselves aggrieved could they be brought to realise that the bugbear of their baby years is common also to the aborigines of Polynesia. So powerful is the spell of early association.

I suppose that most nations, whilst their life has remained primitive, have practised the art of story-telling; and certainly the Scotch were no exceptions to the rule. Campbell of Isla, who wrote about thirty years ago, records that in his day the practice of story-telling still lingered in the remote western islands of Barra; where, in the long winter nights, the people would gather in crowds to listen to those whom they considered good story-tellers. At an earlier date, but still at that time within living memory, the custom of story-telling survived at Pool-Ewe, in Ross-shire; where the young people were used to assemble at night to hear the old ones recite the tales which they had learned from their forefathers. Here, and at earlier dates in other parts of the country also, the demand for stories would further be supplied by pedlars, "gaberlunzie men," or pauper wandering musicians and entertainers, or by the itinerant shoemaker or tailor,—both of which last were accustomed to travel through thinly-populated districts, in the pursuit of their calling, and put up for the night at farm-houses,—where, whilst plying their needles, they would entertain the company with stories. The arrival of one of these story-tellers in a hamlet was an important event. As soon as it became known, there would be a rush to the house where he was lodged, and every available seat would quickly be appropriated. And then, for hours together, the story-teller would hold his audience spell-bound. During his recitals, the emotions of the reciter were occasionally very strongly excited, as were also those of his listeners,—many of whom, no doubt, firmly believed in all the extravagances narrated. And such rustic scenes as these have by no means been without their marked effect upon Scottish literature.

Perhaps the most characteristic of the Highland tales are those which deal with heroes and giants. But these are generally very long, and, truth to tell,—with all the repetitions of dialogues, all the reproductions of what is practically the same situation, which distinguish them,—they are apt to appear to us wearisome. The shortest kind of popular tales are those which the Folk-Lore Society calls Beast Tales,—the stories, namely, which are concerned with the dumb animals. The Highlands, in particular, are rich in such stories; and it is easy to understand how the common country-people—living so near to nature as they do—may come to have an insight into, and an appreciation of, the characters of the brute animals, and a sympathy with them in their tussle for existence, which is not attainable by those who lead a more artificial life. Some of the fables and traits of animal life in which this knowledge and appreciative sympathy have been embodied are decidedly naive and quaint. Nor are they without a human application.

The class of stories which we may consider next—the Fairy Tales—display a higher degree of fancy. And it would be a mistake to imagine that this quality of fancy is anything less than a characteristic attribute of the minds of many of the Scottish peasantry. It displays itself, for instance, in its simplest form, in their nomenclature—in the names which they have given either to natural objects, or to places which are characterised by some striking natural feature. For example: a waterfall in Dumfriesshire, where the water, after pouring dark over a declivity, dashes down in white foam among rocks, is known as The Grey Mare's Tail; twin hills in Roxburghshire, which have beautifully-rounded matched summits, have been christened Maiden's Paps. Then, the cirrus, or curl-cloud, is in rustic speech "goat's hair"; the phenomenon of the Northern Lights, among the fishermen of Shetland, is the "Merry Dancers"; the Pleiads are the "Twinklers"; the constellation of Orion, with its star *iota* pendant as if from a girdle, is the "King's Ellwand," or yard-measure; the noxious froth which adheres to the stalks of vegetation at midsummer is the "witches' spittle."

There is a root of poetry, I think, in this aptitude for giving names; and, as a matter of fact, in the Lowlands of Scotland, rustic poets and rhymesters are far from uncommon. Nor are the peasantry, in their name-giving, wanting in literary allusiveness—allusiveness, that is, to the only book which has ever obtained universal currency among them. Thus, among the fishermen of the East Coast, the black mark below the gills of a codfish, or haddock, is "Peter's Thumb;" whilst a coarse field-plant called by botanists *Polygonum persicaria*, which has its leaves strangely clouded and stained, as with droppings of some dark liquid, is locally known on the Borders as the "Flower that grew at the Foot of the Cross."

Perhaps the deepest thinkers among a people who have their philosophers as well as their dreamers, are to be found among the hill-shepherds. And it is chiefly through the instrumentality of one of these that we can now enter the Fairyland of the Scottish peasant. James Hogg, the Ettrick Shepherd, was one of those common men, *plus* genius, who every now and then in the history of literature give to a whole world of floating thought, tradition, fancy, a permanent substantial form. No man in literature is his master in the weird tale. No man but Shakespeare, not even excepting Drayton, has written so well of the fairies. Hogg was born in the Arcadia of Scotland, Ettrick Forest—where, as Scott tells us, the belief in fairies lingered longer than elsewhere—about the year 1770. As he grew up, the spirit of emulation was stirred in his breast by the example of the poet Burns. And so, as he wandered through the pastoral solitudes keeping his sheep, he carried an ink-horn slung from his neck, and taught himself to write, and so committed to paper his first poem. And as he thus wandered and mused, he tells us that he one day fell asleep upon a green hill-side, to dream the dream of Kilmeny, and to bear her image in his heart for ever after.

The story of Kilmeny is that of a girl of poetic temperament, a lover of solitude, who, wandering alone at twilight, disappears in a wild glen among the hills. She is sought for by her parents; but no trace of her is found. Years pass, and the mystery remains unsolved. But at the close of the seventh year, in the same twilight hour in which she had vanished, Kilmeny returns to her home. She has been rapt away by fairies, with whom the intervening years have been spent. But in the midst of Fairyland, her heart still yearns tenderly to her home; and when seven years have expired, and the fairies have no longer power to detain her against her will, she chooses to leave the life of pleasure which she leads among them to return to the common world. This is an outline of the story; but the story is the least part of the poem. Its charm lies in its exquisitely flowing and melodious verse, in its suggestion of the twilight world and of a world of shadows—a land "where all things are forgotten,"—in its wistful tenderness; in one word, in the unique and perfect aptness of the style to the subject. So magical, indeed, are the fairy touches throughout the writings of the Ettrick Shepherd, that one might almost be tempted to dream that the experience with which tradition credits Thomas the Rhymer had been shared by this rhymer of a later day.

As in England, tales of fairies caught sight of on the country green, at twilight or by moonlight, of services rendered by mortals to fairies and gratefully and gracefully repaid, find a place among the fables of the Scottish peasantry. But it is by no means in such airy, gracious, and harmless if not beneficent, creations as this that the genius of the Scottish nation finds its fancy's most congenial food. That genius is, upon the whole, essentially a sombre one,—relieved, indeed, by a rough humour; but tending most to an affinity with gloom. The malevolence, the hostility, of Nature, its permanence as contrasted with the transient character of man, its victoriousness in the never-ending battle waged against it by man,—a battle in which he fights for life, in which he gains a few trifling and temporary advantages, but in which he must recognise from the first that he fights against impossible odds: these are facts which a barren soil and a bleak and stormy climate have thrust forcibly upon the Scottish popular imagination, and which have impressed themselves deeply upon it. This gloomy view of Nature has tinged the superstitious beliefs of the peasantry, and through them their stories. And upon the back of this gloomy view of Nature, has come a sense—stronger perhaps than is felt by any other nation—of fate and doom, of the mystery of life and death, of the cruelty of the inevitable, the pain of separation, the darkness which enshrouds the whole. In this sense the Scotch are a nation of pessimists. They have found their religious vocation in Calvinism; and the spirit which embraced Calvinism like a bride informs their mythology and their fireside tales. Their tendency to devil-worship, to the propitiation of evil spirits, is illustrated by the hideous usage of the Good-man's Croft,—a plot of ground near a village which was left untilled—set apart for, and dedicated to, the Powers of Evil, in the hope that their malignity might be appeased by the sacrifice, and that so they might be induced to spare the crops on the surrounding fields. Of the state of superstitious dread in which some Scotchmen passed their lives, Mrs. Grant of Laggan gives a curious illustration when she tells us that in the Highlands of her day, to boast, or to congratulate a friend, was to rashly court retribution; to praise a child upon the nurse's arm was to incur suspicion of wishing to bring down ill upon its head.

Holding these beliefs, it is not to be wondered at if, in their stories, the Scotch are the past-masters of the *weird*. And, as a matter of fact, their very nursery-tales—many of them—would appear to have been conceived with a view to educating, for some strange purpose or other, the passions of horror and of sorrow in the child to whom they are told. Such rhymes, for instance, as "The Tempted Lady," and "The Strange Visitor," are uncanny to a degree. In the former, the Evil One himself appears, in specious guise. The Strange Visitor is Death. The nursery ballad of "The Croodin Doo"* is as full of combined piteousness and sinister suggestion of underhand wickedness as any little tragedy of its length could well be. The suggestion is that of a man's childless lawful wife bearing a bitter grudge against his child borne by another woman. The babe returns from a day's outing, and is questioned by his slighted mother as to where he has been and what he has done. But he is tired, and cries out to be put to bed. The jealous woman, however, persists in her interrogatory, in the course of which she asks him what he had for dinner. He replies that he dined off "a little four-footed fish." (The eft, or newt, is, like the toad, in the common superstition, venomous). "And what was done with the bones of this singular fish?" asks the woman. They were given to the lap-dog. And what did the dog do? After eating them, he "shot out his feet and died." There, with admirable art, the ballad ends. Its effect is immensely heightened by a burthen, or refrain, in which, at the close of every verse, the child, with wearisome iteration and with child-like importunity, cries out to his mother to "make his bed soon." This ballad of child-life is queer fare to set before a child.

Stoddart, the tourist, long ago remarked the contrast between the fairies of the English popular mythology and those of the Scotch; and certainly the delicate, joyous, tricksy, race of moonlight revellers whom we meet in Shakespeare are scarcely to be recognised as belonging to the same family with the soul-less, man-stealing, creatures of the Scottish peasant's fancy. The effect exercised upon popular superstition by the ruling passion of Calvinistic religion is one of the most striking things in Scottish folk-lore. The belief in fairies, for example, did not cease to exist. It was not even universally discountenanced by the Church; for we find recorded instances of Ministers of the Gospel combining with their parishioners to take measures for the restitution of infants which the fairies had changed at nurse, or for the recovery of women who had been spirited away. And certainly

* A term of affection applied to a child.

two of the most curious pieces of composition known to me are, a pamphlet on the Second Sight written by a Minister of Tiree, and an article on the Fairies written by a Minister of Aberfoyle,—both in the Seventeenth Century. Both writers were firm believers in the superstitions upon which they wrote; and in both cases the gross ignorance and darkness of the writer's mind is only equalled by the authoritative weight and pedantry of his style.

The fairies, however, and that rough, grotesque, humoursome, but good-natured figure, the Brownie, occupy but a small space in the popular mythology in comparison with such shapes of awe, of terror, or of ill-omen, as the ghosts, "more real than living men," which the Highland Ezekiel saw borne past him on the wind in Morven, or as the witch, the wraith, the "warning," the water-kelpie, the man or woman who has the second-sight.

The characteristic rough humour of the Scotch peasant, as it affects the creations of the fancy, embodies itself almost exclusively in the Brownie. The Brownie was a wild, half-human, creature, whose custom it was to devote himself to domestic service in a particular family. But he worked from perfectly disinterested motives; and so strained was his sense of self-respect that, on the slightest attempt to recompense his services, he would disappear for ever. The Brown Man of the Moors is another of these twilight, or half-seen, creations; but he is not of a domestic character. Wanderers upon lonely moors might, on rare occasions, catch a glimpse of him lurking in a hollow,—a short, squat, powerful figure, earth-coloured, or of the tint of the surrounding ling. "Shellycoat" dwelt in the waters. His coat was hung with shells, which clattered as he moved; and his delight was in mischief,—such as, for instance, like Will-o'-the-Wisp, in leading travellers astray. "Nuckelavee," the Sea-Devil of the Orkney Islanders, a more formidable phantom, seems to be shaped like a man above and like a horse below; and his peculiar horror lies in the fact that, being skinless, his raw red flesh is exposed to view. Then there is the River Horse, a supernatural being supposed to feed, in the shape of a horse, on the shores of Loch Lochy, and when disturbed to plunge into its waters. The River Bull it is who emerges from the lake to visit the cow-pastures; and cow-herds pretend that they can distinguish the calves of which he is the sire. But a more awe-inspiring water-spirit than any of these was the Kelpie; whose appearances were generally timed either to give warning of death by drowning, or to lure men to a watery grave; and who illustrates the feeling—as I have already observed, so insistent throughout Scottish mythology—of the inveterate hostility of Nature. The elements are our enemies, and wage an internecine war.

Perhaps the most valuable element in the peasant-tales, considered from the poetic standpoint, is the human element. The juxta-position of the supernatural brings out in extraordinary strength certain traits of the human. For instance: the strangest, the most startling, and to us the most incomprehensible, of all the Scotch superstitions is that which prescribed a belief in the periodical return of the dead to their former homes—not as night-walking spectres encountered only by those who were alone and in the dark—but as *social beings*, come back to join the family circle and share in its festivities,—in short, in the old phrase, come back "to dine and dance with the living." How anything so incredible should ever have come to be believed, we may well be at a loss to understand. Yet believed it seems to have been. There are two of the old ballads which are concerned with the belief, and they are two of the finest which have come down to us. The fragment entitled "The Wife of Usher's Well" tells how a thriving country-woman made provision for her three sons by sending them to sea. But they have not been long away from her, when she hears that they have perished in a storm. Then, in the madness of her grief, she puts up a blasphemous prayer to Heaven,—praying that the conflict of wind and wave may never cease until her sons come home to her in their likeness as she knew them of old. Her prayer is heard; and answered. When the long dark nights of Martinmas come round, the sons return to their home. In outward seeming they are unchanged; but the hats they wear, as we are told, are of a birk, or birch-tree, which is not of earthly growth. Rising to a height of simple, unconscious, tragic irony, the ballad goes on to detail the preparations which are made by the mother to fête the home-coming of her sons. In a fever of happiness, she issues her orders to her maids. The fatted calf is slain; and a brief hour of joy goes by. Then, as it grows late, the young men betake themselves to rest. The mother has prepared their bed with her own hands. But the dawn draws near—the period of their sojourn is almost up. The cock crows. They recognise the signal which binds them under penalty to return whence they came, and with a few touching words of leave-taking they depart as they had come. In this case the superstition of the return of the dead to their homes, to visit their friends, is complicated with the idea of punishment for a rash utterance or impious prayer. But in "The Clerk's Twa Sons of Oxenford"—the other ballad which deals with the same theme—in which the home-coming of the dead is timed at Christmas, the fundamental idea appears in its simplest form. These two tales are perhaps the wildest in the whole range of Scottish popular story; but, wild as they are, they contain, I think, a distinct and deep human significance. It will be observed that, in either case, the return of the dead to their homes is fixed at a season of relaxation and festivity. At such seasons the thoughts of the working-people, being set free from their daily occupations, are at liberty to wander; and it is a fact that the annual recurrence of such landmarks in time, with their familiar accompaniment of usages and ceremonies, brings bygone years before the mind with a peculiar clearness—or, at least, brings them before the minds of people who lead simple monotonous lives with few events to mark them. Nothing is commoner at such seasons than to hear the country-people refer to the friends whom they have lost since that time last year, dwelling upon particular acts of theirs, and upon their ways and characters generally. Well, from this peculiar vividness of mental realisation, it is, for a bold and poetic imagination, but a single step to conjure up the actual bodily presence of the departed. Hence may have arisen these wild stories; and hence, no doubt, arose the fancy—a beautiful and touching one—of the dead returning to their homes at a season of festivity, "to dine and dance with the living."

To sum up;—the more striking characteristics of the Scottish peasant-tales generally would appear to be: First, an ever lively and inventive fancy. Secondly, a powerful imagination. The Scottish peasant story-teller is, like Homer, εὐφαντασίωτος—" qui sibi res, voces, actus, secundum verum, optime fingit," as Quintilian renders it. And this powerful imagination is apt to be gloomily affected, and at times distempered, by the natural features and conditions of the country, and by the broodings of the national mind. Thirdly, a love of humanity, coupled with a keen sense of the hardness of its lot,—manifesting itself in a poignant pathos. Of course, in a country of mixed races like Scotland, the general characteristics of the stories differ widely in the different parts of the country. In general terms, it may perhaps be said that the Highland tales display a more inexhaustibly luxuriant fancy, whilst those of the Lowlands have the more clearly defined outline and enjoy a monopoly in depth of human significance.

To glance now at the effect which has been exercised upon literature by these tales. The Tales of the Scottish Peasantry have enjoyed particular advantages in the fact that the rich mine which they afford has been well and admirably worked by modern Scottish writers. Indeed, from the date of Smollett's death onward, the Scottish prose belles-lettres may be said to have been largely "a growth of the soil." And the Scottish writers who have worked the field of popular tradition have not worked in the spirit of such German authors as, for instance, Musæus, Tieck, and Fouqué,—making the popular tale a mere foundation upon which to rear their own structures of philosophy and fancy, and often transforming it almost, if not quite, beyond recognition. Neither have they worked upon the lines of such a writer as Théophile Gautier, who, though he would sometimes use the popular tale as material to work upon, was guided in his choice of subject by a purely artistic instinct. The Scottish writers are, in the first place, objective; and, in the second, national.

Foremost amongst these writers is, of course, Sir Walter Scott. In comparison with his other works, his "Border Minstrelsy" has been neglected; yet, in all probability, he produced no more highly characteristic book; whilst, of that great literature of fiction of which he afterwards became the author, the best and most vital parts may, I think, truly be said to have their roots in the hearts of the people. And the further he departs from that source of his inspiration, the less valuable his work becomes. Although not born in the peasant class, Sir Walter knew the Scottish peasantry, in his own way, as few men have known them; and he lived on terms of friendly intimacy with his valued Tom Purdies and others, and of close literary confidence with such men as William Laidlaw. The two writers who rank next in the group were, however, peasants born. I have already spoken of James Hogg. Allan Cunningham, born in 1784, was a son of the land-steward on the estate on which Robert Burns occupied a farm,—a fact which no doubt had its effect in stimulating the poetic impulse that was in him. His "Traditional Tales of the English and Scottish Peasantry" is perhaps the best of the many books which he wrote, and is especially distinguished by the sweetness of his style, and by the picturesque traits of old-fashioned country life, and the delightful touches of fresh nature-painting in which it abounds. After Cunningham, comes Campbell of Isla, born in 1822. He was of gentle birth, but understood and sympathised with the peasantry. He spoke the Gaelic language, and travelling on foot through the West Highlands, was able to get the people to tell him stories, which he accurately noted down. In his collection, therefore, we get the stories as nearly as possible in the words in which they were told. Then, among lesser writers in the same class, there are, Dougal Graham, the chap-book writer, who has been called the Scottish Rabelais; Robert Chambers, whose fame as a publisher has somewhat obscured his well-earned fame as an author; besides many others, some of them of merely local reputation.

Literature takes the life of tradition, and then embalms the dead body. To-day the old stories, which introduce the supernatural, have ceased to be believed or told. But, in their place, there is still to be found a body of genuine peasant-tales which do not tax credulity quite too far. And it is a fact worthy of attention that, though these stories may and do deal in horrors, yet they never descend to the merely "sensational"; being invariably raised by some touch of fancy, of character-painting, of the picturesque, into the region of poetic fiction.

In conclusion, what is there in these "old wives' tales" to justify their withdrawal, even for an hour, from the limbo of forgotten things? They have a place, though it be a very humble one, in the history of the workings of the human mind. They are the manifestation, in one of its simplest forms, of the literary or art impulse; and nothing that has been thus generated, and that has stood the test of time as these tales have stood it, can ever, I believe, be unworthy of our study. These simple stories were the outcome of faint stirrings in the human breast of two passions—the Love of Beauty, and the Thirst for Fame. "One touch of Nature makes the whole world kin"; and the lapse of centuries does not prevent our entering into the feelings of the peasant story-teller. Art is not only a thing of bound volumes and of exhibitions; and perhaps the Scottish peasant has shown as keen a sense of it—of the story-teller's art, at least—as his mental development and the conditions of his existence would admit. [G. D.]

GENERAL MONTHLY MEETING,

Monday, February 1, 1892.

Sir James Crichton-Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

Leopold Field, Esq. F.C.S.
James Macdonald Horsburgh, Esq. M.A.
Sir Philip Magnus,

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned for the following Donations:—

Sir Benjamin Baker, £50,
L. M. Rate, Esq. £50,
J. W. Swan, Esq. £21,
Wm. Anderson, Esq. £25,

for carrying on investigations on Liquid Oxygen.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz:—

FROM

Governor-General of India—Records, Vol. XXIV. Part 4. 8vo. 1891.
The Lords of the Admiralty—Nautical Almanac for 1895. 8vo. 1891.
The Secretary of State for India—The Tribes and Castes of Bengal. By H. H. Risley. Vols. I. II. Anthropometric Data. 8vo. Calcutta, 1891.
Abel, Sir Frederick, K.C.B. F.R.S. M.R.I. (the Author)—Mining Accidents and their Prevention. 8vo. New York, 1889.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta: Rendiconti. 2° Semestre, Vol. VII. Fasc. 9–12. 8vo. 1891.
Memorie, Vol. IX. 4to. 1891.
Atti, Anno 44, Sess. IV^{a}.–VI^{a}. 4to. 1891.
Agricultural Society of England, Royal—Journal, Vol. II. Part 4. 8vo. 1891.
American Geographical Society—Bulletin, Vol. XXIII. Nos. 1–3. 8vo. 1891.
Astronomical Society, Royal—Monthly Notices, Vol. LII. Nos. 1–3. 8vo. 1891.
Bankers, Institute of—Journal, Vol. XII. Part 9; Vol. XIII. Part 1. 8vo. 1891–92.
Basel, Naturforschenden Gesellschaft—Verhandlungen, Band IX. Heft 2. 8vo. 1891.
Birmingham and Midland Institute—Report for 1891. 8vo.
British Architects, Royal Institute of—Transactions, New Series, Vol VII. 4to. 1891.
Index to First Series of Transactions. 4to. 1891.
Proceedings, 1891–2, Nos. 5–7. 4to.
Buckton, George B. Esq. F.R.S. M.R.I. (the Author)—Monograph of the British Cicadæ or Tettigidæ, Part 8. 8vo. 1891.

Cambridge Philosophical Society—Proceedings, Vol. VII. Part 5. 8vo. 1892.
Transactions, Vol. XV. Part 2. 4to. 1891.
Canada, Geological and Natural History Survey of—Contributions to Canadian Micro-Palæontology, Part 3. 8vo. 1891.
Chemical Industry, Society of—Journal, Vol. X. Nos. 11, 12. 8vo. 1891.
Chemical Society—Journal for Dec. 1891 and Jan. 1892. 8vo.
Cracovie, l'Academie des Sciences—Bulletin, 1891, Nos. 8–10. 8vo.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I.—Journal of the Royal Microscopical Society, 1891, Part 6. 8vo.
Editors—American Journal of Science for Dec. 1891 and Jan. 1892. 8vo.
Analyst for Dec. 1891 and Jan. 1892. 8vo.
Athenæum for Dec. 1891 and Jan. 1892. 4to.
Brewers' Journal for Dec. 1891 and Jan. 1892. 4to.
Chemical News for Dec. 1891 and Jan. 1892. 4to.
Chemist and Druggist for Dec. 1891 and Jan. 1892. 8vo.
Electrical Engineer for Dec. 1891 and Jan. 1892. fol.
Electricity for Dec. 1891 and Jan. 1892. 8vo.
Engineer for Dec. 1891 and Jan. 1892. fol.
Engineering for Dec. 1891 and Jan. 1892. fol.
Engineering Review, Vol. I. Nos. 1–9. 8vo. 1891–92.
Horological Journal for Dec. 1891 and Jan. 1892. 8vo.
Industries for Dec. 1891 and Jan. 1892. fol.
Iron for Dec. 1891 and Jan. 1892. 4to.
Ironmongery for Dec. 1891 and Jan. 1892. 4to.
Monist for Dec. 1891 and Jan. 1892. 8vo.
Murray's Magazine for Dec. 1891. 8vo.
Nature for Dec. 1891 and Jan. 1892. 4to.
Open Court for Dec. 1891 and Jan. 1892. 4to.
Photographic News for Dec. 1891 and Jan. 1892. 8vo.
Revue Scientifique for Dec. 1891 and Jan. 1892. 4to.
Telegraphic Journal for Dec. 1891 and Jan. 1892. fol.
Zoophilist for Dec. 1891 and Jan. 1892. 4to.
Ex Libris Society—Journal for Jan.–Feb. 1892. 4to.
Fayrer, Sir Joseph, K.C.S.I. M.D. F.R.S. (the Author)—Presidential Address to Congress of Hygiene and Demography. 8vo. 1891.
Florence Biblioteca Nazionale Centrale—Bolletino, Nos. 143-146. 8vo. 1891.
Elenco delle Pubblicazioni Periodiche Italiane del 1891. 8vo.
Franklin Institute—Journal, Nos. 792, 793. 8vo. 1891–2.
Geographical Society, Royal—Proceedings, Vol. XIV. Nos. 1, 2. 8vo. 1891.
Geological Society—Quarterly Journal, No. 189. 8vo. 1891.
Glasgow Philosophical Society—Proceedings, Vol. XXII. 8vo. 1890–91.
Groth, Lorentz Albert, Esq. M.R.I. (the Author)—Application of Electricity to Tanning, as proved by Groth's system. 8vo. 1891.
Harlem, Société Hollandaise des Sciences—Archives Néerlandaises, Tome XXV. 3me–4me Livraison. 8vo. 1891.
Horticultural Society, Royal—Journal, Vol. XIII. Part 3. 8vo. 1891.
Hughes, Professor D. E. F.R.S. M.R.I. (the Author)—Electric and Magnetic Researches. 8vo. 1878–86.
Institute of Brewing—Transactions, Vol. V. No. 2. 8vo. 1891.
Jablonowski'sche Gesellschaft, Leipzig—Preisschriften, Mathematisch-naturwissenschaftlichen Section, No. XI. 4to. 1891.
Johns Hopkins University—University Circulars, No. 94. 4to. 1891.
American Chemical Journal, Vol. XIII. No. 8. 8vo. 1891.
Linnean Society—Journal, No. 196. 8vo. 1891.
Madrid Royal Academy of Sciences—Memorias, Tome XV. 4to. 1890-91.
Manchester Geological Society—Transactions, Vol. XXI. Part 12. 8vo. 1891.
McClean, Frank, Esq. M.A. M.R.I. (the Author)—Comparative Photographic Spectra of the Sun and the Metals. fol. 1891.

McKendrick, Professor J. G. M.D. LL.D. F.R.S. (the Author)—A Text Book of Physiology. 2 vols. 8vo. 1888-89.
Meteorological Office—Hourly Means, 1888. 8vo. 1891.
Ten Years' Sunshine in the British Isles, 1881-90. 8vo. 1891.
Harmonic Analysis of Hourly Observations. 4to. 1891.
Meteorological Society, Royal—Quarterly Journal, No. 80. 8vo. 1891.
Ministry of Public Works, Rome—Giornale del Genio Civile, 1891, Fasc. 10, 11, 8vo. And Designi. fol. 1891.
New South Wales Agent-General—Annual Report of the Department of Mines (N.S.W.) for 1890. fol. 1891.
North of England Institute of Mining and Mechanical Engineers—Transactions, Vol. XL. Part 4. 8vo. 1891.
Odontological Society of Great Britain—Transactions, Vol. XXIV. Nos. 2, 3. 8vo. 1891.
Payne, Wm. W. Esq. (the Publisher)—Astronomy and Astro-Physics for January 1892. Edited by W. Payne and Geo. Hale. 8vo.
Pennsylvania Geological Survey—Report, F. 3, 1888-89. 8vo. 1891.
Atlases, A.A. Parts 3, 4, 6. 8vo. 1889.
Pharmaceutical Society of Great Britain—Journal for Dec. 1891 and Jan. 1892. 8vo.
Photographic Society of Great Britain—Journal, Vol. XV. Nos. 5-9; Vol. XVI. No. 4. 8vo. 1891-2.
Physical Society of London—Proceedings, Vol. XI. Part 2. 8vo. 1891.
Richardson, B. W. M.D. F.R.S. M.R.I. (the Author)—The Asclepiad, Vol. VIII. No. 32. 8vo. 1891.
Rio de Janeiro, Observatoire Imperiale de—Revista, Nos. 10, 11. 8vo. 1891.
Royal Irish Academy—Proceedings, 3rd Series, Vol. II. No. 1. 8vo. 1891.
Transactions, Vol. XXIX. Part 17. 4to. 1891.
Royal Society of London—Catalogue of Scientific Papers, Vol. IX. ABA-GIS (1874-83). 4to. 1891.
Proceedings, Nos. 303, 304. 8vo. 1892.
Saxon Society of Sciences, Royal—Mathematische-Physischen Classe: Abhandlungen, Band XVIII. Nos. 1, 2. 4to. 1891.
Berichte, 1891, No. 3. 8vo. 1891.
Philologisch-Historischen Classe: Abhandlungen, Band XIII. No. 3. 8vo. 1891.
Scottish Society of Arts, Royal—Transactions, Vol. XIII. Part 1. 8vo. 1891.
Selborne Society—Nature Notes, Vol. III. No. 26. 8vo. 1891.
Society of Architects—Proceedings, Vol. IV. Nos. 3, 4, 5. 8vo. 1891.
Society of Arts—Journal for Dec. 1891 and Jan. 1892. 8vo.
Statistical Society, Royal—Journal, Vol. LIV. Part 4. 8vo. 1891.
St. Pétersbourg Académie Impériale des Sciences—Mémoires, Tome XXXVIII. Nos. 4-6. 4to. 1891.
Tacchini, Professor P. Hon. Mem. R.I. (the Author)—Memorie della Societá degli Spettroscopisti Italiani, Vol. XX. Disp. 10ª, 11ª. 4to. 1891.
United Service Institution, Royal—Journal, Nos. 167, 168. 8vo. 1891.
United States Department of Agriculture—Monthly Weather Review for September-October 1891. 4to.
United States Geological Survey—10th Annual Report, Parts 1, 2. 4to. 1890.
Veneto, L'Ateneo—Revista, Serie XIV. Vol. II. Fasc. 1-6; Serie XV. Vol. I. Fasc. 1-6. 8vo. 1890-91.
Vereins zur Beförderung des Gewerbfleises in Preussen—Verhandlungen, 1891: Heft 10. 4to. 1891-92.
Weber, Hermann, M.D. M.R.I.—Guide du Voyageur à Ephèse. Par Prof. G. Weber. 8vo. Smyrna, 1891.
Zurich Naturforschenden Gesellschaft—Veerteljahrschrift, Jahrang XXXVI. Heft 1, 2. 8vo. 1891.
Neujahrsblatt, XCIV. 4to. 1892.

SPECIAL GENERAL MEETING,

Monday, February 1, 1892.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

The following Address to Her Majesty the Queen, Patron, and His Royal Highness the Prince of Wales, Vice-Patron of the Royal Institution, in reference to the decease of His Royal Highness the Duke of Clarence and Avondale, Honorary Member of the Institution, was read and unanimously adopted:

The Members of the Royal Institution of Great Britain, in Special General Meeting here assembled, desire most respectfully to offer to Her Majesty the Queen (Patron of the Royal Institution) and the Prince of Wales (Vice-Patron of the Institution) the expression of their unfeigned sorrow at the loss which Her Majesty, His Royal Highness, and the Nation have sustained by the death of His Royal Highness the Duke of Clarence and Avondale (Honorary Member of the Institution), and they ask to be allowed to tender their heartfelt sympathy and condolence with Her Majesty and His Royal Highness the Prince of Wales in their bereavement.

Letters of regret for non-attendance were read from His Grace the President, the Earls of Derby and Ducie, Sir John Fowler, and many others.

EXTRA EVENING MEETING,

Thursday, February 4, 1892.

SIR FREDERICK BRAMWELL, Bart. D.C.L. F.R.S. Hon. Secretary and Vice-President, in the Chair.

NIKOLA TESLA, Esq.

Currents of High Potential and of High Frequency.

[Abstract Deferred.]

WEEKLY EVENING MEETING,

Friday, February 5, 1892.

Sir Frederick Abel, K.C.B. D.C.L. F.R.S. Vice-President, in the Chair.

Professor W. C. Roberts-Austen, C.B. F.R.S. *M.R.I.*

Metals at High Temperatures.

I propose this evening to consider, first, the methods of measuring high temperatures, and, second, to describe certain effects they produce on metals.

Geber, writing in the eighth century, gives directions for obtaining high temperatures, but points to the difficulties that arise in practice, "because fire is not a thing which can be measured, '*sed quoniam non est res ignis, quæ mensurari possit.*'" * It is not sufficient to attain temperatures that are not known to be high; it is necessary, for the purpose of modern investigation, to measure them with accuracy; and few of the early chemists in this country did more in affording a basis for the study of metals at high temperatures than Robert Boyle, the application of whose well-known law to solutions of metals in each other has been made evident by recent work. The 30th December last was the third centenary of his death; it is well, therefore, that this lecture should begin with a tribute to his memory. He suggested improvements in the ordinary mercurial thermometer, † constructed what would appear to be the first air thermometer with an index; and although he did not do much for thermometry at high temperatures, he appears to have been struck by what must have been a quaint device for regulating high temperatures, for he points out that "the great mechanic, Cornelius Drebel ‡ made an automatous musical instrument and a furnace which he could regulate to any degree of heat by means of the same instrument." He indicates various degrees of intensity of heat by reference to the colour of a glowing mass of fuel, and says that, § "tho' we vulgarly say in English, 'a thing is red hot,' to express a superlative degree of heat, yet, at the forges and furnaces of artificers, by a white heat they understand a further degree of ignition than by a red one." It is not a little strange that for three centuries after his death the same vague expressions have constantly been used in describing high temperatures.

---

* From the edition of his 'Summa Perfectionis Magisterii,' p. 28, published in Venice, 1542.

† Boyle's Works, Shaw's edition, vol. i. p. 575, 1738.

‡ Cornelius van Drebel, 1572–1634, Boyle, loc. cit. vol. iii. p. 38, 1738.

§ Loc. cit. vol. ii. p. 28.

A great step in advance was made in 1701 by Sir Isaac Newton,* who applied the law of cooling to the measurement of temperatures beyond the range of the mercurial thermometer, and in the notes which accompany his "Scala graduum caloris" he showed that he knew that the freezing-point of lead differs slightly from its melting-point.

Eighty years later, Josiah Wedgwood (1782),† aided by one of my predecessors, Mr. Alchorne, Assay Master of the Mint, determined a few melting-points of metals, and, in communicating a description of his "thermometer for measuring the higher degrees of heat" to the Royal Society, we find him, one thousand years after Geber had said that "fire cannot be measured," still lamenting the want of suitable instruments, saying: "How much it is to be wished that the authors (to whom he refers) had been able to convey to us a measure of the heat made use of in their valuable processes; . . . a red heat, a bright red, and a white heat are," Wedgwood adds, "indeterminate expressions, and even though the three stages are sufficiently distinct from each other, they are of too great latitude, and pass into each other by numerous gradations which can neither be expressed in words nor discriminated by the eye." Another ninety years brings us to the last time that the measurements of high temperatures formed the subject of a Friday evening discourse in this Institution. On March 1st, 1872, the late Sir William Siemens addressed you on the measurement of "heat by electricity"; ‡ and, speaking of the mercurial thermometer, said: "When we ascend the scale of intensity we soon approach a point at which mercury boils, and from that point upwards we are left without a reliable guide, and the result is that we find, in scientific books on chemical processes, statements to the effect that such and such a reaction takes place at a 'dull red,' such another at a 'bright red,' or a 'cherry red,' or a 'white heat,'—expressions which remind one," he adds, "of the days of alchemy rather than of chemical science at the present day."

It is not a little singular that the same lament should have been uttered, with so long an interval between, by two prominent technical men, and it suggests that but little experimental work had been done in the meantime with a view to the measurement of high temperatures. This is, however, far from being the case. A vast amount of work was done by physicists and metallurgists whose chief masters were "indefatigable labour, the closest inspection, and hands that were not afraid of the blackness of charcoal"; and their more noteworthy efforts were based on the employment of the air thermometer, in which the expansion of air replaces the expansion of the mercury in the ordinary thermometer, the bulb being of some fire-resisting material.§ For this purpose, Princep (1827) used a bulb of gold,

---

* 'Phil. Trans. Roy. Soc.' vol. xxii. p. 824. † Ibid. vol. lxxii. p. 305.

‡ 'Roy. Inst. Proc.' vol. vi. p. 438, 1872.

§ See the excellent bibliography given by C. Barus, 'Bull. Geological Survey, U.S.A.' No. 54, 1889.

Pouillet (1836) one of platinum, and, Deville and Troost, in a truly splendid series of investigations, adopted bulbs of porcelain, with iodine vapour as the elastic fluid. They ultimately reverted to the use of air.

You will remember that old mercurial thermometers had much information, supposed to be useful, engraven on their scales, and such statements as "water freezes," "water boils," "blood heat," "fever heat," "summer heat," were considered indispensable. It is by exposure to known temperatures that a thermoscope can be converted into a pyrometer for measuring intense heat; and the air or gas thermometer has, in the hands of Deville and Troost, rendered excellent service by enabling such gradations to be effected. The gas thermometer is not, in itself, a handy appliance, for it requires much subsidiary apparatus, and elaborate corrections of various kinds have to be introduced into the numerical data it affords; but it has given many fixed temperatures—such as melting-points and boiling-points of elements, and of compounds—which may safely be made use of in graduating pyrometers. For very high temperatures, 900° C. and over, we rely on the excellent work of M. Violle * on the specific heats of platinum, silver, gold, palladium, and iridium, which have enabled the melting-points of the respective metals to be calculated.

The determinations of temperatures between 300° and 1000°, which are now generally accepted, also rest upon data accumulated by the aid of the air thermometer, which has thus enabled the graduation to be effected of instruments widely differing from it, that can be trusted to give rapid and accurate indications in daily use. I can only bring before you two of the many kinds which have been devised; they are, however, by far the best that are available, and for the determination of temperatures up to the melting-point of platinum, leave little to be desired.

(1) A pyrometer which depends on the increase in the resistance of a heated conductor through which a divided electrical current is passing; and

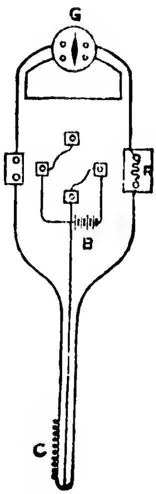
(2) One in which the strength of an electric current, generated by the heating of a thermo-junction, is used as a measure of the heat applied to the thermo-junction.

The principle of the electrical resistance pyrometer was indicated by Sir William Siemens ('Collected Papers,' vol. ii. 'Electricity,' p. 84, 1889) in a letter addressed to Dr. Tyndall, dated December 1860, and the nature of the instrument may be made clear by the accompanying diagram, Fig. 1. A divided current passes from the battery B, to a platinum wire, C, coiled round a clay cylinder, and to a resistance coil, R. At the ordinary temperature the resistance of the platinum coil is balanced by the standard resistance R. If, however, the platinum coil be heated, its resistance will be increased, and, this increase of resistance, which can be measured in various

* 'Comptes Rendus,' vol. lxxxix. p. 702, 1879; vol. xcii. p. 866, 1881.

ways, indicates the temperature of the coil, C. The coil itself may be adequately protected and exposed to temperatures which have been determined by the air thermometer; the deflection of a suitable (differential) galvanometer G, will then indicate temperatures directly. For instance, the temperature at which zinc boils has been accurately fixed at 940° C., and if the coil is heated in the vapour of boiling zinc, the angle through which the galvanometer mirror is deflected marks the temperature of 940° C.

FIG. 1.



The Report of a British Association Committee showed, in 1874, that the instrument is liable to changes of zero, but Mr. H. L. Callendar has recently (1887) restored confidence in the method which had been shaken by the Committee. He has proved that if sufficiently pure platinum wire be used, and if the wire be carefully annealed and protected from strain and contamination,* resistance pyrometers may be made practically free from changes of zero even when used at temperatures as high as 1000° C. He attributes the changes of zero to which the Siemens pyrometers are liable to the action on the wire of the clay cylinder on which it is wound, and of the iron tube in which it is inclosed. As the result of his experiments he has introduced certain modifications, which render the instrument not only trustworthy but very sensitive. He winds the platinum wire on a thin plate of mica, and incloses it in a doubly glazed tube of hard porcelain. He uses the zero method of measuring the resistance; but for these and other details of manipulation his own very interesting papers must be consulted. I will only add that I have had the pleasure of working with him in the Mint Laboratory, and I am satisfied that at temperatures about 1000° the *comparative* results afforded by his method are accurate to the tenth of a degree, a result which would certainly have been deemed impossible a year or two ago.†

---

* 'Phil. Trans. Roy. Soc.' vol. clxxviii. 1887, A, pp. 161–233, and vol. clxxxii. 1891, A, pp. 119–157; 'Phil. Mag.' vol. xxxii. July 1891, p. 104, and vol. xxxiii. Feb. 1892, p. 220.

† As this statement has been received with some surprise, it may be as well to state briefly how this degree of accuracy and sensitiveness is attained. The resistance-box is compensated for changes of temperature, and changes of resistance in the wires leading to the pyrometer are automatically eliminated. The resistance itself is measured by a modification of the well-known Carey-Foster method. The balancing resistance of the Wheatstone bridge employed, is composed partly of resistance coils and partly of a bridge-wire along which a contact key slides. The resistance of a centimetre of this wire is made to correspond to the increase of resistance of the pyrometer produced by a rise of 1° C. The galvanometer can easily be made sensitive to one-hundredth of a centimetre of this bridge-wire, so that one-tenth of a centimetre, which corresponds to one-tenth of a degree, can, of course, be measured with certainty.

The necessity for working with small volumes of fused metals, into which the tube of Callendar's pyrometer could not be plunged, has led me to prefer to adopt a method that would be classified under the second heading I have given. A very small thermo-junction may, in fact, be employed in such cases. The use of thermo-junctions for measuring high temperatures appears to have been suggested in 1826 by Becquerel, and adopted by Pouillet in 1836,* who advocates the use of iron in conjunction with platinum; but of all the varied combinations of metals and alloys which have been tried from time to time, that proposed by H. Le Chatelier possesses many advantages, on which I have elsewhere dwelt.† It consists of a platinum wire twisted at its end with a wire of platinum alloyed with 10 per cent. of rhodium. Such a couple may be used for some time without change of zero, and if the junction becomes injured it may be cut off, and the severed ends of the wires may be twisted together again. I am satisfied that it can afford comparative results which are accurate to 1° at temperatures of over 1000°. The diagrams given later (Figs. 4, 5, and 6) show the disposition of the apparatus. The spot of light indicating the deflections of the galvanometer needle is caused, for the illustrations of this lecture, to fall on to a graduated scale 45 feet long on the wall of the theatre. The thermo-junction has been calibrated with the aid of certain known temperatures, and the long scale is inscribed after the manner of the old thermometer scales, with certain fixed points, which are, of course, far higher than those it was possible to indicate by the expansion of mercury in a glass tube. (These fixed points were:—"water boils" (100°), "lead melts" (326°), "zinc boils" (940°), "gold melts" (1045°), "palladium melts" (1500°), "platinum melts" (1775°). On heating the thermo-junction to bright redness in a Bunsen flame, the spot of light moved rapidly to the point marked "zinc boils.") For laboratory experiments the scale is a short transparent one, rigidly fixed in relation to the galvanometer.

In leading up to the experiments which follow, in the course of which metals will be exposed to high temperatures, I would remind you that if an ordinary thermometer be plunged into water which is gradually losing its heat to a cold environment, the mercury will fall until the water begins to freeze, but directly this happens the mercury remains stationary until all the water is frozen; so that if the rate of fall be measured with a chronograph, there will be a steady fall to the freezing point of water, then a long arrest, followed by a renewed fall. If these readings be plotted, a well-known time-temperature curve will be obtained. Exactly the same effect is produced when a fluid metal "freezes," and before proceeding further it may be well to determine experimentally the freezing-

---

* 'Comptes Rendus,' vol. iii. p. 782, 1836.

† British Association Lecture, 'Nature,' vol. xli. 1889, pp. 11–32; Report Inst. Mech. Eng. Oct. 1891, p. 543.

point of gold. Beneath this little mass of pure gold, A (Fig. 2), a thermo-junction, B, is protected by a very thin layer of clay from the metal. The oxyhydrogen flame is made to play on the gold, there is a rapid movement of the spot of light over almost 25 feet of the scale, there is a diminution in the rate of rise near the point marked 1045°, the melting-point of gold, and then, when the metal becomes fluid, the temperature rapidly rises as more heat is given to the little mass. The source of heat is now removed, the temperature falls, there is an arrest just at 1045° C., the freezing-point of gold, and then the spot of light resumes its course as the gold cools down to the temperature of the room. The melting-point and freezing-point of palladium, 1500° C., were then shown in the same way. It should be observed, however, that when a small fragment of palladium is fused in the naked flame of the oxyhydrogen blow-pipe, hydrogen

FIG. 2.

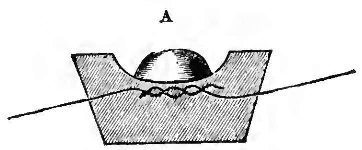
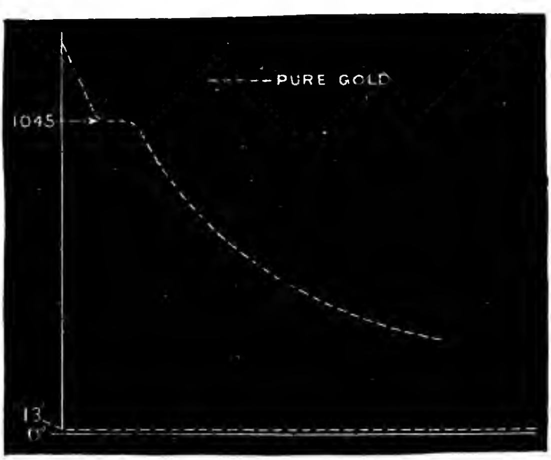


FIG. 3.



appears to be absorbed by the metal; and this absorption of gas lowers the freezing-point materially, and makes it far less steady than when a fresh piece of metal, cut from a large mass, is fused for the first time.

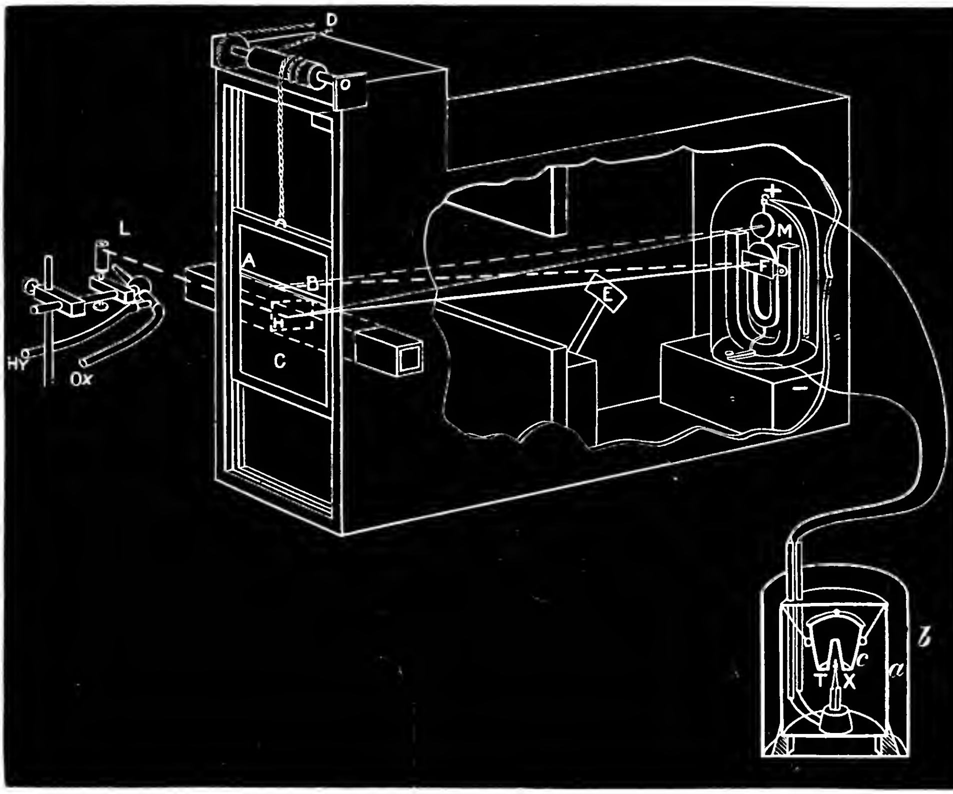
When the spot of light is allowed to fall on a sensitised plate in a suitable camera,* the time-temperature curve, of the cooling metal, traced on a moving plate will be of the form shown in Fig. 3.

It may be useful to show the method by which these autographic curves are obtained: the following diagram, Fig. 4, is therefore added.

* Proc. Roy. Soc. vol. xlix. 1891, p. 347.

The arrangement consists in inclosing a galvanometer of the Deprez and d'Arsonval type in a large camera; a fixed mirror F, being placed below the movable mirror M, of the galvanometer, so that the light from the lime cylinder L, reflected in the mirror H, passes to both mirrors, F and M, and is reflected in the direction of a fine horizontal slit A B, behind which a sensitised photographic plate C, is drawn vertically past the slit by means of gearing, D, driven by clock-work. The ray from the fixed mirror is interrupted periodically by the vane E,

FIG. 4.



and a beaded datum line is given, which enables any irregularity in the advance of the plate to be detected.

The amount of divergence from its datum line of the spot of light reflected by the movable mirror at any given moment bears a relation (which can readily be found by calibration) to the temperature to which the thermo-junction X, is heated, and the variations of temperature are recorded by a curve which is the resultant of the upward movement of the plate and the horizontal movement of the spot of light. A crucible *c*, which may be filled with molten metal, is provided with a tubulure, T, for the insertion of the thermo-junction. The crucible is suspended by wires in a double jacket of tin plate, *a b*.

It will have been evident that the thermo-junction of platinum and platinum-rhodium could not be used for measuring temperatures higher than the melting-point of the platinum of which it is made. Metals with higher fusion-points than platinum are, however, available; thus iridium will only just melt in the flame produced by the combustion of pure and dry hydrogen and oxygen. By the kindness of Mr. Edward Matthey, a thin rod of iridium has been prepared with much labour, and it can be used as a thermo-junction with a similar rod of iridium alloyed with 10 per cent. of platinum. The junction may be readily melted in the electric arc, and by this means a temperature may be registered which careful laboratory experiments show to be close to 2000°, and this agrees with the estimate of the melting-point of iridium which Violle * deduced from calorimetric experiments. [This experiment was shown, a different scale being employed for the screen, as the thermo-electric constants of the iridium, and iridium-platinum couple, are different from those of the platinum and rhodium one previously used.]

It is interesting to remember that within a year, in this Institution, temperatures ranging from − 200 to + 2000° have been mapped out, the lower temperature by Prof. Dewar in his memorable Faraday Lecture; the higher point is now measured in public for the first time.

How difficult it is for us to realise what this range of temperature really means! for we have but little power of appreciating temperatures beyond those we can conveniently bear. We, perhaps, know the meaning of extreme cold better than great heat, but even the vivid imagery of Dante, who might have been expected to afford some guidance, gives us singularly little help. I think in depicting the terror of torture inflicted by extreme cold he succeeds better than when he describes the suffering of those who are exposed to flames. His words (Canto xxxiii.)—

> "Blue, pinched, and shrined in ice the spirits stood"—

mark the highest suffering drawn in the "Inferno." It is, however, probable that my failure to appreciate the descriptive powers of Dante may be the result of resentment, for I read with regret that he consigns to the tenth chasm of Hell, not only the coiner who

> "falsified
> The metal with the Baptist's form impressed," †

but also an honest metallurgist, Cappoccio of Sienna, who,

> "by the power
> Of alchemy, . . . aped creative Nature by his subtle art";

and deserved a better fate.

We are now in a position to consider certain other effects of high temperature on metals. Many years ago, my colleague Mr. Lockyer, and I, conducted an investigation on the spectra of the vapours of certain metals ‡ at the highest temperatures we could

---

* Loc. cit. † The golden florin of Florence.
‡ Proc. Roy. Soc. vol. xxiii. p. 344, 1875.

produce, with the aid of the oxyhydrogen flame. We distilled silver, zinc, cadmium, and volatilised iron and other metals, from a lime crucible, and caused their vapours to pass into a horizontal tube of strongly-heated lime. By these experiments we satisfied ourselves that the molecular structure of metals is gradually simplified as higher temperatures are employed; and we came to the conclusion that each molecular simplification is marked by a distinctive spectrum, and that there is also an intimate connection between the facility with which the final stage is reached, the group to which the element belongs, and the place which it occupies in the solar atmosphere. At the highest temperature of the oxyhydrogen flame, molecules of metals are simplified, but their constituent atoms remain unchanged. Mr. Lockyer has, however, since done far more: he has shown that the intense heat of the sun carries the process of molecular simplification much further; and, if we compare the complicated spectra of the vapours of metals produced by the highest temperatures available here with the very simple spectra of the same metals as they exist in the hottest part of the sun's atmosphere, it is difficult to resist the conclusion that the atom of the chemist has itself been changed. My own belief is that these "atoms" are changed, and that iron, as it exists in the sun, is not the vapour of iron as we know it upon earth. We will not dwell in this lecture on the effects of very high temperatures on metals, but rather on the influence of comparatively low temperatures—that is, below whiteness—in changing the number and arrangement of the atoms in metallic molecules. A profound change must occur when the viscous form of sulphur passes spontaneously at the ordinary temperature into the yellow crystalline variety, but the change is accompanied by but little thermal disturbance. In the case of metals there is also abundant evidence that molecular change may take place at low temperatures. Take the fusible alloy of bismuth, lead, and tin, which bears Newton's name, and contains—

Bismuth	50·00
Lead	31·25
Tin	18·75
	100·00

It fuses at 90°; it may be cast round a thermo-junction, and plunged in water and cooled thoroughly until the observer is certain that the mass has returned to the atmospheric temperature; take it out of the water, dry it rapidly, and in a few moments it will become too hot to hold. The "fracture" of the metal is totally different before and after the molecular change which is the cause of this evolution of heat has taken place. The change, moreover, takes place in the solid metal, and is not due to the release of the latent heat of fusion. The mass, solid as it appears to be, must be the scene of an internal struggle between the molecules in the effort to attain a state of equilibrium, and this conflict is but a type of the action that takes place in many metals and alloys which are of vast industrial importance.

Time will only permit me to deal with three cases of the action of high temperatures on atoms and molecules of metals. In the first case, the arrangement of the atoms in the molecule of a metal, iron, is disturbed, and the result is of great industrial importance. In the second case, the atoms of a metal, gold, appear to combine with those of another metal; and the result, while it is mainly of interest in connection with the history of science, has nevertheless an important bearing upon art. The third case relates to the molecular bombardment which takes place when a small quantity of metal is dissolved in a mass of metallic solvent, and is of interest in connection with modern views both as to osmotic pressure and solution generally.

(1) The pyrometric couple is inserted in the centre of a little mass of steel, which is being slowly raised to a bright red heat; when the flame is withdrawn, the spot of light will turn towards the zero end of the scale, falling slowly until a temperature of 655° is reached, and then there will be an abrupt and prolonged arrest. The metal has never been near its melting-point, and the evolution of heat must be due to a molecular change in the solid metal. In the case of this particular sample of steel, the evolution of heat is mainly the result of a change in the relation between the carbon and the iron; but by laboratory experiments and careful chronographic records, Osmond has shown that, in the case of certain varieties of steel, it can be demonstrated that what here appears as a single change, attended by an evolution of heat, is really an exceedingly complex one. I have shown that it occurs in the purest iron the chemists can prepare by electrolysis, and I agree with Osmond in believing that the change which occurs in pure iron at 855° is a molecular one, independent of the presence of impurity. If the mass of steel (Fig. 5, *a*) be heated again and allowed to cool, you will observe that the point of "recalescence" appears to be that at which the iron regains its magnetic property;* for a magnetised needle, *b*, is attracted at the moment the arrest of the spot of light on the pyrometer scale marks the temperature at which the change occurs, and at that precise moment a second spot of light, from a mirror mounted on the magnetic needle, will rapidly move away from its zero. I have elsewhere† dwelt on the importance of the molecular change in iron and steel, and can now only summarise the significant facts.

It is unnecessary to point to the extreme industrial importance

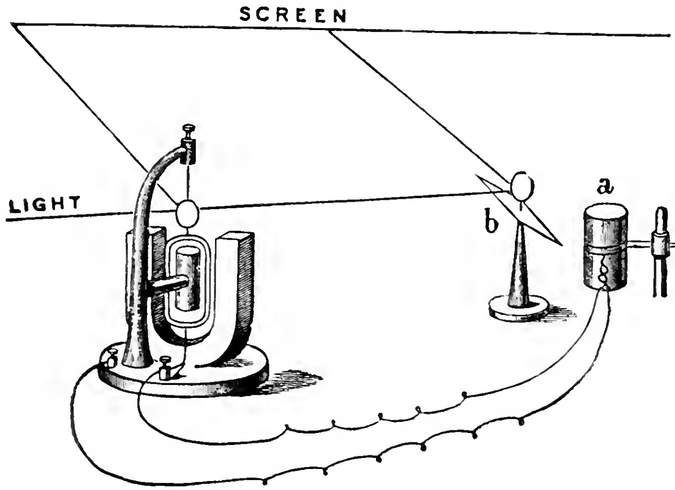
* The temperature at which these molecular changes take place in iron and steel was first demonstrated to an audience in my Newcastle lecture, 1889; but my friend Prof. Reinold, of the Royal Naval College, first arranged an experiment for lecture purposes, which showed the magnetic change simultaneously wih the thermal one.

† Report to the Institution of Mechanical Engineers, 'Proceedings,' 1891, p. 543.

of the property steel possesses, of becoming hard when it is quenched from redness in a fluid which will abstract its heat with more or less rapidity.

The changes which take place at 855° and 650° have to be arrested, as it were, by rapidly cooling the mass of steel; and if this is done, the steel will be more or less hard according to the rapidity

Fig. 5.

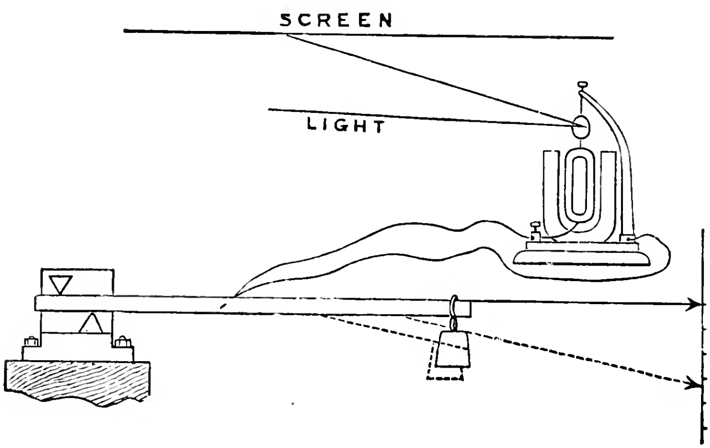


with which the progress of the molecular change has been stopped. It is, however, useless to attempt to harden steel if the temperature of the mass has fallen below 650°. In "oil hardening" or cooling a large mass of steel, like the "A" tube of a gun, which may be 30 feet long, great care should be taken to insure that the temperature of the mass is as uniform as possible; for, if part of the mass is hotter than 650°, while part is colder, the oil will really be cooling a mass of steel which is itself passing through various stages of complex molecular change, and the operation of "hardening" arrests, as it were, the atoms in the midst of a conflict incidental to their attempt to group themselves into one or other of the molecular modifications of iron. By cooling a mass of steel which is not at uniform temperature, stresses of great complexity and intensity are set up, stresses that may greatly reduce the effective strength of the gun.* The result is told in failures, by which many lives have been sacrificed; but I need hardly say that the Director-General of Ordnance is fully sensible of the national importance of studying the behaviour of iron and steel at high temperatures, and, at Dr. Anderson's suggestion, the Institution of Mechanical Engineers appointed a Committee, and have intrusted me with a large portion of the inquiry.

* 'Internal Stresses in Cast Iron and Steel,' by Nicholas Kalakoutsky, 1888.

In the next experiment, Fig. 6, a bar of steel, $\frac{1}{2}$ inch in section and 18 inches long, was heated to bright redness and fixed firmly at one end; a weight of about two pounds is rapidly hung to the free end, a light pointer is added to magnify the motion of the bar, and the thermo-junction is rapidly introduced into a small hole drilled in what is arranged to be the hottest part of the bar. The bar is not softest at a red heat; it remains perfectly rigid until it has cooled down to dull redness, and the temperature, as measured by the spot of light from the galvanometer, shows that "recalescence" has occurred. At that moment of molecular weakness in the bar, the weight has power to bend it, and the pointer falls. By such

FIG. 6.



experiments the exact temperature at which the metal becomes weak, in different varieties of steel, can readily be determined.

(2) Evidence will now be given in support of the second case it was proposed to treat, and it will be shown that at high temperatures the atoms of metals may truly combine with each other; in fact, taking gold as a basis for the experiments, compounds may be formed which would, had they been known centuries ago, have strangely affected the history of science. When the alchemists subjected the metals to high temperatures, their efforts were mainly directed to the discovery of some substance that would either change base metals to the colour of gold, or would give them the brilliancy of silver. The mediæval chemists believed that there were two distinct substances that would effect this, "one for the white" and another "for the red." Many of their writings might be quoted in support of this view, but a reference to Geber, who wrote in the eighth century, will be sufficient. He pointed out that the transmuting agent "has a tincture of itself so clear and splendid, white or red, clean and incombustible, stable and fixed, that fire cannot prevail against it; . . . and a property of the medicine is to give a splendid colour, white or intensely citrine," to metals to which it is added.

That was the effect expected from the transmuting agent, but do not think that the attempt to produce gold arose entirely from the love of gain. The colour of gold and purple impressed men strangely, and the search for the transmuting agent was most eagerly pursued in times when people lived for art, in a dream of colour. The effort to find the secret of the tint of gold is due to the same impulse which made the French in the thirteenth century manifest a keen "sensitiveness to luminous splendour and intensity of hue," so that, as Sir Frederic Leighton tells us, "a stained glass window, by Cousin, was limpid with hues of amethyst, sapphire, and topaz, and fair as a May morning." The chemists were able to stain glass ruby and purple with gold: why should they not impart the same glories to metals? I could not hope to interest you in what follows, did I not call artists to my aid; and many will remember the glowing words Mr. Ruskin uses,* calling purple a "liquid prism and stream of opal," reminding us of the crimson and purple of the poppy, the scarlet and orange of fire and the dawn. No wonder he chides us with turning the lamp of Athena into the safety-lamp of the miner, and with getting our purple from coal instead of, as of old, from the murex of the sea; "and thus grotesquely," he says, "we have had forced on us the doubt that held the old world between blackness and fire, and have completed the shadow and the fear of it by giving to a degraded form of modern purple a name from battle—'Magenta.'"

You will remember that Faraday showed that gold, when finely divided, is brilliantly coloured scarlet and purple. Here is a solution of chloride of gold. Add a little dissolved phosphorus, and the gold is precipitated in an extremely fine state of division, which tinges the solution crimson, but if you try to remove this suspended gold you will only gain a brownish mud. However, I will give you the secret by which any one who possesses a blowpipe, a bead of gold, and a fragment of one of the most widely diffused metals, aluminium, may stain gold purple through and through. But if you add aluminium to molten gold, you obtain many things, as this coloured diagram and series of specimens show. [This diagram cannot be reproduced without colour.]

The series of specimens showed that as the proportion of aluminium is increased, the golden colour of the precious metal is lessened, and when an alloy is formed with about 10 per cent. of aluminium, the fractured surface of the mass is brilliantly white: from this point forwards, as aluminium is added, the tint deepens, until flecks of pink appear, and when seventy-eight parts of gold are added to twenty-two of aluminium a splendid purple is obtained, in which intensely ruby-coloured opaque crystals may readily be recognised. Then, as the quantity of aluminium is still further increased, the alloys lose their colour, and pass to the dull grey hue of the aluminium itself. Per-

* 'The Queen of the Air,' ed. 1887, p. 129; 'Times,' December 11, 1891.

haps the most remarkable point about the purple alloy is its melting-point, which I have shown to be some degrees higher than that of gold itself.* See diagram, Fig. 7, in which curves of several constants of these alloys are given. This fact affords strong evidence that the alloy $AuAl_2$ is a true compound, having analogies to the sulphides, for in every other series of alloys the melting-points of all the members of the series are lower than that of the least feasible constituent. There is one other fact of much interest connected with this alloy. When it is treated with dilute hydrochloric acid, chloride of aluminium is formed, and gold is released in a singularly voluminous form. The heat of formation of the gold-aluminium alloy has not been determined, but hydrochloric acid, which will not attack gold, will readily split up this compound, of which more than three-fourths is gold; the compound, in fact, behaves like a distinct metal, having special heats of oxidation and chlorination of its own.

(3) Lastly, we come to the question of solutions of metals in each other. One very remarkable instance of the behaviour of metals at high temperatures reveals the fact that the presence of a small amount of metal in a mass of another lowers the freezing-point of the mass. In the industrial world this has long been known. Cellini tells us, for instance, that when the bronze for his great figure of Perseus, at Florence, was running out of the furnace, it suddenly showed signs of setting, and he therefore threw pewter plates and dishes into the ducts through which the metal had to pass—"a thing," he says, "never before done." The fluidity of the metal was immediately increased, and he found every part of the casting "to turn out to admiration."

The excellent work of Heycock and Neville,† on the lowering of the freezing-points of metals by the addition of other metals should, I would suggest, form the subject of a lecture in this Institution at an early day. I cannot attempt to deal with the matter here. In leading up to these questions of solution, as applied to metals, I would remind you that Lord Rayleigh told us a few evenings since that it was by no means certain that a gas rushing into a vacuous globe ever completely fills it, as there may still be tiny spaces into which "odd molecules" fail to find room to vibrate in. If it is difficult for a gas to entirely fill a vacuous space, you would think it impossible for a small quantity of a metal to rapidly permeate a fluid mass of another metal; nevertheless, so far as analysis can detect, this does happen.

It may be incidentally observed that the relations of the ordinary gases to metals are far more intimate than they were formerly supposed to be, and this was proved by Graham's work on the absorption of gases by metals, which has often been dealt with in this Institution. To take only the case of iron, more than twenty years ago

* Proc. Roy. Soc. vol. i. 1891, p. 367.

† Chem. Soc. Journ. vol. lv. 1889, p. 666; vol. lvii. 1890, pp. 376, 656; vol. lix. 1891, p. 936; vol. lxi. 1892, p. 888.

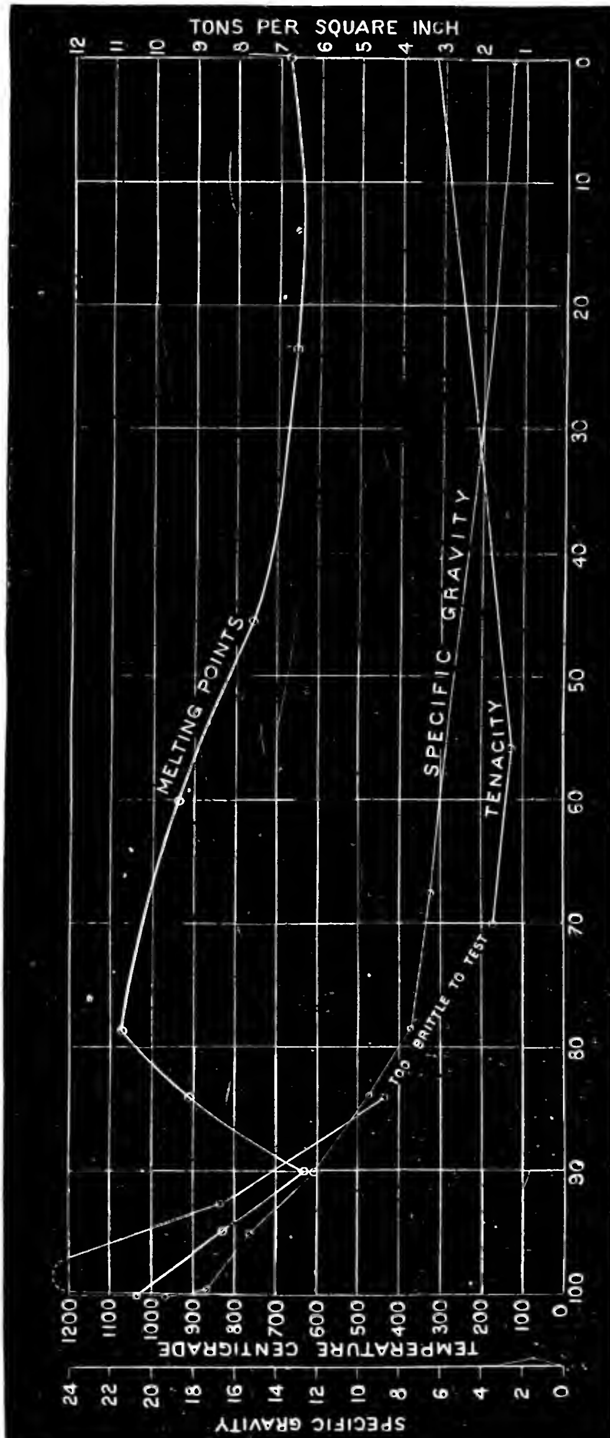

TONS PER SQUARE INCH
MELTING POINTS
SPECIFIC GRAVITY
TENACITY
TOO BRITTLE TO TEST
TEMPERATURE CENTIGRADE
SPECIFIC GRAVITY

FIG. 7.

Sir Lowthian Bell showed that the relations between carbonic oxide and iron are of singular interest. Ludwig Mond, Quincke and Langer have since found and isolated most interesting compounds of iron and carbonic oxide.* But to return to the solution of metals in metals.

The method of taking autographic curves of the cooling of masses of metal has already been indicated in Fig. 4,† and they ought to enable much information to be gained as to what is taking place throughout the mass. Such curves should render it possible to ascertain which of the rival theories as to the nature of solution, as applied to salts, is supported by the behaviour of a metal dissolved in a metal. When, for instance, a little aluminium dissolves in gold, is the analogue of a hydride formed, and, if so, is the curve of freezing-points of a series of aluminium-gold alloys a continuous one? On the other hand, does the theory advocated by Van't Hoff, Arrhenius, and Ostwald gain support, and do the molecules of the dissolved metals act independently of the solvent—that is, does osmotic pressure come into play? It will be remembered that the law which regulates osmotic pressure has exactly the same form as Boyle's law—that is, the pressure is proportional to the density of the gas or of the solution. Is the view of Arrhenius correct—that, if a solution be very dilute, the molecules of the dissolved substance are dissociated, act independently of each other, and behave like a perfect gas?

It will require years of patient work before these questions can be answered; but it appears certain, from the admirable experiments of Heycock and Neville,‡ to which reference has already been made, that, taking metals with low melting-points (such as tin or lead) as solvents, the lowering of the freezing-point of the solvent is really due to the bombardment exerted by the molecules of the dissolved metals.

I have extended this investigation by employing as a solvent a mass of fluid gold, which has a high melting-point, and is not liable to oxidation, and the results confirm those obtained by Heycock and Neville.

There is yet one other question: When metals are added in small quantities to a metallic mass, may the solvent remain inert? Here is a mass of 1000 grammes of lead, and to it 15 grammes of gold, or 1·6 atoms for every 100 atoms of lead will now be added. It could be shown that the gold is readily dissolved, and remains dissolved, even if the lead be solidified. Now, to the fluid lead sufficient aluminium will be added to form the purple alloy with the dissolved gold; the mass will be well stirred, but the aluminium will not unite with the lead; it will nevertheless find out the gold, and, after uniting with it, will carry it to the surface of the bath. Thence it can be removed, and the purple colour of the alloy identified, or the gold it contains

---

* Chem. Soc. Journ. vol. lix. 1891, pp. 604, 1090.

† Proc. Roy. Soc. vol. xlix. p. 347, 1891.

‡ Loc. cit.

can be revealed by the method Prof. Hartley * has given us for detecting the presence of gold in an alloy by volatilising the alloy in a torrent of sparks from an induction coil, and condensing the vapour on mica.

The union of the aluminium and the gold must, however, be peculiar. Crookes† has shown that when this alloy is used as an electrode in a vacuum tube, the gold is volatilised from the alloy and deposited as a film on the glass, leaving the aluminium behind.

The purple alloy presents us with the most interesting case yet known of a molecule built up of purely metallic atoms, but we are certain that the atoms are still those of gold and aluminium—that is, the atoms of the united metals remain unchanged. The interest in this substance is deepened if it be remembered that our aim at the present day is the same as that of the alchemists, for we are striving, as they did, to attack and change the chemist's atoms themselves. We seek, as truly as they, to effect the transmutations, which, as Boyle said, would "be none the less real for not being gainful," and employ high temperatures in the hope of simplifying the molecular structure of metals. We no longer consider gold to be the "sum of perfection," but still retain the belief expressed by Geber, eleven hundred years ago, that, "if we would change metals, we must needs use excess of heat." A poet also appears to have felt this, for George Herbert writes in the seventeenth century—

> "I know . . . what the stars conspire,
> What willing Nature speaks, what forced by fire";

thus comparing the ordinary response of nature to the investigator, with the evidence he elicits from her by heat.

By fusing gold, and staining it "the purple of the dawn," a new interest has been given to the metal which the alchemists always connected with the sun; and for further proof that metallic atoms may be changed, we must turn to the sun itself, as to the great metallurgical centre, where "all the elements shall melt with fervent heat."

* Proc. Roy. Soc. vol. xlvi. 1889, p. 88.
† Ibid. vol. l. 1891, p. 88.

[W. C. R.-A.]

WEEKLY EVENING MEETING,

Friday, February 12, 1892.

BASIL WOODD SMITH, Esq. F.R.A.S. F.S.A. Vice-President, in the Chair.

G. J. SYMONS, Esq. F.R.S. Sec. R.M.S.

Rain, Snow, and Hail.

[No Abstract.]

WEEKLY EVENING MEETING,

Friday, February 19, 1892.

Sir Dyce Duckworth, M.D. LL.D. F.R.C.P. Vice-President, in the Chair.

Professor Percy F. Frankland, Ph.D. B.Sc. F.R.S.

Micro-organisms in their Relation to Chemical Change.

Almost exactly on this day twenty-two years ago the subject of micro-organisms was introduced to the audience of the Royal Institution in one of those charming discourses, which so many of us well know were always to be heard from Dr. Tyndall. The title of his discourse on that occasion was "Dust and Disease," and its contents should be studied by all interested in this departure of science, forming, as it does, a part of the classical literature of the subject in which it marks the commencement of a new epoch.

It has probably rarely, if ever happened before, that in so short a period as twenty-two years any science has undergone such a marvellous advance, such a many-sided development, as that which has taken place in the case of bacteriology, the science which is devoted to the study of those low forms of life which we group together under the name of *micro-organisms*. This advance has been made through the ungrudging expenditure of self-denying labour by a great body of earnest workers of nearly every nationality. The subject is indeed one calculated to draw forth interest and enthusiasm, for the problems involved are not only of high scientific importance, but are also of incalculable moment to mankind and indeed to the entire living creation.

The great impetus which this new science received at its outset was imparted by Pasteur, who has not only laid the foundations, but has also added, and is still adding, so much to the superstructure of its many mansions.

The side of bacteriology with which the general public is most commonly brought in contact is that which relates to disease, but of this I propose saying absolutely nothing to-night. It has been dealt with by others in this place, and notably by my friend Dr. Klein.

There is a second side of bacteriology which has also a special interest for at least a portion of the public in consequence of the invaluable assistance which it has afforded to some sections of the industrial world. Indeed, chronologically, this industrial department of bacteriology was the first which claimed attention, for the growers of wine, the brewers of beer, and the manufacturers of fermented liquors of all kinds from the highest antiquity have been practical bacteriologists, of the same spontaneous order, it is true, as M. Jourdain was an unconscious author of prose. It was Pasteur also who first infused science into the operations of the wine-vat and the fermenting-tun, by his classical 'Études sur la Bière et sur le Vin.' It was he who first showed that the normal work of the brewery was accomplished by particular forms of micro-organisms, known as yeast, and that the frequent failures to produce beer or wine of the desired quality were occasioned by the presence of foreign forms of micro-organisms giving rise to acidity and other undesirable changes in these beverages.

In these researches of Pasteur's on beer and wine, we are almost for the first time brought face to face with the precise nature of some of the chemical changes which micro-organisms bring about. The time-honoured vinous fermentation of sugar, the products of which had been valued and indulged in by man even from the days of Noah, is for the first time so accurately studied as to be definable almost with the precision of a chemical equation.

Similar attention was also given by Pasteur to some of the other micro-organisms which deteriorate the quality of the beer, thus more especially to the bacterium which causes the *acetic* or vinegar fermentation, which is a process of *oxidation*, transforming alcohol into vinegar; to the bacillus inducing the *lactic* fermentation, which is a process of *decomposition*, in which sugar yields lactic acid; as well as to that which brings about the *butyric* fermentation, a process of *reduction* in which butyric acid is formed.

These are the foundations and scaffolding on which subsequent investigators of the phenomena of fermentation have laboured. Thus making use of more refined methods than those which were at the disposal of Pasteur, Christian Hansen, of Copenhagen, has enormously extended our knowledge of the alcohol-producing organisms or yeasts; he has shown that there are a number of distinct forms, differing indeed but little amongst themselves in shape, but possessing very distinct properties, more especially in respect of the nature of certain minute quantities of secondary products to which they give rise, and which are highly important as giving particular characters to the beers produced. Hansen has shown how these various kinds of yeast may be grown or cultivated in a state of purity even on the industrial scale, and has in this manner revolutionised the practice of brewing on the Continent. For during the past few years these pure yeasts, each endowed with particular properties, have been grown with scrupulous care in laboratories equipped expressly for this purpose, and these pure growths are thence despatched to breweries in all parts of the world, particular yeasts being provided for the production of particular varieties of beer. In this manner scientific accuracy and the certainty of success are introduced into an industry in which before much was a matter of chance, and in which nearly everything was subordinated to tradition and blind empiricism.

The Bacteria connected with the Soil.

It is, however, with regard to the bacteria connected with other industries than those of alcoholic fermentation that our knowledge has particularly advanced during the last few years. Thus some of the most important phenomena in agriculture have recently received a most remarkable elucidation through the study of bacteria.

Scientific agriculturists are generally agreed that one of the most important plant-foods in the soil is *nitric acid*, indeed they inform us that if a soil were utterly destitute of this material it would be incapable of growing the barest pretence of a crop *either of corn, or of roots, or of grass*, even if the soil were in other respects of the most superb texture, however favourably it might be situated, however well drained, tilled, and supplied with the purely mineral ingredients of plant-food, such as *potash*, *lime*, and *phosphoric acid.*

Yet, notwithstanding the commanding importance of this substance nitric acid to vegetation, it is present in ordinary fertile soils in but little more than homœopathic doses.

These facts are gathered from those grand experiments which have during the past half century been going on at Rothamsted under the direction of Sir John Lawes and Dr. Gilbert, and which have rendered the Hertfordshire farm a luminous centre of the whole agricultural world.

From these experiments it appears that sometimes there is in fertile soil under 1 part, and often under 10 parts, of nitrate of lime per million of soil.

Indeed, in order to detect and estimate these minute quantities, the most refined methods of chemical analysis have to be called into requisition. [Demonstration of the presence of nitric acid in soil by diphenylamine test.]

Now the cause of such minute quantities only of nitric acid being found in soils is due partly to this material being washed away by the rain and partly to its being so eagerly taken up by plants for the purposes of nutrition; for it has long been known that by suitable means the quantity can be enormously increased if no vegetation is maintained, and the ground properly protected from rain. The soil in fact, under ordinary circumstances, continuously generates this nitric acid from the various nitrogenous manures which are applied to it, and it is in the form of nitric acid that the nitrogen of manures principally gains access as nutriment to the plant.

It was in the year 1877 that two French chemists, Schloesing and Müntz, showed that this power of soils to convert the nitrogen of nitrogenous substances into nitric acid was due to low forms of life—to micro-organisms or bacteria. The proof which they furnished of this statement was of a very simple character, and consisted essentially in demonstrating that this production of nitric acid, or process of *nitrification*, as it is generally called, is promptly inhibited or brought to a standstill by all those materials which have the property of destroying micro-organisms, and which we call *antiseptics*, whilst similarly the process is stopped by heat and other influences, which are known to be fatal to life in general.

These results of Schloesing and Müntz were confirmed and greatly extended in this country by Mr. Warington and Dr. Munro, but although the vital nature of the process was fully established, little practical advance was until recently made in the identification or isolation of the particular bacteria responsible for this remarkable and invaluable transformation.

In 1886, however, a very important step was made by Dr. Munro, who showed that this process of nitrification could take place in solutions practically destitute of organic matter, or, in other words, that the vital activity of the bacteria of nitrification could be maintained without nutriment of an organic nature.

In 1885, I had myself already established the fact that some micro-organisms can actually undergo enormous multiplication in ordinary distilled water:—

MULTIPLICATION OF MICRO-ORGANISMS IN DISTILLED WATER.*

Hours after Introduction of Micro-organisms.	Number of Micro-organisms found in 1 c.c. of Water.
0	1,073
6	6,028
24	7,262
48	48,100

In taking up the subject of nitrification in conjunction with my wife in the autumn of 1886, I determined to avail myself of this remarkable property of the nitrifying organisms to grow in the absence of organic matter, thinking that in this way it would be possible to achieve a separation of the nitrifying organisms from other forms which can only grow if organic food materials are supplied to them.

Proceeding on these lines, we have carried on the process of nitrification over a period of upwards of four years without the nitrifying organisms being supplied with any organic food materials whatsoever:—

COMPOSITION OF SOLUTION EMPLOYED FOR NITRIFICATION.

Ammonium chloride	·5 grm.	In 1000 c.c. of distilled water.
Potassium phosphate	·1 ,,	
Magnesium sulphate	·02 ,,	
Calcium chloride	·01 ,,	
Calcium carbonate	5·0 ,,	

In a solution of this composition the process of nitrification was carried on over a period of upwards of four years, as indicated in the following table:—

* Proc. Roy. Soc., 1885.

EXPERIMENTS ON CONTINUOUS NITRIFICATION IN MINERAL SOLUTIONS.

Generation.	Date of Inoculation.	Quantity taken for Inoculation.	Date when Nitrification first observed.
I.	9 5 1887	Original garden soil	20 5 1887
II.	25 6 1887	3 needle-loops from I.	30 6 1887
III.	1 7 1887	" II.	7 7 1887
IV.	14 7 1887	" III.	23 7 1887
V.	25 7 1887	" IV.	17 8 1887
VI.	26 8 1887	" V.	1 10 1887
VII.	3 10 1887	1 needle-loop from VI.	7 10 1887
VIII.	7 10 1887	1 needle-point from VII.	17 10 1887
IX.	17 10 1887	" VIII.	29 10 1887
X.	7 11 1887	" IX.	30 11 1887
XI.	1 12 1887	" X.	15 12 1887
XII.	16 12 1887	" XI.	13 1 1888
XIII.	28 1 1888	" XII.	20 2 1888
XIV.	29 2 1888	" XIII.	5 4 1888
XV.	7 4 1888	" XIV.	27 4 1888
XVI.	30 4 1888	" XV.	10 5 1888
XVII.	12 5 1888	" XVI.	26 5 1888
XVIII.	19 7 1888	" XVII.	3 9 1888
XIX.	3 9 1888	" XVIII.	1 10 1888
XX.	11 10 1888	" XIX.	20 11 1888
XXI.	24 11 1888	" XX.	26 2 1889
XXII.	26 2 1889	" XXI.	4 5 1889
XXIII.	28 6 1889	" XXII.	18 10 1889
XXIV.	4 11 1889	" XXIII.	17 12 1889
XXV.	27 12 1889	" XXIV.	25 4 1890
XXVI.	16 5 1890	" XXV.	2 7 1890
XXVII.	15 7 1890	" XXVI.	30 1 1891
XXVIII.	3 3 1891	" XXVII.	28 5 1891

In carrying on this series of experiments it was soon evident that although a number of forms foreign to the nitrification-process were being eliminated, there were still some remaining alongside of the nitrifying organisms, or, in other words, that a pure culture of the nitrifying organisms had not been obtained. From various considerations, however, we came to the conclusion that the nitrifying organisms probably differed from the other forms which were still present along with them in being unable to grow on the common cultivating medium employed by bacteriologists, and known as gelatin-peptone.

The separation from these foreign forms was ultimately effected by enormously diluting one of these nitrifying solutions, and then taking out small portions of this diluted material and introducing each of these portions into separate ammoniacal solutions. In some of these nitrification was established, in others not, whilst amongst those in which nitrification *was* established, some contained organisms which grew upon gelatin, whilst one refused to give any growth on the gelatin at all, although it was seen under the microscope to contain abundantly bacteria of the form shown in the diagram. [Lantern-slide of Nitrifying Bacillococcus (Frankland).]

These results, which were published in March 1890, were followed in about a month by a communication in the 'Annales de l'Institut Pasteur,' by M. Winogradsky, who had also separated a very similar, if not identical, nitrifying organism, and a few months later, again a similar separation was made by Mr. Warington.

But these discoveries had not completely unravelled the problem of nitrification, for the organisms separated in these three independent investigations possessed only the property of *converting ammonia into nitrous and not into nitric acid.* The nitrous acid is an intermediate body which curiously is rarely found excepting in very minute quantities in soil. The changes will be more clearly understood by reference to the chemical equations:—

$$(1)\quad \underset{\text{(Ammonia)}}{NH_3} + \underset{\text{(Oxygen)}}{3O} = \underset{\text{(Water)}}{H_2O} + \underset{\text{(Nitrous Acid)}}{NHO_2}$$

$$(2)\quad \underset{\text{(Nitrous Acid)}}{NHO_2} + O = \underset{\text{(Nitric Acid)}}{NHO_3}$$

The organisms separated by Winogradsky, by Warington, and by myself, possessed only the property of effecting the first of these changes, they were absolutely destitute of the power of bringing about the second.

Now the curious thing is that the first of these changes is by far the most difficult to accomplish by *purely chemical means*, whilst the second can be brought about with the greatest facility. [Demonstration of addition of acid permanganate to solution of ammonium sulphate, colour not discharged.] [Demonstration of addition of acid permanganate to solution of potassium nitrite, colour discharged.]

Thus the potassium permanganate has no action on the ammonia, whilst the nitrite it oxidises to nitrate.

In order to bring about the first change, we have to employ one of the most powerful oxidising agents known to chemists, viz. ozone. [Demonstration: ozone from a Siemens-tube was passed through strong solution of ammonia; the production of nitrous and nitric acids was exhibited by the formation of white fumes, as well as by the sulphanilic acid and diphenylamine tests.]

We thus see that the power of oxidation possessed by our nitrifying organism is altogether unique, and does not find its parallel amongst purely chemical agents of oxidation. *But how then is the nitric acid found in the soil produced, when these organisms yield only nitrous acid?*

At the time when I found that the organism which I had separated produced nitrous acid exclusively, I pointed out that it was doubtless explicable on one of two hypotheses: (1) that nitrous and nitric acids are produced by totally distinct organisms; or (2) that the same organism produces the one or the other according to the conditions under which it is growing.

More recent researches of Winogradsky have shown that the first of these two alternative hypotheses is the correct one, for by making cultivations of soil in a solution containing nitrous acid and no ammonia, Winogradsky has succeeded in isolating a micro-organism which possesses the power of converting nitrous acid into nitric acid, but has no power of attacking ammonia. [Lantern-slide of Nitric Ferment (Winogradsky.)]

This second organism or *nitric ferment*, as we may call it, resembles in its activity the purely chemical oxidising agent—*potassium permanganate*—which, as we have seen, has no action on ammonia, but readily converts nitrous into nitric acid.

The process of nitrification in the soil now becomes intelligible in its entirety. It is the work of two independent organisms, the first of which converts ammonia into nitrous acid, whilst the second transforms into nitric acid the nitrous acid produced by the first.

There is a point in connection with the distribution of nitric acid in nature which is exceedingly remarkable, and which forces itself upon the attention of every student of the process of nitrification. Although nitric acid is generally so scantily present in the soil, there is one notable exception to this rule, for in the rainless districts of Chili and Peru there are found immense deposits of nitrate of soda, or Chili saltpetre, as it is called, which would appear to represent the result of a gigantic nitrification process in some previous period of the earth's history. The vast quantities of this material which occur in these regions of South America can be gathered from the fact that its exportation has for years being going on at the rate indicated by the following figures:—During the first six months of 1890 there were brought to the United Kingdom 90,000 tons, and to the European Continent 480,000 tons.

From the presence of such altogether enormous quantities, one is almost tempted to hazard the suggestion that in this particular region of the earth, under some special circumstances of which we know nothing, the nitrifying organisms must have been endowed then and there with very much greater powers than they possess to-day, and it is particularly noteworthy that in a recent examination of soils from nearly all parts of the earth, one coming from Quito, and therefore not far distant from these nitrate fields, was found to possess the power of nitrification in a degree far beyond that exhibited by any other soil hitherto experimented with. Is it not possible, perhaps, that we have in these vigorous nitrifying organisms of the soil of Quito, the not altogether unworthy descendants of that Cyclopean race of nitrifying bacteria, which must have built up the nitrate wealth of Chili and Peru, and thus countless ages ago founded the fortunes of our nitrate kings of to-day?

But these nitrifying organisms have also assisted in teaching us a highly important lesson in connection with the maintenance of life. The facts which I have already referred to concerning the multiplication of micro-organisms in distilled water, and the continuation of the nitrification-process over a period of four years in purely mineral solutions, are strong presumptive evidence in favour of these bacteria being able to gain a livelihood in the entire absence of organic food-stuffs. I refrained, however, from promulgating such a revolutionary doctrine until I should have had an opportunity of repeating these experiments with materials in which the absence of even the merest traces of organic matter had been assured, for as chemists well know, even distilled water may contain traces of organic matter.

Such a rigid proof as I had contemplated has, however, in the meantime been attempted by M. Winogradsky, also in connection with his experiments on nitrification, and he has indeed found that the nitrifying organisms flourish, multiply, and actually build up living protoplasm in a solution from which organic matter has been most rigorously excluded. Now this living protoplasm in the experiments in question must have been elaborated by these bacteria from carbonic acid as the source of the protoplasmic carbon, and from ammonia and nitrous or nitric acids as the source of the protoplasmic nitrogen. If these experiments are correct, and they were undoubtedly performed with great skill and much caution, they are subversive of one of the fundamental principles of vegetable physiology, which denies to all living structures, save those of green plants alone, the power of building up protoplasm from such simple materials.

I had occasion to mention in connection with these nitrifying organisms that they refuse to grow on the ordinary solid cultivating media employed by bacteriologists, a fact which presents a great obstacle to their isolation in a state of purity, for it is just by means of these solid culture media that micro-organisms are most easily obtained in the pure state.

This difficulty has, however, been overcome in a most ingenious manner, originally devised by Prof. Kühne, in which the solid medium is wholly composed of mineral ingredients, the jelly-like consistency being obtained by means of silica. [Demonstration of preparation of silica-jelly, consisting of ammonia sulphate, potassium phosphate, magnesium sulphate, calcium chloride, magnesium carbonate, and dialysed silicic acid.]

Fixation of Free Nitrogen by Plants.

But whilst the study of the bacteria giving rise to nitrification has thus led to the subversion of what was regarded as a firmly established principle of vegetable physiology (*viz. the incapacity of any but green plants to utilise carbonic acid in the elaboration of protoplasm*), the same science has received another shock of perhaps equal if not greater violence through researches which have been carried on with other micro-organisms flourishing in the soil.

For nearly a century past agricultural chemists and vegetable physiologists have been debating as to whether the free nitrogen of our atmosphere can be assimilated or utilised as food by plants. This question was answered in the negative by Boussingault about fifty years since; the problem was again attacked by Lawes, Gilbert, and Pugh about thirty years ago, and *their* answer was also in the negative. In the course, however, of their continuous experiments on crops, Lawes and Gilbert have frequently pointed out that whilst the nitrogen in most crops can be accounted for by the combined nitrogen supplied to the land in the form of manures and in rain water, yet in particular *leguminous* crops, such as peas, beans, vetches, and the like, there is an excess of nitrogen which cannot be accounted for as being derived from these obvious sources. The origin of this excess of nitrogen in these particular crops they admitted could not be explained by any of the orthodox canons of the vegetable physiology of the time. The whole question of the fixation of atmospheric nitrogen by plants was again raised in 1876 by a very radical philosopher, in the person of M. Berthelot, whilst the most conclusive experiments were made on this subject by two German investigators, Prof. Hellriegel and Dr. Wilfarth, who have not only shown that this excess of nitrogen in leguminous crops is obtained from the atmosphere, but also that this assimilation of free nitrogen is dependent upon the presence of certain bacteria flourishing in and around the roots of these plants, for when these same plants are cultivated in sterile soil the fixation of atmospheric nitrogen does not take place. Moreover, the presence of these microbes in the soil occasions the formation of peculiar swellings or tuberosities on the roots of these plants, and these tuberosities, which are not formed in sterile soil, are found to be remarkably rich in nitrogen, and swarming with bacteria. [Lantern-slide of nodules on roots of sainfoin (Lawes and Gilbert).]

Extremely important and instructive in this respect are the experiments of Prof. Nobbe, who has not only confirmed the results mentioned, but has endeavoured to investigate the particular bacteria which bring about these important changes, and he has indeed succeeded in showing that in many cases each particular leguminous plant is provided with its particular micro-organism which leads to its fixation of free nitrogen. Thus he found that if pure cultivations of the bacteria obtained from a pea-tubercle were applied to a pea plant there was a more abundant fixation of atmospheric nitrogen by this pea-plant than if it was supplied with pure cultures of the microbes from the tubercles of a lupin or a robinia; whilst similarly the robinia was more beneficially affected by the application of pure cultures from robinia-tubercles than by those from either pea-tubercles or lupin-tubercles. [Lantern-slides exhibiting Nobbe's experiments on pea and robinia.]

This subject of the source of nitrogen in leguminous plants has again been taken up by Sir John Lawes and Dr. Gilbert at Rothamsted, and their recent results fully confirm the observations of these foreign investigators that it is partially derived from the free atmospheric nitrogen through the agency of bacteria in the soil.

To micro-organisms again then we must ascribe the accomplishment of this highly important chemical change going on in the soil, although it has not hitherto been so fully illuminated as the process of nitrification.

Selective Action of Micro-organisms.

Any of the ordinary plants and animals with which we are familiar may be regarded as analytical machines, and we ourselves, without any knowledge of chemistry, are constantly performing analytical tests; thus we can all distinguish between sugar and salt by the taste, between ammonia and vinegar by the smell, whilst by a more elaborate investigation we distinguish, for instance, between the milk supplied from two different dairies by ascertaining on which we or our children thrive best. In fact, such analytical or selective operations are amongst the first vital phenomena exhibited by an organism on coming into this world. It is, however, particularly surprising to find this analytic or distinguishing capacity developed in an extraordinarily high degree amongst micro-organisms. From the power which we have seen that some possess of flourishing on the extremely thin diet to be found in distilled water, we should be rather disposed to think that caprice would be the very last failing with which they would be chargeable. As a matter of fact, however, the perfectly unfathomable and inscrutable caprice of these minute creatures is amongst the first things with which the student of bacteriological phenomena becomes impressed. Let me call your attention to a striking example of this which I have recently investigated.

I have here two substances, which have the greatest similarity:—

Mannite.	Dulcite.
Occurrence. Numerous plant-juices	Ditto, but less frequently.
Taste. Sweet	Ditto, but less so.
Melts. 166° C.	188° C.
Crystalline form. Large rhombic prisms ..	Large monoclinic prisms.

Not only, however, do these two substances possess such a strong external resemblance to each other, but in their chemical behaviour also they are so closely allied that one formula has to do duty for both of them, for so slight is the difference in the manner in which their component atoms are arranged that chemists have not yet been able with certainty to ascertain in what that difference consists. Under these circumstances it would have been anticipated that bacteria would be quite indifferent as to which of these two substances was presented to them, and that they would regard either both or neither as acceptable. But such is by no means the case; some micro-organisms, like ordinary yeast, have *no action upon either*, whilst *others will attack mannite, leaving dulcite untouched, others again* *being less discriminating, attack both;* representatives of a fourth possible class *which would act upon dulcite but not upon mannite* are as yet undiscovered. [Lantern-slide and Plate-culture of *B. ethaceticus.*]

This bacillus, I have recently shown, has the property of breaking down the mannite molecule into alcohol, acetic acid, carbonic anhydride, and hydrogen, but leaves the dulcite molecule untouched.

More recently I have, in conjunction with my late assistant, Mr. Frew, succeeded in obtaining a micro-organism which decomposes both mannite and dulcite into alcohol, acetic and succinic acids, carbonic anhydride, and hydrogen. [Lantern-slide and Plate-culture of *B. ethacetosuccinicus.*]

Optically Active Substances.

But these are by no means the ultimate limits to which the selective or discriminating powers of micro-organisms can be pushed, for although mannite and dulcite are extremely similar substances, they are not chemically identical. We are acquainted, however, with substances which, though chemically identical, are different in respect of certain physical properties, and are hence known as *Physical Isomers.* It is in explanation of this physical isomerism that one of the most beautiful of chemical theories was propounded by Le Bel and Van't Hoff in 1874, and which remains unsupplanted to the present day.

This theory depends upon taking into consideration the dissymmetry of the molecule which is occasioned by the presence in it of a carbon-atom which is combined with four different atoms or groups of atoms, and is most easily intelligible from an inspection of these two models. [Demonstration of tetrahedral models of Asymmetric Carbon-atom.]

This molecular dissymmetry is specially exhibited in the crystalline form of such substances, and in their action upon polarised light.

The molecule arranged according to the one pattern has the property of turning the plane of polarisation in one direction, whilst the molecule arranged according to the other pattern has invariably the property of turning the plane through precisely the same angle in the opposite direction. The molecular dissymmetry ceases when two such molecules combine together, the resulting molecule having no action on polarised light at all.

The interest of these phenomena in connection with micro-organisms lies in the fact that they are sometimes possessed of the power of discriminating between these physical isomers. Although this remarkable property was demonstrated years ago by Pasteur in respect of the tartaric acids, it has only comparatively rarely been taken advantage of. Recently, however, chemical science has been enriched in several instances by successfully directing the energies of micro-organisms in such work of discrimination.

During the past few years no chemical researches have commanded more interest, both on account of their theoretic importance and the fertility of resource exhibited in their execution, than those of Emil Fischer's, which have led to the artificial preparation in the laboratory of several of the various forms of sugar occurring in nature, as well as of other sugars not hitherto discovered amongst the products of the animal or vegetable kingdoms. The natural sugars are all of them bodies with dissymmetric molecules, powerfully affecting the beam of polarised light, but when prepared artificially they are without action on polarised light because in the artificial product the left-handed and right-handed molecules are present in equal numbers, the molecules of the one neutralising the molecules of the other and thus giving rise to a mixture which does not affect the polarised beam either way. By the action of micro-organisms, however, on such an inactive mixture, the one set of molecules is searched out by the microbes and decomposed, leaving the other set of molecules untouched, and the latter now exhibit their specific action on polarised light, an active sugar being thus obtained.

The most suitable micro-organisms to let loose, so to speak, on such an inactive mixture of sugar-molecules, are those of brewers' yeast, which decompose the sugar molecules with formation of alcohol and carbonic anhydride. Their action on these inactive artificial sugars of Fischer's is particularly noteworthy.

One of the principal artificial sugars prepared by Fischer is called *fructose*, it is inactive, but consists of an equal number of molecules of oppositely active sugars called *lævulose.*

One set of these lævulose-molecules turns the plane of polarisation to the right, and we may call them *right-handed lævulose*, whilst the other set of *lævulose-molecules* turns the plane of polarisation to the left, and we may call them *left-handed lævulose.*

The left-handed lævulose occurs in nature, whilst the right-handed lævulose, as far as we know, does not. Now, on putting brewers' yeast into a solution of the fructose, the yeast-organisms attack the left-handed lævulose molecules and convert them into alcohol and carbonic anhydride, whilst the right-handed lævulose is left undisturbed. The yeast organisms thus attack that particular form of lævulose of which their ancestors can have had experience in the past, whilst they leave untouched the right-handed lævulose molecules, which being a new creation of the laboratory, they have no hereditary instinct or capacity to deal with.

This selective power is possessed also by other forms of micro-organisms besides the yeasts, which are indeed only suitable for the separatory decomposition of sugars, and by means of bacterial forms a much greater variety of substances can be attacked in this manner. Thus I have recently found that glyceric acid can be decomposed by the *B. ethaceticus*, to which I have already referred this evening.

This glyceric acid is thus represented by chemists :—

$$C_3H_6O_4 \quad \text{or} \quad \begin{array}{c} (CH_2OH) \\ | \\ (H)\text{—}C\ OH) \\ | \\ (COOH) \end{array}$$

and this should, according to Le Bel and Van't Hoff's theory, be capable of existing in two physically isomeric forms, as easily shown by our models.

The ordinary glyceric acid known to chemists is, however, quite inactive to polarised light, and must consist, therefore, of a combination in equal molecules of a right-handed and left-handed glyceric acid. Now when the *B. ethaceticus* is put into a suitable solution of the calcium salt of this glyceric acid, it multiplies abundantly and completely consumes the right-handed molecules of the salt, but leaves the left-handed molecules entirely intact, a powerfully active glyceric acid being thus obtained.

[Demonstration of the lævorotary power of solution of new zinc glycerate with projection-polariscope.]

A number of derivatives of this new active glyceric acid have recently been prepared in my laboratory :—

DERIVATIVES OF ACTIVE GLYCERIC ACID.

Formula.	*Specific Rotation.* $[\alpha]_D$
$(C_3H_5O_3)_2$ Ba + 2 H_2O	− 9°
$(C_3H_5O_3)_2$ Sr + 3 H_2O	− 10
$(C_3H_5O_3)_2$ Ca + 2 H_2O	− 12
$(C_3H_5O_3)_2$ Cd + $1\frac{1}{2}$ H_2O	− 14
$(C_3H_5O_3)_2$ Zn + H_2O	− 22
$(C_3H_5O_3)_2$ Mg + H_2O	− 18·5
$C_3H_5O_3$ Na	− 16
$C_3H_5O_3$ Am	− 20
$C_3H_5O_3$ K	− 15
$C_3H_5O_3$ Li	− 20·5
$C_3H_5O_3$ Me	− 4·8
$C_3H_5O_3$ Et	− 9·2
$C_3H_5O_3$ Pr (*n*)	− 13·0

Here again then chemistry has been enriched by a number of new compounds, which we owe entirely to the unaccountable caprice of this micro-organism.

Individuality of Micro-organisms.

Although micro-organisms are thus becoming more and more indispensable reagents in the chemical laboratory, essential as they are for the production of many bodies, it is always necessary to bear in mind that by virtue of their vitality their nature is infinitely more complex than that of any inanimate chemicals which we are accustomed to employ. In a chemically pure substance we believe that one molecule is just like another, and hence we expect perfect uniformity of behaviour in the molecules of such a pure substance under prescribed conditions. In a pure cultivation of a particular species of a micro-organism, however, we must not expect such rigid uniformity of behaviour from each of the individual organisms making up such a cultivation, for there may be and frequently are great differences amongst them, in fact each member of such a pure culture is endowed with a more or less marked individuality of its own, and these possible variations have to be taken into consideration by those who wish to turn their energies to account. In fact, experimenting with micro-organisms partakes rather of the nature of legislating for a community than of directing the inanimate energies of chemical molecules. Thus frequently the past history of a group of micro-organisms has to be taken into account in dealing with them, for their tendencies may have become greatly modified by the experiences of their ancestors.

Of this I will give you an instance which has recently come under my observation:—

Here is a bacillus, which has the property of fermenting calcium citrate; I have found that it can go on exerting this power for years. On submitting this fermenting liquid to plate-cultivation, we obtain the appearances which you see here.

[Lantern-demonstration of plate-culture of bacillus which ferments calcium citrate.]

If one of these colonies be transferred to a sterile solution of calcium citrate, it invariably fails to set up a fermentation of the latter, the bacillus having thus by mere passage through the gelatin-medium lost its power to produce this effect. If, however, we take another similar colony and put it into a solution of broth containing calcium citrate, fermentation takes place; on now inoculating from this to a weaker solution of broth containing calcium citrate, this also is put into fermentation, and by proceeding in this manner we may ultimately set up fermentation in a calcium citrate solution which absolutely refused to be fermented when the bacilli were taken directly from the gelatin-plate.

Phenomena of this kind clearly indicate that there may be around us numerous forms of micro-organisms of the potentiality of which we are still quite ignorant; thus if we were only acquainted with the bacilli I have just referred to from gelatin cultures we should be quite unaware of their power to excite this fermentation of calcium citrate, which we have only been enabled to bring about by pursuing the complicated system of cultivation I have indicated. It is surely exceedingly probable, therefore, that many of the micro-organisms with which we are already acquainted may be possessed of numerous important properties which are lying dormant until brought into activity by suitable cultivation.

This power of modifying the characters of bacteria by cultivation is, I venture to think, of the highest importance in connection with the problems of evolution, for in these lowly forms of life in which, under favourable circumstances, generation succeeds generation in a period of as little as 20 minutes, it should be possible through the agency of selection to effect metamorphoses, both of morphology and physiology, which would take ages in the case of more highly organised beings to bring about.

We hear much about the possibility of altering the human race through training from the enthusiastic apostles of education, but even the most sanguine cannot promise that any striking changes will be effected within several generations, so that such predictions cannot be tested until long after these reformers have passed away. In the case of micro-organisms, however, we can study the effect of educational systems consequentially pursued through thousands of generations within even that short span of life which is allotted to us here.

[P. F. F.]

WEEKLY EVENING MEETING,

Friday, February 26, 1892.

Sir Frederick Bramwell, Bart. D.C.L. F.R.S. Honorary Secretary and Vice-President, in the Chair.

Sir David Salomons, Bart. M.A. *M.R.I.*

Optical Projection.

The intention of this lecture is to give a general survey of the subject of Optical Projection, which now takes its position in science, and to present examples of what may be done by this method. It would be difficult to determine which subject claims a first place. Some scientists say the microscope should have the preference, while others take a different view. For my own part, I think the microscope and polariscope stand foremost, on account of the facility with which these branches of science may be pursued for the benefit of a large number, without multiplying expensive apparatus; also because of the convenience in saving the eyes from undue strain. Indeed, to many persons, looking at objects in the table microscope is little short of a painful operation, and consequently the study of small objects becomes to them impossible. The projection method immediately brings the required relief.

For general instruction, projection methods are invaluable, such as for instance, showing diagrams, photographs, and other slides, upon the screen; as well as for spectrum analysis. In fact, the subjects which can be illustrated by means of optical projection are innumerable; but time will allow me to present only a few examples, and I trust that, when I approach the end of my lecture, my view of the importance of this subject will be held in equal estimation by you.

Probably the only people in the world that benefit by the experience of their predecessors are those who pursue the study of science. They are free from the accusation of robbing the brains of other men, when they take up methods or apparatus already known and improve upon them or employ them for their own work. In such cases, however, it is always understood that honour should be given where honour is due, and accordingly I have no wish to represent to you any piece of apparatus as of my own devising, when in reality it belongs to another.

Few men have had a larger experience, and attained greater success in optical projection, than has Mr. Lewis Wright, who has embodied in his most recent forms of apparatus all that was good in designs existing until his time. I have, therefore, started from his models, making such modifications as I thought to be desirable.

Mr. Wright does not appear—if I may say so—to have had much experience with the electric arc light as a radiant, and I found, at a very early stage, that great difficulties had to be encountered when this light was used, chiefly because the radiant approaches more nearly to what theory requires. That which was easy with the lime light became almost impossible with the arc lamp, and these difficulties had to be conquered.

Many scientific men are dissatisfied with the projection microscope, on the ground that very high magnification does not give that resolution and that sharpness which is found in the usual methods of observation. This want I fully admit. At the same time it is scarcely right to condemn a particular method, because you try to apply it to an unsuitable purpose. Hundreds of thousands of subjects may be shown with the projection microscope with far greater profit to the student than was possible in the old way. The very fact that the professor can place his pointer upon any part of the picture on the screen is invaluable to the students. I shall, therefore, attempt to show you only a series of microscopical subjects suitable for projection, and shall not employ very high magnification.

In regard to some substances very high powers may be used with advantage, but much time would be lost in getting them into the field and focussing them upon the screen. These, consequently, I omit, so that a larger number of subjects may be illustrated.

It is fair to state that most of the apparatus used to-night has been constructed by Messrs. Newton, of Fleet Street, and the luminous pointer by Messrs. Steward, of the Strand. The arc lamp is a Brockie's projector. Messrs. Baker, Watson, and others, have also come to my assistance.

I will first show, on the screen, a picture of the lantern carrying its various apparatus; and then a few systems of lenses, which may be employed for the projection microscope, as well as a diagram of the microscope itself.

Sub-stage condensers and objectives are, as a rule, made to suit the table microscope. When projecting, by means of an objective alone, in consequence of the screen distance being very great—or, in other words, the microscope tube being exceedingly long as compared with the table instrument—the objective has to be approached very close to the slide; in fact, with the higher powers, closer than the cover-glass will allow. This close working distance renders necessary special sub-stage condensers, and in many cases a special one is required for every screen distance with each objective. This requisite would seem to be a complete stumbling-block to microscope projection work. With the limelight the difficulty does not enter in the same degree as with the arc light, and as we are now dealing with the latter, further reference need not be made to the oxy-hydrogen light. There are two ways of surmounting the difficulty; one by the use of plano-concave lenses, introduced in such a way as to be equivalent to greatly lengthening the focus of the objective on the screen side, while it enables, as a consequence, the objective to be slightly further removed from the slide; i.e. giving what is termed a greater working distance. The objection to this method is that, even when these plano-concave lenses are corrected, the result, though greatly improved, is not perfect. The second way, which is a perfect one, is that of introducing an eye-piece. In both these methods, that the best results may be obtained, the objective is made to occupy a position not very different from that which it would do if employed on the table microscope.

In the eye-piece method almost the exact conditions can be complied with for which the objective was made. I propose, therefore, to show the subjects by the eye-piece method. The only objectives which will be used are: (1) Zeiss's 35 millimetre projection objective, with a sub-stage condenser, 4 inches focal length, placed a considerable distance from the slide; (2) Newton's 1-inch projection objective, the sub-stage condenser as in the first case; and (3) Zeiss's $\frac{1}{4}$-inch achromatic objective, the sub-stage condenser being Professor Abbe's three-lens condenser with the front lens removed. In all three cases the eye-pieces used are Zeiss Huyghens No. 2 and No. 3.

In each instance I will mention the magnification in diameters, as well as the number of times when reckoned by area, for the appreciation of those who estimate by area; and I will also give the size to which a penny postage-stamp would be increased, supposing it to be made of indiarubber, and stretchable to any extent in all directions. In presenting these figures I do not pretend that they are absolutely correct, but as they have been ascertained under conditions similar to those now existing the errors will not be very great.

In consequence of the field not being quite flat, and the sections having a certain thickness, although extremely thin in most cases, the whole of the object cannot be in focus upon the screen at the same time. By shifting the focussing screw slightly all parts may be brought into focus successively. So-called greater depth of focus is obtained by using an increased working distance; and for projection work over-correction for flatness can alone give a sharp picture all over with very considerable depth of focus; the difficulty of over-correction being that, unless extreme care is taken, certain forms of distortion may be introduced. By stopping down the objective greater flatness of field may be secured, but at the expense of light. There is thus a choice of difficulties, and the least one should be taken.

Turning now to the polariscope. Polarized light teaches us a great deal concerning the structure of matter; it is also a means of confirming the undulatory theory of light. This subject is so large that no attempt can be made to give even a general idea of the field it covers, and the experiments, which will be shown in the polariscope, may be taken simply as a few illustrations of the subject and nothing more; but they will, at any rate, be suggestive of the large field to which this method of analysis can be applied. A vast amount of mathematical proof can be illustrated graphically by various experiments with polarized light. I will show on the screen a diagram of the polariscope. (Shown.)

With reference to showing the spectrum. The method of projecting a spectrum, I think, is new, as I have not seen it described anywhere. It gives practically a direct spectrum with an ordinary prism, without turning the lantern round to an angle with the screen; and here is a diagram of the method.

The details of the apparatus, as well as those of the methods of working, I have modified in almost every instance, for five reasons:—(1) That more certain results may be ensured; (2) that rapidity may be obtained; (3) that only one operator may be needed; (4) that, as far as possible, all parts of the apparatus may be interchangeable and (5) that loose screws and pieces may be dispensed with.

There were then shown by projection a number of slides illustrating various microscopic optical systems, and a number of microscopic slides, followed by a series of general polariscopic projections, some of them to illustrate the strains existing in many forms of matter; also a spectrum by a carbon disulphide prism, in conjunction with a reflecting prism and with a mirror, which, apart from any other result, demonstrates that the loss of light with a reflecting prism is less than with an ordinary glass mirror. Slides and other projections were also thrown upon the screen.

The details are as follows:—

The Microscope.—Screen distance, 21 feet. First, 35 millimetres Zeiss projection objective, 4-inch sub-stage condenser, Zeiss Huyghens eye-piece 2; 500 diameters = 250,000 times = penny stamp stretched to cover about 147 square yards. Subjects shown: proboscis of blow-fly; permanent molar displacing milk-tooth (kitten); human scalp, vertical; human scalp, surface; fossil ammonites and belemnite. Second, 1-inch Newton's projection objective, 4-inch sub-stage condenser, Zeiss Huyghens eye-piece 2; 1000 diameters = 1,000,000 times = stamp stretched to about 588 square yards. Objects shown: proboscis of blow-fly; foot of a caterpillar; section of human skin, showing the sweat ducts; phylloxera vastatrix of the vine. Third, 1 inch Newton's projection objective, 4-inch sub-stage condenser, Zeiss Huyghens eye-piece 3; 1300 diameters = 1,690,000 times = stamp stretched to about one-fifth of an acre. Slides shown: proboscis of blow-fly; wings of bee (showing hooklets and ridge); sting of bee (showing the two stings, sheath, and poison-sack); sting of wasp (showing same as last slide); eye of beetle (showing the facets). Fourth, $\frac{1}{4}$-inch Zeiss's achromatic objective; Abbé's 3-lens sub-stage condenser, with top lens removed; Zeiss Huyghens eye-piece 3; 4500 diameters = 20,250,000 times = stamp extended to nearly $2\frac{1}{2}$ acres. Slides shown, proboscis of blow-fly; hair of reindeer (showing cell structure); hair of Indian bat (showing the peculiar funnel-like structure); sting of bee (showing the barbs); foot of spider; stage of the micrometer (the closest lines ruled to thousandths of an inch, which measure $4\frac{1}{2}$ inches apart under this magnification); a wave length $\frac{1}{40000}$ inch, therefore, on screen measures about $\frac{1}{9}$ inch.

The Polariscope.—Shown with parallel light; plain glass; glass under pressure; chilled glass (round, oval, and waved peripheries); Prince Rupert's drop (broken in the field); horn; selenites (overlapped); butterfly (selenite); bunch of grapes (selenite); bi-quartz, with $\frac{1}{4}$-wave plate (the $\frac{1}{4}$-wave plate in this experiment produces the same effect upon the bi-quartz as if a column, 20 centimetres long, of a $7\frac{1}{2}$ per cent. solution of cane sugar were placed between the polarising nicol and the bi-quartz. The analyser has to be rotated about 10°); a piece of sapphire to show asterism. Shown with convergent light; hemitrope (cut in a plane, not at right angles to the axis); ruby; topaz; grape sugar (diabetic); cane sugar; quartz; superposed right and left-handed quartz (spirals); calcite and phenakite superposed (showing transition from negative to positive crystal, passing through the apopholite stage).

The Solidiscope.—New form of apparatus for showing solids, and consisting of two reflecting prisms and suitable projecting lenses. With this instrument were shown:—Barton's button, the works of a watch, a coin.

Spectrum Analysis.—Spectrum thrown by means of a disulphide prism, combined with a reflecting prism; the result being that a good spectrum is thrown upon the screen direct without turning the lantern. There were shown:—The spectrum; absorption bands of chlorophyll, &c.; effects produced by passing the light through coloured gelatine films.

Projection of Slides.—Decomposition of water; expansion of a wire by means of heat; combination of colours to form white light; various diagrams, coloured photographs of a workshop, &c. As an extra experiment there was shown, in the polariscope, with a convergent light, Mitscherlisch's experiment (illustrating the changes which take place in a selenite under the influence of heat).

There are but few who would disagree with me in the opinion that the microscopic world, as regards its design and its molecular structure, is quite as wonderful as the great works around us seen with the unaided eye. A magnifying glass of low power opens up a world far larger than that which we are accustomed to see. At the present time, even with the most perfect apparatus that exist, only a small portion of the universe is known to us.

Scientific study should be pursued by all in a greater or less degree. It teaches more important lessons than the most impressive discourse ever preached. During the investigation of what is generally termed the invisible world, men should at times pause to reflect, and ask themselves such questions as these: What is the meaning of, and to what end is, creation? Is it all mere chance? Were such wonderful designs and properties created at the beginning? Was there in matter at the beginning an inherent, or implanted, power of development? Simple as these questions may seem, man in the flesh will never be able to find the true answers. The extraordinary design and structure which have existed in the unseen world for millions of years, or possibly in all past time, and even at the present day known to so few, demonstrate at least that the Great Power has bestowed the same care upon what appear to us the most insignificant portions of creation, as upon what we think are the greatest works in the universe. These silent sermons must surely influence the mind, and set it thinking of the supernatural and of our duties during life.

It may now with truth be said that science gives us means, such as never before existed, of appreciating the greatness of the Supreme Spirit, by enabling us to read fresh chapters in the book of nature.

[D. S.]

WEEKLY EVENING MEETING,

Friday, March 4, 1892.

Basil Woodd Smith, Esq. F.R.A.S. F.S.A. Vice-President, in the Chair.

Professor L. C. Miall, F.L.S. Professor of Biology at the Yorkshire College, Leeds.

The Surface-film of Water, and its relation to the Life of Plants and Animals.

It is necessary to the exposition of my subject that I should begin by reminding you of some well-known properties of the surface of water. These are familiar to every student of physics, and are set forth in many elementary books. They are well explained and illustrated, for instance, in Prof. Boys's deservedly popular book on "Soap-bubbles." But there may be some persons here who have not quite recently given their thoughts to this subject, and it will only cost us a few minutes to repeat a few simple experiments, which will establish some fundamental facts relating to the surface-film of water.

The following experiments were then shown:—

(1) Mensbrugghe's float. Proves that the surface-film of water offers resistance to the passage of a solid body from beneath.

(2) Aluminium wire made to float on water. Proves that the surface-film of water offers resistance to the passage of a solid body from above. The resistance is proportional to the length of the line of contact of the solid with the water.

(3) Copper gauze made to float on water. Here, a number of intersecting wires are employed instead of a single wire, and the consequent increase in the length of the line of contact greatly increases the weight which can be supported.

(4) Frame with vertical threads, carrying a light plate of brass. The threads hang vertically at first, but when the whole is dipped into soapy water, the adhering film exerts a pull upon the sides of the frame, draws the threads into regular curves, and raises the brass plate. When the film is broken, the threads resume their previous vertical position, and the plate falls.

(5) Aluminium wire supported by vertical copper wires. Each end of the aluminium wire forms a loop, which fits loosely to one of the copper wires. When the apparatus is dipped into soapy water, the contraction of the film draws the aluminium wire upwards. After pulling it down with a thread, the wire can be again drawn up. This is another illustration of the tendency of the film to contract. We use soapy water, because the film lasts for a considerable time, but the surface-film of pure water, though less viscous than that of

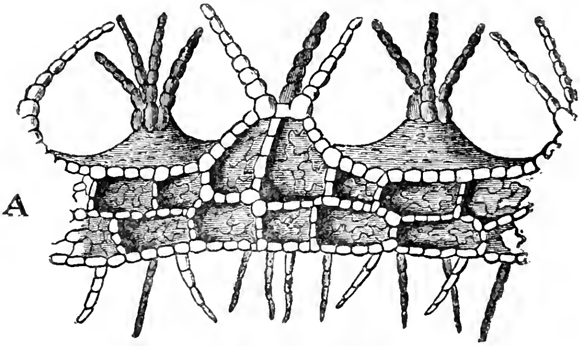
soapy water, is even more contractile. We have already seen that the surface-film clings with considerable tenacity to any solid body introduced into it, and that its hold increases with the length of the line of contact. It is for this reason that fine meshes offer so great a resistance to the passage of the surface-film. Air can pass through the meshes with perfect ease; water also, if not at the surface, can pass through readily enough, but the surface-film in contact with air will only pass through with difficulty, and if there is water behind it, the water may thus be restrained from passing through the meshes.

(6) Muslin bag hung in front of the lantern. Water poured into the bag (a large spoonful) does not flow out; but when the muslin beneath the water is rubbed with a rod, it becomes wetted, the surface-film passes to the outside of the bag, and the water trickles through.

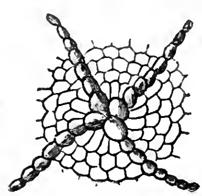
There are many plants which take advantage of this property of the surface-film of water, viz. that it will not penetrate small spaces, in order to keep themselves dry. You must have observed how the hairy grasses repel water. The surface-film is unable to pass into the fine space between the hairs, and accordingly the water above the surface-film is kept from contact with the leaf. This simple artifice is often employed by plants which float at the surface of water. Here it is important that they should keep dry, not only for the purpose of respiration, but for another reason too. They commonly have great power of righting themselves when accidentally submerged, and this self-righting property depends upon the fact that the under surface of each leaf is always wet, while the upper surface is incapable of being wetted. The microscopic hairs which thickly cover the upper surface are sufficient to exclude the water. A leaf of Pistia is now submerged, and shown as an opaque object in the lantern. You see by the gleaming of its surface that it is overspread by a continuous flat bubble of air, which looks like quicksilver beneath the water. I will next invert a leaf of Pistia by means of a rotating lever. It is now brought up beneath the surface of the water in an inverted position, and you see that, notwithstanding its buoyancy, it is unable to free itself and rise to the surface, because of the air-bubble, which adheres both to the leaf and to the disk at the end of the lever, and ties both together. Complete separation of the leaf from the disk would involve the division of the air-bubble into two smaller bubbles, one adhering to the leaf and the other to the disk. In this operation the surface-film would necessarily be extended directly in opposition to its natural tendency to contract. Several other water-plants exhibit the same properties as Pistia. I will mention two of the water-ferns —Salvinia and Azolla. Salvinia is found floating on still water in the warmer parts of Europe, as well as in other quarters of the globe. The leaves are attached on opposite sides of a horizontal stem. Long hairy roots (or what look like roots, and really answer the same purpose) hang down into the water. Salvinia has in a remarkable degree the power of rising when submerged, of always rising with its leaves up and its roots down, and of rising with the upper surface of its leaves perfectly dry. It is obvious that these qualities are most useful to a plant which may be pressed under water or drenched with rain. Its nutrition, like that of all green plants, depends largely upon substances extracted from the air; and to be overspread with water, which disappeared only by a slow process of evaporation, would be disadvantageous, especially if the water were not absolutely clean. Every leaf of Salvinia is, to begin with, excavated by a double layer of air-spaces, which lodge so much air as to give it great buoyancy. On the upper surface are placed at regular distances a

FIG. 1.

A



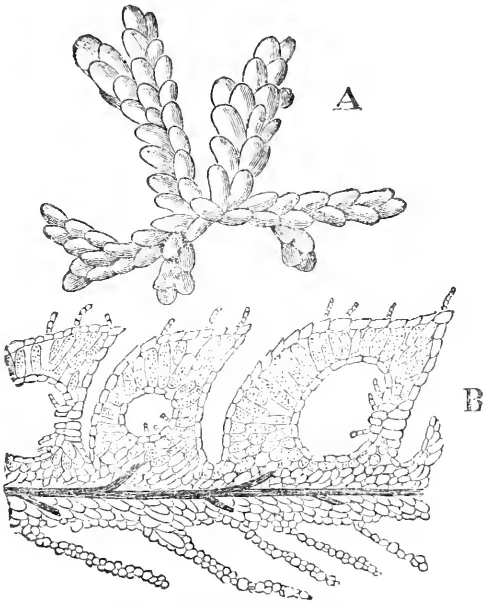
B



Salvinia natans. A, combined surface-view and section of floating leaf, modified from a figure in Sachs's 'Botany,' showing the air-cavities, the submerged hairs of the lower surface, and the groups of stiff hairs on the upper surface. These latter inclose spaces into which water cannot enter, even when the leaf is completely submerged. B, one group of hairs from the upper surface, seen from above.

number of prominences, each surmounted by a group of about four stiff, spreading hairs, which keep the water from reaching the surface of the leaf. When forcibly depressed, the Salvinia takes down with it a layer of air, which forms a flat bubble over the leaf, and of course gives great power of self-righting, for the specific gravity of the upper side is greatly reduced, while the lower side is weighted, as before, by the long, water-logged roots. Once restored to the surface, the bubble bursts, and the little drops into which it is instantly resolved roll off like drops of quicksilver. Azolla, which is found in most hot countries, and is often grown in hothouses, behaves in a very similar way. Here the leaves are far smaller, and crowded together upon a branching stem of minute size. There are a few hairs upon the upper surface, and between the leaves are narrow clefts, connected with globular cavities, which occupy the centre of every leaf. These cavities, which are often closed, and never possess more than an outlet of extreme minuteness, are always filled with air; so are the

Fig. 2.



Azolla caroliniana. A, stem with leaves, magnified; B, longitudinal section through part of ditto, highly magnified. The air-cavities of the leaves are shown, the narrow spaces between the leaves, into which water cannot enter, the fine hairs of the upper surface, the submerged leaf-lobes, and the vascular bundles.

clefts between the leaves. No water can lodge on the upper surface, apparently because the surface-film is stretched from the raised edge of one leaf to that of the next; and thus buoyancy, self-righting, and repulsion of water are efficiently secured.

Many plants which ordinarily float on the surface of the water (Salvinia, Azolla, Duckweed, *Potamogeton natans*, &c.) sink on the approach of winter. At this time it is very curious to see how completely they lose both their buoyancy and their power of repelling water. I do not know how this change is brought about, but the result is one of obvious advantage. The leaves, or in some cases the entire plants, sink to the bottom, and hibernate there, out of the reach of frost. Many perish; some are broken up by decay into isolated buds. When spring returns, the few survivors float up, and soon cover the surface with leaves. It would be interesting to know something of the mechanism by which these seasonal changes are effected.

One of the commonest objects in Nature, which is apt to escape our notice on account of its minute size, for it is less than one-quarter of an inch in length, is the egg-raft of the gnat. This was beautifully described 150 years ago by Réaumur. The eggs of the gnat are cigar-shaped, and 250 or 300 of them are glued together, so as to make a little concave float, shaped like a shallow boat. The upper end of each egg is pointed; the lower end is provided with a lid, through which the larva will ultimately issue into the water. The gnat in all stages, even while still in the egg, requires an ample supply of air. It is therefore necessary that the egg-raft should float at the surface; it is also necessary that it should always float in the same position, so as to facilitate the escape of the larva. This is effectually secured by a provision of almost amusing simplicity. Let us first notice how efficient it is. If we take two or three of these tiny egg-rafts, and place them in a jug of water, we may pour the water into a basin again and again; every time the egg-rafts float instantly to the surface; and the moment they come to the top, they are seen to be as dry as at first. The fact is that the surface-film cannot penetrate the fine spaces between the pointed ends of the eggs. The cavity of the egg-raft is thus overspread by an air-bubble, which breaks the instant it comes to the top. The larva of the gnat, when it escapes from the egg, floats at the surface, and it is enabled to do so in consequence of the properties of the surface-film. When the larva changes to a pupa, it becomes buoyant, and floats at the surface, except when alarmed. To enable it to free itself without unnecessary effort from the surface of the water, the respiratory tubes of the pupa are furnished with a valvular apparatus, which can cut the connection with the air in a moment, and restore it at pleasure, when the pupa again floats to the surface.*

Another Dipterous insect, whose larva inhabits rapid streams, makes an ingenious use of the properties of the surface-film. This is the larva of Simulium, of which I have given some account in the lecture just quoted. At the time of the delivery of that lecture, I was wholly unable to explain how one difficulty in the life of the insect is surmounted. The larva clings to the water-weeds found in brisk and lively streams. The pupal stage is passed in the same situation. But a time comes when the fly has to emerge. Now the fly is a delicate and minute insect, with gauzy wings. How does it escape from the rushing water into the air above, where the remainder of its life has to be passed? This was a question upon which I had spent much thought, but in vain. It appeared to me for many months completely insoluble. However, I was informed last year by Baron Osten Sacken of a paper written by Verdat, seventy years ago, in which the emergence of the fly of Simulium is described. Guided by Verdat's description, I had little difficulty in seeing for myself how

* The larva and pupa of the gnat are more fully described in my British Association lecture on "Some Difficulties in the Life of Aquatic Insects," reported in 'Nature,' vol. lxiv. p. 457.

the difficulty is actually overcome. During the latter part of the pupal stage, the pupa-case becomes inflated with air, which is extracted from the water, and passed through the spiracles of the fly into the space immediately within the pupal skin. The pupal skin thus becomes distended with air, and assumes a more rounded shape in consequence. At length it splits along the back, in the way usual among insects, and there emerges a small bubble of air, which rises quickly to the surface of the water and there bursts. When the bubble bursts, out comes the fly. It spreads its hairy legs, and runs upon the surface of the water to find some solid support up which it can climb. As soon as its wings are dry, it flies to the trees or bushes overhanging the stream.

A very interesting inhabitant of the waters, which makes use of the properties of the surface-film to construct for itself a home beneath the surface, is the water-spider (*Argyroneta aquatica*). This interesting little animal has been described by many naturalists, some of whom, judging from their accounts, had no personal acquaintance with its habits. But among the number is the eminent naturalist Félix Plateau, son of the physicist to whom we are so much indebted for our knowledge of the phenomena of surface-tension. I need hardly say that in his account of the water-spider, Prof. Plateau gives a full and adequate account of the scientific principles concerned in the formation of its crystalline home.* Plateau remarks that the water-spider, like all other spiders, is an air-breathing animal. It dives below the surface, and spends nearly its whole life submerged. In order to do this without interruption to its breathing, the spider carries down a bubble of air, which overspreads the whole abdomen as well as the under side of the thorax. These parts of the body are covered with branched hairs, so fine and close that the surface-film of water cannot pass between them. The spider swims on its back, and the air lodges in the neighbourhood of the respiratory openings, which are placed on that surface which floats uppermost. When the spider comes to the top, as it does from time to time to renew its supply of air, it pushes the abdomen out of the water, and we can then see that this part of the body is completely dry. When it sinks, the water closes in again at a little distance from the body, and the bubble forms once more.

It would be inconvenient to the water-spider to be obliged to come frequently to the surface for the purpose of breathing. A predatory animal on the watch for its victims must lie in ambush close to the spot where they are expected to appear, and the water-spider accordingly requires a lurking-place filled with air, beneath the surface of the water. It has its own way of supplying this want. Relying on the fact, already illustrated by our muslin bag, that the surface-film of water will not readily pass through small openings, the spider proceeds as follows. It begins by drawing together some

* "Observations sur l'Argyronète aquatique," Bull. Acad. Roy. de Belgique, 2me sér. tom. xxiii. 1867.

water-weeds with a few threads, in such a way that they meet at one or more points. It then fetches from the surface a fresh supply of air, and squeezes part of it out by pressing together the bases of its last pair of legs. The bubble rises, but is detained by some of the threads previously spun across its path. Then the spider returns to the surface to fetch another bubble, and repeats the operation as often as is necessary. Now and then she secures the growing bubble by additional threads, and before long has a bubble nearly as big as a walnut, inclosed within an invisible silken net, which imprisons the air as effectually as a dome of glass would do. The spider takes care to conceal her home from observation, and before long the minute Algæ, growing all the more vigorously because of the air brought to them, effectually conceal the habitation. The mouth of the dome, which is of course beneath, is narrowed to a small circle, and Plateau has observed a cylindrical horizontal tube, seven to eight millimetres in diameter, by which the spider is enabled to enter or leave her home without being observed. The air within is renewed as required, by the visits of the spider to the surface.

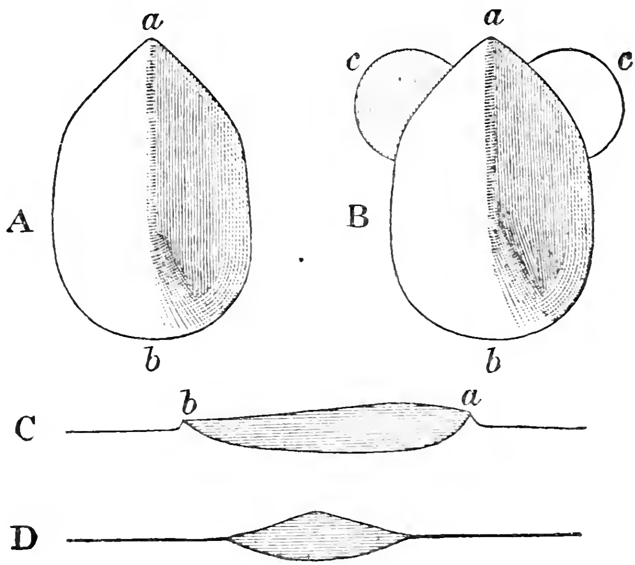
Besides this home, which is the ordinary lurking-place of the spider, another is required at the time when the young are hatched. The new-born spiders are devoid of the velvety covering of hairs, and would drown in a moment if placed in a nursery with a watery floor. The female spider therefore makes a special nest for this particular occasion, which floats on the surface of the water, rising well above it. It is bell-shaped and strongly constructed. The upper part is partitioned off, and contains the eggs. Beneath the floor of the nursery the mother takes her station, and watches over the safety of her brood, defending them against the predatory insects which abound in fresh waters. It is interesting to see how the faculty of spinning silk, used by the house-spider for her snares, and at other times for the fluffy cocoon in which the eggs are enveloped, furnishes to the water-spider the materials of her architecture. It is not less interesting to observe the economy of material which results from the use of the tenacious and contractile surface-film, in place of a solid wall.

We will next consider another property of the surface-film, which is turned to account in the daily life of the very commonest of our floating plants, I mean the duckweed, which overspreads every pond and ditch. A number of the green floating leaves of duckweed are now placed in a shallow dish in the field of the lantern, and I will ask you to observe how they are grouped. They have spontaneously arranged themselves in a very irregular fashion, forming strings and chains which spread hither and thither over the surface of the water. This is not the way in which most floating bodies behave. Let us remove the duckweed, and replace it by another dish of water in which I will put a number of small disks of cork.* You will see

* In order to avoid the inconvenience caused by the attraction of the sides of the vessel, the dish should be over-full of water.

that the bits of cork are attracted one to another and crowd together in one place. Let us inquire why the floating bits of cork are thus attracted towards one another. If any solid capable of being wetted by water is partly immersed in water, the liquid rises round it in an ascending capillary curve. If the solid is not wetted by water, the curve will turn downwards. We may get ascending or descending capillary curves in other ways. If, for instance, I were to lay a sheet of paper upon water, and turn its edges up at certain places, we should get marked ascending curves at these points. The raising of some parts of the surface causes other parts to sink, and may bring about descending curves, or make previously formed descending curves more marked. We shall find it helpful in our experiments to notice one very simple plan of producing a descending capillary curve round the edge of a vessel. If we take a glass of water, and fill it until the water is level with the brim, we naturally speak of the glass as *full;* but if we are careful to avoid rude shaking, we may still add a considerable quantity of water without spilling any. The glass will then become what we may call *over-full*, and its surface will be bounded by a descending capillary curve. Now, it is of immediate importance to us to observe that *like* capillary curves, whether ascending or descending, attract one another, and that *unlike* curves repel one another. The theoretical explanation of this point is not difficult, but it must not detain us here. To place the fact itself beyond dispute, we will try a little experiment. A circular dish of water is now placed in the field of the lantern, and we will introduce into it a small disc of wood. Both the disc and the side of the vessel are wetted by water, and an ascending capillary curve rises round each. The result is that the two bodies attract one another. Every time the disc is moved away it is powerfully drawn towards the side of the vessel. With a little syringe we will add water to the dish in sufficient quantity to raise the level above the edge of the vessel. You will observe that the wooden disc is now repelled by the edge of the vessel, and floats free in the centre. By sucking up a little water, it becomes attracted once more, and so we may go on, causing it to be attracted or repelled, according as we add or subtract a small quantity of water. But what has all this to do with the duckweed? In order to explain the behaviour of duckweed, I must ask you to examine a careful representation of its form. This common plant has not, to my knowledge, been faithfully represented in any botanical book. You will see that the leaf is of an irregular oval shape, broader at one end than at the other, and that the narrow end is pointed. A raised ridge extends along the length of the leaf, from the point to the middle of the opposite or rounded border. Duckweed almost invariably propagates itself by budding. New leaves are pushed out symmetrically on each side of the point. They grow bigger and bigger, and gradually free themselves. The point upon each leaf marks the place where it was last attached to the parent leaf. Sometimes the budding is so rapid, that, before a fresh pair of leaves have become free, they have already budded out a second pair, which we may call the grand-daughters of the parent leaf. The pointed end of the leaf, and also the opposite end of the ridge, are raised above the general level, and very marked capillary curves ascend from the general water-level to these points. The free edge of every bud is also raised above the general water-level, and a capillary curve ascends to meet

FIG. 3.



Duckweed (*Lemna minor*), magnified. A, single frond; *a*, scar of attachment to parent. A ridge extends from *a* to *b* across the upper surface of the frond, gently subsiding towards *b*. B, frond, budding-out two new fronds. C, longitudinal section from *a* to *b* (A) showing ascending capillary curves at *a* and *b*. D, transverse section at right angles to the last. The margins of the frond in this plane are level with the surface of the water. N.B.—The form of the fronds is somewhat variable. Minor inequalities occur along the margin, but the principal ascending curves, which are also centres of attraction, are at *a*, *b*, and *c*.

it. Hence, when a number of leaves of duckweed are floating freely on water, they are powerfully attracted one to another at certain points, while at intervening points they are relatively inert. If you take a floating leaf of duckweed, and bring near it a clean needle or a pencil-point, or any similar object, provided that it is not greasy, you will see that the leaf is at once attracted towards the point, but it always turns itself so as to bring one of its ascending curves round to the needle or pencil. We all see in the lantern how readily a leaf of duckweed is made to rotate rapidly by causing a needle-point to revolve round it, without ever touching it. Let us now try to imitate the behaviour of the leaves by some rude models. I have here some elliptical paper floats, cut out with a pair of scissors, and having each of the pointed ends a little turned up. We place these one by one on the surface of the water, and you see in the lantern how they are attracted to one another, point to point, and how they form long chains, which have a tendency to break up into stars. It is the existence of such points of attraction on the margin of the leaves which causes the duckweed to form chains and strings, so long as there is any unoccupied surface in the pond. A moment's consideration shows how profitable this tendency is to the plant. Were the duckweed to crowd together like the floating bits of cork, the pressure towards the centre of any considerable mass of plants would be so great that the new leaves budded out would find no room in which to expand: but, by virtue of one very simple provision, viz. the existence of inequalities of level among the edges of the leaves, clear spaces and lanes are left between the floating leaves, so long as any unoccupied space remains.

Long exposure to the air, especially in still weather, affects the life of duckweed in a material way. Dust and decaying organic substances give rise to a pellicle, which is most mischievous to floating plants; and I think I could show, if time allowed, how much the habits of duckweed have been altered thereby. But, apart from visible impurities, mere exposure to air gives, as Lord Rayleigh has taught us, a considerable degree of superficial viscosity to water. Hence, the leaves of duckweed, when the surface is contaminated, will tend to lie in whatever positions they may be thrown by accidental causes, such as wind, and the attractions due to capillarity will be more or less impeded. But the effect of the superficial viscosity will in time be overcome by the attractive forces, so that it probably does not in the long run greatly affect the distribution of the leaves over the surface of water.

Many other floating plants, but not all, behave more or less like duckweed, and for the same reason. As yet I know of none which space themselves quite so effectually, and the extreme abundance of the common duckweed, as well as its world-wide distribution, may be partly due to the completeness of its adaptation to capillary forces. Some dead objects may accidentally take a shape which causes them to spread out over water, but I have met with none which have particularly struck me. Floating natural objects, such as sticks or seeds, behave, in many cases at least, very differently, and become densely massed. My attention was first called to this subject by seeing how different was the grouping of duckweed from that of some seeds of *Potamogeton natans*, which were floating in the same pond.

The capillary forces which spread the leaves of duckweed or Azolla upon the surface of the water are indirectly concerned in the transport of these and like plants to fresh sites. If we put a stick into water overspread with duckweed, we cannot fail to notice how the leaves cling to the stick. They cling in a particular way, which enables them to bear transport more safely. The wetted surface, for obvious physical reasons, is attracted to the wetted stick; and the water-repellent surface, which is that which best resists drying, is outwards. The tenacity with which duckweed clings to the legs of water-birds, and the position which it almost inevitably takes under such circumstances, may have a good deal to do with the safe transport of the plant to distant pools. It is not, I think, too much to say that the prosperity of duckweed depends very largely upon the capillary forces which come into play at the surface of water.

We have now exhausted our time, though I have been obliged to leave unnoticed many special adaptations of living things to the peculiar conditions which obtain on the surface of water. Had time allowed, I should have been glad to say something about the aquatic animals which creep on the surface-film as on a ceiling, and about the insects which run and even leap upon the surface-film without wetting their minute and hairy bodies.* All small animals and plants which float on water necessarily come into contact with the surface-film, and have to deal with the difficulties which result from it. We have seen that they generally manage in the long run to convert these natural difficulties into positive advantages.

I have to thank my colleague, Dr. Stroud, for his frequent explanations of the physical principles upon which these adaptations depend, and also for much practical and valuable help in the preparation of suitable experiments. [L. C. M.]

* See 'Nature,' vol. xliv. p. 457.

GENERAL MONTHLY MEETING,

Monday, March 7, 1892.

Sir James Crichton-Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

The Right Hon. Lord Brougham and Vaux,
Eric Bruce, Esq. M.A.
Arthur N. Butt, Esq.
Percy C. Gilchrist, Esq. F.R.S. M. Inst. C.E.
Frederick Hovenden, Esq. F.L.S. F.G.S.
Herbert Edgell Hunt, Esq.
Alphonse Normandy, Esq.
W. R. Pidgeon, Esq.
Mrs. W. R. Pidgeon,
Sir Frederick Montagu Pollock, Bart.
Alan A. Campbell Swinton, Esq.
The Right Hon. Lord Watson,
Mrs. Wigan,
Arthur W. Williams, Esq.

were elected Members of the Royal Institution.

The following Letters were read :—

"SANDRINGHAM, NORFOLK,
"*12th February*, 1892.

"Sir Francis Knollys is desired to convey to the Members of the Royal "Institution of Great Britain the thanks of the Prince and Princess of Wales for "the sympathy they have expressed on the occasion of Their Royal Highnesses' "bereavement."

"WHITEHALL,
"*20th February*, 1892.

"SIR,

"I have had the honour to lay before The Queen the loyal and dutiful "Resolution which has been adopted by the Members of the Royal Institution of "Great Britain on the occasion of the death of His Royal Highness The Duke of "Clarence and Avondale, K.G., and I have to inform you that Her Majesty was "pleased to receive the Resolution very graciously.

"I have the honour to be,
"Sir,
"Your obedient Servant,
"HENRY MATTHEWS.

"Sir J. C. Browne, M.D. LL.D., &c.

The Special Thanks of the Members were returned for the following Donations :—

Sir Frederick Bramwell, Bart.	£100	0	0
Sir Douglas Galton	10	10	0
Mrs. Bloomfield Moore	20	0	0
Sir Frederick Abel	21	0	0
C. Meymott Tidy, Esq.	10	10	0
Sir James Douglass	10	10	0
D. E. Hughes, Esq.	5	0	0
George Berkley, Esq.	5	0	0
Basil Woodd Smith, Esq.	10	10	0
Sir Archibald Campbell, Bart.	25	0	0
Professor Dewar	120	0	0
Sir David Salomons, Bart.	50	0	0
J. T. Brunner, Esq., M.P.	50	0	0

for carrying on investigations on Liquid Oxygen.

The following Arrangements for the Lectures after Easter were announced :—

ON THE SCULPTURING OF BRITAIN—ITS LATER STAGES. By PROFESSOR T. G. BONNEY, D.Sc. LL.D. F.R.S. F.G.S. Professor of Geology in University College, London. (The Tyndall Lectures.) Two Lectures on *Tuesdays*, April 26, May 3.

ON PHOTOGRAPHY IN THE COLOURS OF NATURE. By FREDERICK E. IVES, Esq. Two Lectures on *Tuesdays*, May 10, 17.

ON SOME ASPECTS OF GREEK POETRY. By PROFESSOR R. C. JEBB, M.P. Litt.D. Regius Professor of Greek in University of Cambridge. Three Lectures on *Tuesdays*, May 24, 31, June 7.

ON THE CHEMISTRY OF GASES. By PROFESSOR DEWAR, M.A. F.R.S. *M.R.I.* Fullerian Professor of Chemistry, R.I. Four Lectures on *Thursdays*, April 28, May 5, 12, 19.

On Faust. By R. G. Moulton, Esq. M.A. Three Lectures on *Thursdays*, May 26, June 2, 9.

On J. S. Bach's Chamber Music: I. Suites and Partitas; II. Sonatas and Concertos; III. Preludes and Fugues; IV. Toccatas and Variations. By E. Dannreuther, Esq. (With many Musical Illustrations.) Four Lectures on *Saturdays*, April 30, May 7, 14, 21.

On Some Modern Discoveries in Agricultural and Forest Botany. (Illustrated by Lantern.) By Professor H. Marshall Ward, Sc.D. F.R.S. F.L.S. Professor of Botany at the Royal Indian Engineering College, Coopers Hill. Three Lectures on *Saturdays*, May 28, June 4, 11.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The New Zealand Government—Statistics of the Colony of New Zealand, 1890. fol. 1891.
The French Government—Documents Inédits sur l'Histoire de France:
Lettres de Catherine de Médecis, Tome IV. 1570–74. Par Cte. Hector de la Ferrière. 4to. 1891.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quinta: Rendiconti. 1° Semestre, Vol. I°. Fasc. 1, 2. 8vo. 1892.
American Geographical Society—Journal, Vols. III.-XXII. 8vo. 1870–90.
Antiquaries, Society of—Proceedings, Vol. XIII. No. 4. 8vo. 1891.
Aristotelian Society—Proceedings, Vol. II. No. 1, Part 1. 8vo. 1892.
Bankers, Institute of—Journal, Vol. XIII. Part 2. 8vo. 1892.
Botanic Society of London, Royal—Quarterly Record, No. 48. 8vo. 1892.
British Architects, Royal Institute of—Proceedings, 1891–2, Nos. 8, 9. 4to.
British Association for Advancement of Science—Report of Meeting at Cardiff, 1891. 8vo.
California Publishing Company—Californian Illustrated Magazine, Vol. I. No. 3. 8vo. 1892.
Chemical Industry, Society of—Journal, Vol. XI. No. 1. 8vo. 1891.
Chemical Society—Journal for February, 1892. 8vo.
Cracovie, l'Academie des Sciences—Bulletin, 1892, No. 1. 8vo.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I.—Journal of the Royal Microscopical Society, 1892, Part 1. 8vo.
Duckworth, Sir Dyce, M.D. LL.D. M.R.I. (the Author)—On the Higher Education of Women. 8vo. 1891.
East India Association—Journal, Vol. XXIV. No. 1. 8vo. 1892.
Editors—American Journal of Science for February, 1892. 8vo.
Analyst for February, 1892. 8vo.
Athenæum for February, 1892. 4to.
Brewers' Journal for February, 1892. 4to.
Chemical News for February, 1892. 4to.
Chemist and Druggist for February, 1892. 8vo.
Educational Review, New Series, Vol. I. No. 4. 8vo. 1892.
Electrical Engineer for February, 1892. fol.
Electricity for February, 1892. 4to.
Electric Plant for February, 1892. 4to.
Engineer for February, 1892. fol.
Engineering for February, 1892. fol.
Engineering Review for February, 1892. 8vo.
Horological Journal for February, 1892. 8vo.
Industries for February, 1892. fol.
Iron for February, 1892. 4to.
Ironmongery for February, 1892. 4to.
Lighting for February, 1892. 4to.

Monist for February, 1892. 8vo.
Nature for February, 1892. 4to.
Open Court for February. 1892. 4to.
Photographic News for February, 1892. 8vo.
Revue Scientifique for February, 1892. 4to.
Surveyor for February, 1892. 8vo.
Telegraphic Journal for February, 1892. fol.
Zoophilist for February, 1892. 4to.

Electrical Engineers, Institution of—Journal, No. 95. 8vo. 1892.
Ex-Libris Society—Journal for February, 1892. 4to.
Florence Biblioteca Nazionale Centrale—Bolletino, No. 147. 8vo. 1892.
Franklin Institute—Journal, No. 794. 8vo. 1892.
Geographical Society, Royal—Proceedings, Vol. XIV. No. 3. 8vo. 1892.
Geological Institute, Imperial, Vienna—Verhandlungen, 1891, Nos. 15–18; 1892, No. 1. 8vo.
Georgofili, Reale Accademie—Atti, Quarta Serie, Vol. XIV. Disp. 4. 8vo. 1891.
Harlem, Société Hollandaise des Sciences—Œuvres Complètes de Christiaan Huygens, Tome IV. Correspondance, 1662–63. 4to. 1891.
Institute of Brewing—Transactions, Vol. V. Nos. 3, 4. 8vo. 1892.
Johns Hopkins University—University Circulars, No. 95. 4to. 1892.
American Chemical Jounral, Vol. XV. No. 1. 8vo. 1892.
Lawes, Sir J. Bennet, F.R.S. and Gilbert, Dr. J. H. F.R.S. (the Authors)—The Sources of the Nitrogen of our Leguminous Crops. 8vo. 1892.
Observations on Rainfall, Percolation, and Evaporation. 8vo. 1891.
Manchester Geological Society—Transactions, Vol. XXI. Part 13. 8vo. 1892.
Mechanical Engineers, Institution of—Proceedings, 1891, No. 5. 8vo.
Meteorological Office—Communications from the International Polar Commission, Part 7. 4to. 1891.
Ministry of Public Works, Rome—Giornale del Genio Civile, 1891, Fasc. 12. 8vo. And Designi. fol. 1891.
Odontological Society—Transactions, Vol. XXIV. No. 4. 8vo. 1892.
Payne, W. W. and Hale, G. E. (the Editors)—Astronomy and Astro-Physics for February, 1892. 8vo.
Pharmaceutical Society of Great Britain—Journal, February, 1892. 8vo.
Calendar, 1892. 8vo.
Phipson, T. L. Esq. Ph.D. F.C.S. (the Author)—On Noctilucine; the phosphorescent principle of Luminous Animals. 8vo. 1875.
Wilson W. Phipson. A Memoir. 8vo. 1892.
Photographic Society of Great Britain—Journal, Vol. XVI. No. 5. 8vo. 1892.
Preussische Akademie der Wissenschaften—Sitzungsberichte, Nos. XLI.–LIII. 8vo. 1891.
Rio de Janeiro, Observatoire Impérial de—Revista, No. 12. 8vo. 1892.
Robinson, Professor William (the Author)—Petroleum Oil Engines. (British Association.) 8vo. 1891.
Selborne Society—Nature Notes, Vol. III. No. 27. 8vo. 1892.
Society of Architects—Proceedings, Vol. IV. Nos. 6, 7. 8vo. 1892.
Society of Arts—Journal for February, 1892. 8vo.
St. Bartholomew's Hospital—Report, Vol. XXVII. 8vo. 1891.
Tacchini, Prof. P. Hon. Mem. R.I.—Memorie della Società degli Spettroscopisti Italiani, Vol. XX. Disp. 12ª; Vol. XXI. Disp. 1ª. 4to. 1892.
United Service Institution, Royal—Journal, No. 168. 8vo. 1892.
United States Geological Survey—Bulletins, Nos. 62, 65, and 67–81. 8vo. 1890–91.
Vereins zur Beförderung des Gewerbfleises in Preussen—Verhandlungen, 1892: Heft 2. 4to.
Victoria Institute—Transactions, No. 97. 8vo. 1892.
Wright & Co. Messrs. J. (the Publishers)—Medical Annual for 1892. 8vo.
Pye's Surgical Handicraft. 3rd edition. 8vo. 1891.
Epitome of Mental Diseases. By Dr. J. Shaw. 8vo. 1892.

WEEKLY EVENING MEETING,

Friday, March 11, 1892.

Sir James Crichton-Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

F. T. Piggott, Esq.

Japanesque.

I have been very severely and very properly called to order by the learned Secretary of this Institution for coining a new word "Japanesque," to meet the exigencies of the occasion of my appearing before you. But the word "Japanese" has already three uses. It is both singular and plural, applying to one and to all the inhabitants of Japan; and as an adjective it is applied to the products of the islands, connoting probably some slight idea of artistic excellence. I could not very well append to these a fourth use, applying it to things in the Japanese style, to things 'Japanesy' in fact, because, as I shall endeavour to show you this evening, it could not in such use connote any idea of art at all; it would, on the contrary, connote a great deal that is terribly inartistic. Therefore, again craving your pardon for having coined it, I will use the word Japanesque to signify, not the European facsimiles of Japanese work, which are indeed fairly well executed, but a certain bastard form of ornament which has seized on our art wares, and which has derived some sort of inspiration from Japan. The history of the rise and progress of Japanesque is well-known in the potters' trade: I believe that the precise date of its first appearance has been chronicled on the sherds which form the archives of that trade. For this reason my examples of Japanesque will be drawn from plates, the decoration of which indeed, holding as they do no unimportant place among our household gods, is a matter of considerable interest. But it will probably not be necessary for me to point out that the potters having once set the example, as indeed they have done in decorative art through many centuries, Japanesque has been adopted by the carvers, the weavers, the broiderers, and the candlestick makers. I shall endeavour to point out to you the chief points in which Japanesque misses the mark, commits unpardonable sins against the art of Japan. If I should conclude with a panegyric on that art itself, I trust you will find me sufficient excuse.

Here are some Japanesque plates, the designs upon them are as far removed from Japanese as this Island of Fogs which has produced them is from the Islands of the Sun. And yet, no one who is familiar, as who amongst us is not, with the crude formalities of true Victorian decoration, with the parodies of the Renaissance grotesques with which so much of our street architecture is plastered, no one but can fail to discern in these extraordinary designs a striving after something new, an effort to be original, a desire to shake off the fetters of formalism and pseudo-classicism. You will not need, I think, to be reminded that these plates are fair samples of a species of art-work with which we are now familiar: nor will you fail to recognise the cause which called these artistic strivings and desires into being. A light had dawned suddenly on British artists in design, at a time when they were beginning to weary of the stencil plates and tracing-papers which are the handmaids of formalism. That light dawned from the far-eastern Islands of Japan, which, though their existence had been long known, were for us practically discovered not more than forty years ago. And in the pathway of this light there came these revelations, which to the designers of those days must have been simply startling: that it was not then essential to good ornament to divide a circle into eight, sixteen, or thirty-two parts, in each of which the same amount of pattern was to be stencilly repeated; that it was not then essential to find the true centre of an object, and fixing a flower there to let all the stems radiate from it in ordered arrangement; that it was not then essential to make everything balance truly from that centre, of equal size, and at equal distances. In the work which came from Japan these and many other traditional rules which we deemed to be of the essence of art were cast to the winds, ignored in most insolent fashion: and yet the work was strangely delightful: had a grace of infinitely more charm than anything our traditions had ever produced for us: revealed a feeling of proportion and balance whose laws, if indeed there were any, were wrapped in deepest mystery. The effect of this on the Western workman was strange. Broad and pleasant paths of artistic wickedness seemed to open straight before him: to walk in them, he forsook the narrow path which his education bade him follow; he too, would cast his traditions and his stencil-plates to the winds. And when he had so done his work became sprawly, his designs began to lack unity, he cultivated irregularity, and in proportion as he developed these peculiarities his execution deteriorated: a dabby, spotty, eccentricity pervaded everthing he did. The great art-spirit of the Japanese had been true to the first of artistic principles: she had concealed herself. Without any very great attempt to track her to her hiding-place we contented ourselves with a poor parody of her, and formulated a style which has no laws to guide it, and which for want of a better name I have christened Japanesque.

I think I am not exaggerating when I describe the result as execrable; nor in saying that the revelation which came with the dawning light was startling am I using a mere rhetorical figure of speech. 'The Times' of 1854 contains an account of one of, if not the earliest, exhibitions of Japanese things in London. A certain Dutch merchant "wishing to ascertain whether the taste of the English nation was in accordance with the works of Japan," held an exhibition in Pall Mall in January of that year. 'The Times' wondered "whether the same thing would happen to the Japanese as had already happened to the Hindoos, whose art work had been depreciated by the stupid conceit of the European, who must needs tie down their exuberant fancy to his own meagre or vulgar designs. . . . Such delicate and beautiful ornamentation had never been seen in the same perfection in this country. . . . Our papier-maché manufacturers ought to feel themselves under special obligations to this Dutch merchant for enabling them to see how immeasurably the artisans of that barbarous island (as we thought it then) excel them." Thus 'The Times': and I have heard too how in this theatre, very nearly thirty years ago, the audience was so enraptured with a lecture delivered by Mr. Leighton on Japanese art, and with the objects he exhibited, that it was past midnight before this staid Institution could close its doors.

I may attack at once one of the chief vices of Japanesque, which is an abortive attempt to grasp one of the leading characteristics of Japanese work; it is the fragmentary spray work with which our pots, and pans, and plates are nowadays ornamented, the angulate sprig of flower or fruit which comes from nowhere in particular and sprawls everywhere in general. Without putting you in darkness again let me picture to your mind's eye a very familiar dessert-service, the shape of dishes and plates unexceptional, the price a sum unmentionable, the colour a greenish-grey, which is itself but a weak vibration of the pure celadon of the East. Thereon straggle various boughs and sprays of flowers coming, "*à la Japonaise*," from beyond the edge, with birds hovering about them. But the drawing of these birds is horridly impossible, and as to the proportion between them and the flowers, why, either they must have been snared in Lilliput, or the flowers culled in Brobdignagian gardens. But even this is a small matter compared with the execution of them. The whole subject is first drawn in black outline, and the petals, stalks, and leaves simply dabbed or splashed with colour. This is not an unfair description of average Japanesque work.

Now the points which the draughtsman fights for, on the supposed authority of Japanese art, are: visible irregularity of design coupled with haphazard composition; a suggestion of an invisible shrub growing somewhere, which has allowed one of its branches to trail across the plate; and a sort of conventional naturalism which serves as an excuse for hasty and poor workmanship. In this respect, indeed, his work does not approach even that of those palmy old days of British art when the potters painted just in the centre of a plate a posy of flowers as like to nature as they could make them. The Japanese work which this feeble stuff attempts to copy belongs essentially to the domain of *pictorial* art, and is governed by the same laws as the pictorial art of Japan. The greatest purist among decorators would never deny that pictorial art may very properly be applied to the purposes of decoration; but he would insist, and rightly, that when it is so applied it should conform to the principles of pictorial art, and must be judged by its standards. A base form of pictorial work, work without artistic vitality, is not to be tolerated merely because technical difficulties stand in the way of getting good execution on clay. If I paint a landscape on a potter's vessel I shall not be forgiven its crudity, its lack of depth and light; nor if it be a human face, its vacuity, simply because of the difficulties which my materials set in my way. Admittedly the work is much more difficult than if I painted my subject on paper or canvas; but art is power, and if it succeed, I may look for greater praise for my picture, because of the great technical difficulties which I have overcome; but the attempt to overcome them was of my own seeking, and if I fail, I cannot insist that, by reason of these technical difficulties, my art finds legitimate expression in a lower range of feeling. Nor, again, for art is long, can the length of time which work would occupy were it well done, be an excuse for doing it badly. Is it essential to my happiness to cover my walls with red roses and ribbons, and my ceilings with chubby cupids and all the winged hierarchy of artistic space? I may do so, and infringe no real or imaginary law. But the cost, if it be the work of men's hands, or the difficulty of getting good results, if it be processed, will not be an answer to kind friends who tell me that my variegated patches of colour are hideous and mere nothingness, or that my cherubs are up aloft in positions of anatomic impossibility. Decorative art in its lowest form supplies the means of obviating those terrible productions in which our fathers of an age not so long past seem to have delighted.

Let me turn your attention now for a moment to the pictorial art of Japan. Much of it depends for its charm on the simplicity of the means by which it produces its effects. It revels in suggestion. The works of one of the greatest of its schools is in great part in monochrome. The treatment of leaves and flowers often approaches very closely to the conventional treatment necessary to ornament. In these simple black and white pictures much of the detail, even of the foreground, is left to the imagination; the middle distance is veiled in a misty cloud; the distance is suggested by a few delicate, almost disappearing, touches. Now the technical difficulties in the way of executing pictures such as these in materials less easy to manipulate than ordinary pigments, are obviously much diminished.

The canons of the art can be observed as faithfully by an artist working with lacquer and gold dust as by one who uses water and Chinese ink. The wood-carver and the metal worker, the embroiderer and the dyer, know that the masses of colour which their materials produce may be made to correspond entirely with the masses of full tone in a picture. Again, that wonderful dexterity of workmanship, which surpasses all we have ever dreamed of in the West, looks upon the hammer, the chisel, the needle, the knife, as no less facile instruments for producing sweeping swelling lines than the brush. And, yet again, whether they work with liquid pigment or stiff enamel, with threads of silk or with metal inlay, all craftsmen alike possess a complete mastery over the gradation of their tones, even to the vanishing point. And thus the art of all of these craftsmen is identical both in spirit and in execution with the art of the painter; the result, monochrome pictures in shades of gold or steel, in patina of varied lustres, in dyes or in silk embroidery, which are as effective, and which are endowed with the same charms as the painted picture in black and white. Thus it comes about that the lacquer boxes, the porcelain, the silk or cotton raiment, and all the thousand things which add to the charm of life to the Japanese, are embellished with pictures, executed in precisely the same way, with the same firm lines and evenly-covered surfaces, with the same gradations of tone, the same dark shadows and fleecy clouds, as those which came from the studios of the Kano masters.

I will ask you to look at my Japanesque plates once more. Mr. Lennox has very kindly turned them upside down; he evidently thought it made the boughs of the trees a little truer to nature, though the birds now fare rather badly. They are full of extraordinary angles, which, considered as a mere arrangement of lines, are bad enough, but looked at as an interpretation of nature are impossible. There is little of either Eastern or Western pictorial art about any of them; the one in which the spray comes on at three places might perhaps be taken as giving the idea of a plate hidden in a bush.

The Japanesque designer delights to dwell on the fragmentariness of his design, insists on the existence of the remainder of the boughs; his designs are clumsily composed, and still more clumsily set upon the surface. These are among the chief characteristics of Japanesque which I will ask you to bear in mind.

Now let me endeavour to explain to you how essentially this Japanesque work differs from the Japanese work upon which it is based. In point of mere execution it lacks two of its essential qualities, the smooth swelling surfaces which are produced by gradual pressure on the long pliant brush, and the strength and sweep of the lines. I will not dwell upon this point; but I think you will follow me at once when I say that the free lines which are drawn by a man kneeling, as the Japanese do, over his paper laid upon the floor, must be essentially different from the free lines drawn by a man facing his canvas, and working with a mahl-stick, or sitting on a chair with his material on a table in front of him. The part of the body which forms the axis of the free lines is in each case different. The line drawn, for example, with the axis at the wrist must have a different character from that drawn with the axis at the shoulder. The methods of drawing in the East and the West have each produced characteristic results. I do not wish to compare them, only the quality of Japanese work is so intimately connected with the method of its execution, that it is impossible quite to catch the spirit of it unless we adopt the method, *pro hac vice* of course.

But the drawing of most Japanesque work is execrable from a Western point of view. Western art does not sanction smudges as substitutes for rose-leaves; Western art requires some observance of proportion between the different objects of a picture; therefore in the most elementary essentials, Japanesque sins against both the East and West. That it cannot catch the trick of the Eastern line is, as I have said, not exactly the fault of the draughtsmen. They do not profess to copy Japanese models, but rather to adapt Japanese principles to their own work. And the first point, on which I would not have dwelt so long did it not pervade the whole subject, is that they have missed the pictorial quality of this form of Japanese decoration, and have parodied it with crude outlines and haphazard smudging. It is the failure to recognise the pictorial quality of this Japanese work that has led to the Japanesque arrangement of branches which makes them appear as if they came from somewhere else, and the insistence on the existence of the rest of the branch, both of which are entirely foreign to the Japanese idea.

Now, in the first place, it must be clearly understood that the Japanese do both profess and practise a rigid adherence to the structural principle of ornament. Their art never allows them so to ornament a surface as to make it appear part of something which is non-existent. Perhaps the corresponding principle in pictorial art may be stated concisely, thus: a picture should not look as if it were a slice cut out of a panorama, with more to follow at either end.

Japanese pictorial art, in its very nature, delighting to paint one incident, not many, enables the principles of focus to be more carefully observed than they are in the West.

Now the bough of plum or cherry-tree, with its buds and blossoms, is one of the most familiar subjects in Japanese paintings, for a very sufficient reason; it is one of the most familiar objects in a Japanese house. In the floral pictures with which, week by week, they beautify their homes, the Japanese use everything that nature gives them, not the bud and flower alone, nor the twig and leaf alone, but the whole bough. Whole branches of flowering shrubs are set in their vases, and these branches reappear in their pictures. And I think the simple secret of their being set close to the edge of the paper or the plate, is, that that is precisely how the branch is seen in the house, cut in a sharp line by the edge of the vase or hanging basket in which it is placed. Then too a sort of artificial focus is obtained by the pruning of twigs and flowers at the base of the branch. But of suggestion of the rest of the tree, the suggestion of which Japanesque is so full, there is not the slightest trace. Such an idea as that a spray should come on to the plate in three different places would be quite impossible for a Japanese artist even to imagine.

Far more important even than the points I have already touched upon is the composition of the design for picture or ornament. And it is in this, as I think you will have seen, that Japanesque with its astonishing angles, true neither to nature nor to art, so terribly misses the mark: it is in this that the Japanese so entirely excel.

I cannot but digress to dwell for a short space upon the Japanese art of beautiful arrangement, out of which composition in design springs. You are all probably familiar with one of the commonest forms of Japanese ornament, that in which the surface is sprinkled with heraldic devices, or with fragments or patches of ornament; often in the case of a continuous design it is broken in upon here and there by imaginary, or slightly indicated, cloud-masses. The principle is the same in both cases. The devices or patches may be few, or many, but they are set upon the surface irregularly, with apparent hap-hazard, or, as the Japanese say, "jiggi-jiggi." The effect is invariably charming; in spite of the very small material used to produce it, there is a sense of completeness which is the more surprising as it seems to conflict with every principle known to our own symmetrical art. And yet there is nothing hap-hazard about it, it is the result of a most finished study, guided by a most refined taste; the taste which enters into the minutiæ of every day life in Japan; whether it be the coolie setting-out minute maple trees on the slopes of a miniature Fujiyama, the maiden settling gew-gews in her raven tresses, your "boy" arranging flowers for your table, the journeyman painter daubing colour on the commonest fan, all alike know the mysterious secret, and act upon it. You cannot live a week in Japan without noticing that it is deeply-rooted in the people's instincts. I verily believe that, with some inclinings of the head, and not a few soft interjaculatory reflections, a Japanese could put a postage-stamp on an envelope artistically.

I pray you to forgive the banality of this word "artistically," but I use it deliberately. When the Japanese are said to be "*so* artistic" the speaker usually refers to this skill of theirs in beautiful arrangement. And those æsthetic souls who pose as the high priests of something they call "high art," they too have their little fantasies of arrangement; they love to set things all askew, thinking thereby to redeem themselves from the curse of commonplace, which indeed, they do; they are certainly dimly conscious of one fact, that it is possible not to be commonplace. Beyond this they do not go, deeming "all anyhow" to be the perfect rule of art. "Culture" with its inept niaiseries, its tawdry and fade conceits, struggles still in the primers of a science of which the Japanese have long ago formulated every rule, and daily practise the examples. If they put one little flower-vase on a table, or paste on a screen a dozen differently shaped poetry papers, the result is always effective, always charming; in their subtle arrangements there is a most admired disorder, they seem indeed "to snatch a grace beyond the reach of art."

Though I travel somewhat "beyond the reach of art," let me indicate, in the lightest and most Japanese method of suggestion, how wrapped up are the subtleties of this charm with intellectual pleasure. The delight which we derive from following a graceful curve, or from contemplating a perfectly balanced arrangement, seems to grow as the proportion between the "x" and "y" of the curve's equation, or between the "x" and "$1 - x$" of the rectilinear arrangement increases in subtlety. There appear to be three distinct degrees of this pleasure. The pleasure of knowledge first, as of the proportions of a circle, or of a vase in the centre of a table; but this is of a very low order, often indeed approaching contempt. The mind knows at once how *that* is done. Then there is the repose and satisfaction of not knowing and not wanting to know. But when this satisfaction gives place to curiosity and the desire to know, the trouble of finding out produces cerebral fidgets; there is the excitement of discovery, probably the annoyance that the discovery was not worth the trouble after all, and the general subversion of that repose which beauty engenders.

Some such intellectual processes seem to be the origin of the effects we call commonplace, beautiful, eccentric.

And so coming back to my vase and my table, the Japanese know not the one, but the hundred and one, places where to set the vase, so that the conditions are satisfied of subtle proportion and avoidance of curious or eccentric effect.

And here let me add is an example of the abundant latitude which is left to individual expression in spite of the cloud of rules in which tradition and convention have wrapped this and every other Japanese subject.

* * * * * * *

Precisely the same principle pervades the composition of all Japanese pictures, though one form of it very often leads to large spaces of blank, or as it is often called, wasted paper. If the paper were cut down it would be so much nicer, so much more suited to form one of the crowd of pictures which jostle one another on our walls. Well, but the Japanese consider, in setting out the scheme of the picture, its relation to the area it has to cover; the balance of lines or of masses may, for instance, possibly require one shaft of bamboo to be carried up isolated into an otherwise blank space. The blank or slightly tinted paper seems to me as sufficient for all the requirements of art as the wilderness of black, red, or blue paint with which we cover similar spaces on our canvases, calling them "backgrounds" of wall, curtain, or sky.

The ugly necessities of primed canvas compel us to cover it all over with pigment; the necessities of the rapidly absorbent paper, or prepared silk, which the Japanese use, preclude their doing so; thus each necessity has produced its own law.

But it is not in the somewhat lavish use of blank spaces alone, but in every line of the drawing that this art of balanced arrangement is discernible, making for wholeness and completeness and repose. It is the absence of this that makes Japanesque work so sprawly, distorted, and abortionate. And verily it is not to be wondered at;

it is not on our bizarre porcelain alone that this same quality of composition is lacking, but (I speak this with very bated breath) all our art seems to lack it altogether.

* * * * * * *

Time permits me to dwell only on one other feature of Japanesque work which greatly grieves the souls of the elect. Of recent years panels of doors have been decorated with boughs of trees which begin in space, sprawl across one panel, are continued in imagination behind the joint, to reappear and wander on across the second panel.

It violates our own canons of art, but it is supposed to derive authority from the Japanese; yet like every other Japanesque notion it is entirely contrary to their idea. If a Japanese artist has to ornament two panels of a screen with a continuous picture, he lays them close together on the floor, and treating them as one, proceeds to paint his picture. Afterwards the panels are separated and put into the frame of the screen. There is thus no notion of the design being continued behind the frame. You will find the same principle in any Japanese picture-book. The double-page illustrations are deliberately cut down the middle and printed on opposite pages.

Double-page illustrations are a vexation of the spirit. We have four ways of dealing with them, stitching-in, folding-down, printing sideways, and mounting on a guard. I do not say that in this case the Japanese way is the best, but at least it is not more ruthless than stitching-in or folding-down, and is certainly more convenient than printing sideways.

* * * * * * *

I have, with your kind indulgence, abused Japanesque sufficiently for one evening. I will endeavour to sum up its misdemeanours in a single sentence. It conceived the Japanese art of ornament to be merely the embodiment of eccentricity, and therefore to be judged by other standards than those with which we had long been familiar: it cultivated eccentricity in its turn, and substituted for simplicity of style and vigour of execution crude designs and coarse workmanship.

I have talked more of Japanesque than of Japanese; but I must now carry out my threat of saying a few words on Japanese art, and of showing you a few more specimens of it chiefly drawn from the Shiba and Nikko temples.

With the form of Japanese decorative art which I have chiefly discussed this evening, the floral spray-work, most people are I think familiar. But I do not think it is so generally known that the art of the country, eclectic as is everything in Japan, embraces as many different forms of expression as there are styles in current use in the West. It is an eclectic art; but nothing ever passed into the service of the Japanese but it received the impres of the national spirit. A graceful symbolism and an ultimate reference to nature pervade it in all its forms, whether in the more fanciful side which makes it the most delightful of home arts, or in the severely classical side which puts it in the front rank of arts suited to devotion.

For the panels and doorways for the shrine to a departed hero the Buddhist emblems furnish a hundred different diapers, so that the walls and doors glitter and scatter the light in a thousand fanciful ways. For the friezes and the ceilings, not a flower that blows, nor leaf that withers, but is pressed into the service; the pæony and the chrysanthemum bend their stalks into a classic scroll, or the plum blossoms floating down the stream suggest new lines of exquisite tracery. Here a few lines of conventional work, notwithstanding their recurrence at regular intervals, and their almost geometrical treatment, compose a design as restless in its beauty as the sea-waves which have suggested it; and there the oft-repeated scroll-work tells of the summer cloud from which it has borrowed its shape. Dull monotony is packed away along with moulds and stencil-plates, and in its place, imaginativeness, suggestion, devotional reminders, worship of nature, infinite variety everywhere.

That the art-instinct in the East, and among Eastern nations in Japan the highest, is higher than in the West, none will deny. It is not indeed to be wondered at. In that beautiful country life passes under easier and more graceful conditions than with us; there the days are not marred by the ever-presence and worship of the machine which civilisation the king has set up: there the sway of fashion is unknown, only that changeless law, the joy perpetual in the things of beauty; there one learns how great a part in life art may play, rendering every incident of the day more interesting by beautifying its surroundings, making them, when they minister to the wants of the body, minister also to the lust of the eye. It is in great part the lack of this, when one comes back to be ground by the great machine, that causes that Japanostalgia, which all suffer who have once set foot in that land of flowers.

*   *   *   *   *   *

[F. T. P.]

WEEKLY EVENING MEETING,

Friday, March 18, 1892.

Sir James Crichton-Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

George Du Maurier, Esq.

Modern Satire in Black and White.

[No Abstract.]

WEEKLY EVENING MEETING,

Friday, March 25, 1892.

Basil Woodd Smith, Esq. F.S.A. F.R.A.S. Vice-President, in the Chair.

John Evans, Esq. D.C.L. LL.D. Sc.D. P.S.A. Treasurer, R.S.

Posy-Rings.

[No Abstract.]

[The whole discourse is printed in the May number of 'Longman's Magazine.']

WEEKLY EVENING MEETING,

Friday, April 1, 1892.

William Huggins, Esq. D.C.L. LL.D. Ph.D. F.R.S. Vice-President, in the Chair.

Professor Oliver Lodge, D.Sc. LL.D. F.R.S. F.R.A.S.

The Motion of the Ether near the Earth.

Everybody knows that to shoot a bird on the wing you must aim in front of it. Every one will readily admit that to hit a squatting rabbit from a moving train you must aim behind it.

These are examples of what may be called "aberration" from the sender's point of view, from the point of view of the source. And the aberration, or needful divergence between the point aimed at and the thing hit has opposite sign in the two cases—the case when receiver is moving, and the case when source is moving. Hence, if both be moving, it is possible for the two aberrations to neutralise each other. So to hit a rabbit running alongside the train you must aim straight at it.

If there were no air that is all simple enough. But every rifleman knows to his cost that though he fixes both himself and his target tightly to the ground, so as to destroy all aberration proper, yet a current of air is very competent to introduce a kind of spurious aberration of its own, which may be called windage; and that he must not aim at the target if he wants to hit it, but must aim a little in the eye of the wind.

So much from the shooter's point of view. Now attend to the point of view of the target.

Consider it made of soft enough material to be completely penetrated by the bullet, leaving a longish hole wherever struck. A person behind the target, whom we may call a marker, by applying his eye to the hole immediately after the hit, may be able to look through it at the shooter, and thereby to spot the successful man. I know that this is not precisely the function of an ordinary marker, but it is more complete than his ordinary function. All he does usually is to signal an impersonal hit; some one else has to record the identity of the shooter. I am rather assuming a volley of shots, and that the marker has to allocate the hits to their respective sources by means of the holes made in the target.

Well, will he do it correctly? assuming, of course, that he can do so if everything is stationary, and ignoring all curvature of path, whether vertical or horizontal curvature. If you think it over you will perceive that a wind will not prevent his doing it correctly; the line of hole will point to the shooter along the path of his bullet, though it will not point along his line of aim. Also, if the shots are fired from a moving ship, the line of hole in a stationary target will point to the position the gun occupied at the instant the shot was fired, though it may have moved since then. In neither of these cases (moving medium and moving source) will there be any aberration error.

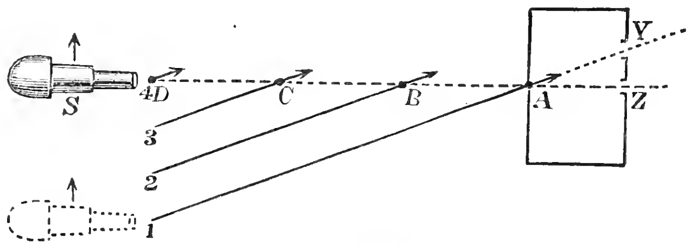
But if the *target* is in motion, on an armoured train for instance, then the marker will be at fault. The hole will not point to the man who fired the shot, but to an individual ahead of him. The source will appear to be displaced in the direction of the observer's motion. This is common aberration. It is the simplest thing in the world. The easiest illustration of it is that when you run through a vertical shower, you tilt your umbrella forward; or, if you have not got one, the drops hit you in the face; more accurately, your face as you run forward hits the drops. So the shower appears to come from a cloud ahead of you, instead of from one overhead.

We have thus three motions to consider, that of the source, of the receiver, and of the medium; and of these only motion of receiver is able to cause an aberrational error in fixing the position of the source.

So far we have attented to the case of projectiles, with the object of leading up to light. But light does not consist of projectiles, it consists of waves; and with waves matters are a little different. Waves crawl through a medium at their own definite pace; they cannot be *flung* forwards or sideways by a moving source; they do not move by reason of an initial momentum which they are gradually expending, as shots do; their motion is more analogous to that of a bird or other self-propelling animal than it is to that of a shot. The motion of a wave in a moving medium may be likened to that of a rowing boat on a river. It crawls forward with the water, and it drifts with the water; its resultant motion is compounded of the two, but it has nothing to do with the motion of its source. A shot from a passing steamer retains the motion of the steamer as well as that given it by the powder. It is projected therefore in a slant direction. A boat lowered from the side of a passing steamer, and rowing off, retains none of the motion of its source; it is not projected, it is self-propelled. That is like the case of a wave.

The diagram illustrates the difference. Fig. 1 shows a moving cannon or machine-gun, moving with the arrow, and firing a succession of shots which share the motion of the cannon as well as their own, and so travel slant. The shot fired from position 1 has reached A, that fired from the position 2 has reached B, and that fired from position 3 has reached C by the time the fourth shot is fired at D. The line A B C D is a prolongation of the axis of the gun; it is the line of aim, but it is not the line of fire; all the shots are travelling aslant this line, as shown by the arrows. There are thus two directions to be distinguished. There is the row of successive shots, and there is the path of any one shot. These two directions enclose an angle. It may be called an aberration angle, because it is due to the motion of the source, but it need not give rise to any aberration. True direction may still be perceived from the point of view of the receiver. Attend

FIG. 1.

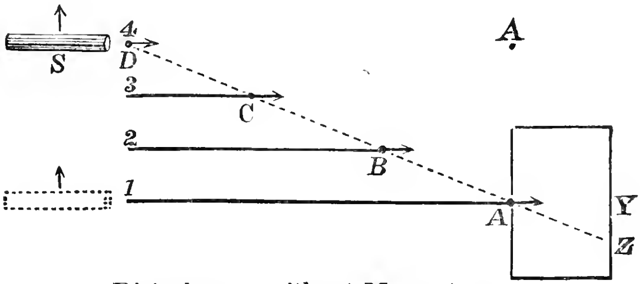


Disturbances with Momentum.

to the target. The first shot is supposed to be entering at A, and if the target is stationary will leave it at Y. A marker looking along Y A will see the position whence the shot was fired. This may be likened to a stationary observer looking at a moving star. He sees it where and as it was when the light started on its long journey. He does not see its present position, but there is no reason why he should. He does not see its physical state or anything as it is now. There is no aberration caused by motion of source.

But now let the receiver be moving at same pace as the gun, as when two grappled ships are firing into each other. The motion of the target carries the point Y forward, and the shot A leaves it at Z, because Z is carried to where Y was. So in that case the marker looking along Z A will see the gun, not as it was when firing, but as it is at the present moment; and he will see likewise the row of shots

FIG. 2.



Disturbances without Momentum.

making straight for him. This is like an observer looking at a terrestrial object. Motion of the earth does not disturb ordinary vision.

Fig. 2 shows as nearly the same sort of thing as possible for the case of emitted waves. The tube is a source emitting a succession of disturbances without momentum. A B C D may be thought of as horizontally flying birds, or as crests of waves; or they may even be thought of as bullets, if the gun stands still every time it fires, and only moves between whiles.

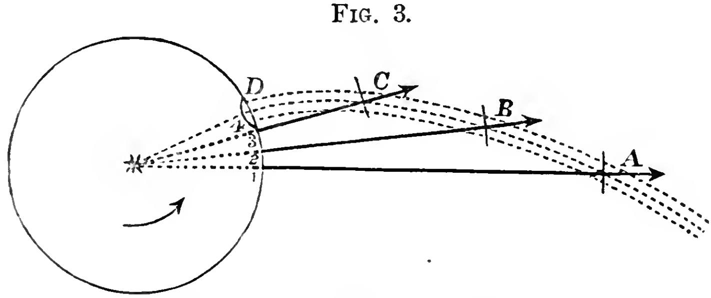
The line A B C D is now neither the line of fire nor the line of aim: it is simply the locus of disturbances emitted from the successive positions 1 2 3 4.

A stationary target will be penetrated in the direction A Y, and this line will point out the correct position of the source when the received disturbance started. If the target moves, a disturbance entering at A may leave it at Z, or at any other point according to its rate of motion; the line Z A does not point to the source, and so there will be aberration when the target moves. Otherwise there would be none.

Now Fig. 2 also represents a parallel beam of light travelling from a moving source, and entering a telescope or the eye of an observer. The beam lies along A B C D, but this is not the direction of vision. The direction of vision to a stationary observer is determined not by the locus of successive waves, but by the path of each wave. A ray may be defined as the path of a labelled disturbance. The line of vision is Y A 1, and coincides with the line of aim; which in the projectile case (Fig. 1) it did not.

The case of a revolving lighthouse, emitting long parallel beams of light and brandishing them rapidly round, is rather interesting. Fig. 3 may assist the thinking out of this case. Successive dis-

Fig. 3.



Beam from a Revolving Lighthouse.

turbances A, B, C, D, lie along a spiral curve, the spiral of Archimedes; and this is the shape of the beams as seen illuminating the dust particles, though the pitch of the spiral is too gigantic to be distinguished from a straight line. At first sight it might seem as if an eye looking along those curved beams would see the lighthouse slightly out of its true position; but it is not so. The true rays or actual paths of each disturbance are truly radial; they do not coincide with the apparent beam. An eye looking at the source will not look tangentially along the beam, but will look along A S, and will see the source in its true position. It would be otherwise for the case of projectiles from a revolving turret.

Thus, neither translation of star nor rotation of sun can affect direction. There is no aberration so long as the receiver is stationary.

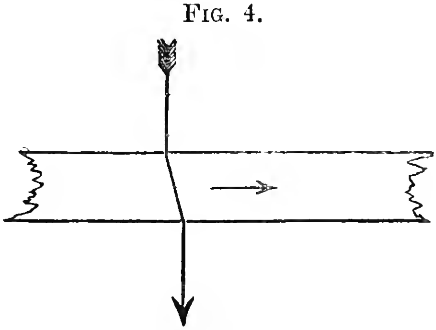
But what about a wind, or streaming of the medium past source and receiver, both stationary? Look at Fig. 1 again. Suppose a row of stationary cannon firing shots, which get blown by a cross wind along the slant 1 A Y (neglecting the curvature of path which would really exist): still the hole in the target fixes the gun's true position, the marker looking along Y A sees the gun which fired the shot. There is no true deviation from the point of view of the receiver, although the shots are blown aside and the target is not hit by the particular gun aimed at it.

With a moving cannon, combined with an opposing wind, Fig. 1 would become very like Fig. 2.

(N.B.—The actual case, even without complication of spinning, &c., but merely with the curved path caused by steady wind-pressure, is not so simple, and there would really be an aberration or apparent displacement of the source towards the wind's eye: an apparent exaggeration of the effect of wind as shown in the diagram.)

In Fig. 2 the result of a wind is much the same, though the details are rather different. The medium is supposed to be drifting down across the field opposite to the arrows. The source is stationary at S. The arrows show the direction of waves *in the medium;* the dotted slant line shows their resultant direction. A wave centre drifts from D to 1 in the same time as the disturbance reaches A, travelling down the slant line D A. The angle between dotted and full lines is the angle between ray and wave movement. Now, *if the motion of the medium inside the receiver is the same as it is outside*, the wave will pass straight on along the slant to Z, and the true direction of the source is fixed. But if the medium inside the target or telescope is stationary, the wave will cease to drift as soon as it gets inside, under cover as it were; it will proceed along the path it has been really pursuing *in the medium* all the time, and make its exit at Y. In this latter case, of different motion of the medium inside and outside the telescope, the apparent direction, such as Y A, is not the true direction of the source. *The ray is in fact bent where it enters the differently-moving medium* (as shown in Fig. 4).

FIG. 4.



Ray through a Moving Stratum.

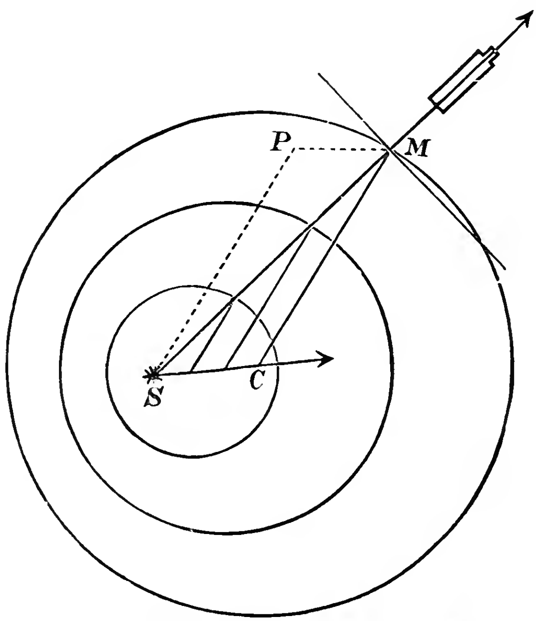
A slower moving stratum bends an oblique ray, slanting with the motion, in the same direction as if it were a denser medium.

A quicker stratum bends it oppositely. If a medium is both denser and quicker moving, it is possible for the two bendings to be equa and opposite, and thus for a ray to go on straight. Parenthetically I may say that this is precisely what happens, on Fresnel's theory, down the axis of a water-filled telescope exposed to the general terrestrial ether drift.

In a moving medium waves do not advance in their normal direction, they advance slantways. The direction of their advance is properly called a ray. The ray does not coincide with the wave-normal in a moving medium.

All this is well-shown in Fig. 5.

FIG. 5.



Successive Wave Fronts in Moving Medium.

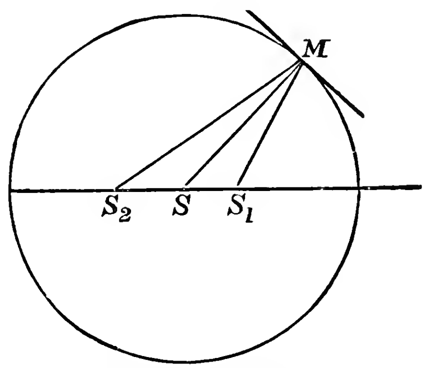
S is a stationary source emitting successive waves, which drift as spheres to the right. The wave which has reached M has its centre at C, and C M is its normal; but the disturbance, M, has really travelled along S M, which is therefore the ray. It has advanced as a wave from S to P, and has drifted from P to M. Disturbances subsequently emitted are found along the ray, precisely as in Fig. 2. A stationary telescope receiving the light will point straight at S. A mirror, M, intended to reflect the light straight back must be set normal to the ray, not tangential to the wave front.

The diagram also equally represents the case of a moving source in a stationary medium. The source, starting at C, has moved to S, emitting waves as it went, which waves as emitted spread out as simple spheres from the then position of source as centre. Wave-normal and ray now coincide: S M is not a ray, but only the locus of successive disturbances. A stationary telescope will look not at S, but along M C to a point where the source was when it emitted the wave M; a moving telescope, if moving at same rate as source, will look at S. Hence S M is sometimes called the *apparent* ray. The angle S M C is the aberration angle.

Fig. 6 shows normal reflection for the case of a moving source. The mirror M reflects light received from S_1 to a point S_2, just in time to catch the source there.

Parenthetically I may say that the time taken on the double journey, $S_1 M S_2$, is not quite the same as the double journey S M S, when all is stationary, and that this is the principle of Michelson's great experiment.

FIG. 6.



Normal reflection.

For the rest of the lecture I am going to call the medium which conveys light, "ether" simply. Every one knows that ether is the light conveying medium, however little else they know about the properties of that tremendously important material.

We have arrived at this: that a uniform ether stream all through space causes no aberration, no error in fixing direction. It blows the waves along, but it does not disturb the line of vision.

Stellar aberration exists, but it depends on motion of observer, and on motion of observer only. Etherial motion has no effect upon it, and when the observer is stationary with respect to object, as he is when using a terrestrial telescope, there is no aberration at all.

Surveying operations are not rendered the least inaccurate by the existence of a universal ethereal drift; and they therefore afford no means of detecting it.

But observe that everything depends on the ethereal motion being uniform everywhere, inside as well as outside the telescope, and along the whole path of the ray. If stationary anywhere it must be stationary altogether. There must be no boundary betweeen stationary and moving ether, no plane of slip, no quicker motion even in some regions than in others. For (referring back to the remarks preceding Fig. 4) if the ether in receiver is stagnant while outside it is moving, a wave which has advanced and drifted as far as the telescope will cease to drift as soon as it gets inside, but will advance simply along the wave normal; and in general at the boundary of any such change of motion a ray will be bent, and an observer looking along the ray will see the source not in its true position, not even in the apparent position appropiate to his own motion, but lagging behind that position.

Such an aberration as this, a lag or negative aberration, has never yet been observed; but if there is any slip between layers of ether, if the earth carries any ether with it, or if the ether, being in motion at all, is not equally in motion everywhere throughout every transparent substance, then such a lag or negative aberration must occur: in precise proportion to the amount of the carriage of ether by moving bodies.

On the other hand, if the ether behaves as a perfectly frictionless inviscid fluid, or if for any other reason there is no rub between it and moving matter, so that the earth carries no ether with it at all, then all rays will be straight, aberration will have its simple and well-known value, and we shall be living in a virtual ether stream of 19 miles a second, by reason of the orbital motion of the earth.

It may be difficult to imagine that a great mass like the earth can rush at this tremendous pace through a medium without disturbing it. It is not possible for an ordinary sphere in an ordinary fluid. At the surface of such a sphere there is a viscous drag, and a spinning motion diffuses out thence through the fluid so that the energy of the moving body is gradually dissipated. The persistence of terrestrial and planetary motions shows that ethereal viscosity, if existent, is small; or at least that the amount of energy thus got rid of is a very small fraction of the whole. But there is nothing to show that an appreciable layer of ether may not adhere to the earth and travel with it, even though the force acting on it be but small.

This, then, is the question before us:—

Does the earth drag some ether with it? or does it slip through the ether with perfect freedom? (Never mind the earth's atmosphere; the part it plays is not important.)

In other words, is the ether wholly or partially stagnant near the earth, or is it streaming past us with the opposite of the full terrestrial velocity of nineteen miles a second? Surely if we are living in an ether stream of this rapidity we ought to be able to detect some evidence of its existence.*

It is not so easy a thing to detect as you would imagine. We have seen that it produces no deviation or error in direction. Neither does it cause any change of colour or Doppler effect; that is, no shift of lines in spectrum. No steady wind can affect pitch, simply because it cannot blow waves to your ear more quickly than they are emitted. It hurries them along, but it lengthens them in the same proportion, and the result is that they arrive at the proper frequency. The precise effects of motion on pitch are summarised in the following table:—

Changes of Frequency due to Motion.

Source approaching shortens waves.
Receiver approaching alters relative velocity.
Medium flowing alters both wave-length and velocity in exactly compensatory manner.

---

* The word "stationary" is ambiguous. I propose to use "stagnant," as meaning stationary with respect to the earth, i. e. as opposed to stationary in space.

What other phenomena may possibly result from motion? Here is a list:—

Phenomena resulting from Motion.

(1) Change or apparent change in direction; observed by telescope, and called aberration.
(2) Change or apparent change in frequency; observed by spectroscope, and called Doppler effect.
(3) Change or apparent change in time of journey; observed by lag of phase or shift of interference fringes.
(4) Change or apparent change in intensity; observed by energy received by thermopile.

Motion of either source or receiver can alter frequency, motion of receiver can alter apparent direction, motion of the medium can do neither; but surely it can hurry a wave so as to make it arrive out of phase with another wave arriving by a different path, and thus produce or modify interference effects.

Or again, it may carry the waves down stream more plentifully than up stream, and thus act on a pair of thermopiles, arranged fore and aft at equal distances from a source, with unequal intensity.

And again, perhaps the laws of reflection and refraction in a moving medium are not the same as they are if it be at rest. Then, moreover, there is double refraction, colours of thin plates and thick plates, polarisation angle, rotation of the plane of polarisation; all sorts of optical phenomena.

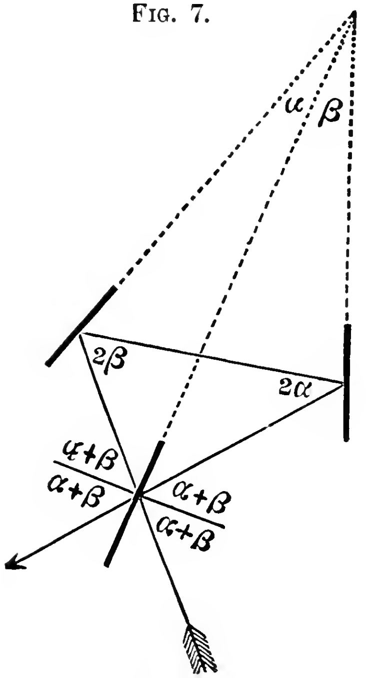
It may be, perhaps, that in empty space the effect of an ether drift is difficult to detect, but will not the presence of dense matter make it easier?

Consider No. 3 of the phenomena tabulated above. I expect that every one here understands interference, but I may just briefly say that two similar sets of waves "interfere" whenever and wherever the crests of one set coincide with and obliterate the troughs of the other set. Light advances in any given direction when crests in that direction are able to remain crests, and troughs to remain troughs. But if we contrive to split a beam of light into two halves, to send them round by different paths, and make them meet again, there is no guarantee that crest will meet crest and trough trough; it may be just the other way in some places, and wherever that opposition of phase occurs there there will be local obliteration or "interference." Two reunited half-beams of light may thus produce local stripes of darkness, and these stripes are called interference bands.

If I can I will produce actual interference of light on the screen, but the experiment is a difficult one to make visible at a distance, partly because the stripes or bands of darkness are usually very narrow. I have not seen it attempted before. [Very visible bands were formed on screen by three mirrors, one of them semi-transparent, as in Fig. 7.]

Now a most interesting and important, and I think now well-known experiment of Fizeau proves quite simply and definitely that if light be sent along a stream of water, travelling inside the water as a transparent medium, it will go quicker with the current than against it. You may say that is only natural; a wind helps sound along one way and retards it the opposite way. Yes, but then sound travels in air, and wind is a bodily transfer of air, hence, of course, it gives the sound a ride; whereas light does not really travel in water, but always in ether. It is by no means obvious whether a stream of water can help or hinder it. Experiment decides, however, and answers in the affirmative. It helps it along with just about half the speed of the water; not with the whole speed, which is curious and important, and really means that the moving water has no effect whatever on the ether of space, though it would take too long to make clear how this comes about. Suffice for present purposes the fact that the velocity of light inside moving water, and therefore presumably inside all transparent matter, is altered by motion of that matter.

Fig. 7.

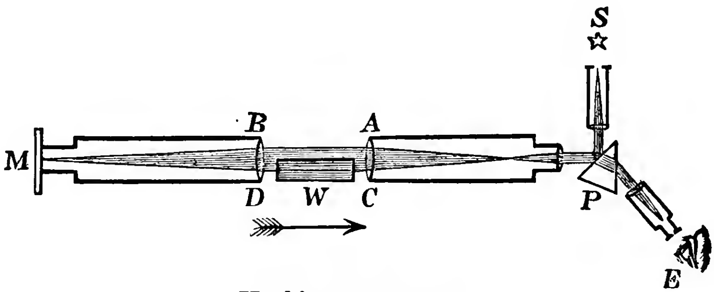


Plan of Interference Kaleidoscope.

Does not this fact afford an easy way of detecting a motion of the earth through the ether? Here on the table is water travelling along nineteen miles a second. Send a beam of light through it one way and it will be hurried; its velocity, instead of being 140,000 miles a second, will be 140,009 miles. Send a beam of light the other way, and its velocity will by 139,991; just as much less. Bring these two beams together; surely some of their wave-lengths will interfere. M. Hoek, Astronomer at Utrecht, tried the experiment in this very form; here is a diagram of his apparatus (Fig. 8). Babinet had tried another form of the experiment previously. Hoek expected to see interference bands, from the two half-beams which had traversed the water, one in the direction of the earth's motion and the other against it. But no interference bands were seen. The experiment gave a negative result.

An experiment, however, in which nothing is seen is never a very satisfactory form of a negative experiment; it is, as Mascart calls it, "doubly negative," and we require some guarantee that the condition was right for seeing what might really have been in some sort there. Hence Mascart and Jamin's modification of the experiment is prefer-

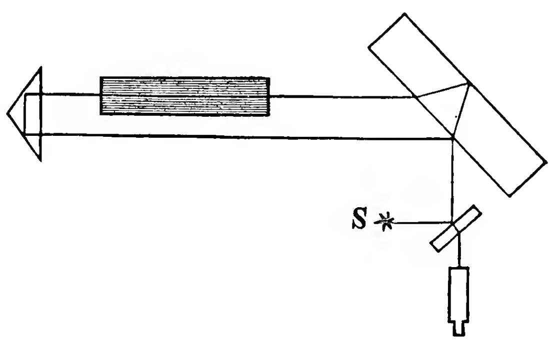
Fig. 8.



Hoek's arrangement.

able (Fig. 9). The thing now looked for is a shift of already existing interference bands, when the above apparatus is turned so as to have different aspects with respect to the earth's motion; but no shift was seen.

Fig. 9.



Arrangement of Mascart and Jamin.

Interference methods all fail to display any trace of relative motion between earth and ether.

Try other phenomena then. Try refraction. The index of refraction of glass is known to depend on the ratio of the speed of light outside, to the speed inside, the glass. If then the ether be streaming through glass, the velocity of light will be different inside it according as it travels with the stream or against it, and so the index of refraction will be different. Arago was the first to try this experiment by placing an achromatic prism in front of a telescope on a mural circle, and observing the deviation it produced on stars.

Observe that it was an *achromatic* prism, treating all wave-lengths alike; he looked at the *deviated* image of a star, not at its *dispersed* image or spectrum, else he might have detected the change-of-frequency-effect due to motion of source or receiver first actually seen by Dr. Huggins. I do not think he would have seen it, because I do not suppose his arrangements were delicate enough for that very small effect; but there is no error in the conception of his experiment, as Prof. Mascart has inadvertently suggested there was.

Then Maxwell repeated the attempt in a much more powerful manner, a method which could have detected a very minute effect indeed, and Mascart has also repeated it in a simple form. All are absolutely negative.

Well, what about aberration? If one looks through a moving stratum, say a spinning glass disk, there ought to be a shift caused by the motion (see Fig. 4). The experiment has not been tried, but I entertain no doubt about its result, though a high speed and considerable thickness of glass or other medium is necessary to produce even a microscopic apparent displacement of objects seen through it.

But the speed of the earth is available, and the whole length of a telescope tube may be filled with water; surely that is enough to displace rays of light appreciably.

Sir Geo. Airy tried it at Greenwich on a star, with an appropriate zenith-sector full of water. Stars were seen through the water-telescope precisely as through an air telescope. A negative result again.

Stellar observations, however, are unnecessarily difficult. Fresnel had said that a terrestrial source of light would do just as well. He had also (being a man of exceeding genius) predicted that nothing would happen. Hoek has now tried it in a perfect manner and nothing did happen.

Since then Prof. Mascart with great pertinacity has attacked the phenomena of thick plates, Newton's rings, double refraction, and the rotatory phenomenon of quartz; but he has found absolutely nothing attributable to a stream of ether past the earth.

The only positive result ever supposed to be attained was in a very difficult polarisation observation by Fizeau in 1859. As this has not yet been repeated, it is safest at present to ignore it, though by no means to forget that it wants repeating.

Fizeau also suggested, but did not attempt, what seems an easier experiment, with fore and aft thermopiles and a source between them, to observe the drift of a medium by its convection of energy; but arguments based on the law of exchanges* tend to show, and do show as I think, that a probable alteration of radiating power due to motion through a medium would just compensate the effect otherwise to be expected.

We may summarise most of these statements as follows:—

---

* Lord Rayleigh, 'Nature,' March 25, 1892.

Summary.

Source alone moving produces	A real and apparent change of wave-length. A real but not apparent error in direction. No lag of phase or change of intensity, except that appropriate to altered wave-length.
Medium alone moving, or source and receiver moving together, produces	No change of frequency. No error in direction. A real lag of phase, but undetectable without control over the medium. A change of intensity corresponding to different distance, but compensated by change of radiating power.
Receiver alone moving produces	An apparent change of wave-length. An apparent error in direction. No change of phase or of intensity, except that appropriate to different virtual velocity of light.

I may say, then, that not a single optical phenomenon is able to show the existence of an ether stream near the earth. All optics go on precisely as if the ether were stagnant with respect to the earth.

Well then perhaps it *is* stagnant. The experiments I have quoted do not prove that it is so. They are equally consistent with its perfect freedom and with its absolute stagnation; though they are not consistent with any intermediate position. Certainly, if the ether were stagnant nothing could be simpler than their explanation.

The only phenomena then difficult to explain would be those depending on light coming from distant regions through all the layers of more or less dragged ether. The theory of astronomical aberration would be seriously complicated; in its present form it would be upset. But it is never wise to control facts by a theory; it is better to invent some experiment that will give a different result in stagnant and in free ether. None of those experiments so far described are really discriminative. They are, as I say, consistent with either hypothesis, though not very obviously so.

Mr. Michelson, however, of Harvard, U.S., has invented a plan that will discriminate; and, what is much more remarkable, he has carried it out.

That it is an exceptionally difficult experiment you will realise when I say that the experiment will fail altogether unless one part in 400 millions can be clearly detected.

Mr. Michelson reckons that by his latest arrangement he could see 1 in 4000 millions if it existed (which is equivalent to detecting an error of $\frac{1}{1000}$ of an inch in a length of 40 miles); but he saw nothing. Everything behaved precisely as if the ether was stagnant; as if the earth carried with it all the ether in its immediate neighbourhood. And that is his conclusion. If he can repeat it and get a different result on the top of a mountain, that conclusion may be considered established. At present it must be regarded as tentative.

I have not time to go into the details of his experiment (it is described in 'Phil. Mag.,' 1887), but I may say that it depends on no doubtful properties of transparent substances, but on the straightforward fundamental principle underlying all such simple facts as that—It takes longer to row a certain distance and back up and down stream than it does to row the same distance in still water; or that it takes longer to run up and down a hill than to run the same distance laid out flat; or that it costs more to buy a certain number of oranges at three a penny and an equal number at two a penny than it does to buy the whole lot at five for twopence.

Hence, although there may be some way of getting round Mr. Michelson's experiment, there is no obvious way; and I conjecture that if the true conclusion be not that the ether near the earth is stagnant it will lead to some other important and unknown fact.

The balance of evidence at this stage seems to incline in the sense that the earth carries the neighbouring ether with it.

But now put the question another way. *Can* matter carry neighbouring ether with it when it moves? Abandon the earth altogether; its motion is very quick, but too uncontrollable, and it always gives negative results. Take a lump of matter that you can deal with, and see if it pulls any ether along.

That is the experiment I set myself to perform, and which in the course of the last year, I have performed.

I take a steel disk, or rather a couple of steel disks clamped together with a space between. I mount it on a vertical axis and spin it like a teetotum as fast as it will stand without flying to pieces. Then I take a parallel beam of light, split it into two by a semi-transparent mirror (Michelson's method), a piece of glass silvered so thinly that it lets half the light through and reflects the other half; and I send the two halves of this split beam round and round in opposite directions in the space between the disks. They may thus travel a distance of 20 or 30 or 40 feet. Ultimately they are allowed to meet and enter a telescope. If they have gone quite identical distances they need not interfere, but usually the distances will differ by a hundred-thousandth of an inch or so, which is quite enough to bring about interference.

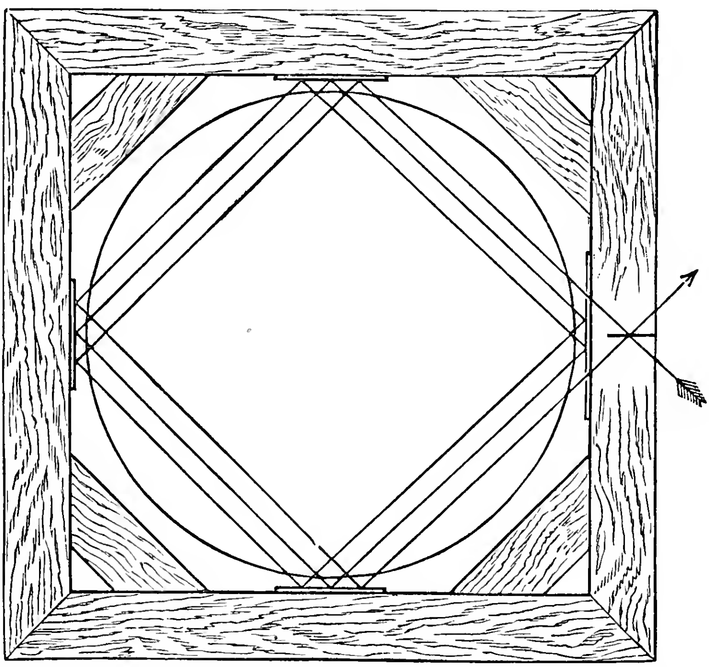
The mirrors which reflect the light round and round between the disks are shown in Fig. 10. If they form an accurate square the last two images will coincide, but if the mirrors are the least inclined to one another at any unaliquot part of 360° the last image splits into two, as in the kaleidoscope is well known, and the interference bands may be regarded as resulting from those two sources. The central white band bisects normally the distance between them, and their amount of separation determines the width of the bands. There are many interesting optical details here, but I shall not go into them.

The thing to observe is whether the motion of the disks is able to replace a bright band by a dark one, or *vice versâ*. If it does, it means that one of the half beams, viz. that which is travelling in the same direction as the disks, is helped on a trifle, equivalent to a shortening of journey by some quarter millionth of an inch or so in the whole length of 30 feet; while the other half beam, viz. that travelling against the motion of the disks, is retarded, or its path virtually lengthened, by the same amount.

If this acceleration and retardation actually occurs, waves which did not interfere on meeting, before the disks moved, will interfere now, for one will arrive at the common goal half a length behind the other.

Now a gradual change of bright space to dark, and *vice versâ*, shows itself, to an observer looking at the bands, as a gradual change of position of the bright stripes, or a shift of the bands. A shift of

Fig. 10.



Plan of Steel Disks, one yard in diameter, and Optical Frame.

the bands, and especially of the middle white band, which is much more stable than the others, is what we look for.

At first I saw plenty of shift. In the first experiment the bands sailed across the field as the disks got up speed until the crosswire had traversed a band and a half. The conditions were such that had the ether whirled at the full speed of the disks I should have seen a shift of three bands. It looked very much as if the light was helped along at half the speed of the moving matter, just as it is inside water.

On stopping the disks the bands returned to their old position. On starting them again in the opposite direction, the bands ought to have shifted the other way too; but they did not; they went the same way as before.

The shift was therefore wholly spurious; it was caused by the centrifugal force of the blast of air thrown off from the moving disks. The mirrors and frame had to be protected from this. Many other small changes had to be made, and gradually the spurious shifts have been reduced and reduced, largely by the skill and patience of my assistant, Mr. Davies, until now there is barely a trace of them.

But the experiment is not an easy one. Not only does the blast exert pressure, but at high speeds the churning of the air makes it quite hot. Moreover, the tremor of the whirling machine, in which some four or five horse-power is sometimes being expended, is but too liable to communicate itself to the optical part of the apparatus. Of course elaborate precautions are taken against this. Although the two parts, the mechanical and the optical, are so close together, their supports are entirely independent. But they have to rest on the same earth, and hence communicated tremors are not absent. They are the cause of all the slight residual trouble.

The method of observation now consists in setting a wire of the micrometer accurately in the centre of the middle band, while another wire is usually set on the first band to the left. Then the micrometer heads are read, and the setting repeated once or twice to see how closely and dependably they can be set in the same position. Then we begin to spin the disks, and when they are going at some high speed, measured by a siren note and in other ways, the micrometer wires are reset and read—reset several times and read each time. Then the disks are stopped and more readings are taken. Then their motion is reversed, the wires set and read again; and finally the motion is once more stopped and another set of readings taken. By this means the absolute shift of middle band and its relative interpretation in terms of wave-length are simultaneously obtained; for the distance from the one wire to the other, which is often two revolutions of a micrometer head, represents a whole wave-length shift.

In the best experiments I do still often see something like a fiftieth of a band shift, but it is caused by residual spurious causes, for it repeats itself with sufficient accuracy in the same direction when the disks are spun the other way round.

Of real reversible shift, due to motion of the ether, I see nothing. I do not believe the ether moves. It does not move at a five-hundredth part of the speed of the steel disks. I hope to go further, but my conclusion so far is that such things as circular-saws, flywheels, railway trains, and all ordinary masses of matter do not appreciably carry the ether with them. Their motion does not seem to disturb it in the least.

The presumption is that the same is true for the earth; but the earth is a big body, it is conceivable that so great a mass may be able to act when a small mass would fail. I would not like to be too sure about the earth. What I do feel already pretty sure of is that if moving matter disturbs ether in its neighbourhood at all, it does so by some minute action, comparable in amount perhaps to gravitation, and possibly by means of the same property as that to which gravitation is due—not by anything that can fairly be likened to ethereal viscosity. [O. L.]

GENERAL MONTHLY MEETING,

Monday, April 4, 1892.

Sir James Crichton-Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

His Grace The Duke of Devonshire, M.A. LL.D.
The Right Hon. Lord Herschell, D.C.L.
Peter Brotherhood, Esq. M. Inst. C.E.
A. H. Brown, Esq. M.P.
W. Le Geyt Dudgeon, Esq. B.A.
George King, Esq.
Allen Lee, Esq.
Sir Joseph Lister, Bart. M.D. D.C.L. LL.D. F.R.S.
Arthur Henry Renshaw, Esq.
George James Snelus, Esq. F.R.S. F.C.S.
Sir George Tryon, G.C.B.
Corbet Woodall, Esq. M. Inst. C.E.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned for the following Donations:—

J. Emerson Dowson, Esq.	£20	0	0
Captain Noble	50	0	0
George Matthey, Esq.	25	0	0
Sir William Bowman, Bart.	5	0	0
Sir Henry Doulton	10	10	0

for carrying on investigations on Liquid Oxygen.

The Chairman reported, That at the Meeting of the Managers held this day the decease of Sir William Bowman, Bart. *M.R.I.* on the 29th of March last, was announced, and that the following Resolution had been passed:—

"Resolved: That the Managers of the Royal Institution desire to express their most sincere sympathy with Lady Bowman in the deep sorrow which has befallen her by the death of their friend and former colleague, Sir William Bowman, and to put on record their sense of the unvarying interest he for over 34 years took in the welfare of the Institution, and their appreciation of the valuable services rendered by him as Manager and as Honorary Secretary."

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—South Indian Inscriptions. By E. Hultzsch. Vol. II. Part 1. 4to. 1891.

British Museum Trustees—Catalogue of Arabic Glass Weights. 8vo. 1891.
Catalogue of the Cuneiform Tablets in the Kouyunjik Collection, Vol. II. 8vo. 1891.

The British Museum (Natural History)—Catalogue of Birds, Vol. XX. 8vo. 1891.
The Madras Government—Madras Meridian Circle Observations, 1871-73. By C. Michie Smith. 4to. 1892.
The New Zealand Government—Statistics of the Colony of New Zealand, 1890. fol. 1892.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quinta: Rendiconti. 1° Semestre, Vol. I. Fasc. 3-4. 8vo. 1892.
Agricultural Society of England, Royal—Journal, Vol. III. Part 1. 8vo. 1892.
American Philosophical Society—Proceedings, No 136. 8vo. 1891.
Astronomical Society, Royal—Monthly Notices, Vol. LII. No. 4. 8vo. 1892.
Bankers, Institute of—Journal, Vol. XIII. Part 3. 8vo. 1892.
Bavarian Academy of Sciences, Royal—Sitzungsberichte, 1891, Heft 3. 8vo.
British Architects, Royal Institute of—Proceedings, 1891-2, Nos. 10, 11. 4to.
Carmichael, C. H. E. Esq. For. Sec. R.S.L.—History of the Royal Society of Literature. By E. W. Braybrook. 8vo. 1891.
Report of Royal Society of Literature, 1890-91. 8vo.
Chemical Industry, Society of—Journal, Vol. XI. No. 2. 8vo. 1892.
Chemical Society—Journal for March, 1892. 8vo.
Editors—American Journal of Science for March, 1892. 8vo.
Analyst for March, 1892. 8vo.
Athenæum for March, 1892. 4to.
Brewers' Journal for March, 1892. 4to.
Chemical News for March, 1892. 4to.
Chemist and Druggist for March, 1892. 8vo.
Educational Review. Vol. I. No. 5. 8vo. 1892.
Electrical Engineer for March, 1892. fol.
Electric Plant for March, 1892. 8vo.
Electricity for March, 1892. 8vo.
Engineer for March, 1892. fol.
Engineering for March, 1892. fol.
Horological Journal for March, 1892. 8vo.
Industries for March, 1892. fol.
Iron for March, 1892. 4to.
Ironmongery for March, 1892. 4to.
Manufacturer's Engineering and Export Journal, Vol. I. Nos. 1-3. 4to. 1892.
Monist for March, 1892. 8vo.
Nature for March, 1892. 4to.
Open Court for March, 1892. 4to.
Surveyor for March, 1892. 8vo.
Telegraphic Journal for March, 1892. fol.
Zoophilist for March, 1892. 4to.
Electrical Engineers, Institution of—Journal, No. 96. 8vo. 1892.
Ex Libris Society—Journal for March, 1892. 4to.
Florence Biblioteca Nazionale Centrale—Bolletino, Nos. 149, 150. 8vo. 1892.
Franklin Institute—Journal, No. 795. 8vo. 1892.
Geographical Society, Royal—Proceedings, Vol. XIV. No. 4. 8vo. 1892.
Institute of Brewing—Transactions, Vol. V. No. 5. 8vo. 1892.
Johns Hopkins University—University Circulars, No. 96. 4to. 1892.
Keeler, James E. Esq. (the Author)—The Star Spectroscope of the Lick Observatory. 8vo. 1892.
Elementary Principles governing the efficiency of Spectroscopes for Astronomical Purposes. 8vo. 1891.
Linnean Society—Journal, No. 198. 8vo. 1892.
Maryland Medical and Chirurgical Faculty—Transactions, 93rd Session. 8vo. 1891.
Medical and Chirurgical Society, Royal—Transactions, Vol. LXXIV. 8vo. 1891.
Ministry of Public Works, Rome—Giornale del Genio Civile, 1892, Fasc. 1. 8vo. And Designi. fol. 1892.

Motti, G. A. M. S. Esq. (the Author)—Risoluzioni della Quadratura del Circolo. 8vo. Pavia, 1892.
North of England Institute of Mining and Mechanical Engineers—Transactions, Vol. XLI. Part 1. 8vo. 1892.
Numismatic Society—Chronicle and Journal, 1891, Parts 3 and 4. 8vo. 1892.
Odontological Society of Great Britain—Transactions, Vol. XXIV. No. 5. 8vo. 1892.
Payne, Wm. W. Esq. and Hale, Geo. E. Esq. (the Editors)—Astronomy and Astro-Physics for March, 1892. 8vo.
Pharmaceutical Society of Great Britain—Journal for March, 1892. 8vo.
Richards, Admiral Sir S. H. K.C.B. F.R.S. &c. (the Conservator)—Report on the Navigation of the River Mersey, 1891. 8vo. 1892.
Richardson, B. W. M.D. F.R.S. M.R.I. (the Author)—The Asclepiad, Vol. IX. No. 33. 8vo. 1892.
Royal Society of London—Philosophical Transactions, Vol. CLXXXII. 4to. 1892.
Proceedings, No. 305. 8vo. 1892.
Saxon Society of Sciences, Royal—Mathematische-Physischen Classe
Abhandlungen, Band XVIII. Nos. 3, 4. 4to. 1892.
Berichte, 1891, No. 4. 8vo. 1892.
Philologisch-Historischen Classe:
Berichte, 1891, Nos. 2, 3. 8vo. 1892.
Selborne Society—Nature Notes, Vol. III. No. 28. 8vo. 1892.
Society of Architects—Proceedings, Vol. IV. Nos. 8, 9. 8vo. 1892.
Society of Arts—Journal for March, 1892. 8vo.
Solar Physics Committee (Department of Science and Art)—Measures of Positions and Areas of Sun-spots and Faculae on Photographs at Greenwich, Dehra-Dun, and Melbourne, 1878-81. fol. 1891.
St. Pétersbourg Académie Impériale des Sciences—Bulletin, Tome XXIV. No. 3. 4to. 1892.
Tacchini, Professor P. Hon. Mem. R.I. (the Author)—Memorie della Societá degli Spettroscopisti Italiani, Vol. XXI. Disp. 2ª. 4to. 1892.
United Service Institution, Royal—Journal, No. 169. 8vo. 1892.
United States Department of Agriculture—Monthly Weather Review for November-December, 1891. 4to. 1892.
Vereins zur Beförderung des Gewerbfleises in Preussen—Verhandlungen, 1892 Heft 3. 4to. 1892.
Wild, Dr. H. (the Director)—Annalen der Physikalischen Central Observatorium, 1890, Theil II. 4to. 1891.
Repertorium fur Metéorologie, Band XIV. 4to. 1891.
Yorkshire Archæological and Topographical Association—Journal, Part 45. 8vo 1892.

WEEKLY EVENING MEETING,

Friday, April 8, 1892.

David Edward Hughes, Esq. F.R.S. Vice-President, in the Chair.

Professor W. E. Ayrton, F.R.S.

Electric Meters, Motors, and Money Matters.

[Abstract deferred.]

WEEKLY EVENING MEETING,

Friday, April 29, 1892.

Sir James Crichton-Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

B. W. Richardson, M.D. LL.D. F.R.S. *M.R.I.*

The Physiology of Dreams.

> "We are such stuff
> As dreams are made of, and our little life
> Is rounded with a sleep."

If we take the word "stuff" as meaning our bodies living and moving in what we consider the activity of consciousness, and if we consider sleep as resembling the infinite repose of the space through which we are being carried by the planet, then truly the great poet is right to the letter. Every one of us is dreaming now. Between that which we are now doing and that which we are doing when we are said to dream in sleep there is this simple difference only, if it be a difference, that in the present or wakeful state the will directs or moves with the phantasy, and that in the dream of sleep the will is passive, neither suggesting nor directing the course of the story.

There is a romance about dreams in sleep; and whoever—poet, novelist, historian, philosopher—would investigate the influence of dreams of sleep on dreams of wakefulness would find subject matter for a life of labour. Some of the mightiest events in the whole of human life, some of the most persistent, have had their origin in the shortest of involuntary, sleeping dreams. Men do not seem to will dreams; dreams come without will, that is to say without being willed consciously, therefore they are dreams; but having come, they do not necessarily pass away when the conscious will returns into activity. Some dreams do—and this is fortunate—but all do not, and it is because some come without being summoned and remain afterwards that they are a mystery. For this reason they were ghostly messengers to the larger part of mankind in days of simple life, when science had no presence, no explanatory reason. Why should dreams come if they are neither wanted, expected, nor called? They must be provoked by messengers unseen and to the world at large unknown. Little wonder that they should have played their marvellous parts, and that, to fervid natures, immortal spirits, gods themselves, should have spoken, as it has been assumed, to men in dreams, when gods and spirits, formulated by the minds of men, were accepted as firmly by belief as if they were veritable fact; as if believing and knowing were one and the same thing, which practically they are in an immense number of matters, even in a modern community that boasts the possession of the Royal Institution itself!

Great men, as well as little, have put their interpretations on dreams according to their cherished beliefs. Hannibal the mighty, with his one eye, wished to steal—I am bound in honesty to use the unvarnished term "wished to steal"—from one of the temples of Juno a pillar of gold which he had drilled into and found to be the metal unalloyed. Thereupon Juno appeared to Hannibal in a dream, and like a virago—she had the credit of being of that stamp even to Jupiter—threatened Hannibal that if he dared to take away the pillar of gold from her temple she would have her reprisal; she would take good care to have his remaining eye removed from the temple of his brain. Juno had the further credit, founded on belief, of always keeping her promises; so Hannibal, a wise and discreet man, did the correct act when he woke out of his dream. He was a great general, a brave general, and a filching general, loving spoil; but his belief in Juno was equal to knowledge, and he was afraid of her, notwithstanding the weakness of her sex. Instead therefore of bearing away the pillar of gold, he had the dust he had drilled out of it made into an ornament—some say a ring, but I should rather say a calf (buculam)—and left it on the top of the pillar, where it will no doubt still be found should the pillar be turned up by some enthusiastic antiquarian.

I have, I confess, a strong dislike to say a word that shall break through any divine enchantment. It seems such a letting down of man from heaven to earth, such a transformation of him from the spiritual to the mechanical, to teach that dreams after all may be nothing more than the common vibrations of terrestrial media acting upon a corporeal vibratorium. Yet so, it is to be feared, the fact stands. There is a string or a wire in tension on some instrument of music. If we were to strike that wire, it would give us a sound which could be determined by our will into a living or rational sound, an extension of ourselves, consciously extended. We might, if we had the skill of a man like Paganini, make that string discourse in beautiful language. We cease to strike it and cease to hear a sound. But the vibration has not ceased. We bring our friend Professor Hughes's microphone into use, and hear still, for a time, the lost sound; a dream on the part of the instrument. All musical instruments dream after we cease to play on them.

We may do something more with the tense string: we may pass over it a current or breath of air. If the breath be sufficient, the instrument will speak out like a thing of life; if it be not sufficient, the instrument dreams; and if we bring the microphone into use, we hear the dream so long as the gentlest vibration is sustained.

If in some part of this room we should make a string emit a sound in harmony with the sound which another string near that could be made to give out, we should set the second into sympathetic vibration, and after we had ceased to hear the sympathetic note by our ordinary sense of hearing, we should be able by the microphone to hear it, for again the instrument would dream. Or, once more, if we were to put into vibration other strings, we could bring out a series of discordant noises, confused, unpleasant, and audible by the microphone, even when we did not appreciate them if I may so say, by the naked sense.

In these similes I have cast a glance at certain vibratory movements which occur and are the causes of some dreams in men, women, and animals. Dreams are vibrations varying from those of the waking dream down to the most delicate and accidental. The sleep that is free from dreams is therefore that which is most clear of any vibratory movement derived either from within the body or from without. Some physiologists have contended that no sleep is quite free from dream, but that many dreams are existent that make no impression on the memory, and are therefore as if they had no existence. Whether this hypothesis be sound or unsound is difficult to say, for although it be true that the memory carries dreams that are so intense as to be like waking dreams, it is also true that the body often wakes with weariness for which there is no accounting except the weariness of dreamings that have left no impression on the memory. Again, acts are sometimes performed in sleep which are not carried in memory—a fact which indicates that active dreaming may leave no story.

The most perfect sleep, the sleep least molested with dreams, is one in which the sleeper sinks into repose and wakes again after several hours, having no more consciousness of lost time than if he had closed his eyes and opened them once more. This is what Wordsworth called "the twinkling of oblivion," and a healthful twinkling it is. It belongs perhaps always to the happiest days of childhood. In a condition of body leading to such an elysium, the balance of all the organic parts is being accurately timed and sustained. There is no jarring chord within the organism to disturb by its own motion; the senses rest too soundly to allow of the conveyance of any vibration from without. If then there be a dream, it must be of the lightest kind, so light that not even the responsive movement of a limb is perceivable. In adult life such sleep is exceptional, and marks out an exceptionally healthy and strong individual, in whom mental placidity balances physical strength, one who even during waking hours is less perturbed than others about him by the clamours of life. I know an individual of this kind who can lay himself down in a Pullman car and sleep from London to Glasgow without being conscious of the journey and without recalling a dream on the awakening—the nearest perfection, perchance, of adult repose in nervous serenity. Such persons are to be envied; they never grow prematurely old; and when they are stricken with disease of a passing kind, they are led to believe with Menander that sleep is the natural cure for all diseases.

There are not many of this dreamless nature; the majority of men and women dream, and some so intently that they work, it may almost be said, as earnestly by night as by day, waking in fact as fatigued as when they resigned themselves to rest. A limited few acquire so much the habit of dreaming, they fail to distinguish between dreaminess and wakefulness. They call themselves insomniacs, declare they never sleep at all, and wonder why it is that after they have risen from what they believe to be a perfectly sleepless night they are able next day, improving hour by hour as the day progresses, to carry out their ordinary labours and duties with fair activity; as if, without knowing it, they had slept, after all, in some part of their nervous organisation.

In referring to these different classes of sleepers and dreamers, I am bordering on the topic of causes of the phenomena of dreams, about which it will be necessary to speak at some length. But it will be most philosophical, before noticing causes, to dwell upon the phenomena themselves, the understanding of phenomena being ever the true preliminary method of divining the cause or causes of them. A grand field opens to me here. I might, for lecture upon lecture, stand in this place reporting stories of dreams, stories written or told by the dreamers themselves. Unfortunately recorded dreams thought out and written out by their authors are too suspicious, as a general rule, to be accepted as reliable authority. The most vivid dreams and the most circumstantial are often the most fleeting, and when the dream seems to be borne in the memory, recollection, which is not the same as memory, fails to supply the true account. The recollection of one memory commingles with other memories, and the general tendency is to put together a mixture of dream and fancy. Paul Richter in his narrative of a dream of the universe, the most magnificent story of its kind ever told, affords illustration of this fact. He had been thinking, he says, of the mighty space of the universe, until, lost in the immensity of the contemplation, he fell asleep and dreamed of an angel coming to him who, ordering him to be stripped of his robes of flesh and divested of his gravitating body, led him through the unfathomable abysses of space until he cried, "Insufferable is the glory of God. Let me lie down and hide myself from the infinite. Angel, I can bear it no longer." But this is not the narrative of a simple dream of sleep; it is the dream of a poet touched with the most refined delicacy; a picture drawn with unmeasured care, conceived as celestial, and beyond the grasp of any but of the imaginative scholar, who can make a new heaven and a new earth out of the little sphere in which he lives; the dream of a constructive Ovid, singing a new and immortal song.

The phenomena of dreams I have to present, though they lack all fancy and are the most commonplace fact, have this advantage: that they are scientifically valid. Throwing aside for my work of observation all fancy, I determined, nearly half a century ago, to make notes of such phenomena of dreams as should in my professional life be told to me while yet the phenomena were fresh in the minds of those who experienced them. When I had, in this manner, collected a goodly number of phenomena, I began to classify them, and in the end I have come to a classification which, although it may be imperfect, is perhaps more nearly perfect than any that has gone before or been recorded in physiological literature. The classification, easily remembered, is set forth under three heads:—

1. There is a dream which is the effect of an external vibration acting upon the sleeper. In this dream there is always an outside object telling upon the sensorium. We might call it the dream of Queen Mab, in which, as you remember, the sleeper is always assailed by the busy Fairy tickling "the parson's nose as he lies asleep," or some other sense of some other sleeper. It is best to describe this as the *objective* dream, the dream in which an external movement or thing excites the phenomena.

2. There is a dream in which, without provocation of an external kind, something progressing in the body of the sleeper himself provokes the phenomena. This I call the *subjective* dream, because it is dependent on changes in the subject who dreams.

3. There is another dream, in which the phenomena are lighted up partly by external vibrations conveyed to the sleeper, and partly by conditions belonging to the sleeper himself which favour the external interference or exalt its influence. This I would call the *compound* dream.

The objective dream is not uncommon, and is, as a rule, most distinctive. It is a dream produced by vibration started from the outside of the body. It is a dream through a sound or through an impression on a sensitive open surface. It is never, I believe, through the sight, a fact probably due to the circumstance that the closed eyelids shut out impressions of sight. This, however, is not all, for I once knew a person who slept with his eyes open, and although he dreamed, he never dreamed as if he had been led to dream from the sight of an object. I have seen, also, a somnambulist with the eyes wide open, but I could never find afterwards that she had seen any object during the sleep, and I am sure it is a mistake to assume that, guided by sight, somnambulists pick their course over obstacles that lie in the way. They put out their hands as if to touch, but they neither see nor hear in the sleep, although they sometimes perform, automatically, feats which convey the idea that their faculties are awake and on the alert.

We are all more or less familiar with the phenomenon of the objective dream. Some experience it from slight causes, and in nearly all the physiological experiments conducted to ascertain the time and duration of dreams the action of an excitement on the sensitive surface of the skin, or on the sense of smell, or on the sense of hearing, has been traceable. Thus the effect of pressure on the body produced by weight, or by pressure on the limbs from the weight of the body itself during sleep, has been found to induce dreams of struggle and wrestle, as if for liberty. The presence of odours has produced excitement, leading sometimes to the most pleasant, sometimes to the most terrible, dream. A friend of mine who was sleeping in a strange house woke one night in a state of actual exhaustion from the deathly struggle of a dream, in which he thought that he was overpowered by birds of prey. His dream carried him to the gardens of the dead in India; and, what was more strange, it carried him to the story of Philip Quail,—a hero of the Robinson Crusoe type,—a story he had not read for many years. He thought the dream must have resulted from indigestion; but it recurred night after night, and at last he began to suspect, from a kind of reasoning that took place in the dream itself, that the down pillow in which his head was half buried was connected with the cause. He inquired into the matter, and at once discovered that the pillow was stuffed with feathers undergoing decomposition and giving forth a peculiar odour so soon as the warmth of his body produced diffusion of the odour. The pillow changed, he was no longer troubled with the dream. All the details of the mystery were also unravelled. The fact of the odour was disclosed, and the story of Philip Quail, from his recollection of it, supplied a similar incident, in that a pillow stuffed with the decomposing feathers of wild birds induced, in Philip, his dream.

Sounds, particularly when they are suddenly brought to bear on the sleeper, produce the strangest dreams. In my early days of professional life the sound of the night-bell, familiar as I was with it, always produced a vivid, but necessarily brief, dream—for I was up in a minute—that varied with the manner of the messenger who rang the bell. A violent clang led once to the idea of a fall from a height, with noise of thunder, as if I were falling down on an avalanche; a series of gentle ringings led me back to school days, when, at the close of term, I took part with my companions in a peal on the bells of the village church, and that so vividly, I seemed to have the rope of the tenor bell still in my hand.

In these objective dreams, expectation and long watching play an important part; the briefest dream may then become the intensest and most real. A few seconds of sleep—nine winks, literally—will come over the wearied watcher, and afford, from the effect of some outer impression, a dream of long duration, with events occurring in it of the most striking nature. Such dreams have deceived many a sleeper, and have led him, unconscious of the transient "twinkle of oblivion," to give to the world tales of manifestations made to himself that have not merely sounded like miracles, but have acted like miracles, in their after influence. The Hegira itself might count amongst dreams of this nature.

In these brief objective dreams it would seem as if one or more of the senses may be awake whilst the others sleep. My learned and finely observant friend Dr. Hack Tuke dreamt that a gentleman called at his house and stood at the front door. He saw the gentleman from the window, and as no one let him in, he (the dreamer) went to the top of the kitchen stairs, where there was a bell, which he shook forcibly; the bell moved, and the tongue of it moved, but no sound proceeded from it. The visual centre was dreaming; that of hearing was dead asleep.

The subjective dream, the result of some vibration within the body of the sleeper, is the dream of indigestion, of pain, of fever, of self-investigation or of self-contention.

The dream of indigestion is a dream always of trouble, of fear, of anxiety, of depression. It is, occasionally, attended by the distressing phenomenon of nightmare, *incubus*, with its difficult breathing, sense of weight on the chest, palpitation, intermittency, or temporary stoppage of the circulation, and a feeling of some terrible blackness or thunder-cloud over-shadowing life, a coldness of body, and an awakening in a severe alarm, as if death itself were impending and inevitable. The disturbed dreams of childhood are often of this alarming nature, and many a peculiarity of character, many a superstitious fear of later life, is implanted into sensitive natures by frequently repeated dreams of this order, and especially when, at the early term of life, the surroundings each day are of sombre cast, leading during waking hours to gloomy thoughts, forebodings, and strained contritions for trifling and imaginary offences, against morals, doctrines, or habits, breeding mental doubts and terrors. These subjective dreams are mental discords, disturbances between the voluntary and involuntary nervous systems, which, extending by reflected vibrations from one centre to another, bring all the centres of sensation during the dream into confusion and clamour, until perfect consciousness, with return to common life, is restored.

The subjective dream of pain differs from the dream of disturbance. The dream of pain is the continuance of pain through sleep. It differs according to the character of the pain. After burns, in which vibration at the seat of injury is most intense and continuous, and during which sleep, if it be not artificially induced, comes on from sheer weariness, the dream is literally one of fire and flame. The dream of toothache or of neuralgia is of accident, of struggle attended with a fall, a crush, a blow, or a stab.

Fever leads to a variable dream—variable, I believe, according to the varying degrees of febrile heat. The active dream, when fever is high, is one of rambling exaltation of mind, followed by depression when fever is low, like the flow and ebb of a tide, from the sensorial organs being, from time to time, at different tensions. The fever dream is attended by sensations which extend through the whole of the body, as if the common sensibility were appreciable by touch, with concentration of the sensibility in particular parts. I remember this experience in the course of a nervous remittent to which I was subjected in early life, and I have often heard repetition of the sensation from others. The most remarkable illustration of the kind is one I have published in the 'Asclepiad' for 1884, pages 294–5, and which will be found in the library. In this instance a most intelligent observer suffering from enteric fever reported to me the recollection of his dreams so soon as he had sufficiently recovered to undertake the task. His dreams were, to those who observed him, deliriums in sleep; but to him it seemed that his brain was passive to everything external. He was aware that any voluntary activity would be at the expense of his vital centres, on which he depended for life; whilst at the same time, there was an intelligent connection going on between those parts, so that each could feel what the other was thinking of. After long trouble in this manner, he suddenly woke up in a state of profuse perspiration, and with that the dreams passed away. He was exhausted in the extremest degree, but he felt, as he expressively and graphically described it, a "tingling feeling of life," whereupon he felt he had taken a turn and would live through the disease.

Amongst these subjective dreams there is one which accompanies extremest exhaustion: a state when the body lies between life and death, when the consciousness of all worldly things has faded away so completely that neither words nor acts nor persons are any longer recognisable, but when movements, sights, and sounds are translated in exaltation perhaps through every sense, and certainly through the senses of sight and hearing. I had occasion once to see a gentleman in this condition of temporary death produced by a mechanical arrest of the circulation of the blood in the outgoing current of blood from the heart. Nothing except the dislodgment of the obstructing cause could save life, and hope of this relief was practically given up. The condition was that of a person rapidly passing away in a struggling dream. By a change—I had almost said an accident—which would not occur once in a thousand instances of this nature, the cause of obstruction did suddenly become loosened and carried away, with almost instant return of consciousness, followed ultimately by complete recovery to a life afterwards prolonged, in fair health, for twenty-two years. From the lips of this gentleman, whilst yet his memory was fresh, I took down the particulars of his dream. All the world had faded from him, and he had not the least knowledge that I had visited him in his sleep and had endeavoured to rouse him to consciousness. He was by profession a farmer, a fact which accounts for much of what he experienced through a long and terrible dream, in which he had been struggling with all kinds of imaginary foes, human and animal: with burglars, horses, and bulls, that got away from him after he had conquered them, allowing him to go to sleep again, and then returning to the contest. These were the causes of the struggles we had witnessed as he lay in the sinking sleep so near to actual dissolution. I asked him how he struggled, and he repeated to the letter the unconscious struggles we had witnessed.

There is a subjective dream occurring mostly amongst dyspeptics, and which may be considered a dream of regret or of despair. It is a melancholic dream, in which, in the most mysterious manner, conscience, as the dreamer may explain, seems to whisper some strong and yet unintelligible message, or in which some indescribable doubt is conjured up, with so much effect that when the dreamer first awakes he feels as if an insuperable difficulty, which quickly clears away, lay straight before him. This is the dream of apprehension, and is nothing more than an attack during sleep which visits many dyspeptic persons in their waking hours, and which seems to suggest the idea of panic, of an impending anxiety or danger, for the occurrence of which there is no conceivable reason. Allied to the same dream is another of desire to escape from an imaginary pain, peril, or pressure, a desire well expressed in the despairing words, "Oh that I had the wings of a dove! for then would I fly away and be at rest." I knew one subject of this dream who was often heard to mutter portions of these words while asleep, and who frequently woke with them on his lips, saying them audibly to himself.

Lastly, under this head of subjective dreaming, there is what may be designated the dream of contention, in which, in Pauline paraphrasing from philosophy, the flesh seems to war against the spirit, and the spirit against the flesh, so that the dreamer is bound in bonds, unable to do the things that he would, a very mischievous dream in the history of human action. Some dreams have counterparts, but the dream of contention has none. The most conceited dreamer rarely, if ever, awakes from a dream assured by it of his own self-importance, a discount on dreaming which is good all round.

The compound dream, in which subjective phenomena combine with external impressions, is the most common dream. The impression is often made during the period of dreaming, but it may be made before going to sleep, or it may be revived in the period of sleep, and it may take part in the phenomena of the dream. Vibration of motion is in this manner conveyed. After being on the sea for a time, the rocking or vibration of the vessel may be communicated to the body so strongly that afterwards, during sleep on land, the bed seems to take the motion of the vessel, and the sleeper wakes from a realistic dream astonished to find himself once more on *terra firma*. This is a commonplace illustration, but many more are at hand, one of which I will relate, because it bears particularly on a physiological point at which I shall arrive at a later stage. In 1884 Mr. Edward Payson Weston, the pedestrian, put himself under the feat of walking five thousand miles in one hundred days—50 miles per day. He was so finely trained that on the last day of the trial he walked from Brighton to London without once sitting down or ceasing to travel. He placed himself the while under scientific observation, in which I was allowed to take part; and one of the questions I kept in view was the dreams he experienced after walks in all weathers and on all kinds of roads from the beginning to the end of his task. In all the time he could recall no dream; in fact, he fell asleep at once, and woke each day from dreamless sleep, one of the decisive aids in the accomplishment of his success. But—and here comes the fact I wish to emphasise—in a later feat he did the walking in a circle, on a path in which there were many laps to a mile. Then dreaming began.

As he sank into repose those laps recurred in a painful form, as if he were making somersaults. He would wake up under this dream, and through all the ordeal experienced the same interruption. One day he told me he could not get it out of his mind that on the previous night he really did make a somersault, and that although he knew it was impossible, because he neither got out of bed nor raised himself, and because he felt sure that the bedstead was not likely to play a trick of the sort, yet nothing could clear his mind from the sensation that he had veritably turned a forward somersault, not once, but several times before he awoke to consciousness. More striking still, he felt, after he awoke to consciousness, that same muscular strain and momentary fatigue which would follow exercise of the kind named. In this instance the impression was, so to speak, set in the body before going to sleep; then came a dream which would, ordinarily, have been forgotten, but in the course of which the impression was liberated perhaps, nay probably, by a muscular movement of the body—raising the head on the pillow, for instance—and the phenomena of the somersault were developed.

The compound dream is frequently a continuation of a waking dream. We go to sleep with something on the mind, and either the mental labour continues, or, after a short rest, the subject of contemplation returns and is, or seems to be, sustained until awakening. Two very different lines of mental work are thus sustained: the purest reasoning, the mathematical, and the purest imaginative. I knew a scholar who after a dreary night, in which he slept well physically, woke in the morning with different and difficult problems quite solved, but mentally worn out and ready only for some active sport. I know another scholar who, engaged in a work of imagination, and thinking himself to sleep at the junction of cross roads of thought, has often risen in the morning well advanced on one of them, and inspired to write, as fast as his pen would let him, a passage or a poem.

The compound dream is often one of memory. The memory may be of things recent, but more frequently it is of things, events, or persons, remembered from long ago, but impressed and fixed in the mind. Dreams thus become, as it were, reminders of the past. I inquired of a blind person who had an extensive knowledge of other persons in his own unhappy condition whether he or they ever had dreams of visible things. He explained to me that he had, and that some like himself had, but that they always were dreams of objects with which the mind had become familiar before the loss of the sense of sight. These are dreams of memory purely, dreams called up by current circumstances that have led to trains of thought connected with the condition of former life when sight aided comprehension. I notice that the prince of physiological writers, Muller, relates that the distinguished Huber, who had been blind from his eighteenth year, in his sixty-sixth year still dreamed of objects which appeared distinctly visible to him, though these dreams referred to the time at which he was possessed of vision. In a similar manner the deaf occasionally dream of the sounds they heard before the period when deafness was established in them; and I remember an instance in which a child, who was dumb, in consequence of his becoming quite deaf in his second year, would call out in his dreams words which he never pronounced when awake.

The compound dream is now and then one of suspicion which may attach itself to some particular person who is being dreamed about. This, I have no doubt, is the true explanation of the dream of a woman that Corder was the culprit in the case of the murder of Maria Martin, the victim of what was called the "Red Barn Murder." Here a process of reasoning goes on in the dream, in the course of which many circumstances combine, as in the construction of circumstantial evidence. Such dreams have been considered as revelations, and received as manifestations of special character and importance.

Again, the compound dream may be one of hope or of fear, and, according to its nature in these respects, may lead to conclusions which seem like predictions, but which are nothing more than reasonings from possible or probable data. Once in some thousands of such instances the conclusion drawn may in some degree prove correct, as it might if it had been arrived at in wakefulness; and, thereupon, the importance of the dream has been magnified to the last degree.

Amongst the class of compound dreams, we have to take in all those which are developed when sleep has been artificially induced by the action of the narcotic series of chemical bodies, such as opium, Indian hemp, mandragora, chloroform, chloral, methylene, amylene, the ethers, carbonic acid, mercaptan, alcohol, coal gas, and other narcotising substances. Some years ago I wrote a paper on this topic in which I showed that each distinctive substance produces its own peculiar dream, probably from the effect it has over the action of the heart, as well as from the direct effect exerted on the sensorium. The subject is rich in interest; but I must not dwell upon it beyond the relation of one or two facts.

I found that the vapour of the substance called amylene induced a sleep attended with a dream which might be called somnambulistic, during which acts the most natural were performed by the sleeper without consciousness of them or remembrance of them afterwards, the consciousness being, for the time, so obliterated that a painful surgical operation called forth no expression of pain or anxiety.

In investigating the action of another sleep-producing vapour, methylic ether, I discovered a still more curious dream; one, namely, in which the dreaming person would perform acts under the direction of the observer while quite unconscious of pain and without after-remembrance of that which had occurred during the sleep. This was a kind of artificially induced hysteria, not unlike the condition known as hypnotism presented by very susceptible individuals.

There is a medicinal plant which in the days of the Greek physicians, and from them up to the thirteenth century, was used as a narcotic. It was called Mandragora. Dioscorides gave a formula for making a wine, which got the name of *Morion* or death-wine, by steeping the root of the plant, Atropa Mandragora,—a plant which grows in the islands of the Grecian Archipelago, and is akin to our Atropa Belladonna, or deadly nightshade—in wine. Morion, or death-wine, when swallowed was capable of producing such a deep sleep that it veritably led to the phenomena of "shrunk death." This was the wine Juliet took. It caused a dream from which the sleeper awoke with screams and terror. They who by habit drank this substance uttered shrieks on waking, and from that probably came the idea, ridiculous enough, that the plant itself shrieked when it was torn from the earth. I made this wine some years ago, and found that its properties were as stated. It produces its own special dream, a dream, even in lower animals that are susceptible to its influence, of anxiety, excitement, and alarm, ending in sudden return to consciousness in the midst of excitement.

A dream of a special and remarkable kind is induced by cannabina, the active principle of Indian hemp. My friend the late Professor Polli, of Milan, who experimented on himself with cannabina, found that the dream was recurrent; and the peculiarity of it was that within a brief period, even of a few seconds, events occurred to the mind that seemed actually to occupy an eternity of time. The facts gave direct experimental proof of the theory that in dreams time plays no important part in the phantasy, but that a whole lifetime of story may be compressed into a minute of existence.

From the study of the phenomena of dreams we may pass now to that of causes of the phenomena, why we dream and wherefore?

In considering this question with the scientific spirit we are led to see that the dream is a pure physical phase of life; that it depends on two conditions: our own corporeal organisation and the state, for a time, of the surroundings of our life. There is in it no more mystery, no more prescience, no more power, than there is in the dream of the wakeful day. We dream according to our nature, our habit, and our environment. Hannibal dreamt of Juno: he could not have dreamt of the Virgin, because he did not know of her; a good Catholic, in these days, would never dream of Juno, because she is to him a non-entity, but he might dream reverently of her who, to him, is both Queen and Mother. And so with all else in the way of dream; it is a partial mental activity combined with more or less complete physical repose.

The seat of dreaming is in the locked-up closet of mental impressions, the brain and spinal column, commonly called the cerebro-spinal centre, the absorbing centre of vibrations from the surrounding universe, the retainer of those vibrations, and the sender forth of them by the energy employed in thought, deed, and word.

In the course of its vital activity this nervous centre wears out from its motor work, its power of keeping the great muscles in play, more decisively than from mental labour. So the muscular eyelids droop, and the limbs fail, and all except the vital involuntary movements of the heart and breathing muscles sink into rest for vital repair of their own structure, whilst the central battery, to use a simile as distinct from an identity, recruits itself. In this "twinkling of oblivion," the sentinels, the senses of sight, hearing, smell, taste, touch, common sensibility, which through their nervous cords pulsate the impressions they receive to their centres, more or less cease to vibrate. The eyes close first, for if they did not, sleep would be well-nigh as impossible as it is in the fish, which never seems to sleep; the ear reposes less readily, for if it did not, we should be exposed to many dangers we are aroused from by noise; the sense of odour, having its origin in the open nasal cavity, never closes, a fact which accounts for odours having so powerful an influence in dreams; the sense of taste closes; that of touch, situated in the finger tips, is in abeyance, but the common sensibility from the nervous expanse of the skin and mucous membranes is ever imperfectly closed, a fact which accounts for pressure and movement causing dreams or actual awakenings to full life. Thus between the outer world and the great centres of thought and feeling there are vibrations even in sleep, and when these reach their central points there is wakefulness, activity there, and that imperfect argument which we call dream, in which some centres are more or less active and some more or less absolutely passive, as if, for the time, dead.

These explanations of communication betwixt the outer and inner world of man account for the objective dream; but there are other communications, more personal if I may so express myself, which deserve to be considered. We have two nervous systems: one our own, by which we will and do; the other nature's, which goes on with our vital work whether we will or no. When we lay open the nervous casket, we see, as now on the screen, two brains, cerebrum and cerebellum, with their spinal cord and nerves communicating with sensitive surfaces and with muscles, all under our own rule and governance. But there is the other system, belonging to nature, centred within the trunk of the body, not in the closed box of the skull and spinal column, but in the line of the great viscera, to which its nerves are distributed, and in which it communicates with the nerves of the cerebral system, which are our own. In this second system lies the governance of the heart, of the digestive organs, of the breathing organs, to a considerable extent, and of the great secreting glands. How extensive this second nervous distribution is can only be understood when it is fully laid out before us in dissection, or in this faithful picture before us. In one set of organs alone, those concerned in digestion, such a view conveys, at a glance, the richness of the supply of these involuntary nerves. Strangely also, these organic nerves combine with a nerve that wanders down to them from the cerebrum itself. Vesalius, the first great anatomist, traced this wandering nerve, or *par vagum*, and depicted it, not knowing of the organic nerves with which it comes into communion. Dissected out, this true wanderer runs, as you will observe, to the larynx, the œsophagus, the heart, the stomach, conveying intelligence to the brain of any local disturbance in those organs, and rousing up the great nervous centres to exert themselves, and by a reflex notice to the muscles, force the muscles to do their best to remove any intruding cause of evil.

We have not yet finished. If we follow those filaments of organic nervous centres which are not our own and have nothing to do with our royal wills, we find them accompanying the arteries which carry the vital blood to the extremest destination of the blood-serving or arterial system, governing those vessels up to the minutest twig, and, as the late Sir Thomas Watson simply but finely defined it, regulating the supply of blood to every part, as a gas tap regulates flame, so that the arterial vessels, in senseless pulsation during our lives, are quickened, or slowed, by insensible direction, into various stages, from the surface redness of rage to the pallor of death.

In these mechanisms we see the origin of the subjective dream. That indigestion, perturbation in the richly nerved digestive organs, should lead to the transmission of vibrating and startling messages to the mental centres, is simple truth enough; that fever should excite, and that every influence or disturbance—friction, distension, heat—should disturb, in parts, the sensorium and conjure up a phantasy, is no longer a mystery. It is a phenomenon that must be.

Touching the effect of external influences I have one word more to add. Warmth of the air breathed by the sleeper favours dreams, while coldness of the air disfavours them. Hence the vivid, brilliant dream of the Asiatic, the dull dream of the Northern blood. I have been assured by one eminent Arctic explorer, the late Sir Edward Belcher, that the Esquimaux do not know what dreaming means, and our distinguished colleague Dr. Rae, in a letter I have before me, says he does not recollect hearing of the phenomenon, although he thinks such an imaginative people as the Esquimaux may dream.

The reference to the effects of cold brings me to an experimental demonstration bearing on motor and on sensory dreams. We all know from our common experiences that there are in us two powers: one of mind, the other of motion. If I were to enter fully into this experience I might be able to prove that the two powers are essentially one. It is enough now to explain, from what has preceded, that in the dream the motor powers sleep, as a rule, uninterruptedly, whilst the mental may be dreaming actively. We have, however, seen exceptions to this rule; and I may add that we can bring out such exceptions by experiment.

In some of my early experiments on the effects of extreme cold on nervous function, I found that the centres of nervous action could be reduced to such inertia by cold that deepest sleep was inducible, sleep leading to unconsciousness and perfect repose of all parts save those which are under the influence of the organic nervous ganglia and their fibres. Soon afterwards Dr. Weir Mitchell, of Philadelphia, and I, simultaneously and independently discovered that by putting different centres to sleep by cold of different intensities we could produce variations of motion, by influencing the motor parts of the brain that balance each other. For example, we found that in birds the cerebellum, which in full activity impels the body to forward movements, is balanced by two great ganglia in the fore part of the cerebrum, called by the old anatomists the corpora striata, and that if the cerebellum be put to sleep the body makes backward somersaults; while if the ganglia in the fore part of the cerebrum be made to sleep, the body is impelled forward in a similar mode of motion. There was thus produced one of the same conditions which Mr. Weston experienced in his dream after walking many short laps. He had wearied his cerebral centres by the constant jerk he encountered in his long exercise on the short circuit; and when he passed into sleep, his propelling centre, the cerebellum, which in its action had become almost automatic, seemed to force him forward imperatively. In this way dreams often become automatic where balance is not correct. In some persons a kind of sudden dream occurs when they look down a steep height; the controlling centre in the brain is for the moment overpowered, while, the propelling centre continuing unaffected, the danger is occasionally realised, of precipitation into the space below.

This is the dream of the motor centres in a state of broken balance, but there is a dream also of the reasoning centres due to broken balance in them, in which one nature in man seems to struggle with another, as if indeed two were contending, a dream of weariness and strife, such as is commonly present in the unhappy during waking dreams, as well as in dreams of sleep. I have called this the dream of contention. The break of balance in this instance is between the two hemispheres of the brain, which, like the two hands, the two eyes, the two lungs, are independent organs, and which, as Dr. Wigan taught nearly fifty years agone, are by their independency the cause of the dual nature of the mind. If these hemispheres are closely akin in function, either for strength or weakness, we have the evidence of the single mind for strength or for weakness. But in very few is there such equality: in the majority of persons one hemisphere is strong, the other less strong, the stronger ruling until it is so wearied that it gives way to the feebler. The apparent contradictions of human nature are readable, by this key, in the dream of contention, dream of resolution, of contrition, of remorse, in some cases of confession of real or imaginary offences against common or moral law. The sleeping dreams of the insane, like their waking dreams, are specially of this character, and afford the best insight we have into the meaning of what is called the unbalanced or insane mind.

My reading of dreams, their phenomena, and their causes would be incomplete were I not able to draw from the study some useful practical lessons. We can draw many such, and here are a few.

Dreams are all explainable on physical grounds; there is no mystery about them save that which springs from blindness to natural facts and laws. We make our dreams as we make our lives. They are reflexes of that which we take into our organisation.

Absence of dream in sleep is a sign, all other things being natural, of sound health physically, mentally, and morally.

Dreams occurring in childhood are, invariably, signs of disturbed health, and should be regarded with anxiety. If they are subjective they indicate derangement of body; if they are objective they tell of some mischief to the developing mind.

A night of dream relating to events of the day is a sure sign of mental overstrain; and the dream of continuation of mental work is a sign of danger which should never be disregarded. It becomes very quickly automatic in its course and injurious in its effect.

Dreams are a cause of mental weariness extending into waking hours, and when that fact is experienced the grand remedy is exercise of body. Exercise calls into play the centres of motion which have rested; and whilst they, with new associations, are in play, the mental centres rest and recuperate, the truest *re*-creation.

It is an open question whether a dream ever leads to permanent disturbance of mental equilibrium, that is to say insanity. Dr. Hack Tuke has supplied me with a history which gives colour to an affirmative view of this question. I am uncertain on the point; but I am certain that every circumstance leading to dreams should be removed from persons of unbalanced mind.

To avoid wearying and wearing dreams, all objective influences which excite the mental centres should be under control. Sleep, in short, should be in the most noiseless atmosphere, where thieves break not in and steal the repose. How shall a man sleep dreamlessly who, by excitement of any kind for finding sleep, makes those arteries on which his brain is built beat two beats to one, hammering away at his senses and putting on them ten, fifteen, twenty foot-tons of pressure from the heart, as if it were a good experiment to find how quickly the delicate brain structure may be beaten into a solidity which natural vibration shall fail to call into natural function?

In this temple of science it is our business to converse with the universe, using experiment as our interpreter. If we cannot explain we confess we are ignorant, and must remain ignorant until time and circumstance bring insight. If we acquire knowledge we must speak of it with so much understanding as is given to us to plane and square and fit it into a shape that is understandable. Beyond this we cannot pass. Pardon me therefore if I have ventured to-night to speak of the "stuff" that dreams are made of on physical principles, and none other. It is the spirit of our craft, and must be implicitly obeyed.

[B. W. R.]

ANNUAL MEETING,

Monday, May 2, 1892.

Sir James Crichton-Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

The Annual Report of the Committee of Visitors for the year 1891, testifying to the continued prosperity and efficient management of the Institution, was read and adopted. The Real and Funded Property now amounts to above 83,000*l.* entirely derived from the Contributions and Donations of the Members.

Fifty new Members were elected in 1891.

Sixty-three Lectures and Twenty-one Evening Discourses were delivered in 1891.

The Books and Pamphlets presented in 1891 amounted to about 248 volumes, making, with 517 volumes (including Periodicals bound) purchased by the Managers, a total of 765 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer, and the Honorary Secretary, to the Committees of Managers and Visitors, and to the Professors, for their valuable services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:

President—The Duke of Northumberland, K.G. D.C.L. LL.D.
Treasurer—Sir James Crichton-Browne, M.D. LL.D. F.R.S.
Secretary—Sir Frederick Bramwell, Bart. D.C.L. F.R.S. M. Inst. C.E.

Managers.

Sir Frederick Abel, K.C.B. D.C.L. F.R.S.
Captain W. de W. Abney, R.E. C.B. D.C.L. F.R.S.
George Berkley, Esq. M. Inst. C.E.
Shelford Bidwell, Esq. M.A. F.R.S.
Joseph Brown, Esq. Q.C.
Arthur Herbert Church, Esq. M.A. F.R.S.
Sir Andrew Clark, Bart. M.D. LL.D. F.R.S.
Sir Douglas Galton, K.C.B. D.C.L. LL.D. F.R.S.
The Rt. Hon. Lord Halsbury, M.A. D.C.L. F.R.S.
William Huggins, Esq. D.C.L. LL.D. F.R.S.
David Edward Hughes, Esq. F.R.S.
The Rt. Hon. Lord Kelvin, D.C.L. LL.D. Pres.R.S.
Hugo Müller, Esq. Ph.D. F.R.S.
John Rae, M.D. LL.D. F.R.S.
William Chandler Roberts-Austen, Esq. C.B. F.R.S.

Visitors.

Thomas Buzzard, M.D. F.R.C.P.
Michael Carteighe, Esq. F.C.S.
Andrew Ainslie Common, Esq. LL.D. F.R.S. F.R.A.S.
James Farmer, Esq. J.P.
Robert Hannah, Esq.
George Herbert, Esq.
Donald William Charles Hood, M.D. M.R.C.P.
James Mansergh, Esq. M. Inst. C.E.
Lachlan Mackintosh Rate, Esq. M.A.
John Callander Ross, Esq.
Arthur William Rücker, Esq. M.A. F.R.S.
Sir David Salomons, Bart. M.A. F.R.A.S. F C.S.
John Bell Sedgwick, Esq. J.P. F.R.G.S.
John Isaac Thornycroft, Esq. M. Inst. C.E.
Robert Wilson, Esq. M. Inst. C.E.

WEEKLY EVENING MEETING,

Friday, May 6, 1892.

Sir Frederick Bramwell, Bart. D.C.L. F.R.S. Honorary Secretary and Vice-President, in the Chair.

Captain W. de W. Abney, C.B. R.E. D.C.L. F.R.S.

The Sensitiveness of the Eye to Light and Colour.

There may be some here who have had the pleasure—or the pain—of rising very much betimes in a Swiss centre of mountaineering in order to gain some mountain peak before the sun has had power enough to render the intervening snow-fields soft, or perhaps dangerous. Those who have, will recollect what were the sensations they experienced as they sallied out of the comfortable hotel, after endeavouring to swallow down breakfast at 2 a.m., into the darkness outside. Perhaps the night may have been moonless, or the sky slightly overcast, and the sole light which greeted them have been the nervous glimmer of the guides' lanterns. By this feeble light they may have picked their way over the stony path, and between the frequent stumbles over some half hidden piece of rock lying in the short grass they may have had time to look around and above them, and notice that the darkness of the night was alone broken by stars which gave a twinkle through a gap in the clouds, or if the sky were cloudless, every star would be seen to lie on a very slightly illuminated sky of transparent blackness. Although giant mountains may have been immediately in front of them, their outlines would be almost if not quite invisible. As time went on the sky would become a little brighter, and what is termed the *petit jour* would be known to be approaching. The outlines of the mountains beyond would become fairly visible, the tufts of grass and the flowers along the path would still be indistinguishable, and most things would be of a cold grey, absolutely without colour. The guide's red woollen scarf which he bound round his neck and mouth would be black as coal. But a little more light, and then some flowers amongst the grass would appear as a brighter grey, though the grass itself would still appear dark; but that red scarf would still be as black as a funereal garment. The mountains would have no colour. The sky would look leaden, and were it not for the stars above it might be a matter of guesswork whether it were not covered over with cloud.

More light still, and the sky would begin to blush in the part where the sun was going to rise, and the rest would appear as a blue-grey; the blue flowers will now be blue, and the white ones white; the violet or lavender coloured ones will still appear of no particular colour, and the grass will look a green grey, whilst the guide's neck-gear will appear a dull brown.

The sun will be near rising, the white peaks beyond will appear tipped with rose; every colour will now be distinguished, though they would still be dull; and, finally, the daylight will come of its usual character, and the cold grey will give place to warmth of hue.

But there may be others who have never experienced this early rising, and prefer the comfort of an ordinary English tramp to that just described; but even then they may have felt something of the kind. In the soft autumn evening, when the sun has set, they may have wandered into the garden and noticed that flowers which in the daytime appear of gorgeous colourings—perhaps a mixture of red and blue—in the gloaming will be very different in aspect. The red flowers will appear dull and black; a red geranium, for instance, in very dull light, being a sable black, whilst the blue flowers will appear whitish-grey, and the brightest pale yellow flowers of the same tint; the grass will be grey, and the green of the trees the same nondescript colour. A similar kind of colouring will also be visible in moonlight when daylight has entirely disappeared, though the sky will have a transparent dark blue look about it, approaching to green. These sensations, or rather lack of sensations of light and colour, which as a rule attract very little attention, as they are common ones, are the subjects of my discourse to-night.

Experiments which can be shown to a large audience on this subject are naturally rather few in number, but I will try and show you one or two.

We are often told that the different stages of heat to which a body can be raised are black, red, yellow, and white heat, but I wish to show you that there is an intermediate stage between black and red heat, viz. a grey heat. An incandescence lamp surrounded by a tissue paper shade, has a current flowing through it, and in this absolutely dark room nothing is seen, for it is black hot. An increase of the current, however, shows the shade of a dim grey, whilst a further increase shows it as illuminated by a red, and then a yellow light. A bunch of flowers placed in the beam of the electric light shows every colour in perfection; the light is gradually dimmed down, and the reds disappear, whilst the blue colours remain and the green leaves become dark. These two experiments show that there is a colour, if grey may be called a colour, with which we have to reckon.

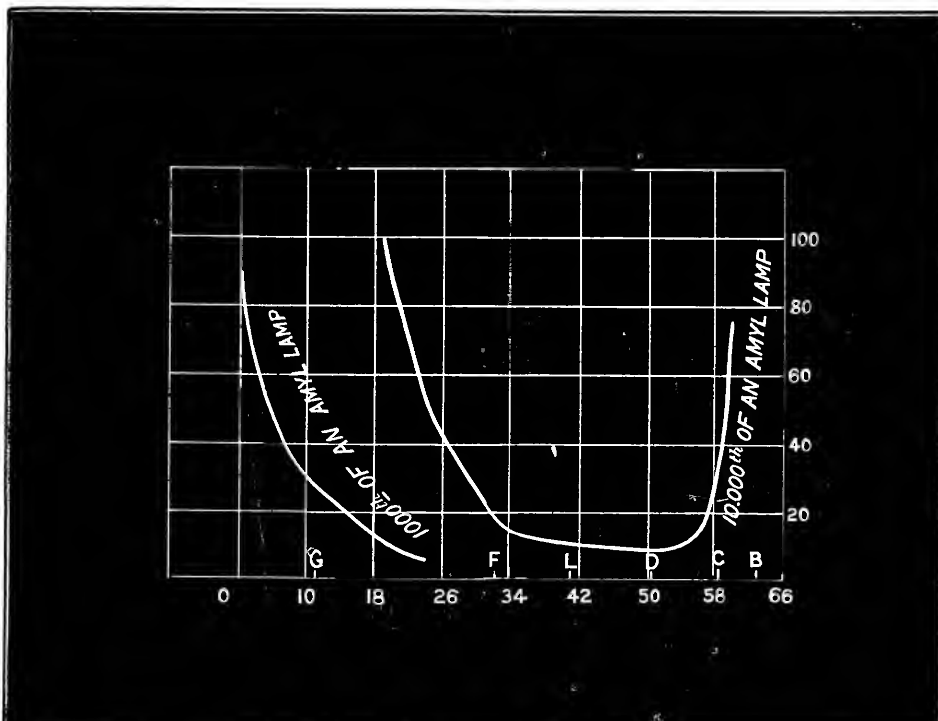
Now the question arises whether we can by any means ascertain at what stage a colour becomes of this grey hue, and at what stage of illumination the impression of mere light also disappears, and whether in any case the two disappear simultaneously.

As all colours in nature are mixed colours, it is at the outset useless to experiment with them in order to arrive at any definite conclusion, hence we are forced—and the forcing in this direction to the experimentalist is a very agreeable process—we are forced to come to the spectrum for information.

The apparatus on this table is one which I have before described in this theatre, and it is needless for me to describe it again. I can only say that it has in all colour investigations been of such service that any attempt on my part to do without it would have been most disadvantageous. The apparatus enables a patch of what is practically pure monochromatic light of any spectrum colour to be placed upon the screen at once, and an equally large patch of white light alongside it, by means of the beam reflected from the first surface of the first prism.

It should be pointed out that this beam of white light reflected from the first prism of the apparatus, having first passed through the collimator, must of necessity diminish with the intensity of the spectrum, when the collimator slit is closed.

FIG. 1.



Extinction of Spectrum Colours.

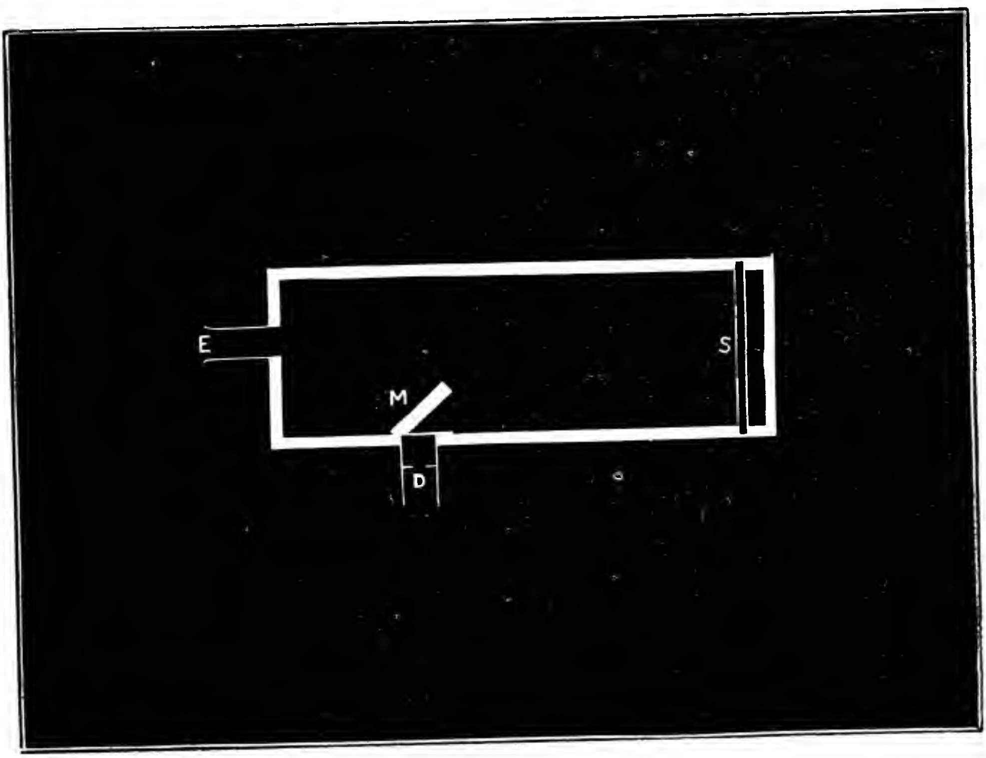
Having got these patches, the next step is to so enfeeble the light that their colour and then their visible illumination disappear.

An experiment which well demonstrates loss of colour is made by throwing a feeble white light on one part of the screen, and then in succession patches of red, green, and violet alongside it. The luminosity of the coloured light gradually diminishes till all the colour disappears, the white patch being a comparison for the loss of colour.

If red, green, and violet patches be placed alongside each other, and they are bedimmed in brightness together, it will be noticed that the red disappears first, then the green, and then the violet; or I may take a red and green patch overlapping, which when mixed form orange, and extinguish the colour: the slit allowing red light to fall on the screen may be absolutely closed, and no alteration in the appearance of the patch is found to occur. This shows, I think, that when all colour is gone from a once brilliant colour, a sort of steel-grey remains behind, and that red fails to show any luminosity when the green still retains its colour.

The measurement of the extinction of colour from the different parts of the spectrum was made on these principles. A box, similar

Fig. 2.



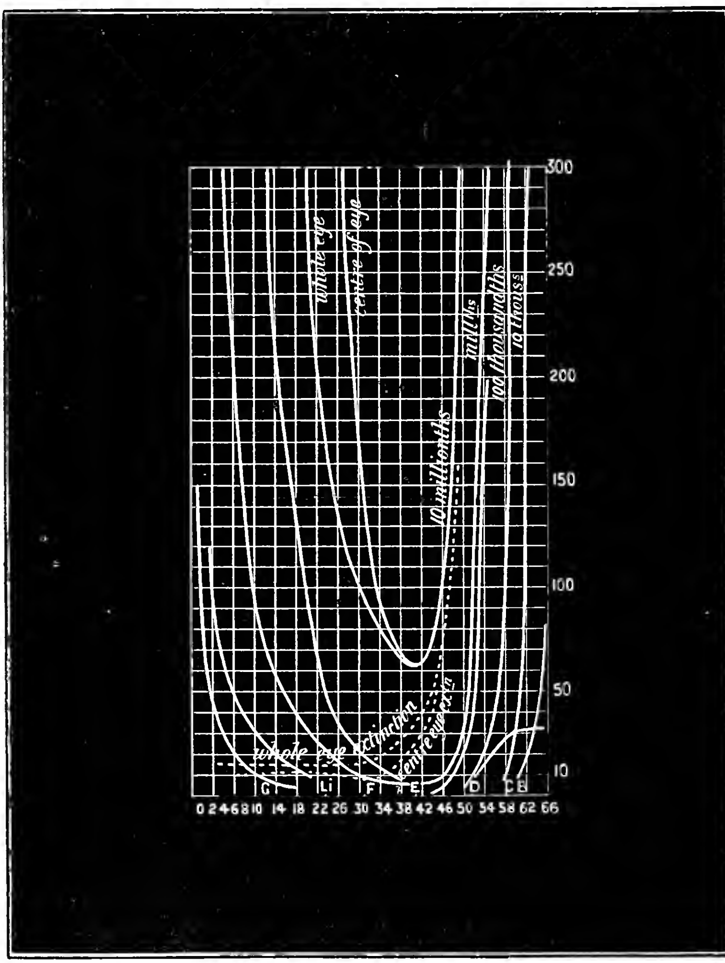
Extinction Box.

to Fig. 2, was prepared, but having two apertures, one at each side. Through one the coloured ray was reflected, and through the other a white beam of light to a white screen. Both beams were diminished, and when the white and coloured patches appeared the same hue, the amount of illumination was calculated. Fig. 1 shows graphically the reduction of illumination, when the D light of the spectrum is the same intensity as one amyl-acetate lamp at one foot from the screen. To measure the extinction of light, a box was made as in the diagram, closed at each end, but having two apertures as shown, Fig. 2:—

E is a tube through which the eye looks at S, which is a black screen with a white spot upon it, and which can be illuminated by light coming through the diaphragm D first falling on a ground glass which closes the aperture, and reflected on to it by M a mirror.

The patch of light of any colour being thrown on D, rotating sectors, the apertures of which could be opened and closed at pleasure, were placed in the path of the beam, thus enabling the intensity of

FIG. 3.



Extinction of the Spectrum.

the patch to be diminished. D could be made of any desired aperture, and thus the illumination of the ground glass would be diminished at pleasure. After keeping the eye in darkness for some time the eye was placed at E when the white spot illuminated by the colour thrown on D was visible, and the sectors closed till the last scintilla of light was extinguished. This was repeated for rays at different parts of the spectrum, and the results are shown in Fig. 3 by the continuous curved lines. The diagram would have been too large had the same scale been adopted throughout for the ordinates, each curve is therefore made on a scale ten times that of its neighbour, counting from the centre.

In the diagram the sodium light of the spectrum before extinction was made of the luminosity of the amyl-acetate lamp (hereafter called A L), which is about ·8 of a standard candle, at 1 foot distance from the source. Before it ceased to cause an impression on the eye, the illumination had to be reduced to $\frac{350}{10,000,000}$ A L.

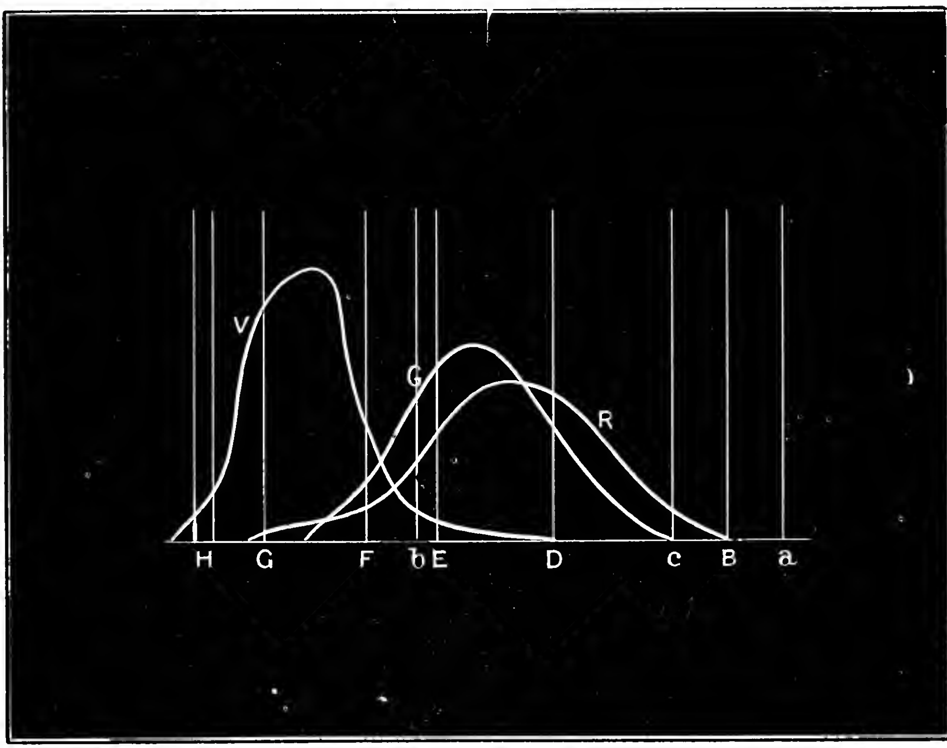
E light	to	$\frac{65}{10,000,000}$		of its spectrum luminosity.
F light	"	$\frac{150}{10,000,000}$	or $\frac{15}{1,000,000}$	" "
G light	"	$\frac{3000}{10,000,000}$	or $\frac{3}{10,000}$	" "
C light	"	$\frac{11,000}{10,000,000}$	or $\frac{11}{10,000}$	" "
B light	"	$\frac{70,000}{10,000,000}$	or $\frac{7}{1000}$	" "

There was one objection which might have been offered to this method, and that was to the use of the rotating sectors, and perhaps to the ground glass. This objection was met by first of all reducing the light by means of a double reflection of the beam forming the patch from one or two plain glass mirrors, and also by using a plain glass mirror in the box instead of a silvered glass. By this plan the light falling on the first plain glass mirror was reduced, before it reached the end of the box, 1000 times; and again, by narrowing the slit of the collimator, and also the slit placed in the spectrum, another similar reduction would be effected. All rays thus enfeebled were within the range of extinction. It was found that neither ground glass nor rotating sectors had any prejudicial effect, and therefore this extinction curve may be taken as correct.

In the curves there are two branches at the violet side, and this requires explanation. One shows the extinction when viewed by the most sensitive part of the eye, wherever that may be, and the other when the central portion of the eye was employed. The explanation of this difference in perception is chiefly as follows :—

In the eye we have a defect—at least we are apt to call it a defect, though no doubt Providence has made it for a purpose—in that there is a yellow spot which occupies some 6° to 8° of the very centre of the retina, and as it is on this central part that we receive any small image, it has a very important bearing on all colour experiments. The yellow spot absorbs the blue-green, blue, and violet rays, and exercises its strongest absorption towards the centre, though probably absent in the very centre, that is, in the "fovea centralis," and is less at the outer edges. That absorption of colour by the yellow spot takes place can be shown you in this way. Any colour in nature can be imitated by mixing a red, a green, and violet together, and with these I will make a match with white and then with brown, two very representative colours, if we may call them colours. Now if I, standing at this lecture table, match a white by mixing these

FIG. 4.



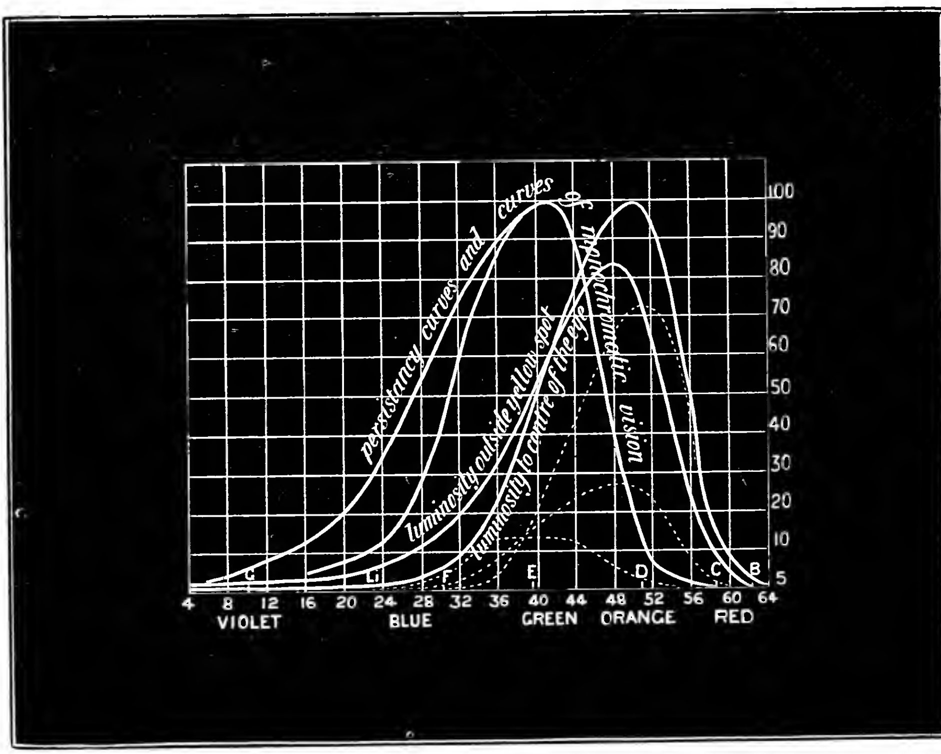
Colour Sensations.

three colours together, using a large patch, the image will fall on a part of the retina of considerably larger area than the yellow spot, and it will appear too green for those at a distance; but it is correct for myself. If I place a mirror at a distance, and make a match again by the reflected image, the match is complete for us all, as we all see it through the yellow absorbing medium. If I look at it direct from where I stand the match is much too pink. It may be asked why the comparison patches and the mixed colours do not always match since both images are received on the same part of the retina. The reason is that the green I have selected for mixture is in the part of the spectrum where great absorption takes place, whilst the comparison white contains the green of the whole spectrum, some parts of which are much less absorbed than others. I may remark that just outside the yellow spot the eye is less sensitive to the red than is the centre, and this is one additional cause of the difference. See Fig. 5.

More on this subject I have not time to say on this occasion, but it will be seen that the extinction of light for the centre and the outside of the eye differs on account of this.

I must take you to a theory of colour vision which, though it may not be explanatory of everything, at all events explains most phenomena—that is, the Young-Helmholtz theory. The idea embodied

FIG. 5.



in it is that we have three *sensations* stimulated in the eye, and that these three sensations give an impression of a red, a green, and a violet. These three *colours* I have said can be mixed to match any other colour, or, in other words, the three sensations are excited in different degrees, in order to produce the sensation of the intermediate spectrum colours, and those of nature as well.

The diagram Fig. 4 shows the three sensations as derived from colour equations made by Kœnig. It will be seen that there are three complete colour sensations, all of which are present in the normal eye. I would ask you to note that at each end of the spectrum only one sensation is present, viz. at the red end of the spectrum, the red sensation, and at the violet end the violet.

This is a matter of some importance, as we shall now see.

It will be recollected that in making the extinctions, the D light of the spectrum was made equal to one amyl-acetate lamp, and the other rays had the relative luminosity to it, which they had in the spectrum before they were extinguished. The luminosity curve of the spectrum is shown in Fig. 5.

Suppose we make all the luminosities of the different rays equal to one A L, we should not get the same extinction value, as shown in the continuous lines in Fig. 3. The violet would have to be much more reduced, but by multiplying the extinction by the luminosity we should get the curve of reduction for equal luminosities, and we get the dotted curves in Fig. 3.

It will be seen that it is the violet under such circumstances that would be the last to be extinguished, and that all the rays at the violet end of the spectrum would be extinguished simultaneously, as would also those at the extreme red. This looks like a confirmation of the Young-Helmholtz theory which I have briefly explained, for we cannot imagine that it can be anything but a single sensation which fails to be excited.

The violet is extinguished when it is $\frac{15}{10,000,000}$ A L, that is, a screen placed 817 feet away and illuminated by an A L violet lamp would be invisible. The blue-green (F) light when it is $\frac{17}{\text{10 millionths}}$ or 770 feet away. The green (E) light $\frac{35}{\text{10 millionths}}$ or 550 feet away. The orange (D) light is extinguished as before at $\frac{350}{\text{10 millionths}}$ or 180 feet away, whilst the red (C) light has only to be reduced to $\frac{2200}{\text{10 millionths}}$ or an A L lamp radiating C light would have to be placed only 67 feet away, whilst the radiation for an A L of the colour of the B light of the spectrum would have to be diminished to but $\frac{2600}{\text{10 millionths}}$ or the screen would have to be placed 60 feet away.

It is therefore apparent that with equal luminosities the violet requires about 175 times more reduction to extinguish it than does the red, and probably about 25 times more than the green.

This being so, I think it will be pretty apparent that, at all events from the extreme violet to the Fraunhofer line D of the spectrum, the extinction is really the extinction of the violet sensation, a varying amount of which is excited by the different colours. If then we take the reciprocals of the numbers which give extinction of the spectrum, we ought to get the curve of the violet sensation on the Young-Helmholtz theory. For if one violet sensation has to be reduced to a certain degree before it is unperceived, and another has to be reduced to half that amount, it is evident that the violet sensation must be double in one case to what it is in the other; that is, the degrees of stimulation are expressed by the reciprocal of the reduction.

Such a curve is shown in Fig. 5 (in which also are drawn the curves of luminosity of the spectrum when viewed with the centre of the retina and outside the yellow spot). And it will be noticed that it is a mountain which reaches its maximum about E. Remember that the height of the curve signifies the amount of stimulation given to the violet sensatory apparatus by the particular ray indicated in the scale beneath.

Turning once more to Fig. 3, it will be noticed that if any one or two of the three sensations are absent, the persons so affected are, what is called, colour blind. Thus if the red sensation is absent they are red blind; if the green, then green blind; if the violet, then violet blind; if both red and green sensations are absent, then the person would see every colour, including white, as violet. The results of the measurement of the luminosity of the spectrum by persons who have this last kind of monochromatic vision should be that they give a curve exactly, or at all events very approximately, of the same form as the curve given by the reciprocals of the extinction curve obtained by the normal eye, as the violet sensation is that which is last stimulated.

It has been my good fortune to examine two such persons, and I find that this reasoning is correct, the two coinciding when the curves for the centre of the retina are employed.

Further, I examined a case of violet blindness, and measured the luminosity of the spectrum as apparent to him. Now if the Young-Helmholtz theory be correct, then in his case the violet sensation ought to be absent, and the difference between his luminosity and that of the normal eye ought to give the same curve as that of the violet sensation. This was found to be the case.

Again, the reciprocal of the extinction curves of the red blind and green blind ought to be the same as those of the normal eye, for the violet sensation must be present with them also. This was found to be so. We have still one more proof that the last sensation to disappear is the violet.

If we reduce the intensity of the spectrum till the green and red disappear to a normal eye, and measure the luminosity of the spectrum in this condition, we shall find that it also coincides with the persistency curve. On the screen we have a brilliant spectrum, but by closing the slit admitting the light and placing the rotating sectors in the spectrum and nearly closing the apertures, we can reduce it in intensity to any degree we like. The whole spectrum is now of one colour and indistinguishable in hue from a faint white patch thrown above it. If the luminosity of this colourless spectrum be measured we shall get the result stated. The curve obtained in this way is in reality identical with the other curves. By these four methods then we arrive at the conclusion that the last colour to be extinguished is the sensation which when strong gives the sensation of violet, but which when feeble gives a blue-grey sensation.

One final experiment I may show you. It has been remarked that moonlight passing through painted glass windows is colourless on the grey stone floor of a cathedral or church.

We can imitate the painted glass and moonlight. Here is a diaper pattern of different coloured glasses and by means of the electric light lantern we throw its coloured pattern on the screen. The strength of moonlight being known we can reduce the intensity of the light of the lamp till it is of the same value. When this is done it will be seen that the pattern remains, but it is now colour-less, showing that the recorded observations are correct, and I think you are now in a position to account for the disappearance of the colour.

I have now carried you through a series of experiments which are difficult to carry out perfectly before an audience, but at any rate I think you will have seen enough to show you that the first sensation of light is what answers to the violet sensation when it is strong enough to give the sensation of colour. The other sensations seem to be engrafted on this one sensation, but in what manner it is somewhat difficult to imagine. Whether the primitive sensation of light was this and the others evolved, of course we cannot know. It appears probable that even in insect life this violet sensation is predominent, or at all events existent. Insects whose food is to be found in flowers seek it in the gloaming when they are comparatively safe from attack. Professor Huxley states that the greatest number of wild flowers are certainly not red but more or less of a blue colour. This means that the insect eye has to distinguish these flowers at dusk from the surrounding leaves which are then of a dismal grey; a blue flower would be visible to us whilst a red flower would be as black as night. That the insects single out these flowers seems to show that they participate in the same order of visual sensations. I venture to think, without adopting it in its entirety, that these results at all events give an additional probability as to the general correctness of the Young-Helmholtz theory of colour vision. Where the seat of colour sensation may be is not the point, it is only the question as to what the colour sensations make us feel which the physicist has to deal with. The simpler the theory, the more likely is it to be the true one, and certainly the Young-Helmholtz theory has the advantage over others of simplicity.

[W. de W. A.]

GENERAL MONTHLY MEETING,

Monday, May 9, 1892.

Sir James Crichton-Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

The following Vice-Presidents for the ensuing year were announced:—

Sir Frederick Abel, K.C.B. D.C.L. F.R.S.
Sir Douglas Galton, K.C.B. D.C.L. LL.D. F.R.S.
The Right Hon. Lord Halsbury, M.A. D.C.L. F.R.S.
William Huggins, Esq. D.C.L. LL.D. F.R.S.
David Edward Hughes, Esq. F.R.S.
The Right Hon. Lord Kelvin, D.C.L. LL.D. Pres. R.S.
Sir James Crichton-Browne, M.D. LL.D. F.R.S. Treasurer.
Sir Frederick Bramwell, Bart. D.C.L. F.R.S. Hon. Secretary.

Harry Spencer Ashbee, Esq. F.S.A.
Charles Ballance, Esq. F.R.C.S.
Francis Elgar, Esq. LL.D. M. Inst. C.E.
Montague Ellis, Esq.
S. H. Wells Foote, Esq.
J. E. H. Gordon, Esq. M. Inst. C.E.
Sir Robert Jardine, Bart. M.P.
Harry E. Jones, Esq. M.Inst. C.E.
Alexander B. W. Kennedy, Esq. F.R.S. M. Inst. C.E.
William Macnab, Esq. F.C.S.
Colonel L. J. Oliphant,
C. D. F. Phillips, M.D.
Mrs. Shield,
Sydney Francis Staples, Esq.
R. Palmer Thomas, Esq.

were elected Members of the Royal Institution.

The following Letter was read:—

Corporation House, Bloomsbury Place, W.C.,
Wednesday, 6th April, 1892.

Dear Sir Frederick Bramwell,

My mother, Lady Bowman, desires me to acknowledge the receipt of your letter, enclosing the copy of a Resolution passed at a meeting of the Board of Managers of the Royal Institution, held on Monday last. The testimony borne by many friends to the affectionate regard, no less than to the high esteem, in which they held my dear father, helps greatly to soften the blow which has fallen upon us, and we are further consoled in the memory of the peaceful death which ended, and seemed to complete, his active and useful life. His services to the Royal Institution were a source of very great pleasure to himself, and we are glad to know that they were appreciated by his colleagues. You will please do my mother the favour of conveying to the Managers her most grateful acknowledgments for the Resolution which has been placed upon the minutes.

Believe me, very truly yours,
W. Paget Bowman.

The Special Thanks of the Members were returned for the following Donation :—

Sir Lowthian Bell, Bart. (collection by) .. £50 0 0

for carrying on investigations on Liquid Oxygen.

The Special Thanks of the Members were returned to the Sub-Committee of the Forrest Engraving and Lectureship Fund for the presentation of an Engraving of a Portrait of Mr. James Forrest.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

The Governor-General of India—Geological Survey of India: Records, Vol. XXV. Part 1. 4to. 1892.
The Secretary of State for India—Report on Public Instruction in Bengal, 1890-91. fol. 1891.
Great Trigonometrical Survey of India, Vols. XXII.-XXIV. 4to. 1891.
The Madras Government—Madras Meteorological Results, 1861-90. 4to. 1892.
Academy of Natural Sciences, Philadelphia—Proceedings, 1891, Part 3. 8vo.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quinta: Rendiconti. 1° Semestre, Vol. I°. Fasc. 5. 8vo. 1892.
American Geographical Society—Bulletin, Vol. XXIII. No. 4; Vol. XXIV. No. 1. 8vo. 1891-92.
Astronomical Society, Royal—Monthly Notices, Vol. LII. No. 5. 8vo. 1892.
Bankers, Institute of—Journal, Vol. XIII. Part 4. 8vo. 1892.
Basset, Alfred B. Esq. M.A. F.R.S. M.R.I. (the Author)—Treatise on Physical Optics. 8vo. 1892.
Batavia Observatory—Magnetical and Meteorological Observations, 1890, Vol. XIII. 4to. 1891.
Rainfall in East Indian Archipelago, 1890. 8vo. 1891.
Birt, William, Esq.—Official Guide to the Great Eastern Railway. 8vo. 1892.
Bischoffsheim, M. R. L.—Monographie de l'Observatoire de Nice. Par C. Garnier. fol. 1892.
British Architects, Royal Institute of—Proceedings, 1891-2, No. 12. 4to.
Chemical Industry, Society of—Journal, Vol. XI. No. 3. 8vo. 1892.
Chemical Society—Journal for April, 1892. 8vo.
Cracovie, l'Academie des Sciences—Bulletin, 1892, No. 3. 8vo.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I.—Journal of the Royal Microscopical Society, 1892, Part 2. 8vo.
East India Association—Journal, Vol. XXIV. No. 2. 8vo. 1892.
Editors—American Journal of Science for April, 1892. 8vo.
Analyst for April, 1892. 8vo.
Athenæum for April, 1892. 4to.
Chemical News for April, 1892. 4to.
Chemist and Druggist for April, 1892. 8vo.
Educational Review for April, 1892. 8vo.
Electrical Engineer for April, 1892. fol.
Engineer for April, 1892. fol.
Engineering for April, 1892. fol.
Engineering Review for April, 1892. 8vo.
Horological Journal for April, 1892. 8vo.
Industries for April, 1892. fol.
Iron for April, 1892. 4to.
Ironmongery for April, 1892. 4to.
Nature for April, 1892. 4to.
Telegraphic Journal for April, 1892. fol.
Zoophilist for April, 1892. 4to.

Electrical Engineers, Institution of—Journal, No. 97. 8vo. 1892.
Evans, John, Esq. D.C.L. LL.D. F.R.S. (the Author)—Posy Rings. 8vo. 1892.
Ex-Libris Society—Journal for April, 1892. 4to.
Florence Biblioteca Nazionale Centrale—Bolletino, Nos. 151, 152. 8vo. 1892.
Franklin Institute—Journal, No. 796. 8vo. 1892.
Geneva, Société de Physique et d'histoire Naturelle—Mémoires, Vol. supplémentaire Centenaire de la Fondation. 4to. 1891.
Geological Institute, Imperial, Vienna—Verhandlungen, 1892, Nos. 2–5. 8vo.
Geological Society—Quarterly Journal, No. 190. 8vo. 1892.
Georgofili, Reale Accademie—Atti, Quarta Serie, Vol. XV. Disp. 1. 8vo. 1892.
Horticultural Society, Royal—Journal, Vol. XV. Part 1. 8vo. 1892.
Iron and Steel Institute—Journal for 1891. 8vo.
Johns Hopkins University—University Circulars, No. 97. 4to. 1892.
Studies in Historical and Political Science, Ninth Series, Nos. 9–12; Tenth Series, Nos. 1–3. 8vo. 1891–92.
American Journal of Philology, Vol. XII. Nos. 2, 3. 8vo. 1891.
American Chemical Journal, Vol. XIII. No. 7; Vol. XIV. No. 2. 8vo. 1891–92.
Annual Report. 8vo. 1891.
Linnean Society—Journal, Nos. 199, 200. 8vo. 1892.
Mechanical Engineers, Institution of—Proceedings, 1892, No. 1. 8vo.
Meteorological Society, Royal—Quarterly Journal, No. 81. 8vo. 1892.
Meteorological Record, No. 41. 8vo. 1892.
Miller, W. J. C. Esq. (the Registrar)—The Medical Register for 1892. 8vo.
The Dentists' Register for 1892. 8vo.
Odontological Society—Transactions, Vol. XXIV. No. 6. 8vo. 1892.
Payne, W. W. and Hale, G. E. (the Editors)—Astronomy and Astro-Physics for April, 1892. 8vo.
Pharmaceutical Society of Great Britain—Journal, April, 1892. 8vo.
Photographic Society of Great Britain—Journal. Vol. XVI. No. 6. 8vo. 1892.
Physical Society of London—Proceedings, Vol. XI. Part 3. 8vo. 1892.
Royal Society of London—Proceedings, No. 306. 8vo. 1892.
Russell, The Hon. Rollo, M.R.I. (the Author)—Epidemics, Plagues, and Fevers; their Causes and Prevention. 8vo. 1892.
Saxon Society of Sciences, Royal—Mathematisch-physischen Classe, Berichte, 1891, No. 5. 8vo. 1892.
Philologisch-historischen Classe, Band XIII. No. 4. 8vo. 1892.
Selborne Society—Nature Notes, Vol. III. No. 29. 8vo. 1892.
Smithsonian Institution—Bureau of Ethnology:
Catalogue of Prehistoric Works. 8vo. 1891.
Omaha and Ponka Letters. 8vo. 1891.
Society of Architects—Proceedings, Vol. IV. No. 10. 8vo. 1892.
Society of Arts—Journal for April, 1892. 8vo.
Statistical Society, Royal—Journal, Vol. LV. Part 1. 8vo. 1892.
St. Pétersburg Académie Impériale des Sciences—Bulletin, Tome XXXIV. No. 4. 4to. 1892.
Memoires, Tome XXXVIII. Nos. 7, 8; Tome XXXIV. 4to. 1891.
Tacchini, Prof. P. Hon. Mem. R.I.—Memorie della Società degli Spettroscopisti Italiani, Vol. XXI. Disp. 3ª. 4to. 1892.
Teyler Museum—Archives Serie II. Vol. III. Fasc. 7. 4to. 1892.
United Service Institution, Royal—Journal, No. 170. 8vo. 1892
Vaughan, Henry, Esq. M.R.I.—Illustrated Catalogue of Bookbindings at the Exhibition of Burlington Fine Arts Club. 4to. 1891.
Vereins zur Beförderung des Gewerbfleises in Preussen—Verhandlungen, 1892: Heft 4. 4to.
Zoological Society of London—Transactions, Vol. XIII. Part 4. 4to. 1892.
Proceedings, 1891, Part 4. 8vo. 1892.
Index to Proceedings, 1881–90. 8vo. 1892.

WEEKLY EVENING MEETING,

Friday, May 13, 1892.

Sir James Crichton-Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

William Huggins, Esq. D.C.L. LL.D. Ph.D. F.R.S. *M.R.I.*

The New Star in Auriga.

We depend so absolutely at every moment, and in every action, upon the uniformity of Nature, that any event which even appears to break in upon that uniformity cannot fail to interest us. Especially is this the case if a strange star appears among those ancient heavenly bodies by the motions of which our time and the daily routine of life are regulated, and which through all ages have been to man the most august symbols of the unchanging. For, notwithstanding small alterations due to the accumulated effects of changes of invisible slowness which are everywhere in progress, the heavens, in their broad features, remain as they were of old. If Hipparchus could return to life, however changed the customs and the kingdoms of the earth might appear to him, in the heavens and the hosts thereof, he would find himself at home.

Only some nineteen times in about as many centuries have we any record that the eternal sameness of the midnight sky has been broken in upon by even the temporary presence of an unknown star, though there is no doubt that in the future, through the closer watch kept upon the sky by photography, a larger number of similar phenomena will be discovered.

According to Pliny it was the sudden outburst into splendour of a new star in 130 b.c. which inspired Hipparchus to construct his catalogue of stars. Passing at once to more modern times we come to the famous new star of 1572 discovered by Tycho Brahe in the Constellation of Cassiopeia which outshone Venus, and could even be seen as a bright object upon the sky by day. But its brilliancy, like that of the new stars before and since, was transitory; within a few weeks its great glory had departed from it, and it then continued to wane until at last it had fallen back to its original low estate, as a star invisible to the naked eye.

The star of 1886 which, on May 2nd of that year, burst forth as a star of the second magnitude in the Northern Crown, is memorable as the first of those objects which was subjected to the searching power of the spectroscope. Two temporary stars have appeared since, one of the third magnitude in 1876 in Cygnus, and a small star in the Great Nebula of Andromeda in 1885.

It may be asked whether these temporary stars are in reality new stars, the creations of a day, or but the transient outbursts into splendour of small stars usually invisible; and, indeed, whether they may be but extreme cases of the large class of variable stars which wax and wane in periods more or less regular.

In the case of the more modern temporary stars the evidence is forthcoming that they did exist before and do exist still. The star of 1866 may be seen as one of about the ninth magnitude, with nothing to distinguish it from its fellows. So the star of 1876 in Cygnus, which rose to the third magnitude, is still there as a star of about the fourteenth magnitude. To these may be added, perhaps, Tycho's star.

The new star which makes the present year memorable is, indeed, so far as our charts go, without descent. But there is no improbability in assuming that in its usual low estate, to which it has now returned, it is of smaller magnitude than would bring it within our catalogues and charts.

Of great value in similar cases, in the future, will be the plates of the International Star Chart, which begins its existence this year. Such a photographic record, like the partial ones already made at the Cape Observatory and at the Harvard Observatory, will enable us to put back at will the dial of time, and to re-observe the heavens as they appeared when the plates were taken.

The absence of any previous record of the new star of the present year is not necessarily to be regarded as a proof that it did not exist as a star emitting light. Visibility and invisibility in our largest instruments are but expressions in terms of the power of the eye. The photographic plate, untiring in its power of accumulation, has brought to our knowledge multitudes of stars which shine, but not for us. The energy of their radiation is too small to set up the changes in the retina upon which vision depends.

A striking illustration is presented by plates taken of the neighbourhood of η Argus by Mr. Russel at Sydney, and later by Dr. Gill at the Cape. In these photographs a crowd of stars reveal themselves for the first time, which have hitherto shone in vain for the dull eye of man.

It is not improbable that the new star in Auriga did exist as a very faint star; but what were the conditions under which it woke up into sudden splendour? Such information as is forthcoming has been gained chiefly from that particular application of the spectroscope by which we can measure motion in the line of sight. It is not too much to say that this method of observation has opened for us in the heavens a door through which we can look upon the internal motions of binary and multiple systems of stars, which otherwise must have remained for ever concealed from us.

With every increase of telescopic aperture more stars are resolved into double or multiple systems, but no conceivable progress in instrument-making could have put it in our power, as the spectroscope does, to discover within the point-like image of a star, in many cases, a complex system of whirling suns, gigantic in size, and revolving with enormous speed, close about each other. An object-glass, as large in diameter as this theatre, if it could be constructed, would fail to show close systems of stars which the prism easily lays open to our view.

It is as many as twenty-three years ago since I had the honour of describing in this place the first successful application of this mode of using the spectroscope to the heavenly bodies. The method is now too well known for me to say more than that the change of wave-length or pitch of the light shows itself by a shift of the lines in the spectrum; towards the blue for an approach, towards the red for a recession between the light-source and the observer. It is obvious that the prism can take note only of the motions which are precisely in the line of sight. The stars, as seen from the earth, are moving in all directions; the spectroscope selects out of the star's motion, whatever it may be, that part only which is in the line of sight. It is of this component only of the complete motion that we can gain information directly by the spectroscope.

My original observations of the motion of Sirius were made in 1868, and of other stars in the following years, but the advance since then, and especially in recent years, in the improvements of instruments and in the use of the sensitive gelatine plate, has made a much higher degree of accuracy in the determination of motions attainable now, than was then possible. To Prof. Vogel is due the working out of a photographic method by which he has now determined the motions in the line of sight of more than fifty stars.*

This method is applicable not only to the drift of star-systems, but what is of more immediate interest in connection with the new star, to the internal motions within those systems. The simplest case of such systems is where one body only is bright enough to produce a spectrum. Unless the plane of the orbit is across the line of sight the star will have alternate periods of approach and of recession, and the lines in its spectrum will be seen to swing backwards and forwards relatively to a terrestrial line of the same substance in times corresponding to the star's orbital period. A grand example of this state of things was revealed by the discovery at Potsdam of the orbital motion of the bright star of Algol showing that the variation of its light is caused by its being partially eclipsed at intervals by a dusky companion star, the existence and motions of which were thus brought to light.

---

* Photographs of the spectrum of Sirius compared with that of iron, and photographs of the spectra of other stars, showing motions in the line of sight taken at Potsdam were thrown upon the screen.

I wish to express my great obligations to Professor Vogel, Professor Pickering, Professor Holden, M. Deslandres, MM. Henry, Dr. Bélopolsky, Dr. Roberts, and Father Sidgreaves, for photographs of star-motions and of the New Star, and its spectrum, many of which were specially prepared for this lecture. The photographs not suitable for throwing upon the screen were exhibited in the Library.

If the plane of the star-system is inclined to the line of sight, the dark body might pass above or below the bright one as seen from the earth, and not eclipse it. Vogel had the good fortune to discover such a system in Spica, which he showed to consist of a pair of great suns, one bright and the other dark, or nearly so, whirling round their common centre of gravity in about four days.

If, however, in a binary system both stars are bright, the minute stellar point formed in the telescope will contain the light of both stars; and its spectrum will be a compound one, the spectrum of one bright star being superposed upon that of the other. If the spectra are identical, all the lines will be really double, though apparently single when the stars have no relative motion; and will open and close in periods depending upon the stars' motions.

Such a system was first made known to us spectroscopically by Prof. Pickering from his photographs of Mizar, which consists of a pair of gigantic blazing suns, equal together to forty times the sun's mass and whirling round their common centre of gravity with the speed of about 50 miles a second. Then followed at Harvard the discovery in β Auriga of an order of close binary stars hitherto unknown. In Fig. 2 of Plate I. are reproduced the original photographs showing the duplication every second day of the lines in the spectra of this double star; the doubling is well seen in K, which is very narrow in this star.

Now it is to this method of spectroscopic observation that we are indebted for the revelation of the remarkable state of things existing in the new star. I may remark, in passing, that it is not a little surprising that a new star as bright as the fifth magnitude should have burst out almost directly overhead in the heavens, and yet have remained undiscovered for nearly seven weeks. Europe and the United States bristle every clear night with telescopes pointed from open observatories, which are served by an army of astronomers; and yet the honour of the discovery of the new star is due to an amateur, Mr. Anderson, possessed only of a small pocket-telescope and a star-chart. Happily the days are not over when discoveries can be made without an armoury of instruments.

As soon as the news reached Cambridge, U.S., Prof. Pickering, by means of photographs which had been taken there, was able to cause the part of the sky where the new star appeared, to pass again under his examination, precisely as it had appeared at successive intervals during the last six years; but the new star's place had remained unoccupied all that time by any star so bright as of the eleventh magnitude.

For about a year a still closer watch has been kept upon the sky at Cambridge by means of a photographic transit instrument driven by clockwork, which automatically patrols the sky every clear night, and registers upon one plate all stars as bright as of the sixth magnitude, within a great zone 60° in breadth, and three hours of Right Ascension in length. On December 1 the Nova did not appear upon

PLATE I.

Fig. 1.

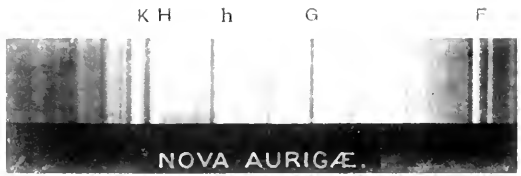
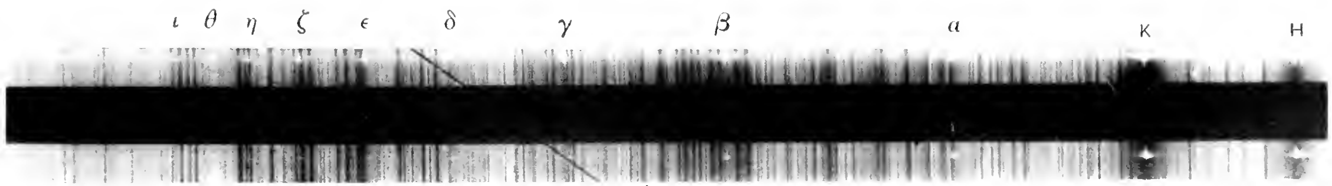


Fig. 2.

Fig. 3.



Spectrum of Prominence, March 4, 1892 (Deslandres).

PLATE II.

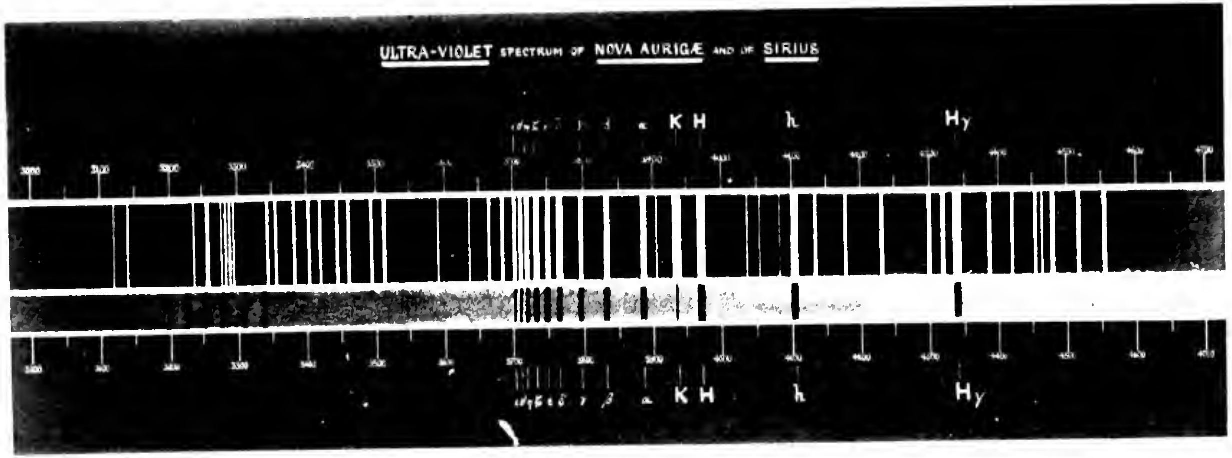


Fig. 2.

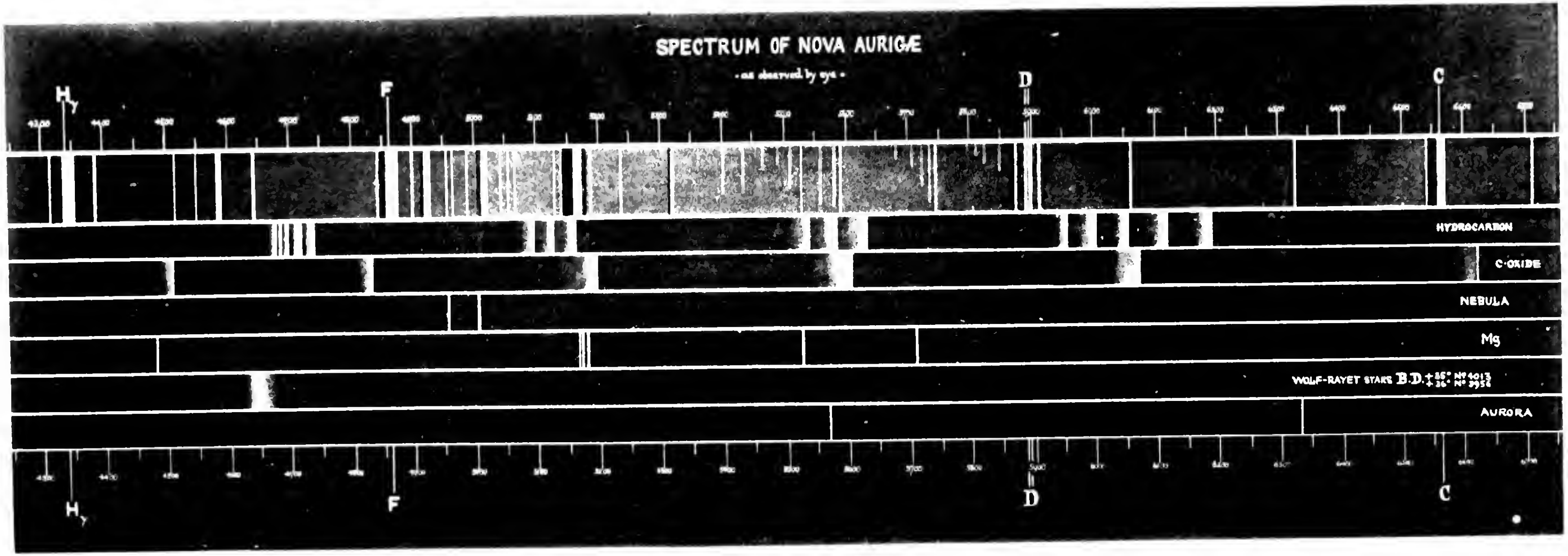


Fig. 1.

the plate, but the next night that was clear, December 10th, the Nova is recorded as of the fifth magnitude. Most fortunately, on December 8, Dr. Max Wolf photographed this part of the constellation of Auriga, but no star so bright as of the ninth magnitude was to be found where the Nova afterwards appeared.

The new star must therefore have sprung up to the brightness of the fifth magnitude between the 8th and the 10th of December last.

At that early time the Nova was not nebulous on Prof. Pickering's plates. The question has been raised since whether the star was surrounded by a faint nebula. To us, in our observations, it appeared like an ordinary star; but the point may be considered set at rest by photographs taken by Mr. Roberts, which by his kindness I am able to throw upon the screen. With an exposure of over three hours, there is only the usual small fringe of nebulosity due to our atmosphere, and which is present also about the other stars on the plate. How searching a test for faint nebulous matter is so long an exposure, is strikingly shown by contrasting a short exposure plate of the Pleiades, where nebula do exist, with a photograph on which the light action has been prolonged for nearly four hours. The Nova is free from nebulosity in photographs which have been sent to me by the Brothers Henry, and by Prof. Holden of the Lick Observatory.

The changes of magnitude of the Nova as shown by photographs taken at Greenwich, from eye-observations at Prof. Pritchard's observatory, and by Mr. Stone and by Mr. Knott, are recorded in the diagram on the wall. These observations show that notwithstanding continual fluctuations a slow but steady decline had set in, carrying the light of the star from nearly the fourth and one-half magnitude down to the sixth magnitude by the early days of March; but after March 7th, these swayings to and fro of its light, set up probably by commotions attendant on the causes of the star's outburst, calmed down, and the light fell rapidly and with regularity to about the eleventh magnitude by March 24th, and then down to 14·5th magnitude by April 1st. On April 26th, however, it was still visible at Harvard observatory as a star of the 14·5th magnitude.

We commenced our observations on February 2nd. The spectrum of the star in the visible region is represented in Fig. 1 of Plate II. Below the star's spectrum are placed the terrestrial spectra with which it was directly compared. The spectrum showed a brilliant array of bright lines, among which four in the green were very conspicuous.

The brightest of these we recognised as the second line of hydrogen, and passing the eye to the red, we saw blazing the first line of hydrogen at C; the blue line near G was also visible. But a remarkable phenomenon presented itself in that each bright line seemed to cast a shadow, for on the blue side of each was a narrow space of intense blackness. When we threw into the spectroscope for comparison the bright lines of hydrogen, the secret of this unusual appearance was revealed. The hydrogen line did not fall upon the middle of the F line, but upon one side. We had before us a magnificent example, on a grand scale, of motions in the line of sight—two mighty streams of hydrogen fleeing from each other; the hotter one, emitting the bright lines, going from us; the cooler producing the dark shadows by absorption, coming towards us, indicating a relative velocity of about 550 miles a second.

Direct comparisons of the bright line near the position of the chief nebular line, with lines of nitrogen and lead, showed that the stellar line was less refrangible than the principal nebular line. The second nebular line was not present in the star. The spectrum of the Nova showed, therefore, no relationship with the well-known spectrum of the bright-lined nebulæ.

A similar want of relationship of the spectrum of the new star with the usual hydro-carbon spectrum of comets was shown by direct comparison with the Bunsen flame. The bright line near *b* differs in position and in character from the beginning of the brightest band of the Bunsen flame spectrum, and no bright lines were found in the star at the positions of the other bright bands of this spectrum.

This bright line in the star falls very near the magnesium triplet at *b*, but a careful comparison of the spark spectrum of magnesium leaves little doubt that it does not owe its origin to this substance.

The sodium line at D is bright in the spectrum of the star, in which appears also a thin bright line at about the position of D_3. The continuous spectrum extended, when the star was brightest, below C in the red, and as far into the blue beyond G as the eye could follow it. The spectrum in Fig. 2 of Plate II. is from a photograph of the spectrum of the Nova which we took on February 22nd, using a mirror of speculum metal and a spectroscope with a prism of Iceland spar and lenses of quartz, so that the extreme violet part of the star's light was not cut off by passing through glass. The brilliant lines followed by absorptions, and the fainter continuous spectrum were found to extend upon the plate nearly as far as the light of Sirius, and not far short of the place where our atmosphere cuts off all celestial light. A photograph of the spectrum of Sirius showing the group of lines near the end of the spectrum has been added for comparison. In the star the whole range of the hydrogen lines, including the ultra-violet series and the calcium lines H and K, were bright, each accompanied on the blue side by a dark absorption band. In this respect, as well as in the positions of the principal bright lines in the visible region, the Nova suggested a state of things not unlike what we find in the erupted matter at the solar surface.

M. Deslandres permits me to reproduce in Fig. 3, Plate I., the photograph of a remarkable prominence taken on March 4th, 1892, in which are reversed not only H and K, and the known hydrogen series, but three additional members are to be seen at the more refrangible end, the positions of which M. Deslandres informs me fall into Balmer's formula for the hydrogen series.*

The resemblance of the spectrum of the Nova to that of the erupted solar surface is further shown in a remarkable feature of great significance in the character of the hydrogen lines both bright and dark. On February 2nd we noticed that the F line was not of uniform brightness throughout its breadth. We soon came to the conclusion that it was divided, not quite symmetrically, by a very narrow dark line. The more refrangible component was brighter, and rather broader than the other. Later on in February, we were sure that small alterations were taking place in this line, and that the component on the blue side no longer maintained its superiority. We suspected, indeed, at times that the line was triple, and towards the end of February and in the beginning of March we had no longer any doubt that it was occasionally divided into three bright lines by the incoming of two very narrow dark lines.

Similar alterations, giving a more or less apparent multiple character to the lines, are to be seen not only in the bright lines, but also in those of absorption in contemporary photographs taken of the spectrum of the star. I may mention those taken at Potsdam, Stonyhurst, and the Lick Observatory. These changes were specially watched and measured by M. Bélopolsky at Pulkova.

Prof. Pickering informs me that on a photograph taken at Cambridge, U.S., on February 27th, H, K, and α are triple, and that Miss Maury recorded, "the dark hydrogen lines rendered double, and sometimes triple, by the appearance of fine bright threads superposed upon the dark bands."

Now, when on the sun's surface, or in the laboratory, portions of the same gas at different temperatures come in before each other, the cooler gas may cause a narrow absorption line to form upon a broader bright line, and thus impart to it the appearance of a double line; or in the case of hotter gas, a narrow bright line upon a dark line. Prof. Liveing and your distinguished Professor of Chemistry, Prof. Dewar—whose researches with the electric arc-crucible have made them specially familiar with the ever-changing guises and disguises of this protean phenomenon of reversal—have recorded cases not only of double reversals, giving apparent triplicity to single bands, but also of threefold reversals. The phenomenon of the unsymmetrical division of the bright and dark lines which was occasionally seen in the Nova frequently presents itself in the laboratory from the unequal expansion on the two sides of the line on which the reversed line falls; and at the solar surface from the relative motions in the line of sight of the hotter and cooler portions of the gas taking part in the phenomenon. Unless we accept this obvious interpretation of the apparent multiple character of the stellar lines, we should have

* M. Deslandres has detected since two more lines, thus adding five new lines to the hydrogen series.

to assume a system of at least six bodies, all moving with different velocities.

In Fig. 1, Plate I., is reproduced a photograph of the blue part of the spectrum taken at Harvard Observatory with a prism placed over the object-glass. The dark absorption lines on the blue side of the bright lines are well shown.

It is of great importance to state that the waning of the star was not accompanied by any material change of its spectrum, but only of such apparent changes as might well come in when parts of an object differ greatly in brightness. On March 24th, when the star's light had fallen so low as to nearly the eleventh magnitude, we could still glimpse the faint continuous spectrum, upon which the remarkable quartet of bright lines still shone out without any great change of relative intensity. Prof. Pickering informs me that on his plates the principal lines in the photographic part of the spectrum "faded in the order K, H, *a*, F, *h*, G, the latter becoming brighter as the star was faint." Omitting the calcium lines H and K, which varied, the order of disappearance agrees with that of the sensitiveness of the plate for these parts of the spectrum, and is in accordance with the view that the star's spectrum remained without material alteration through this great range of magnitude.

How are we to account for the appearance and doings of this new star, or rather stars? For, as we have seen, the great shifts in the spectrum of the bright and dark lines, the bright to the red, and the dark to the blue, appear to show two bodies having relative motion in the line of sight of about 550 miles a second. Now, during the whole time, some seven weeks, that the star was under observation, this relative velocity was maintained without any great alteration, though it is probable that small changes, beyond the reach of our instruments, took place.

A reasonable explanation of these phenomena may perhaps be found if we venture to assume, though with considerable hesitation, as the subject is obscure, two gaseous bodies, or bodies with gaseous atmospheres, moving away from each other after a near approach, in parabolic or hyperbolic orbits, with our sun nearly in the axis of the orbits; the components of the motions of the two bodies in the line of sight, after they had swung round, might well be as rapid as those observed in the new star, and might continue for as long a time without any great change of relative velocity. Unfortunately, information as to the motions of the bodies at the critical time is wanting, for the event through which the star became suddenly bright had been over for some forty days before any observations were made with the spectroscope.

Analogy from the variable stars of long period would suggest the view that the near approach of the two bodies may have been of the nature of a periodical disturbance, arising at long intervals in a complex system of bodies. Chandler has recently shown in the case of Algol that the minor irregularities in the variation of its light are probably caused by the presence of one or more bodies in the system, besides the bright star and the dusky one which partially eclipses it. To a similar cause are probably due the minor irregularities which form so prominent a feature in the waxing and waning of the variable stars as a class. We know that the stellar orbits are usually very eccentric. In the case of γ Virginis the eccentricity is as great as 0·9, and Auwers has recently found the very considerable eccentricity of 0·63 for Sirius.

The great relative velocity of the component stars of the Nova, however, seems to force us to look rather to the casual near approach of bodies possessing previously considerable motion, unless we are willing to concede to them a mass very great as compared with that of our sun. Such a near approach of two bodies of great size is very greatly less improbable than would be their actual collision. The phenomena of the new star scarcely permit us to suppose even a partial collision; though if the bodies were very diffuse, or the approach close enough, there may have been possibly some mutual interpenetration and mingling of the rarer gases near their boundaries.

A more reasonable explanation of the phenomena, however, may be found in a view put forward many years ago by Klinkerfues, and recently developed by Wilsing, that under such circumstances of near approach enormous disturbances of a tidal nature would be set up, amounting it may well be to partial deformation in the case of gaseous bodies, and producing sufficiently great changes of pressure in the interior of the bodies to give rise to enormous eruptions of the hotter matter from within, immensely greater, but similar in kind, to solar eruptions; and accompanied probably by large electrical disturbances.

In such a state of things we should have conditions so favourable for the production of reversals undergoing continual change, similar to those exhibited by the bright and dark lines of the Nova, that we could not suppose them to be absent; while the integration of the light from all parts of the disturbed surfaces of the bodies would give breadth to the lines, and might account for the varying inequalities of brightness at the two sides of the lines.

The source of the light of the continuous spectrum upon which were seen the dark lines of absorption shifted towards the blue, must have remained, as seen by us, behind the cooler absorbing gas, so as to form a background to it; indeed, must have formed with it the body which was approaching us, unless we assume that both bodies were moving exactly in the line of sight, or that the absorbing gas was of enormous extent.

The circumstance that the receding body emitted bright lines, while the one approaching us gave a continuous spectrum with broad absorption lines similar to a white star, may, perhaps, be accounted for by the two bodies being in different evolutionary stages, and consequently differing in diffuseness and in temperature. Indeed, in the variable star β Lyræ, we have probably a binary system, of which one component gives bright lines, and the other dark lines of absorption. We must, however, assume a similar chemical nature for both bodies, and that they existed under conditions sufficiently similar for equivalent dark and bright lines to appear in their respective spectra.

We have no knowledge of the distance of the Nova, but the assumption is not an improbable one that its distance may be of the same order of greatness as that of the Nova of 1876, for which Sir Robert Ball failed to detect any parallax. In this case, the light-emission suddenly set up, certainly within two days and possibly within a few hours, was probably much greater than that of our sun; yet within some fifty days after it had been discovered, at the end of January, its light fell to about 1/300th part, and in some three months to nearly the 1/10,000th part. As long as its spectrum could be observed the chief lines remained without material alteration of relative brightness. Under what conditions could we suppose the sun to cool down sufficiently for its light to decrease to a similar extent in so short a time, and unaccompanied with the incoming of very material changes in its spectrum. It is scarcely conceivable that we can have to do with the conversion of gravitational energy into light and heat. On the theory we have ventured to suggest, the rapid calming down, after some swayings to and fro of the tidal disturbances, and the closing in again of the outer and cooler gases, together with the want of transparency which might come in under such circumstances, as the bodies separated; might account reasonably for the very rapid and at first curiously fluctuating waning of the Nova, and also for the observed absence of change in its spectrum.

I may, perhaps, be permitted to remark that the view suggested by Dr. William Allen Miller and myself, in the case of the Nova of 1866, was essentially similar, in so far as we ascribed it to erupted gases. The great suddenness of the outburst of that star, within a few hours probably, and the rapid waning from the 3·6 magnitude to the 8·1 magnitude in nine days, induced us to throw out the additional suggestion that possibly chemical actions between the erupted gases and the outer atmosphere of the star may have contributed to its sudden and transient splendour, a view which, though not impossible, I should not now, with our present knowledge of the light changes of stars, be disposed to suggest.

The subject is necessarily obscure, but we must not on this account feebly relinquish the hope of conquest. The words of a great Seer may well be taken as the watchword of the Astronomer:—

> . . . "Fervent love,
> And lively hope, with violence assail
> The kingdom of the heav'ns, and overcome"

*   *   *

[W. H.]

WEEKLY EVENING MEETING,

Friday, May 20, 1892.

David Edward Hughes, Esq. F.R.S. Vice-President, in the Chair.

J. Wilson Swan, Esq. M.A.

Electro-Metallurgy.

This is not the first time a lecture has been delivered here on electro-metallurgy. I find that so long ago as January 1841 there was a lecture on the subject by Mr. Brand.

At that time electro-metallurgy was very new and very small. It consisted solely of electro-plating and electrotype. Electro-plating had already begun to be practised as a regular industry, but it was still a question whether the new kind of plating was good, and there were not a few silversmiths who would not offer electro-plate for sale because of its supposed inferiority to plate of the old style. That question has long been definitely settled by the fact that every week more than a ton of silver is deposited in the form of electro-plate.

Electrotype in 1841 was not so far advanced—it had not then been taken hold of by the artisan and manufacturer—it was still in the hands of the amateur.

While the voltaic battery was the cheapest source of electric current, electro-metallurgy was necessarily restricted to artistic metal work, or to those applications where the fine quality of the electrotype cast outweighed the consideration of its cost, or where only a thin film of metal was required for the protection of a baser metal from the action of the air.

Within this limited field, the electro deposition of copper, of gold, of silver, of iron, and of nickel, has been carried on commercially with very great success and advantage for almost the whole period of the existence of the art. But beyond these bounds, set by the limitation of cost, it could not pass.

Now, all this is changed—since engineer and electrician have united their efforts to push to the utmost the practical effect of Faraday's great discovery, of the principle of generating electric currents by motive power. The outcome is the modern dynamo, with its result—cheap electricity. The same cause that has led to electric lighting, and to the electric transmission of power, has also led to a very great development of electro-metallurgic industry, and not only in the old directions but in new. It is no longer a matter of depositing ounces or pounds of metal, but of tons and thousands of tons. And it is no longer with metal deposition merely that electro-metallurgy now deals, but also with the extraction of metals from their ores, and the fusion and welding of metals. Electro-metallurgy has in fact grown so large and many-branching, that it is impossible to treat it in a complete manner in a single hour.

One of the latest developments is electric welding. This, in one of its forms, that invented by Elihu Thomson, has recently been so thoroughly explained and demonstrated by Sir Frederick Bramwell, that it is not necessary for me to do more than mention it as belonging to the subject.

There is also another species of electric welding—that of Dr. Benardos—in which the electric arc is used after the manner of a blow-pipe flame, to obtain the welding of such forms and thicknesses of iron, steel, and other metals, as would be difficult or impossible to weld in any other way; and not only is the electric blow-pipe used for welding, but also for the repair of defects in steel and iron castings, by the fusion of pieces of metal, of the same kind as the casting, into the faulty place, so as to make it completely sound. This new kind of electric welding, as improved by Mr. Howard, is now of sufficient importance to entitle it to the full occupation of an evening. I therefore propose to leave it for detailed description to some other lecturer, and content myself with calling your attention to the interesting collection of specimens on the table, and in the Library (lent by Messrs. Lloyd & Lloyd), showing the results of this process.

Even with this curtailment, the extent of the field is still too great, and I must reduce it further by omitting a considerable section of that portion which relates to the extraction of metals from their ores, and, in this connection, only speak of the extraction of aluminium.

But, in the first place, I am going to speak of the deposition of copper, and you will pardon me if I treat it as if you were unacquainted with the subject.

One of the wonderful things about the electro-deposition of copper, and in fact any other metal deposited from a solution of its salt in water, is, that bright, hard, solid metal, such as we are accustomed to see produced by means of fusion, can, by the action of the electric current, be made to separate from a liquid which has no appearance of metal about it.

The beginning of every electro-deposition process is the making a solution of the metal to be deposited. I am going to dissolve a piece of copper, the most elementary of all chemical operations, but I want to make it quite clear where the metal to be deposited comes from—to show that it is actually in the solution, and actually comes out of it again; for that is an effect so surprising, that it requires both imagination and demonstration to make it evident. There is projected on the screen a glass cell containing nitric acid. Mr. Lennox will put into it a piece of copper. He has done so; it quickly disappears, and a blue solution of copper nitrate is formed. Now, if I pass an electic current through this solution, or through some solution of the same kind, which, to save time, has been prepared beforehand, and immerse in it, a little apart from each other—the positive and negative wires coming from some generator of electric current—this will happen: metallic copper will come out of the solution, and attach itself as a coating to the negative wire, and consequently that wire will grow in thickness. At the other wire—the positive—exactly the reverse action will take place. There, if the positive wire be copper, it will gradually dissolve, and become thinner. The quantity of metal deposited on the negative wire will almost exactly equal the quantity dissolved from the positive, and therefore the solution will contain the same quantity of metal at the end of the experiment as at first, but it will not be the same metal; it will be fresh metal dissolved from the positive wire, and the metal originally contained in the solution will have been deposited as metallic copper.

I will show on the screen this process in operation. Here are the two wires I spoke of. The electric circuit, which includes these two wires, is so arranged that on its completion the thick wire will be the positive, and the thin wire the negative. Now please complete the circuit. One wire (the positive) is carrying an electric current into the copper solution, and the other (the negative) is carrying the current away. The solution is conveying the current between the wires, and one of the incidents of the transport of current from wire to wire by the solution, is electro-chemical decomposition, or electrolysis; and the result of that is, the deposition, out of the solution, of copper, upon one wire, and the dissolving away, or entering into solution, of copper, from the other. Now it can be clearly seen that the wire that was thick is now thin, and the wire that was thin is now thick.

Imagine the growing wire to be an electrotype mould, and that the deposit of copper which formed on the wire has spread over the surface, and formed a nearly uniform film, and that by continuing the process it has become thick, that deposit, stripped from the mould, would be an electrotype.

Or imagine the negative wire to be a thin sheet of pure copper, and the positive wire to be a thick sheet of impure copper, and suppose the action carried on so far that the thin sheet has become thick, by the deposition of copper upon it from the solution, and the thick one thin, by its copper entering into solution, that case would represent the condition of things in electrolytic copper refining.

Allow your imagination to take one more short flight, and suppose that this is not a solution of copper, but one of silver, and that the growing wire is a teapot, to be silvered; and further, suppose that the dissolving electrode is silver, and you will then understand the principle of electro-plating.

It requires very little explanation to make the ordinary arrangement of electrotyping intelligible. Here is a trough containing sulphate of copper solution. Here is a mould, that, through the kindness of Messrs. Elkington, has been prepared for me, this is connected with the negative pole of a battery—and here is a plate of copper, connected with the positive pole. When I immerse the mould in the solution—at about two inches from the copper plate—the electrical circuit is completed, and the same electrolytic action that the experiment illustrated will take place. Copper will be deposited on the mould, and will be dissolved in equal quantity from the copper plate, and the supply of copper in the solution will thus be kept up. As it will take a little time to obtain the result I wish to show, I will put this aside for ten minutes or so, and proceed to speak of different applications of this principle of copper deposition.

For the reproduction of fine works of art in metal, electrotype is unapproachable. The extreme minuteness with which every touch of graver or modelling-tool is copied by the deposited metal film, separates electrotype by a wide space from all other modes of casting. Even the Daguerreotype image is not too exquisitely fine, for electrotype to copy it so perfectly, that the picture is almost as vivid in the cast as in the original.

It is this quality that has given to electrotype a *rôle* which no other process can fill, and, so far, its practical utility is not greatly dependent on the cost of the current. This applies to all those most beautiful things here and in the Library, lent by Messrs. Elkington. These could all have been produced commercially even if there had been nothing better for the generation of the current than Smee's battery; a very good battery, by the way, for small operations in copper deposition. It gives a very low electro-motive force, and that is a defect, but in copper deposition, the half volt or so is generally sufficient to produce, automatically, the required current density.

One of the uses of electrotype, not greatly affected by the cost of deposition, is that of the multiplication of printing surfaces. In these days of illustrated periodicals, electrotype has come more and more into use for making duplicate blocks from wood engravings, which would soon be worn out and useless if printed from direct. It is also employed to make casts from set-up type, to be used instead of ordinary stereotype casts, when long numbers of a book have to be printed; also as a means of copying engraved copper-plates. Here are examples of all these uses of the electrotype process. The electro-blocks are lent by Messrs. Richardson & Co., and the copper-plates by the Director General of the Ordnance Survey Office, Southampton.

The plates illustrate the method employed at Southampton in the map printing department. The original plates are not printed from, except to take proofs. The published maps are all printed from electrotypes. Here is an original plate—here the matrix, or first electro, with, of course, all the lines raised, which are sunk in the original. The second electro is, like the original, an intaglio. Here is a print from it, and here one from the original plate. Practically they are indistinguishable from each other, and bear eloquent testimony to the wonderful power of electrotype to transmit an exceedingly faithful copy of such a surface.

Nickel, has, of late years, come into extensive use for what is termed nickel-plating, as applied to coating polished steel and brass with nickel. Nickel, not only has the advantage over silver of cheapness, but also, in some circumstances, of greater resistance to the action of the air.

Another metal, usually deposited in the form of a coating, is iron. The electrolytic deposit of iron is peculiarly hard—so much so, that it is commonly, but erroneously spoken of as *steel*-facing. The deposition of a film of iron upon engraved copper-plates, as a means of preventing the wear incidental to their use in being printed from, has become almost universal. Valuable etchings, mezzo-tints, and photogravure plates are thus made to bear a thousand or more impressions without injury. By dissolving off the iron veil with weak acid, when the first signs of wear appear on the surface of the plate, and re-coating it with iron, an engraved copper-plate is, for all practical purposes, everlasting.

In this case, of course, the film of iron is extremely thin—one or two hundred thousandths of an inch. But it is possible to produce most of the metals commonly used as coatings, in a more massive form. Here, for example, is an iron rod half-an-inch in diameter, entirely formed by electrolytic deposition. I am indebted to Mr. Roberts-Austen for being able to show this, and also for this other example of a solid deposit of iron, and for this beautiful specimen of electrolytic coating with iron. Here also are solid deposits of silver. This drinking cup is a solid silver electro-deposit.

These are all departments of electro-metallurgy which would have maintained a perfectly healthy industrial existence and growth without the dynamo; but now I come to speak of a branch of the subject—electrolytic copper refining—which, without that source of cheap electricity, could not have existed. This is the most extensive of all the applications of electro-chemistry, and is rendering valuable assistance to electrical engineering by the improvement it has led to in the conductivity of copper wire.

One of the results of this is seen in the raising of the commercial standard of electrical conductivity.

Ten years ago, contracts for copper wire for telegraphy, stipulated for a minimum conductivity of 95 per cent. of Matthiessen's standard of pure copper. Now, chiefly owing to electrolytic refining, a conductivity of 100 per cent. is demanded by the buyer and conceded by the manufacturer.

To show the difference between the past and present state of things in relation to the commercial conductivity of copper, I am going to exhibit on the screen measurements of the resistance of six pieces of wire of equal length and equal cross section—they have been drawn through the same drawplate. Three of the pieces are new, and three are old. The three new pieces are made from electrolytic copper, and are representative of the present state of things. The three old pieces are taken from three well known old submarine telegraph cables, and they show how very bad the copper was when it was first employed for telegraphic purposes, and how great has been the improvement. I will take No. 1 wire as the standard of comparison. It is a piece of the wire about to be supplied to the Post Office Telegraph Department for trunk telephone lines. It will show the very high standard of conductivity that has been reached in the copper of commerce. I am indebted for it, and for two out of three of the old cable wires, to Mr. Preece. No. 2 wire is made from electrolytic copper, deposited in my own laboratory. No. 3 is also electrolytic copper, but such as is commercially produced in electrolytic copper refining, it has been supplied to me by Mr. Bolton, to whom I am also indebted for wire No. 6—a particularly interesting specimen: it is from the first Trans-Atlantic cable—the cable of '58. No. 4 wire is from the Ostend cable of 1860, and No. 5 wire is from the old Dutch cable. These wires are so arranged that I can send a small and constant current partly through any one of them, and partly through a galvanometer. When this is done the result will be a deflection of the spot of light on the scale from the zero point to an extent corresponding to the resistance of the particular wire in the circuit. The worse the wire is, the greater will be the deflection. We will begin with the Post Office sample first. I connect the galvanometer terminals to wire No. 1, you see there is a deflection of ten degrees. I will now shift the contacts to wire No. 2—exactly the same length of wire is included—but now you see there is a deflection of slightly less than ten degrees, showing that this wire has a little lower resistance than No. 1. The difference is very small —it may be 2 per cent.—and 2 per cent. less of it would be required to conduct as well as the No. 1 wire. The next is No. 3. This is Mr. Bolton's wire, and shows a resistance almost equal to the last.

Nos. 1, 2, and 3 are, therefore, nearly alike, and have a degree of conductivity almost as high as it can possibly be.

Now we come to the three old wires.

We will take No. 4 (the Ostend cable). There, you see, is a great difference. Instead of spot of light being on the tenth degree, it is upon the eleventh.

We will now try No. 5 (the Dutch cable). That drives the index to 17.

Now I change to No. 6 (the old Atlantic cable), and we have a deflection of no less than 25 degrees. I suppose we may assume that this wire fairly represents the commercial conductivity of copper in 1858, for it is highly probable that for a work so important as the first Atlantic cable every care would be taken in the selection of the copper.

The result of this experiment shows that the copper of that cable was extremely bad as a conductor, that in fact it is 150 per cent. worse than the best commercial copper of to-day. In other words, it shows that, in point of electrical conductivity, one ton of the copper of to-day will go as far as two-and-a-half tons of such copper as was used for the cable of '58.

This change is largely due to electrolytic copper refining.

The process of electrolytic copper refining is the same in principle as that which produced the thickening of one of the wires and the thinning of the other in my first experiment. To prepare the crude copper for the refining process it is cast into slabs; these form the anodes, and correspond to the wire which in my experiment became thin. The cathodes, corresponding to the wire which became thick, are formed of thin plates of pure copper. Here are plates such as are used in electrolytic copper refining works. They are portions of actual cathodes and anodes, and represent the state of things at the commencement, and at the end, of the depositing operation—an operation that takes several weeks to complete, and effect the great change these plates show. In copper refining works, an immense number of these plates, each having 6 to 10 square feet of superficial area, are operated upon together, in a great number of large wooden vats, containing sulphate of copper solution and a small proportion of sulphuric acid. Electric current from a dynamo, driven by a steam-engine or water-power, is conveyed by massive copper conductors to the vats, arranged in long lines of 50 or 100 or more in series. Thick copper bars connect adjoining vats, and provide a positive and negative support for the plates, which hang in the solution, opposite each other, two or three inches apart. During the process, the impure slabs dissolve, and at the same time pure copper is deposited from the solution upon the thin plates. The deposition and dissolving go on slowly, in some cases very slowly, for a slow action takes less power, and gives purer copper than a more rapid one. The usual rate is one to ten ampères per square foot of cathode surface. You will better realise what these rates of deposit mean, when I say that one ampère per square foot rate of deposition gives for each foot of cathode surface, nearly one ounce of copper in twenty-four hours, and a thickness of one-eight hundredth of an inch; and therefore the production of one ton of copper, at that rate, in twenty-four hours, would require a cathode surface in the vats, in round numbers, of 36,000 square feet. At the higher rate of ten ampères per square foot, which is used where coal is cheap, one-tenth of this area would be required.

The importance of the electrolytic copper refining industry, and the extent of the plant connected with it, may be inferred from the fact that, reckoning the united production of all the electrolytic copper works in the world, nearly one ton of copper is deposited every quarter of an hour.

Very little power is required for copper deposition if the extent of the dissolving and depositing surfaces is large, relatively to the quantity of copper deposited in a given time.

Some of the impurities ordinarily found in crude copper are valuable. Silver and gold are common impurities, and these, and some other impurities, do not enter into solution, but fall down as black mud, are recovered, and go to diminish the cost of the process, or increase the profit; and even those impurities which enter into solution, are, under ordinary conditions, almost completely separated.

Electrolytic copper refining is both an economical and an effective process. The deposited copper is exceptionally pure. At one time it was supposed that it must necessarily be quite pure, but this is not the case; other metals can be deposited with the copper, but it is not difficult to realise in practice a close approximation to absolute purity in the deposited copper. Here is an example of the deposition of a mixed metal—brass, that is, copper and zinc deposited together, and there are in the Library a number of interesting specimens of mixed metal deposition. These deposits of brass and other alloys show that more than one metal can be deposited at the same time. The great enemy to conductivity in copper is arsenic, and the deposition of arsenic as well as copper, is one of the things to be guarded against in electrolytic copper refining. Not only are the chemical characteristics of electrolytically refined copper generally good, but its mechanical properties are largely controllable. Usually electrolytic copper is melted down and cast into billets of the form required for rolling and wire-drawing. This treatment not only involves cost, but the copper is apt to imbibe impurity during fusion; though, if the process is carefully conducted, the deterioration is slight.

But it is evident that the re-melting of the deposited copper is a thing to be avoided, if possible, and the question naturally arises, why, now that deposition costs so little, may not the beautiful principle which comes into play in electrotype, and which enables the most complicated forms to be faithfully copied, be taken advantage of to give to plainer and heavier objects their ultimate form?

There are several reasons why this idea is not more frequently acted upon. One is, that the process of electrolytic deposition is slow; another, that knowledge of the conditions necessary for obtaining a deposit having the required strength, and other qualities, is not very widespread. Moreover, in the electrolytic deposition of copper, and indeed of all metals, there is a strong tendency to roughness on the outside of the deposit, and to excrescent growths, the removal of which involves waste of labour and material. These tendencies can, to a very great extent, be counteracted by careful manipulation, and the use of suitable solutions, and they can also be counteracted by mechanical means. This has been done by Mr. Elmore. He remedies the faults I have mentioned by causing a burnisher of agate (arranged after the manner of a tool in a screw-cutting lathe) to press upon and traverse a revolving cylindrical surface on which the deposit is taking place, and while it is immersed in the copper solution. The result is that it is kept smooth and bright to the end of the process.

But the use of the burnisher is not the only means available for the production of a smooth deposit. It was observed in the early days of electro-plating how great a change was effected in the character of the metal deposited, by the presence of a very small quantity of certain impurities. It was found, for example, that an exceedingly minute dose of bisulphide of carbon, if put into a bath from which silver was being deposited, caused the deposit to change from dull to bright.

I have lately had experience of a similar kind with nickel and with copper. I was working with a hot solution of nickel, and up to a certain point the deposit had the usual dead-grey appearance. Suddenly, and without doing anything more than putting in a new cathode, I found the character of the deposit completely changed. Instead of the grey, tough, adherent deposit, there was produced a brittle, specular deposit, which scaled off in brilliantly shining flakes of metal. I sought for the cause of this extraordinary change, and traced it to the accidental introduction into the solution of a minute quantity of glue.

By adding gelatine to a fresh nickel solution I obtained the same peculiar bright and brittle deposit that had resulted from the accident. I then made a similar addition to a solution of copper, and when I hit the right quantity—an exceedingly minute one—bright copper, instead of dull or crystalline, was deposited. Here are some specimens. These were deposited on a bright surface, and they are bright on both sides.

Not only is the copper made bright, under the conditions I have described, but, if the proportion of the gelatine be carried to the utmost that is consistent with the production of a bright deposit, it becomes exceedingly hard and brittle. Beyond this point the deposit is partly bright and partly dead, the arrangement of the patches of dead and bright being in some cases very peculiar, and suggestive of a strong conflict of opposing forces.

Before I leave the subject of copper deposition, I may mention that I have found the range of current density within which it is possible to obtain a deposit of reguline metal, far wider than is commonly supposed.

The rate of deposition in copper-refining is usually very slow, and it is one of the drawbacks of the process, since slow deposition necessitates large plant. But rapid deposition necessitates a larger consumption of power, and larger cost on that account, and therefore, there is a point beyond which it is not good economy to go, in the direction of more rapid deposition. Still there are cases, where, if we had the power to deposit more rapidly, it might be found useful to exercise it. The subject of more rapid deposition is also interesting from a scientific point of view, I therefore mention an unusual result I have arrived at in this direction.

Taking, as one extreme, the slow rate of deposit, of one ampère per square foot of cathode—a rate not infrequent in copper-refining, I have found that the limit in the other direction is not reached by a rate of deposit one thousand times faster. I have produced, and I hope to be able to produce before you, a perfectly good deposit of copper, with a current density of 1000 ampères per square foot of cathode.

This cell contains a solution of copper nitrate with a small proportion of ammonium chloride. The plate on which I am going to produce a deposit of copper has an exposed surface of 21 square inches. Opposite, at a distance of one inch, is a plate of copper. When I close the circuit, a current of 140 ampères is passing through the solution. I continue this for just one minute. Now I wash it, and remove the outer edge so as to detach the deposit, and as you see, I have a sheet of good copper—an electrotype.

To have produced a deposit of this thickness at the ordinary rate used in electrotyping operations, would have occupied more than an hour.

In this experiment an extreme degree of rapidity of deposition has been shown. I do not intend to suggest such a rate as of practical value. But it is at least interesting, as showing that the characteristic properties of copper are not less perfectly developed when the atoms of metal have been piled up one on the other at this extremely rapid rate than when there is slower aggregation.

I think it probable that a rate of deposit intermediate between this rate and the usual one of about 10 ampères per square foot may frequently be useful, for no doubt the slowness of the rate of deposit has often prevented electrotype from being made use of, where, if the rate could have been increased ten times, it might have been employed with advantage.

Here are some thick plates, deposited at the rate of 100 ampères per square foot. They are as solid and as free from flaw as plates deposited ten times more slowly.

I said that electrolytic copper-refining owed its existence to the discovery and improvement of the dynamo, and that other electro-metallurgic industries had originated from the same cause. One of these industries is the electrolytic production of aluminium.

When Deville produced aluminium by the action of sodium on aluminium chloride, exaggerated expectations were entertained of the great part it was about to play in metallurgy. It was very soon found that aluminium had not all the virtues that its too sanguine friends had claimed for it, but that it had a great many most valuable properties, and, given a certain degree of cheapness, a number of useful applications could be found for it. Some of these are suggested and shown by the various articles made of aluminium, kindly lent by the Metal Reduction Syndicate, and metallurgical research is rapidly extending our knowledge of its importance in connection with the improvement of steel castings, and the production of bronzes and other alloys of extraordinary strength. The cost of the aluminium produced by Deville's process was too great to permit of its use on any large scale for these purposes.

After Davy demonstrated, by the electrolytic extraction of potassium and sodium, the power of the electric current to break down the strong combination existing between the alkaline metals and oxygen, it seemed natural to expect that aluminium would also be reduced by the same means. But Davy did not succeed in producing any appreciable quantity of aluminium by the electrolytic method. Deville and Bunsen were more successful, but they did not possess the modern dynamo: that has made all the difference, between the small experimental results they achieved, and the industrial production of to-day, a production now so large that I suppose every day it amounts to at least one ton, and has resulted in a very great reduction of the price of the metal.

There are two electrolytic processes at work. One is the Hall process—employed at Pittsburg, and at Patricroft, Manchester—and now in experimental operation here. The other, the Herault process, worked at Neuhausen, is not greatly different from the Hall process—the shape of the furnace or crucible is different, and the composition of the bath yielding the aluminium may be different, but, in all essentials these two processes are one and the same. They depend on the electrolysis of a fused bath, composed of cryolite, aluminium fluoride, fluorspar and alumina. In the Hall process this mixture is contained in a carbon-lined iron crucible—the cathode in an electric circuit; and between which and the anode—a stick of carbon immersed in the fused bath—a difference of potential of 10 volts is maintained. In carrying out the process on a manufacturing scale, there are many of these sticks of carbon to each bath. Here, in our experimental furnace, there is only one.

The heat developed by the passing of so large a current as we are using (180 ampères), through an electrolyte of but a few inches area in cross section, is sufficient to melt and keep red-hot the fluorides in which the alumina is dissolved.

The electrolytic action results in the separation of aluminium from oxygen. The metal settles to the bottom of the pot, and is tapped, or ladled out, from time to time as it accumulates. The oxygen goes to the carbon cylinder, and burns it away at about the same rate as that at which aluminium is produced. It is only necessary to keep up the supply of alumina, to enable the operation to be continued for a long time. I mean, of course, in addition to the keeping up of the current, and the supply of carbon at the anode.

By far the greater part of the cost of aluminium obtained by electrolysis, is the cost of motive power, 20 horse-power hours are expended to produce 1 lb. of aluminium. Therefore it is essential for the cheap production of aluminium to have cheap motive power.

There is one feature about the Neuhausen production of aluminium which is very striking, and that is the generation of the electric current by means of water power derived from a portion of the Falls of the Rhine at Schaffhausen.

The motive for making use of water power is economy. But apart from that, it is interesting to see water replacing coal, not only in the production of power, but also in the production of the heat required in a smelting furnace.

Here is the Hall apparatus on a small scale. It is simply a carbon-lined iron crucible, and a thick stick of carbon. As already mentioned, the crucible is the cathode, the stick of carbon the anode.

As the process takes time to get into full operation, it was commenced some hours ago, and at the rate at which it has been working, we should by now have produced several ounces of aluminium. In beginning the process, the charge has first to be melted. This is done by bringing the carbon stick into contact with the bottom of the crucible, so as to allow the current to pass from carbon to carbon to develop heat between the electrodes.

The alumina compound, which, when melted, forms the bath, is added, in powder, little by little, and when sufficient is melted, the carbon stick is raised out of contact with the bottom, and the electrolytic action then commences.

I will now ask Mr. Sample to empty the crucible and let us see the result of the operation, and while he is doing so I take the opportunity of expressing my very sincere thanks for his having so kindly and so successfully carried out this most interesting demonstration of the latest and one of the most important of all the applications of electricity to metallurgical operations.

Here is the result of our experiment. It is not very large, certainly, but it is quite enough for our purpose, which is to illustrate the principle of a newly developed electro-metallurgical industry directly derived from discoveries made at the Royal Institution.

[J. W. S.]

EXTRA EVENING MEETING,

Thursday, February 4, 1892.

Sir Frederick Bramwell, Bart. D.C.L. F.R.S. Honorary Secretary and Vice-President, in the Chair.

Nikola Tesla, Esq.

Alternate Currents of High Potential and High Frequency.

At the first outset this investigation was taken up with the view of studying the effects of rapidly changing electrostatic and electromagnetic stresses. It was thought, from theoretical considerations, that some useful observations would be made in following up this line of experiment by means of properly constructed apparatus; but the anticipations were by far surpassed, for a number of unexpected phenomena were noted, and some novel facts brought to light, which have opened up a new and promising field of research. Some of the results obtained are of special interest on account of their direct bearing upon the problem of producing an efficient illuminant.

The phenomena which are due to the changing character of the stresses are exalted when the time rate of change is increased, hence the study of these phenomena is much facilitated by the employment of apparatus adapted especially for the purpose of carrying on such investigations. With this object in view, several types of alternators were constructed, capable of giving currents of frequencies from five to ten thousand and even more. Currents of much higher frequencies used in some of these experiments, were obtained by disruptively discharging condensers.

The construction of the alternators offered at first great difficulties. To obtain these frequencies it was necessary to provide several hundred polar projections, which were necessarily small and offered many drawbacks, and this the more as exceedingly high peripheral speeds had to be resorted to. In some of the first machines both armature and field had polar projections. These machines produced a curious noise, especially when the armature was started from the state of rest, the field being charged. The most efficient machine was found to be one with a drum armature, the iron body of which consisted of very thin wire annealed with special care. It was, of course, desirable to avoid the employment of iron in the armature, and several machines of this kind, with moving or stationary conductors, were constructed, but the results obtained were not quite satisfactory, on account of the great mechanical and other difficulties encountered. A few of the machines constructed were described in some periodicals of the past year, notably in the *Electrical Engineer*, New York, March 18, 1891.

The study of the properties of the high frequency currents obtained from these machines is very interesting, as nearly every experiment discloses something new.

Two coils traversed by such a current attract or repel each other with a force which, owing to the imperfection of our sense of touch, seems continuous.

An observation, scarcely foreseen, is that a piece of iron, surrounded by a coil through which the current is passing appears to be continuously magnetised. This apparent continuity might be ascribed to the deficiency of the sense of touch, but there is evidence that in currents of such high frequencies one of the impulses preponderates over the other.

As might be expected, conductors traversed by such currents are rapidly heated, owing to the increase of the resistance, and the heating effects are relatively much greater in the iron.

The hysteresis losses in iron are so great that an iron core, even if finely subdivided, is heated in an incredibly short time. To give an idea, an ordinary iron wire of 1/16 inch in diameter inserted within a coil having 250 turns, with a current estimated to be five ampères passing through the coil, becomes within two seconds' time so hot as to scorch wood. Beyond a certain frequency, an iron core, no matter how finely subdivided, exercises a dampening effect, and it was easy to find a point at which the impedance of a coil was not affected by the presence of a core consisting of a bundle of very thin well annealed and varnished iron wires.

Experiments with a telephone, a conductor in a strong magnetic field, or with a condenser or arc, seem to afford certain proof that sounds far above the usually accepted limit of hearing would be perceived if produced with sufficient power.

The arc produced by these currents possesses several interesting features. Usually it emits a note the pitch of which corresponds to twice the frequency of the current, but if the frequency be sufficiently high it becomes noiseless, the limit of audition being determined principally by the linear dimensions of the arc. A curious feature of the arc is its persistency, which is due partly to the inability of the gaseous column to cool and increase considerably in resistance, as in the case with low frequencies, and partly to the tendency of such a high frequency machine to maintain a constant current.

In connection with these machines the condenser affords a particularly interesting study. Striking effects are produced by proper adjustments of capacity and self-induction. It is easy to raise the electro-motive force of the machine to many times the original value by simply adjusting the capacity of a condenser connected in the induced circuit. If the condenser be at some distance from the machine, the difference of potential on the terminals of the latter may be only a small fraction of that on the condenser.

But the most interesting experiences are made when the tension of the currents from the machine is raised by means of an induction coil. In consequence of the enormous rate of change obtainable in the primary current, much higher potential differences are obtained than with coils operated in the usual ways, and, owing to the high frequency, the secondary discharge possesses many striking peculiarities. Both the electrodes behave generally alike, though it appears from some observations that one current impulse preponderates over the other, as before mentioned.

The physiological effects of the high tension discharge are found to be so small that the shock of the coil can be supported without any inconvenience, except perhaps a small burn produced by the discharge upon approaching the hand to one of the terminals.

The decidedly smaller physiological effects of these currents are thought to be due either to a different distribution through the body or to the tissues acting as condensers. But in the case of an induction coil with a great many turns the harmlessness is principally due to the fact that but little energy is available in the external circuit when the same is closed through the experimenter's body, on account of the great impedance of the coil.

In varying the frequency and strength of the currents through the primary of the coil, the character of the secondary discharge is greatly varied, and no less than five distinct forms are observed:—A weak, sensitive thread discharge, a powerful flaming discharge, and three forms of brush or streaming discharges. Each of these possesses certain noteworthy features, but the most interesting to study are the latter.

Under certain conditions the streams, which are presumably due to the violent agitation of the air molecules, issue freely from all points of the coil, even through a thick insulation. If there is the smallest air-space between the primary and secondary, they will form there and surely injure the coil by slowly warming the insulation. As they form even with ordinary frequencies when the potential is excessive, the air-space must be most carefully avoided.

These high frequency streamers differ in aspect and properties from those produced by a static machine. The wind produced by them is small and should altogether cease if still considerably higher frequencies could be obtained.

A peculiarity is that they issue as freely from surfaces as from points. Owing to this, a metallic vane, mounted in one of the terminals of the coil so as to rotate freely, and having one of its sides covered with insulation, is spun rapidly around. Such a vane would not rotate with a steady potential, but with a high frequency coil it will spin, even if it be entirely covered with insulation, provided the insulation on one side be either thicker or of a higher specific inductive capacity. A Crookes' electric radiometer is also spun around when connected to one of the terminals of the coil, but only at very high exhaustion or at ordinary pressures.

There is still another and more striking peculiarity of such a high frequency streamer, namely, it is hot. The heat is easily perceptible with frequencies of about 10,000, even if the potential is not excessively high. The heating effect is, of course, due to the molecular impacts and collisions. Could the frequency and potential be pushed far enough, then a brush could be produced resembling in every particular a flame and giving light and heat, yet without a chemical process taking place.

The hot brush, when properly produced, resembles a jet of burning gas escaping under great pressure, and it emits an extraordinary strong smell of ozone. The great ozonising action is ascribed to the fact that the agitation of the molecules of the air is more violent in such a brush than in the ordinary streamer of a static machine.

But the most powerful brush discharges were produced by employing currents of much higher frequencies than it was possible to obtain by means of the alternators. These currents were obtained by disruptively discharging a condenser and setting up oscillations. In this manner currents of a frequency of several hundred thousand were obtained.

Currents of this kind produce striking effects. At these frequencies, the impedance of a copper bar is so great that a potential difference of several hundred volts can be maintained between two points of a short and thick bar, and it is possible to keep an ordinary incandescent lamp burning at full candle power by attaching the terminals of the lamp to two points of the bar no more than a few inches apart. When the frequency is extremely high, nodes are found to exist on such a bar, and it is easy to locate them by means of a lamp.

By converting the high tension discharges of a low frequency coil in this manner, it was found practicable to keep a few lamps burning on the ordinary circuit in the laboratory, and by bringing the undulation to a low pitch, it was possible to operate small motors.

This plan likewise allows of converting high tension discharges of one direction in low tension unidirectional currents, by adjusting the circuit so that there are no oscillations. In passing the oscillating discharges through the primary of a specially constructed coil, it is easy to obtain enormous potential differences with only few turns of the secondary.

Great difficulties were at the beginning experienced in producing a successful coil on this plan. It was found necessary to keep all air, or gaseous matter in general, away from the charged surfaces, and oil immersion was resorted to. The wires used were heavily covered with gutta-percha and wound in oil, or the air was pumped out by means of a Sprengel pump.

The general arrangement was the following: — An ordinary induction coil, operated from a low frequency alternator, was used to charge Leyden jars. The jars were made to discharge over a single or multiple gap through the primary of the second coil. To insure the action of the gap, the arc was blown out by a magnet or air-blast. To adjust the potential in the secondary a small oil condenser was used, or polished brass spheres of different sizes were screwed on the terminals and their distance adjusted.

When the conditions were carefully determined to suit each experiment, magnificent effects were obtained.

Two wires, stretched through the room, each being connected to one of the terminals of the coil, emit streams so powerful that the light from them allows distinguishing the objects in the room; the wires become luminous even if covered with thick and most excellent insulation. When two straight wires, or two concentric circles of wire, are connected to the terminals, and set at the proper distance, a uniform luminous sheet is produced between them. It was possible in this way to cover an area of more than one meter square completely with the streams. By attaching to one terminal a large circle of wire and to the other terminal a small sphere, the streams are focussed upon the sphere, produce a strongly lighted spot upon the same, and present the appearance of a luminous cone. A very thin wire glued upon a plate of hard rubber of great thickness, on the opposite side of which is fastened a tinfoil coating, is rendered intensely luminous when the coating is connected to the other terminal of the coil. Such an experiment can be performed also with low frequency currents, but much less satisfactorily.

When the terminals of such a coil, even of a very small one, are separated by a rubber or glass plate, the discharge spreads over the plate in the form of streams, threads, or brilliant sparks, and affords a magnificent display, which cannot be equalled by the largest coil operated in the usual ways. By a simple adjustment it is possible to produce with the coil a succession of brilliant sparks, exactly like with a Holtz machine.

Under certain conditions, when the frequency of the oscillation is very great, white phantom-like streams are seen to break forth from the terminals of the coil. The chief interesting feature about them is, that they stream freely against the outstretched hand or other conducting object without producing any sensation, and the hand may be approached very near to the terminal without a spark being induced to jump. This is due presumably to the fact that a considerable portion of the energy is carried away or dissipated in the streamers, and the difference of potential between the terminal and the hand is diminished.

It is found in such experiments, that the frequency of the vibration and the quickness of succession of the sparks between the knobs affect to a marked degree the appearance of the streams. When the frequency is very low, the air gives way in more or less the same manner as by a steady difference of potential, and the streams consist of distinct threads, generally mingled with thin sparks, which probably correspond to the successive discharges occurring between the knobs. But when the frequency is very high, and the arc of the discharge produces a sound which is loud and smooth (which indicates both that oscillation takes place and that the sparks succeed each other with great rapidity), then the luminous streams formed are perfectly uniform. They are generally of a purplish hue, but when the molecular vibration is increased by raising the potential they assume a white colour.

The luminous intensity of the streams increases rapidly when the potential is increased; and with frequencies of only a few hundred thousand, could the coil be made to withstand a sufficiently high potential difference, there is no doubt that the space around a wire could be made to emit a strong light, merely by the agitation of the molecules of the air at ordinary pressure.

Such discharges of very high frequency which render luminous the air at ordinary pressure we have very likely occasion to witness in the Aurora borealis. From many of these experiments it seems reasonable to infer that sudden cosmic disturbances, such as eruptions on the sun, set the electrostatic charge of the earth in an extremely rapid vibration, and produce the glow by the violent agitation of the air in the upper and even in the lower strata. It is thought that if the frequency were low, or even more so if the charge were not at all vibrating, the lower dense strata would break down as in a lightning discharge. Indications of such breaking down have been repeatedly observed, but they can be attributed to the fundamental disturbances, which are few in number, for the superimposed vibration would be so rapid as to not allow a disruptive break.

The study of these discharge phenomena has led to the recognition of some important facts. It was found that gaseous matter must be most carefully excluded from any dielectric which is subjected to great, rapidly-changing electrostatic stresses. Since it is difficult to exclude the gas perfectly when solid insulators are used, it is necessary to resort to liquid dielectrics. When a solid dielectric is used, it matters little how thick and how good it is; if air be present streamers form, which gradually heat the dielectric and impair its insulating power, and the discharge finally breaks through. Under ordinary conditions the best insulators are those which possess the highest specific inductive capacity, but such insulators are not the best to employ when working with these high frequency currents, for in most cases the higher specific inductive capacity is rather a disadvantage. The prime quality of the insulating medium for these currents is continuity. For this reason principally it is necessary to employ liquid insulators, such as oils. If two metal plates, connected to the terminals of the coil, are immersed in oil and set a distance apart, the coil may be kept working for any length of time without a break occurring, or without the oil being warmed, but if air bubbles are introduced, they become luminous; the air molecules, by their impact against the oil, heat it, and after some time cause the insulation to give way. If, instead of the oil, a solid plate of the best dielectric, even several times thicker than the oil intervening between the metal plates, is inserted between the latter, the air having free access to the charged surfaces, the dielectric invariably is warmed and breaks down.

The employment of the oil is advisable or necessary even with low frequencies, if the potentials are such that streamers form, but only in such cases, as is evident from the theory of the action. If the potentials are so low that streamers do not form, then it is even disadvantageous to employ oil, for it may, principally by confining the heat, be the cause of the breaking down of the insulation.

The exclusion of gaseous matter is not only desirable on account of the safety of the apparatus, but also on account of economy, especially in a condenser, in which considerable waste of power may occur merely owing to the presence of air, if the electric density on the charged surfaces is great.

In the course of these investigations a phenomenon of special scientific interest has been observed. It may be ranked among the brush phenomena, in fact it is a kind of brush which forms at, or near, a single terminal in high vacuum. In a bulb with a conducting electrode, even if the latter be of aluminium, the brush has only a very short existence, but it can be preserved for a considerable length of time in a bulb devoid of any conducting electrode. To observe the phenomenon it is found best to employ a large spherical bulb having in its centre a small bulb supported on a tube sealed to the neck of the former. The large bulb being exhausted to a high degree, and the inside of the small bulb being connected to one of the terminals of the coil, under certain conditions there appears a misty haze around the small bulb, which, after passing through some stages, assumes the form of a brush, generally at right angles to the tube supporting the small bulb. When the brush assumes this form it may be brought to a state of extreme sensitiveness to electrostatic and magnetic influence. The bulb hanging straight down, and all objects being remote from it, the approach of the observer within a few paces will cause the brush to fly to the opposite side, and if he walks around the bulb it will always keep on the opposite side. It may begin to spin around the terminal long before it reaches that sensitive stage. When it begins to turn around, principally, but also before, it is affected by a magnet, and at a certain stage it is susceptible to magnetic influence to an astonishing degree. A small permanent magnet, with its poles at a distance of no more than two centimetres, will affect it visibly at a distance of two metres, slowing down or accelerating the rotation according to how it is held relatively to the brush.

When the bulb hangs with the globe down, the rotation is always clockwise. In the southern hemisphere it would occur in the opposite direction and on the (magnetic) equator the brush should not turn at all. The rotation may be reversed by a magnet kept at some distance. The brush rotates best, seemingly, when it is at right angles to the lines of force of the earth. It very likely rotates, when at its maximum speed, in synchronism with the alternations, say 10,000 times a second. The rotation can be slowed down or accelerated by the approach or receding of the observer, or any conducting body, but it cannot be reversed by putting the bulb in any position. Very curious experiments may be performed with the brush when in its most sensitive state. For instance, the brush resting in one position, the experimenter may, by selecting a proper position, approach the hand at a certain considerable distance to the bulb, and he may cause the brush to pass off by merely stiffening the muscles of the arm, the mere change of configuration of the arm and imperceptible displacement being sufficient to disturb the delicate balance. When it begins to rotate slowly, and the hands are held at a proper distance, it is impossible to make even the slighest motion without producing a visible effect upon the brush. A metal plate connected to the other terminal of the coil affects it at a great distance, slowing down the rotation often to one turn a second.

It is hoped that this phenomenon will prove a valuable aid in the investigation of the nature of the forces acting in an electrostatic or magnetic field. If there is any motion which is measurable going on in the space, such a brush would be apt to reveal it. It is, so to speak, a beam of light, frictionless, devoid of inertia.

On account of its marvellous sensitiveness to electrostatic or magnetic disturbances it may be the means of sending signals through submarine cables with any speed, and even of transmitting intelligence at distance without wires.

In operating an induction coil with these rapidly alternating currents, it is astonishing to note, for the first time, the great importance of the relation of capacity, self-induction, and frequency as regards the general result. The combination of these elements produces many curious effects. For instance, two metal plates are connected to the terminals and set at a small distance, so that an arc is formed between them. This arc *prevents* a strong current to flow through the coil. If the arc be interrupted by the interposition of a glass plate, the capacity of the condenser obtained counteracts the self-induction, and a stronger current is made to pass. The effects of capacity are the most striking, for in these experiments, since the self-induction and frequency both are high, the critical capacity is very small, and need be but slightly varied to produce a very considerable change. The experimenter brings his body in contact with the terminals of the secondary of the coil, or attaches to one or both terminals insulated bodies of very small bulk, such as exhausted bulbs, and he produces a considerable rise or fall of potential on the secondary, and greatly affects the flow of the current through the primary coil.

In many of the phenomena observed, the presence of the air, or, generally speaking, of a medium of a gaseous nature (using this term not to imply specific properties, but as contradistinction to homogeneity or perfect continuity) plays an important part, as it allows energy to be dissipated by molecular impact or bombardment. The action is thus explained :—

When an insulated body connected to a terminal of the coil is suddenly charged to a high potential, it acts inductively upon the surrounding air, or whatever gaseous medium there might be. The molecules or atoms which are near it are, of course, more attracted, and move through a greater distance than the further ones. When the nearest molecules strike the body they are repelled, and collisions occur at all distances within the inductive distance. It is now clear that, if the potential be steady, but little loss of energy can be caused in this way, for the molecules which are nearest to the body having had an additional charge imparted to them by contact, are not attracted until they have parted, if not with all, at least with most of the additional charge, which can be accomplished only after a great many collisions. This is inferred from the fact that with a steady potential there is but little loss in dry air. When the potential, instead of being steady, is alternating, the conditions are entirely different. In this case a rhythmical bombardment occurs, no matter whether the molecules after coming in contact with the body lose the imparted charge or not, and, what is more, if the charge is not lost, the impacts are only the more violent. Still, if the frequency of the impulses be very small, the loss caused by the impacts and collisions would not be serious unless the potential were excessive. But when extremely high frequencies and more or less high potentials are used, the loss may be very great. The total energy lost per unit of time is proportionate to the product of the number of impacts per second, or the frequency and the energy lost in each impact. But the energy of an impact must be proportionate to the square of the electric density of the body, on the assumption that the charge imparted to the molecule is proportionate to that density. It is concluded from this that the total energy lost must be proportionate to the product of the frequency and the square of the electric density; but this law needs experimental confirmation. Assuming the preceding considerations to be true, then, by rapidly alternating the potential of a body immersed in an insulating gaseous medium, any amount of energy may be dissipated into space. Most of that energy, then, is not dissipated in the form of long ether waves, propagated to considerable distance, as is thought most generally, but is consumed in impact and collisional losses—that is, heat vibrations—on the surface and in the vicinity of the body. To reduce the dissipation it is necessary to work with a small electric density—the smaller the higher the frequency.

The behaviour of a gaseous medium to such rapid alternations of potential makes it appear plausible that electrostatic disturbances of the earth, produced by cosmic events, may have great influence upon the meteorological conditions. When such disturbances occur both the frequency of the vibrations of the charge and the potential are in all probability excessive, and the energy converted into heat may be considerable. Since the density must be unevenly distributed, either in consequence of the irregularity of the earth's surface, or on account of the condition of the atmosphere in various places, the effect produced would accordingly vary from place to place. Considerable variations in the temperature and pressure of the atmosphere may in this manner be caused at any point of the surface of the earth. The variations may be gradual or very sudden, according to the nature of the original disturbance, and may produce rain and storms, or locally modify the weather in any way.

From many experiences gathered in the course of these investigations it appears certain that in lightning discharges the air is an element of importance. For instance, during a storm a stream may form on a nail or pointed projection of a building. If lightning strikes somewhere in the neighbourhood, the harmless static discharge may, in consequence of the oscillations set up, assume the character of a high-frequency streamer, and the nail or projection may be brought to a high temperature by the violent impact of the air molecules. Thus, it is thought, a building may be set on fire without the lightning striking it.

In like manner small metallic objects may be fused and volatilised—as frequently occurs in lightning discharges—merely because they are surrounded by air. Were they immersed in a practically continuous medium, such as oil, they would probably be safe, as the energy would have to spend itself elsewhere.

An instructive experience having a bearing on this subject is the following:—A glass tube of an inch or so in diameter and several inches long is taken, and a platinum wire sealed into it, the wire running through the centre of the tube from end to end. The tube is exhausted to a moderate degree. If a steady current is passed through the wire it is heated uniformly in all parts and the gas in the tube is of no consequence. But if high frequency discharges are directed through the wire, it is heated more on the ends than in the middle portion, and if the frequency, or rate of charge, is high enough, the wire might as well be cut in the middle as not, for most of the heating on the ends is due to the rarefied gas. Here the gas might only act as a conductor of no impedance, diverting the current from the wire as the impedance of the latter is enormously increased, and merely heating the ends of the wire by reason of their resistance to the passage of the discharge. But it is not at all necessary that the gas in the tube should be conducting; it might be at an extremely low pressure, still the ends of the wire would be heated, as, however, is ascertained by experience, only the two ends would in such case not be electrically connected through the gaseous medium. Now what with these frequencies and potentials occurs in an exhausted tube, occurs in the lightning discharge at ordinary pressure.

From the facility with which any amount of energy may be carried off through a gas, it is concluded that the best way to render harmless a lightning discharge is to afford it in some way a passage through a volume of gas.

The recognition of some of the above facts has a bearing upon far-reaching scientific investigations in which extremely high frequencies and potentials are used. In such cases the air is an important factor to be considered. So, for instance, if two wires are attached to the terminals of the coil, and streamers issue from them, there is dissipation of energy in the form of heat and light, and the wires behave like a condenser of larger capacity. If the wires be immersed in oil, the dissipation of energy is prevented, or at least reduced, and the apparent capacity is diminished. The action of the air would seem to make it very difficult to tell, from the measured or computed capacity of a condenser in which the air is acted upon, its actual capacity or vibration period, especially if the condenser is of very small surface and is charged to a very high potential. As many important results are dependent upon the correctness of the estimation of the vibration period, this subject demands the most careful scrutiny of other investigators.

In Leyden jars the loss due to the presence of air is comparatively small, principally on account of the great surface of the coatings and the small external action, but if there are streamers on the top, the loss may be considerable, and the period of vibration is affected. In a resonator, the density is small, but the frequency is extreme, and may introduce a considerable error. It appears certain, at any rate, that the periods of vibration of a charged body in a gaseous and in a continuous medium, such as oil, are different, on account of the action of the former, as explained.

Another fact recognised, which is of some consequence, is, that in similar investigations the general considerations of static screening are not applicable when a gaseous medium is present. This is evident from the following experiment.

A short and wide glass tube is taken and covered with a substantial coating of bronze, barely allowing the light to shine a little through. The tube is highly exhausted and suspended on a metallic clasp from the end of a wire. When the wire is connected with one of the terminals of the coil, the gas inside of the tube is lighted in spite of the metal coating. Here the metal evidently does not screen the gas inside as it ought to, even if it be very thin and poorly conducting. Yet, in a condition of rest the metal coating, however thin, screens the inside perfectly.

One of the most interesting results arrived at in pursuing these experiments, is the demonstration of the fact that a gaseous medium, upon which vibration is impressed by rapid changes of electrostatic potential, is rigid. In illustration of this result an experiment may be cited.

A glass tube about 1 inch in diameter and 3 feet long, with outside condenser coatings on the ends, was exhausted to a certain point, when, the tube being suspended freely from a wire connecting the upper coating to one of the terminals of the coil, the discharge appeared in the form of a luminous thread, passing through the axis of the tube. Usually the thread was sharply defined in the upper part of the tube and lost itself in the lower part. When a magnet or the finger was quickly passed near the upper part of the luminous thread, it was brought out of position by magnetic or electrostatic influence, and a transversal vibration like that of a suspended cord, with one or more distinct nodes, was set up, which lasted for a few minutes and died gradually out. By suspending to the lower condenser coating metal plates of different sizes, the speed of the vibration was varied. This vibration would seem to show beyond doubt that the thread possessed rigidity, at least to transversal displacements.

Many experiments were tried to demonstrate this property in air at ordinary pressure. Though no positive evidence has been obtained, it is thought nevertheless, that a high frequency brush or streamer, if the frequency could be pushed far enough, would be decidedly rigid. A small sphere might then be moved within it quite freely, but if thrown against it the sphere would rebound. An ordinary flame cannot possess rigidity to a marked degree because the vibration is directionless; but an electric arc, it is believed, must possess that property more or less. A luminous band excited in a bulb by repeated discharges of a Leyden jar must also possess rigidity, and if deformed and suddenly released should vibrate.

From like considerations other conclusions of interest may be made. The most probable medium filling the space is one consisting of independent carriers immersed in an insulating fluid. If through this medium enormous electrostatic stresses are assumed to act, which vary rapidly in intensity, it would allow the motion of a body through it, yet it would be rigid and elastic, although the fluid itself might be devoid of these properties. Furthermore, on the assumption that the independent carriers are of any configuration such that the fluid resistance to motion in one direction is greater than in another, a stress of that nature would cause the carriers to arrange themselves in groups, since they would turn to each other their sides of the greatest electric density, in which position the fluid resistance to approach would be smaller than to receding. If in a medium of the above characteristics a brush would be formed by a steady potential, an exchange of the carriers would go on continually, and there would be less carriers per unit of volume in the brush than in the space at some distance from the electrode, this corresponding to rarefaction. If the potential were rapidly changing, the result would be very different: the higher the frequency of the pulses, the slower would be the exchange of the carriers; finally, the motion of translation through measurable space would cease, and, with a sufficiently high frequency and intensity of the stress, the carriers would be drawn towards the electrode, and compression would result.

An interesting feature of these high frequency currents is that they allow to operate all kinds of devices by connecting the device with only one leading wire to the source. In fact, under certain conditions it may be more economical to supply the electrical energy with one lead than with two.

An experiment of special interest is the running, by the use of only one insulated line, of a motor operating on the principle of the rotating magnetic field enunciated by the author a few years ago. A simple form of such a motor is obtained by winding upon a laminated iron core a primary and close to it a secondary coil, closing the ends of the latter and placing a freely movable metal disk within the influence of the moving field. The secondary coil may, however, be omitted. When one of the ends of the primary coil of the motor is connected to one of the terminals of the high-frequency coil and the other end to an insulated metal plate, which, it should be stated, is not absolutely necessary for the success of the experiment, the disk is set in rotation.

Experiments of this kind seem to bring it within the reach of possibility to operate a motor at any point of the earth's surface from a central source, without any connection to the same except through the earth. If, by means of powerful machinery, rapid variations of the earth's potential were produced, a grounded wire reaching up to some height would be traversed by a current which could be increased by connecting the free end of the wire to a body of some size. The current might be converted to low tension and used to operate a motor or other device. The experiment, which would be one of great scientific interest, would probably best succeed on a ship at sea. In this manner, even if it were not possible to operate machinery, intelligence might be transmitted quite certainly.

In the course of this experimental study special attention was devoted to the heating effects produced by these currents, which are not only striking, but open up the possibility of producing a more efficient illuminant. It is sufficient to attach to the coil terminal a thin wire or filament, to have the temperature of the latter perceptibly raised. If the wire or filament be inclosed in a bulb, the heating effect is increased by preventing the circulation of the air. If the air in the bulb be strongly compressed, the displacements are smaller, the impacts less violent, and the heating effect is diminished. On the contrary, if the air in the bulb be exhausted, an inclosed lamp filament is brought to incandescence, and any amount of light may thus be produced.

The heating of the inclosed lamp filament depends on so many things of a different nature, that it is difficult to give a generally applicable rule under which the maximum heating occurs. As regards the size of the bulb, it is ascertained that at ordinary or only slightly differing atmospheric pressures, when air is a good insulator, the filament is heated more in a small bulb, because of the better confinement of heat in this case. At lower pressures, when air becomes conducting, the heating effect is greater in a large bulb, but at excessively high degrees of exhaustion there seems to be, beyond a certain and rather small size of the vessel, no perceptible difference in the heating.

The shape of the vessel is also of some importance, and it has been found of advantage for reasons of economy to employ a spherical bulb with the electrode mounted in its centre, where the rebounding molecules collide.

It is desirable on account of economy that all the energy supplied to the bulb from the source should reach without loss the body to be heated. The loss in conveying the energy from the source to the body may be reduced by employing thin wires heavily coated with insulation, and by the use of electrostatic screens. It is to be remarked, that the screen cannot be connected to the ground as under ordinary conditions.

In the bulb itself a large portion of the energy supplied may be lost by molecular bombardment against the wire connecting the body to be heated with the source. Considerable improvement was effected by covering the glass stem containing the wire with a closely fitting conducting tube. This tube is made to project a little above the glass, and prevents the cracking of the latter near the heated body. The effectiveness of the conducting tube is limited to very high degrees of exhaustion. It diminishes the energy lost in bombardment for two reasons: firstly, the charge given up by the atoms spreads over a greater area, and hence the electric density at any point is small, and the atoms are repelled with less energy than if they would strike against a good insulator; secondly, as the tube is electrified by the atoms which first come in contact with it, the progress of the following atoms against the tube is more or less checked by the repulsion which the electrified tube must exert upon the similarly electrified atoms. This, it is thought, explains why the discharge through a bulb is established with much greater facility when an insulator than when a conductor is present.

During the investigations a great many bulbs of different construction, with electrodes of different material, were experimented upon, and a number of observations of interest were made.

It was found that the deterioration of the electrode is the less the higher the frequency. This was to be expected, as then the heating is effected by many small impacts, instead of by fewer and more violent ones, which shatter quickly the structure. The deterioration is also smaller when the vibration is harmonic. Thus an electrode, maintained at a certain degree of heat, lasts much longer with currents obtained from an alternator, than with those obtained by means of a disruptive discharge. One of the most durable electrodes was obtained from strongly compressed carborundum, which is a kind of carbon recently produced by Mr. E. G. Acheson. From experience, it is inferred, that to be most durable, the electrode should be in the form of a sphere with a highly polished surface.

In some bulbs refractory bodies were mounted in a carbon cup and pushed under the molecular impact. It was observed in such experiments that the carbon cup was heated at first, until a higher temperature was reached; then most of the bombardment was directed against the refractory body, and the carbon was relieved. In general, when different bodies were mounted in the bulb, the hardest fusible would be relieved, and would remain at a considerably lower temperature. This was necessitated by the fact that most of the energy supplied would find its way through the body which was easier fused or "evaporated."

Curiously enough it appeared in some of the experiments made, that a body was fused in a bulb under the molecular impact by evolution of less light than when fused by the application of heat in ordinary ways. This may be ascribed to a loosening of the structure of the body under the violent impacts and changing stresses.

Some experiences seem to indicate that under certain conditions a body, conducting or nonconducting, may, when bombarded, emit light, which to all appearance is due to phosphorescence, but may in reality be caused by the incandescence of an infinitesimal layer, the mean temperature of the body being comparatively small. Such might be the case if each single rhythmical impact were capable of instantaneously exciting the retina, and the rhythm just high enough to cause a continuous impression in the eye. According to this view, a coil operated by disruptive discharge would be eminently adapted to produce such a result, and it is found in experience that its power of exciting phosphorescence is extraordinarily great. It is capable of exciting phosphorescence at comparatively low degrees of exhaustion, and also projects shadows at pressures far greater than those at which the mean free path is comparable to the dimensions of the vessel. The latter observation is of some importance, inasmuch as it may modify the generally accepted views in regard to the "radiant state" phenomena.

A thought, which early and naturally suggested itself, was to utilise the great inductive effects of high frequency currents to produce light in a sealed glass vessel without the use of leading-in wires. Accordingly, many bulbs were constructed in which the energy necessary to maintain a button or filament at high incandescence, was supplied through the glass either by electrostatic or electrodynamic induction. It was likewise easy to regulate the intensity of the light emitted by means of an externally applied condenser coating connected to an insulated plate, or simply by means of a plate attached to the bulb which at the same time performed the function of a shade.

A subject of experiment, which has been exhaustively treated by Prof. J. J. Thomson, has been followed up independently by the author from the beginning of this study, namely, to excite by electrodynamic induction a luminous band in a closed tube or bulb. In observing the behaviour of gases, and the luminous phenomena obtained, the importance of the electrostatic effects was noted and it appeared desirable to produce enormous potential differences, alternating with extreme rapidity. Experiments in this direction led to some of the most interesting results arrived at in the course of these investigations. It was found that by rapid alternations of a high electrostatic potential, exhausted tubes could be lighted at considerable distance from a conductor connected to a properly constructed coil, and that it was practicable to establish with the coil an alternating electrostatic field, acting through the whole extent of a room and lighting a tube, wherever it was placed in the same. Phosphorescent bulbs may be excited in such a field, and it is easy to regulate the effect by connecting to the bulb a small insulated metal plate. It was likewise possible to maintain a filament or button mounted in a tube at bright incandescence, and in one experiment, a mica vane was spun by the incandescence of a platinum wire.

It is hoped that the study of these phenomena, and the perfection of the means for obtaining rapidly alternating high potentials, will lead to the production of an efficient illuminant.

[NIKOLA TESLA.]

WEEKLY EVENING MEETING,

Friday, May 27, 1892.

Sir Frederick Bramwell, Bart. D.C.L. LL.D. F.R.S.
Honorary Secretary and Vice-President, in the Chair.

Sir James Crichton-Browne, M.D. LL.D. F.R.S. *Treas. R.I.*

Emotional Expression.

It has been argued that any detailed list or description of the emotions is impossible, and that so numerous and varied are the feelings of the mind that it is as futile and unprofitable to catalogue them as it would be to register the ever shifting patterns of the kaleidoscope. These feelings, it is alleged, are not specific mental entities like the old immutable species of plants and animals, but ever changing phases of life which defy analysis and classification. Now, allowing that many psychologists have divided and subdivided the feelings with too much elaboration, have drawn artificial distinctions between them, and have mistaken mere passing phenomena for permanent types, I must still maintain that the feelings do admit of arrangement in certain great natural orders, and that however multifarious or blended their manifestations may be, we can always recognise in them some of the ingredients of which they are made up. When we look at a painting—a landscape, portrait, or historical scene, and delight in its rich and delicate colouring, we soon perceive that the colours that please us are cunningly and perplexingly mixed. Almost every touch is compounded of several pigments, and in no two touches do the same pigments mingle in exactly the same proportion. And yet in these compounded touches we can say in most instances what pigments have gone to their composition, while every here and there, amongst them we discern a point or streak of pure colour, and so in the end we can affirm with tolerable certainty what paints—crimson-lake, burnt-sienna, flake-white, or Vandyke-brown—were on the palette of the painter who produced the picture. And so when we witness on the canvas of the human face, representations of feeling, trivial, stirring, or profound, we are at first struck by the high complexity and infinite variety of their constituent parts. The features move in ever new and subtle combinations, and for no two seconds are their aspects exactly alike. But when we look a little deeper we find that we are able to identify in the flux of feeling many of its constituent elements, while here and there we detect flashes of pure primitive emotion, and the result is that we are able to say with some confidence what feelings—curiosity, rage, hatred, or pride—were in the mind of the man or woman whose agitated countenance we have been watching.

And to carry our analogy a little further, we can by comparing the works of different painters arrive at certain definite conclusions as to their colour proclivities, and satisfy ourselves that one has a partiality for yellow, another prefers rose-madder, and a third neutral tints. And so by comparing the facial manifestations of feeling in different men and women, we can discover their general emotional tone and temperament and put down one as spiteful, another as melancholic, and a third as choleric.

And to take still another step in analogy; we know that the pigments on the palette of the painter are not ultimate colours, but would on final analysis resolve themselves into the primary colours red, blue, or green, and yet we find the recognition and distinction of these pigments convenient, nay, essential in all practical artistic work and in conversation, and so we know that the feelings in a man's mind which we identify by the expressions flitting across his face, are not ultimate states of mind, but might be resolved into simpler and simpler constituents, last of all perhaps into the primitive feelings of defence, accompanying contraction, and of attack, accompanying expansion in the lowest organisms (the negative and positive emotional poles), and yet we find the differentiation and the naming of these several feelings not only convenient but essential in all physiological work, and in all intercourse between man and man. We all know by our daily experience that there are such feelings as jealousy, modesty, compassion, and contempt; and whatever the composition of these feelings may be, or however intricate the enlacements which connect them with other feelings, we treat them as realities, and know tolerably well what we mean when we are talking about them.

Now the feelings of the mind, whether crude or subtle, are expressed in several different ways, but always in movement. Expression in its widest sense includes form, gesture, play of feature, visceral changes, and language, but each of these implies movement. Form, which corresponds with the development of the skeleton and the chiselling of the soft parts, depends on the movements of growth; gesture depends on the movements of the muscles of the trunk and limbs; play of feature on the movements of the muscles of the face; visceral changes on the movements of the blood-vessels supplying internal organs; and language on the movements of the muscles of the larynx, tongue, and lips. Almost every feeling that arises in the mind, we have now reason to believe, expresses itself in all these ways, except in the most evolved and highly symbolic way, that is to say, in speech. The central excitement corresponding with feeling aroused by any object in our environment or recalled in memory, reverberates through the whole organism, and may by delicate instruments be detected, not only in the quivering of the muscles, but in the heart-beats, the respiration, the sweat-glands, the pupils of the eyes, and indeed everywhere. But while each feeling thus diffuses its effects generally throughout the body, each feeling has at the same time special channels, internal and external, through which it principally discharges itself, and makes its existence known. Each feeling has its appropriate expression different from the expression of every other feeling, and our main objects this evening are to inquire what are the appropriate expressions of certain feelings, and how comes it that these appropriate expressions have been attached to certain feelings? What is the origin and significance of emotional expression?

In the investigation of these questions—and few questions are more difficult or obscure—the illustrious Darwin has, as I daresay you know, taken the foremost place. By an elaborate research he endeavours to explain the origin of the expressions, used by man and animals under the influence of emotions, and he arrives at the conclusion that these are to be explained by three principles which he calls the principles of Associated Serviceable Habits, of Antithesis, and of the Action of the Nervous System.

Movements, he argues, which have been found serviceable to escape from danger, or to relieve distress, which were at first voluntarily performed for a definite object, have by frequent repetition become innate or inherited, and are afterwards performed whether of service or not, whenever the desire or sensation out of which they originally arose is again experienced. Horses, which fight with their teeth in their native state, at some remote period found it serviceable to prevent their prominent ears from being bitten or torn; by retracting them and laying them flat against the head when engaged in combat. In the course of generations this, at first purposive and voluntary movement, becomes involuntary and habitual, and now we have the laying back of the ears as the sure sign of anger in the horse—the expression of those emotions which were stirred in it by conflict, even when those emotions recur in a much milder degree, and are provoked by some insignificant cause, such as the tickling of the curry-comb, and when its ears are no longer in danger. When infants scream loudly to intimate hunger or pain, the circulation in the eyeballs and their sockets is affected. These parts become gorged with blood, an uneasy sensation results, and the infant finds that contraction of the muscles in front of the eye—the muscles of the eyelids and eyebrows—is serviceable to relieve this sensation and afford protection. By frequent repetition of this experience through many generations of infants, the action, at first wilful, has become fixed and inherited, so that now, even in grown men and women who have learnt to suppress screaming when in pain or distress, contraction of the muscles round the eyes still betrays the existence of these feelings, contraction of these muscles having become the expression of the emotion of pain or distress. This is Darwin's principle of Associated Serviceable Movements.

But movements, again Darwin argues, which have never themselves been serviceable, but which are the opposites of movements that have been so, have come to be representative of states of emotion opposite to those in which the movements of which they are themselves the opposites arise. The dog, approaching a supposed enemy, in a hostile or savage mood, holds itself erect, has a straight back, rigid tail, ears pricked forward, eyes set in a fixed stare and lips retracted so as to show the teeth, and all these attitudes and movements can be explained as serviceable, being calculated to intimidate the enemy, or to facilitate attack; but when the dog suddenly finds that it is a friend and not an enemy it is approaching, these movements and attitudes are instantly replaced by their opposites. It crouches, its back is curved, its tail is wagged, its ears droop, its eyes are turned upwards, its lips hang loosely, and these movements, Darwin says, can never have been of any service, but are simply the opposites of the movements expressive of anger, and so have come to be expressive of affection. A man who is indignant, or resents an injury, holds his head up, squares his shoulders, expands his chest and clenches his fists, and these movements are serviceable; but a man who manifests helplessness, bends his head, shrugs his shoulders, contracts his chest, opens his hands—exhibits, in short, movements not serviceable but opposite to the movements of indignation or self-assertion, and which have therefore come to be recognised as expressive of the opposite emotion, that of helplessness and self-depreciation. This is Darwin's principle of Antithesis.

But movements, Darwin still further argues, which are independent of the will, which cannot have been acquired by habit, but which are expressive of emotional states, are the direct result of the constitution of the nervous system, and are to be ascribed to its mechanism, or the existence in it of connections between its several parts, and of definite channels through which nerve energy flows. An animal thrown into a state of fear, trembles violently, and this general quivering of the muscles can have been of no service, but is indeed of much dis-service to it, by weakening its power of flight or resistance, and cannot have been voluntarily assumed, as it is not under voluntary control, but must be attributed to a profuse generation of force in the excited nerve-centres, and to the overflow of this through the ordinary outlets. In a woman under a sense of shame the blood-vessels of the face dilate, and a blush suffuses the countenance, and this movement is certainly not serviceable, is not voluntary, for the muscles of the blood-vessels are beyond the control of the will and can only be explained by the existence of a connection between the nerves regulating the calibre of these blood-vessels and the part of the nervous system stimulated in this particular emotion. This is Darwin's principle of the Action of the Nervous System.

Now, as regards these three principles of Darwin, I venture, with the utmost deference, to think that the time has come when they may be subjected to criticism and revision. They have been eminently useful; have let in a flood of light on a very obscure subject; have guided to further elucidative researches; but the very illumination of which they have been themselves the cause, has revealed flaws in their own constitution. The facts bearing on emotional expression which Darwin observed or with scrupulous discrimination collected, remain and must always remain, unassailable, of high value to the naturalist and psychologist, but the laws by which he sought to systematise these facts are open to question.

As regards Darwin's second principle, that of Antithesis, according to which certain movements are expressive of certain emotions, simply because they are the opposites of other movements which are expressive of opposite emotions, I have all along felt some difficulty in accepting it. In physiology we know nothing of antithesis, though we see a good deal of antagonism; and it is difficult to believe that a whole series of movements of a very definite and positive character, betokening mental states, are merely the negations of other movements betokening other mental states. It is difficult to believe also that a series of emotions, those, according to Darwin, antithetically expressed, existed without any appropriate expression of their own, until they had tacked on to them certain movements not really related to them, but contrary to other movements by which the emotions farthest removed from them are displayed. Each emotion, I conceive, must have a language of its own, adequate to its requirements, evolving with its evolution, and cannot be dependent on the inverted echo of the utterances of the emotion with which it is least in sympathy. Then it is difficult, I might say impracticable, to arrange the emotions in pairs of opposites; indeed, only with the very primitive emotions can this be done, for during the development of the mental faculties in the individual and race the emotions branch out in various directions, and it is not possible to trace out any parallelism in the lines they pursue, or to say what is the opposite of a highly differentiated emotion. Humility is the opposite of both pride and vanity, but pride and vanity have entirely distinctive expressions, and it is impossible that the expression of humility can be opposite to the expression of both.

Again, when we study the movements of the body systematically, we discover that opposite movements are not always significant of opposite states of mind. Clenching of the fist, produced by strong flexion of the hand and arm, is undoubtedly expressive of anger; but the opposite movement, strong extension of the hand and arm, is not expressive of the opposite emotion, conciliatory good humour, which is expressed by the open hand—a position intermediate between the two. So far is it from being the case that opposite emotions are necessarily expressed by opposite movements, that the extremes of contrary passions are sometimes expressed by the same actions. Sir Joshua Reynolds noticed this, and pointed as instances to a Bacchante in frantic joy, with a facial expression not unlike that of a Mary Magdalene in overwhelming grief. Excessive happiness and bitter sorrow may both fill the eyes with tears. Laughter may be the outburst of joy, or the cynical mask of poignant suffering. I do not believe that the expressions of different emotions are ever identical.

Strict analysis will always reveal significant differences between them, but superficially they sometimes closely resemble each other, and if not identical are certainly not antithetical.

Without troubling you with further comments on it, I may say that Darwin's second principle of Antithesis may, it seems to me, be dispensed with; and that all the instances of it which he has adduced may be explained by a reference to the principle of Associated Serviceable Habits, or of the Action of the Nervous System, or to another principle to which I shall shortly allude.

Since the time when Darwin wrote on the expression of the emotions, our knowledge of the structure and action of the nervous system, and especially of its crown, the brain, has advanced enormously. His book was published in 1872. At that time the experiments performed by Fritsch and Hitzig, in Germany in 1870, were scarcely known in this country, and Ferrier's fruitful and memorable work had not been begun; and it is since then that the investigations of Munk, Goltz, Schäfer, Horsley, Beevor, Tamburini, and others, have unravelled for us some tangled mysteries of cerebral organisation. Thanks to their labours, we now know that there is localisation of function in the brain, or division of labour between the several parts, and that in one of its parts—what we call the motor area—that division is carried out to a considerable degree of subdivision; so that we have centres presiding over definite groups of muscular movements in the upper and lower extremities—face, neck, and trunk.

The active part of the brain is the cortex, or mantle of grey matter which forms its surface, covering its convolutions or folds, and enclosing the mass of white or medullary substance within. This white or medullary substance within the cortex or grey matter, and, forming the great bulk of the organ, is made up of communicating fibres which are equivalent to marine electric cables or telegraph wires, and convey currents to and from the brain. They carry to it sensory impressions, or information from the organs of sense, the surface of the body, its interior—indeed from every nook and cranny of it; they carry from it the mandates of the will to the muscles, and every tissue under its control; and they carry within it impressions putting the different parts of the brain in communication with each other. In the cortex or grey matter are to be found, of course, those communicating fibres that go to or from it, or traverse it; but its essential structure consists in brain-cells, its active constituents, a diamond-dust of protoplasm arranged in a striatified manner, and of different shapes and sizes—round, stellate, pyramidal. These little cells, scattered amongst the delicate white fibrils, are the true fountains of nerve-energy, into which are gathered the rills of sensory impressions from which flow forth the streams of voluntary and emotional impulses. It is in correspondence with changes in their protoplasm that all mental activities are manifested. These little brain-cells are of different forms, and are differently arranged in different regions of the brain, and they undergo wasting and degeneration in chronic insanity.

What the changes in the protoplasm of the cerebral cells corresponding with psychical activity really are we do not know, but, thanks to the recent researches of Professor Mosso, of Turin, we have now positive evidence that certain chemical changes, with loss of heat, accompany their activity. Discarding the thermopile, Professor Mosso has employed thermometers of extreme delicacy, and by the application of these to the surface of the hemispheres has found that the temperature of the brain—which in deep sleep falls below that of the arterial blood—is raised even during the continuance of sleep by a noise or any sensory impression which infringes upon the brain without causing awakening, or by the direct application of the interrupted current to the cortex, and is raised very decisively by the transition from the sleeping to the waking condition. Professor Mosso has found that the brain of an animal awake is hotter than that of an animal asleep, and that during consciousness the temperature of the brain rises as much as 0·5° C. above that of the arterial blood, a clear indication that the maintenance of consciousness involves chemical action. Strange to say, Professor Mosso has not found that variations in conscious activity are accompanied by variations in the temperature of the brain. Once the temperature corresponding with conscious activity has been attained it remains, according to his experiments, steady, and is uninfluenced by such psychical stimulation as he has been able to induce. But, for a great variety of reasons, I cannot believe that there is any dead level of consciousness or uniform distribution of functional activity in the brain, and I am inclined to believe that Professor Mosso's conclusions on this point will be modified by further experiments; and especially by simultaneous thermometric observations in different regions of the brain during emotional disturbances of different kinds. All analogy and many recorded facts warrant the belief that, even if the hemispheres have a function common to the mass of their grey matter, as far as thought is concerned, there are during consciousness incessant variations in the functional activity of different brain areas, and there must be chemical changes, delicate but probably cognisable, corresponding with these.

An image, in matters scientific that are obscure and difficult to comprehend, is sometimes obstructive and misleading, but it is sometimes useful, and in connection with the functional activity of the brain I would suggest an image drawn from the phenomena of the Polar lights as seen in our northern sky. Think of the brain during unconsciousness steeped in slumber, and imagine it to be the dark segment which forms the core of the aurora, which can sometimes be seen before any luminosity has appeared. Think of the brain, again, as it has passed from the sleeping to the waking condition, and is in common conscious life, and conceive of it as spanned by the luminous arch in constant motion, now rising, now falling, forming a continuous curtain of flickering light over its whole surface. And think of the brain, again, in times of intense conscious elevation, of concentrated attention or agitated feeling, and picture it as sending forth from its luminous arch of common consciousness long brilliant rays of psychical energy, that shoot towards the zenith, now here, now there, now east, now west, green, purple, or violet, according to the emotive complexion of the moment.

But whatever image of the brain in its active and quiescent states we may form, the important point to bear in mind now is that some of its special activities—long rays of function—may be evoked by that electrical stimulation of its surface which Mosso has shown to cause a rise of temperature there. It was the determination of this fact by Fritsch and Hitzig that was the starting point of all the discoveries in cerebral physiology since made. They showed that the surface of the brain, which all previous experimenters had found to yield absolutely no response to any kind of stimulation, mechanical or chemical, that could be applied to it, is excitable to electricity. Before their time, brains of living animals (exposed by removal under chloroform of the skin, skull, and membranes) had been subjected to stimulation, by light, heat, pressure, acid, alkalies, pricking, and burning, and had remained dead and inert, insensible and motionless. But under the inspiration of their genius, brains of living animals, the moment that their surface was touched by the electrodes connected with a battery, sprang as it were into life and moved responsive to every appeal made to them.

One spot on the surface (and let us suppose it is a monkey's brain that is under observation) is touched by the electrodes, and the knee of the opposite side is bowed; the electrodes are removed, and it is straightened. An adjoining spot is touched and the ankle is twisted; the electrodes are raised, and it is straightened. Another spot is touched and the shoulder is raised, another and the elbow is flexed, another and the wrist, and so on, through all the movements of leg, foot, arm, hand, fingers, face, neck, trunk. It is exactly like playing the piano: a key is depressed and a definite note follows; a spot is touched and a definite movement follows. And every time that the same spot is touched, the same movement follows, while an adjacent spot gives an entirely different movement with the same unerring regularity. No puppet responds more accurately to the twitching of the strings attached to it than do the body and limbs of a deeply unconscious monkey to the touches of the electrodes of the experimenter, who can make it perform any movement you may suggest. Numerous elaborate and careful experiments have made it certain that all the movements of the body are localised or have their central springs of action in this middle region of the brain, and that they are localised there in a definite order, movements of the lower limbs being uppermost, those of the upper limbs next in order downwards, and those of the face lower still.

But it may be objected that these movements have been localised in the brains of monkeys, and that we have no guarantee that they are localised in the same manner in the far larger and more complex brain of man, and the answer to that objection is that the ravages of disease have satisfied us that movements are localised in exactly the same way in the human and Simian brains. A man suffers from convulsive twitchings of his thumb; he dies, and on post-mortem examination a small tumour is found setting up irritation in the spot of the brain precisely corresponding with that electrical stimulation of which caused twitching of the thumb in the monkey. Another man suffers from paralysis of the corner of his mouth; he dies, and a patch of softening is found destroying the spot of the brain precisely corresponding with that electrical stimulation of which caused retraction of the angle of the mouth in the monkey. But more than this, it has been experimentally demonstrated that electrical excitation of a certain number of the motor centres in the human brain causes the same movements represented in the homologous parts of the brains of monkeys.

But it must have occurred to you that there are large tracts of brain outside this motor area, before it, behind it, beneath it, about the functions of which nothing has been said. Well, all I shall say about these is that some of them—those in the frontal lobes—probably afford the anatomical substratum for certain intellectual functions, while it is certain that some of them are sensory. It is established that the visual function is localised in the occipital area, it is all but established that hearing is localised in the temporal area, and it is probable that the limbic lobe is sensory.

Now, emotions are compounded of sensations; they are what Herbert Spencer has called centrally initiated sensations, that is to say they are feelings arising in the mind not from any sensory impression conveyed to it from the outer world or from the body, but from the revival of former sensations of a complex character. Of course a sensory impression may call forth an emotion. A blow excites anger, but the emotion—the anger—is quite distinct from the sensation—the pain of the blow. And an emotion, however excited, is evolved by intellectual processes, and differs from a sensation in that it persists for some time after its exciting cause has ceased to operate. And, as emotions are revived or compounded sensations, they must originate in those centres in which the sensations of which they are compounded first arose, and so must have as their starting point the sensory centres of the brain. But in all our sensations and perceptions, even in the simplest sensations, there are motor elements, and so it is certain that in all emotional states, even when they do not pass over into action, these motor centres are concerned. But it is when the emotion demands external manifestations that these motor centres are obviously brought into play, and then by observations of the physical correlatives, the attitude of the body, the cast of the features, &c. we are able to say which centres are principally involved in different emotions.

A great many emotional movements are involuntary, and hence they have often been referred to nerve centres lower than the cerebral hemispheres, but it is to be borne in mind that many emotional acts are also ideo-motor—that is to say there is a mental representation of the act to be performed before it is performed, and ideo-motor acts or movements can have no other origin than in the cerebral hemispheres—the seat of consciousness. And it is further to be borne in mind that a large number of motor acts which have become involuntary by repetition and habit, were at first voluntary, purposely devised or imitative, and as they must then have originated in the highest cerebral centres, it is exceedingly improbable that they have since been transferred to centres at lower levels.

Expression, in its widest sense, is the manifestation of mental by bodily conditions, and includes, as I have already said, language as well as emotional movements. Language or speech—a peculiarly human function, the last to evolve in the animal scale—consists in certain muscular movements which at first thought we would regard as altogether voluntary, but amongst which we see going on just the same process that we have referred to as so much more marked amongst emotional movements, the conversion of the voluntary into the involuntary and automatic. Each of us has by incessant repetitions of certain common words and phrases, such as salutations and exclamations of various kinds, made them so automatic that they blurt forth when required without any voluntary effort, and continue to be uttered when through disease of the brain voluntary speech has been lost.

It is agreed that language or speech is localised in the third frontal convolution of the human brain which lies just at the front of the motor area, and that its voluntary and involuntary expressions alike have here their origin; and it is fair to infer that the simpler language of feature and gesture has also both its voluntary and involuntary utterances localised in the region where the former must unquestionably be placed.

I regard the central region of the brain as a great expressional area, whence proceed all the movements of the body that are an index to conditions of the mind. In this region is placed the intricate and delicate machinery by which emotional movements are produced, and here we must study that action of the nervous system to which Darwin gave a subsidiary, but to which I would give a first place in the explanation of emotional expression.

Every emotion, as has been explained, radiates throughout the organism to its utmost confines. We must each of us have experienced the widespread diffusion of emotional disturbance in that general tremor or shiver which runs down the backbone and along the limbs, under a strong feeling of indignation or alarm; but, besides this general discharge, there are special channels of overflow of emotional disturbance appropriate to each emotion. The motor impulses expressive of each emotion start, as it were, from one centre in the motor area, whence proceeds what may be called its "signal symptoms"; and if the emotion be slight and transient, these alone may suffice for its expression; but if it be strong and sustained, and more particularly if it be gravescent, the nervous vibrations initiated at this centre spread to other centres adjacent, or with which they are in functional connection, and result in varied, complex, and extensive movements.

All emotions of extreme and uncontrolled violence ultimately involve the whole motor area of the brain, and some such emotions, pushed to a pathological conclusion, end in general convulsions. We actually see this conclusion reached when the religious emotion is powerfully stirred in persons of excitable temperament. In the dancing mania of the Middle Ages it was no uncommon occurrence for those who had been caught by the mental contagion, and had passed through a paroxysm of delirious excitement, to fall finally in epileptic convulsions; and to-day, alike in East and West, similar phenomena may sometimes be witnessed. The religious transports of dervishes, beginning in slow, monotonous movements of the hands, and mounting into wild gyrations, not infrequently end in fits; and the negroes of the States, who at their camp meetings pass from slight swaying movements of the body to frantic leapings and vociferous shouting of that refrain which has found favour recently in this cultured metropolis—"Ta-ra-ra-boom-de-ey"—the negroes sometimes tumble down unconscious and in violent spasms.

But, short of this point of universal sudden explosive discharge of the whole motor area of the cerebral cortex, there are always differences in the grouping of the movements by which emotions even in their violent phases are displayed, showing involvement in different degrees, and in different combinations of the motor centres in the expressional area; and in the gradual rise and culmination of certain emotions it is possible even now to trace out in a rough way the lines of diffusion of the cortical excitation which correspond with their gradually extended expression.

[This point was illustrated by photographs of different stages of emotional excitement, as exhibited by muscular action, and by indicating how each of these stages corresponded with the implication of a new centre, or group of centres, in the brain. The photographs shown were those of a young Scotch girl, knowing nothing of stage tricks or conventional artifices, who most kindly assisted by throwing herself, at the word of command, into various emotional attitudes—withdrawing the control of the will, letting herself go, as it were, and slip into the spontaneous expression of each emotion suggested—and being instantaneously photographed while the expression lasted.]

I wish next to direct your attention to the frequent association of face and hand movements in emotional expression. Next to the face, the hand is the great instrument of emotional expression; but perhaps this manual function has been somewhat lost sight of. We all readily recognise the services of the hand in executing the orders of the will, and giving us our control over matter, but we are apt to forget that it moves responsive to passing feelings of the mind, may tell with nice precision of our pains and pleasures, and heighten and elucidate the emotional expression of the face. Our constitutional reserve in this country has led to the systematic suppression of emotional movements in the hand, and we must go to the South of Europe if we would learn how rich and diversified the emotive language of the hand may be. Canon Farrar says of the mimic actors of Rome—who in the time of Nero expressed in movement, and in movement alone, every burning passion and soft desire—that they literally spoke with their hands; and we had recently in London a play without words, "L'Enfant Prodigue," in which a somewhat complicated story, full of incident and feeling, was told by pantomime with musical accompaniment, and in which the hands of some of the actors became articulate, and supplied the place of dialogue. Facial expression counted for little. The two principal characters, the Pierrots, were masked and their faces therefore blank. The music no doubt was suggestive and the large movements of the body conveyed broad effects, but it was the hands that really told the story, clearly and forcibly, and that like tongues of fire flashed and flickered forth the hidden strivings of the spirit. Suppose that the hands of the actors had been cut off, and that they had played in other respects exactly as they did and with the same music, only in stumps and minus hand movements—why, the whole piece would have been absolutely unintelligible.

The education of the motor centres consists in great part in raising them to individuality of action, or to the power of combined action in a definite order. At first these centres tend to discharge themselves in a confused and disorderly way. Present an object of desire—say a red-cheeked apple—to a young infant, and what does it do? Does it take hold of the coveted object? Not at all. It kicks up its heels, crows loudly, and throws its whole body into agitation, and this it does because the sense impression conveyed from the eye to the brain and awakening the emotion of pleasure and desire there, sets up a motor activity which is widely diffused through the motor area and ends not in definite but in wholesale and indiscriminate display. Only gradually does the child learn, by observation, imitation, and experiment during evolution of its brain centres, to cut off one superfluous movement after another, and ultimately to confine itself to those movements that are essential to its purpose, to wit, the stretching out of the arm, the closing of the hand, and the conveyance of the apple to its mouth. All through education this process of specialisation of motor centres goes on. Any one who has watched a dancing class will know what pains are required to put a stop to unnecessary sprawling movements and grimaces, and all through education and long after education is over, illustrations may be noticed of the failure of this process in certain directions, and of what I may call leakage from one centre to another. I daresay most of you have noticed how in children learning to write, their tongues twist about, keeping time with their fingers; how in a woman using a pair of scissors, the jaws will sometimes move synchronously with the fingers (although the opening and closing of the mouth does not in any way facilitate the process of cutting); and how in a man using a corkscrew the corner of his mouth is sometimes drawn down with every turn he gives the instrument. These are instances of leakage from the hand to the mouth centres. It is as if the hand centres had been imperfectly banked up, and permitted of overflow at one part, so that voluntary hand movements are followed by an automatic mouth movement. And overflows in the opposite direction from the mouth to the hand centres are of the most constant occurrence. You all know that it is a word and a blow—a word, a voluntary act dependent on the mouth centre, followed by a blow which may be almost automatic, dependent on the hand and arm centres; and innumerable examples might be given of the way in which emotional conditions first expressed in the mouth centres immediately afterwards involve the hand, the movements of hand and face in these cases being partly voluntary and partly automatic; and it is important to note that while the currents of overflow of purely voluntary into automatic movements are from the hand to the face centres, the currents of overflow of emotional into voluntary expression are from the face to the hand.

[A few more photographs illustrating the rapid diffusion of emotional excitation from face to hand centres were exhibited.]

I can but mention now another great law of emotional expression which affords a key to a large class of emotional movements, and admits of very beautiful demonstration, and which although present to the minds of Piderit and Gratiolet seems to have escaped Darwin —I allude to the law of correlation of movements with ideas, which has been insisted on by Professor Clelland, of Glasgow.

Words indicating position and quantity, represent ideas which relate alike to the physical and the mental world; and emotions expressed by such words are indicated by the attitudes, gestures, and movements of the body, expressed by the same words. Take words relating to height—upward, downward, ascent, descent, elevation, depression, rise, fall—and you will find that you apply them equally to physical and mental conditions. You speak of the elevation of a building, and of elevation of soul; of a depression in the ground, and of depression in spirits; of the ascent of a balloon, and of the ascent of genius; of the descent of a stair, and of descent into vice. Take words expressing magnitude—such as large and small, wide and narrow, expanded and contracted. You speak of large gooseberries and large hearts; small profits and small wits; bodies that are expanded and contracted, and ideas that are the same. And so it is with words expressing direction—forward, backward, advance, retrogression; with words expressing distance—far and near, attraction, repulsion; with words expressing resistance—as strong, weak, hard, soft; with words expressing motion—as quick, slow, tension, relaxation; all are equally applicable to physical and to mental states, and the bond linking these words seems to be universal and necessary, for it is to be found in all languages. Primarily physical in their application, they have, in the course of evolution, had metaphysical and metaphoric meanings attached to them.

But emotions to which such words as I have been referring to have application, are indicated by attitudes, gestures, and movements of the body expressed by the same words. The workings of the feelings are expressed by attitudes, gestures, and movements of the body correlative with them, and we have symbolism in expression. In expressing superiority or authority, the body is drawn up; in expressing inferiority or humility, it is bent down. In expressing liking or attraction, we bend the body forward to be near that which we like; in expressing dislike, or repulsion, we draw the body backward, as if to be far away from that which we dislike; and these movements are made not from any notion of achieving a purpose, still less from an inherited habit founded on their utility to real or supposed ancestors, but simply from the close connection subsisting between movements towards an object and mental attraction, or between movement away from an object and a feeling of repulsion.

We have slight movements of the arms to express the hugging of an idea to the bosom, when nothing but what is thoroughly impersonal is thought of; and we have a sweep downwards and backwards of the arm with the palm of the hand turned away from the body; and no gesture can more thoroughly express the putting away of something vile, and this gesture is applied to the intangible and invisible; by it the cleric puts away the false doctrine, and the fastidious brands a notion as vulgar. And in the direction of the eyes we see the appropriate supplement of gesture in expression. An erect carriage may be given to the body by a sense of authority, by pride, by conceit, by scorn, by reproach, and by ennobling thought, and it is really the direction of the eyes that best distinguishes these.

I have said enough, perhaps, to illustrate the correlations of movements and positions with ideas, and I need only add that this correlation is seen elsewhere in nature besides in the face of man. In the vegetable kingdom the flower holds the place of honour; in vertebrate animals the nervous system, which with the exception of the supra-œsophageal ganglion, has been inferior, in the articulate becomes superior, while in man, the brain becomes superior in every sense.

I have tried to demonstrate that the immediate explanation of emotional expression is to be found in the structure of the nervous system, and that its origin must be sought in the conditions that have determined the constitution and evolution of the brain. In doing so, I have had to consider the emotions solely on their cerebral side, and in connection with their material manifestations, but I would now add that whatever advance in this direction modern research may have secured, we are not one hair's breadth nearer comprehending the real relation between emotional and bodily states. Why emotion or consciousness should arise in connection with the activity of the cerebral hemispheres is as much hidden from us as ever, and the further we pursue the parallelism between mental processes and neural changes, the more deeply convinced do we become that it is a perfect parallelism after all, and that we must reverently bow our heads in the presence of a great and inscrutable mystery.

[J. C. B.]

WEEKLY EVENING MEETING,

Friday, June 3, 1892.

Sir Frederick Abel, K.C.B. D.C.L. F.R.S. Vice-President, in the Chair.

Ludwig Mond, Esq. F.R.S. *M.R.I.*

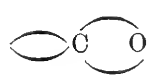
Metallic Carbonyls.

Justus Liebig, perhaps the most prophetic mind among modern men of science, wrote in the year 1834, in the 'Annalen der Pharmacie,' "I have previously announced that carbonic oxide may be considered as a radical, of which carbonic acid and oxalic acid are the oxides, and phosgene gas is the chloride. The further pursuit of this idea has led me to the most singular and the most remarkable results."

Liebig has not told us what these results were, and it has taken many years before the progress of chemical research has revealed to us what may at that early date have been before Liebig's vision. I will to-night bring before you some important discoveries made only within the last few years by following up Liebig's idea.

Carbonic oxide, composed of one atom of carbon and one atom of oxygen, is a colourless gas, without taste or smell, which I have here in this jar. It burns in air with a blue flame. When it acts as a radical, combining with other bodies, we term it carbonyl, and its compounds with other elements or radicals are termed carbonyls. Liebig defined a radical as a compound having the characteristics of a simple body, which would combine with, replace, and be replaced by simple bodies. In more modern times a radical has been defined as an unsatiated body. I am of course speaking of chemical radicals. If we look at it from the modern point of view, carbonyl should be the very model of a radical, because only half of the four valencies of carbon are satiated, the other two remaining free. Carbonic oxide should even be a most violent radical because amongst all organic radicals it is the only one we know to exist in the atomic or free state. All the other organic radicals, even such typical ones as cyanogen and acetylene, are known to us as molecules composed of two atoms of the radical, so that the cyanogen gas and acetylene gas we know should more properly be called di-cyanogen and di-acetylene; they consist of two atoms of the radical cyanogen or of the radical acetylene, the free valencies or combining powers of which satiate or neutralise each other. On the other hand, carbonic oxide gas, as I stated before, makes the sole exception. Its molecule contains only one atom of carbonyl moving about with its free valencies unfettered by a second atom. For all that, carbonic oxide is by no means a violent body, but the very reverse, and instead of being ready to attack with its two free valencies anything coming in its way, until very recently we only knew it to interact and to combine with substances possessing themselves extreme attacking powers, such as chlorine and potassium. Although Liebig had so long ago proclaimed it as a radical, the chemical world was startled when, two years ago, I announced in a paper I communicated to the Chemical Society in conjunction with Drs. Langer and Quincke, that carbonic oxide combines at ordinary temperature with so inactive an element as nickel, and forms a well-defined compound of very peculiar properties.

The fact that carbonic oxide does not possess the chemical activity one would suppose in a radical composed of single atoms may, I believe, be explained by assuming that the two valencies of carbon which are not combined with oxygen do satiate or neutralise each other. Everybody admits that the valencies of two different carbon atoms, which are all considered of equal value, can neutralise each other. I see, therefore, no reason to question the possibility of two valencies of the same carbon atom neutralising each other. On this assumption carbonic oxide may be looked upon as a self satisfied body—one which keeps in check its free affinities within itself. I have tried to explain this by the graphic formula in this diagram.



You have here (see diagram on next page) the typical carbon radicals containing one atom of that element, acetylene, methylene, methyl, cyanogen, and carbonyl. In the second column you have these substances as they are known to us in the free state. You see the carbonyl is the only one which exists in the free state as a single atom, while all the others only exist as molecules, composed of two atoms the free valencies of which neutralise each other. The carbonyl I have represented in the last formula, with the two valencies not combined with oxygen neutralising each other, so that in this way it also becomes a satiated body.

The paper published by Liebig in 1834, from which I have already quoted, was entitled "On the Action of Carbonic Oxide on Potassium." In it Liebig fully described the preparation and properties of the first metallic carbonyl known—a compound of potassium and carbonic oxide. Liebig obtained this compound by the direct action of carbonic oxide upon potassium at a temperature of 80° C., and proved it to be identical with a substance which had been previously obtained as a very disagreeable bye-product of the manufacture of potassium from potash and carbon by Brunner's method. It forms a grey powder which is not volatile, and which on treatment with water yields a red solution, gradually turning yellow in contact with air and from which on evaporation a yellow salt is obtained called potassium croconate, on account of its colour. Liebig showed this

TYPICAL CARBON RADICALS.

I.		II. (Known in the free state.)		
Acetylene	— C — — H	Di-acetylene	H — — C — — C — — H	C_2H_2
Methylene	C — H, — H	Di-methylene (Olefiant gas)	H — C C — H, H — — H	C_2H_4
Methyl	— C — — H, — H, — H	Di-methyl	H — H — — C — C — — H, H — — H, — H	C_2H_6
Cyanogen	— C — — N	Di-cyanogen	N — — C — C — — N	C_2N_2
Carbonyl	C O	Carbonic oxide	C O	CO

salt to consist of two atoms of potassium, five of carbon, and five of oxygen, and not to contain any hydrogen, as had previously been supposed.

Since the publication of Liebig's paper, potassium carbonyl has been studied by numerous investigators, amongst whom Sir Benjamin Brodie deserves particular mention; but it has been reserved to Nietzki and Benkiser to determine finally in the year 1885, by a series of brilliant investigations, its exact constitution and its place in the edifice of chemistry. They have proved that it has the formula $K_6C_6O_6$; that the six carbons in this compound are linked together in the form of a benzole ring; that, in fact, the compound is hexhydroxylbenzole, in which all the hydrogen is replaced by potassium. By simple treatment with an acid it can be converted into the hexhydroxylbenzole, and from this substance it is possible to produce, by a series of reactions well known to organic chemists, the whole wide range of the benzole compounds. The body which Liebig obtained by the direct action of carbonic oxide on potassium has thus enabled us to prepare synthetically in a very simple way from purely inorganic substances—to wit, from potash and carbon, or if we like even from potash and iron—the whole series of those most important and interesting compounds called aromatic compounds, including all the coal-tar colours, which have furnished us with an undreamt of variety of innumerable hues and shades of colour, as well as many new substances of great value to suffering humanity as medicines. Surely a startling result, which alone would have fully justified Liebig's prediction of 1834!

Speaking of coal-tar colours, everybody will be reminded of the great loss the scientific world has recently sustained by the death of August Wilhelm Hofmann, their first discoverer, Liebig's greatest pupil. Hofmann will ever be remembered in this Institution, where he so often delighted the audience by his lucid lectures, and in whose welfare he took the greatest interest, of which he gave us a fresh proof only last year, in the charming letter he wrote on the occasion of his election as an Honorary Member.

Looking back upon the wonderful outcome of Liebig's idea I have referred to, it seems surprising indeed that others should not have followed up his work by attempting to obtain other metallic carbonyls.

A very few experiments were made with other alkaline metals. Sodium, otherwise resembling potassium so closely, has been shown not to combine with carbonic oxide; lithium and calcium are stated to behave similar to potassium. But metals of other groups received little or no attention. The very important rôle which carbonic oxide plays in the manufacture of iron did lead to a number of metallurgists (among whom Sir Lowthian Bell and Dr. Alder Wright are the most prominent) to study its action upon metallic iron and other heavy metals, including nickel and cobalt at high temperatures. They proved that these metals have the property to split up carbonic oxids into carbon and carbonic acid at a low red heat, a result of great importance, which threw a new light upon the chemistry of the blast furnace. None of these investigators, however, turned their attention to obtaining compounds of these metals with carbonic oxide, and, owing to the high temperature and the other conditions under which they worked, the existence of such compounds could not come under their observation. In order to obtain these compounds, very special conditions must be observed, which are fully described in the papers I have published during the last two years in conjunction with Dr. Langer and Dr. Quincke.

The metals must be prepared with great care, so as to obtain them in an extremely fine state of division, and must be treated with carbonic oxide at a *low temperature.* The best results are obtained when the oxalate of the metal is heated in a current of hydrogen at the lowest temperature at which its reduction to the metallic state is possible. I have in the tube before me metallic nickel prepared in this way, and over which a slow current of carbonic oxide is now passing. The carbonic oxide before entering the tube burns, as you see, with a blue non-luminous flame. After passing over the nickel it burns with a highly luminous flame, which is due to the separation of metallic nickel from the nickel carbonyl formed in the tube, which is heated to incandescence in the flame. If we pass the gas through a freezing mixture, you will observe that a colourless liquid is condensed, of which I have a larger quantity standing in this tube. In passing the gas issuing from our tube through a glass tube heated to about 200° C., we obtain a metallic mirror of pure nickel, because at this temperature the nickel carbonyl is again completely resolved into its components, nickel and carbonic oxide. We will by-and-by show you that this mirror consists of pure nickel. This liquid is pure nickel carbonyl, and has the formula $Ni(CO)_4$. It has a specific gravity at ordinary temperature of 1·3185, and boils under atmospheric pressure at the low temperature of 43° C.

It has a very high vapour tension at ordinary temperature and possesses a very high rate of expansion. If cooled to − 25° C. it solidifies, forming needle-shaped crystals. A mixture of the vapour with air explodes readily, sometimes at ordinary temperature, but without violence, as we will show you. The liquid itself in the pure state does not explode, but decomposes into its constitutents when heated sufficiently. The vapour of nickel carbonyl possesses a characteristic odour and is poisonous, but not more so than carbonic oxide gas. Prof. McKendrick has studied the physiological action of this liquid, and has found that, when injected subcutaneously in extremely small doses in rabbits, it produces an extraordinary reduction of temperature, in some cases as much as 12°.

The liquid can be completely distilled without decomposition, but from its solution in liquids of a higher boiling point it cannot be obtained by rectification. On heating such a solution the compound is decomposed, nickel being separated in the liquid, while carbonic oxide gas escapes. I will try to demonstrate this by an experiment.

We have here a solution of the substance in heavy petroleum oil, which you will, in a few minutes, see turns completely black on heating by the separation of nickel, while a gas escapes which is pure carbonic oxide.

In a similar way, when the nickel carbonyl is attacked by oxidising agents, such as nitric acid, chlorine, or bromine, it is readily broken up, nickel salts being formed, and carbonic oxide being liberated. Sulphur acts in a similar way. Metals, even potassium, alkalies, and acids, which have no oxidizing power, will not act upon the liquid at all, nor do the salts of other metals react upon it. The substance behaves therefore, chemically, in an entirely different manner from potassium carbonyl, and does not lead, as the other does, by easy methods to complicated organic compounds. It does not show any one of the reactions which are so characteristic for organic bodies containing carbonyl, such as the Ketones and Quinones; and we have not been able, in spite of very numerous experiments, either to substitute the carbonic oxide in this compound by other bivalent groups, or to introduce the carbonic oxide by means of this compound into organic substances.

By exposing the liquid to atmospheric air a precipitate of carbonate of nickel is slowly formed of varying composition, which is yellowish-white if perfectly dry air is used, and varies from a light green to a brownish colour if more or less moisture is present.

We have found all these precipitates to dissolve easily and completely in dilute acid, with evolution of carbonic acid, leaving ordinary nickel salts behind, and can therefore not agree with the view propounded by Professor Berthelot in a communication to the French Academy of Science, that these precipitates contain a compound of nickel with carbon and oxygen, comparable to the so-called oxides of organo-metallic compounds. In the same paper Professor Berthelot has describod a beautiful reaction of nickel carbonyl with nitric oxide, which Dr. Langer will now show you. You will notice the intense blue coloration which the liquid solution of nickel carbonyl in alcohol assumes by passing the nitric oxide through it. Professor Berthelot has reserved to himself the study of this body, but has so far not published anything further about it.

The chemical properties of the compound I have just described to you are without parallel; we do not know a single substance of similar properties. It became, therefore, of special interest to study the physical properties of the compound.

Professor Quincke, of Heidelberg, has kindly determined its magnetic properties, and found that it possesses in a high degree the property discovered by Faraday, and called by him dia-magnetism, which is the more remarkable, as all the other nickel compounds are para-magnetic. He also found that it is an almost perfect non-conductor of electricity, in this respect differing from all other nickel compounds.

The absorption spectrum, and also the flame spectrum of our compound are at present under investigation by those indefatigable spectroscopists, Professors Dewar and Liveing, by whose kindness I am enabled to bring before you, in advance of a paper they are sending to the Royal Society, some of the interesting results they have obtained. We have here a photograph of the absorption spectrum, obtained by means of a hollow prism through quartz plates filled with nickel carbonyl, through which the spark spectrum of iron is passed, which is photographed on the same plate. You see that the whole of the ultra-violet rays of the iron spectrum have disappeared, being completely absorbed by the nickel carbonyl, which is thus quite opaque for all the rays beyond the wave-lengths 3820.

The spectrum of the highly luminous flame of nickel carbonyl, which I have shown you before, is quite continuous; but if the nickel carbonyl is diluted with hydrogen, and the mixture burnt by means of oxygen, the gases burn with a bright yellowish-green flame without visible smoke; and the spectrum of this flame shows in its visible part, on a background of a continuous spectrum, a large number of bands, brightest in the green, but extending on the red side beyond the red line of lithium, and on the violet side well into the blue. These bands cannot be seen on the photograph which I will now show you, the visible part of the spectrum appearing continuous, but beyond the visible part, the photograph shows a large number—over fifty—of well-defined lines in the ultra-violet. I will show you these lines in another photograph taken with greater dispersion, and on which have also been photographed the spark spectrum of nickel. You will see that all these lines correspond absolutely to lines appertaining to the spark spectrum; in fact the greater part of the lines in the spark spectrum are also shown in this flame spectrum. We have here another and very striking example of the fact discovered on the same day by Professors Dewar and Liveing and by Dr. Huggins, that the spectrum of luminous flames is not always continuous throughout its whole range, a fact which has at one time been much debated and discussed.

One of the most remarkable discoveries made within the precincts of this Institution by that illustrious man whose centenary we celebrated last year was that of the connection between magnetism and light, which manifests itself when a beam of polarised light is sent through a substance while it is subjected to a strong magnetic field, under whose influence the beam of light is rotated through a certain angle. Dr. W. H. Perkin has prosecuted this discovery of Faraday's by a long series of most elaborate researches, and has established the fact that this power of magnetic rotation of various bodies has a definite relation to their chemical constitution, and enables us to gain a better insight into the structure of chemical compounds. Dr. Perkin has been good enough to investigate the power of magnetic rotation of the nickel carbonyl, and has found it quite as unusual as its chemical properties, and to be, with the sole exception of phosphorus, greater than that of any other substance he has yet examined.

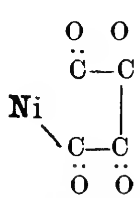
The power of different bodies of refracting and dispersing a ray of light has been shown by the beautiful and elaborate researches undertaken many years ago by Dr. Gladstone—who has given an account of them in this theatre in 1875, and who has since continued them with indefatigable zeal—to throw a considerable light upon the constitution of chemical compounds.

I have investigated the refractive and dispersive powers of nickel carbonyl in Rome, in conjunction with Prof. Nasini. We found that the atomic refraction of nickel in the substance is nearly two and a half times as large as it is in any other nickel compound—a difference very much greater than had ever before been observed in the atomic refraction of any element. To give you some idea how these figures are obtained, Mr. Lennox will now throw on to the screen a beam of light through a double prism, filled partly with nickel carbonyl, and partly with alcohol. You will notice that the top spectrum is turned much further to the left, showing the nickel carbonyl to possess a much greater power of refraction, and you will also notice that it is much wider than the bottom spectrum, which shows the greater dispersive power of the nickel carbonyl.

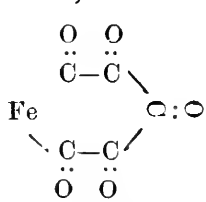
It is now generally supposed that if one element shows different atomic refractive powers in different compounds, it enters with a larger number of valencies into the compound which shows a higher refractive power. In accordance with this view, the very much greater refractive power of the nickel in the carbonyl would find an explanation in assuming that this element, which in all its other known combinations is distinctly bivalent, exercises in the carbonyl the limit of its valency, viz. 8, assigned to it by Mendeleeff, who placed it into the eighth group in his Table of Elements. This would mean that one atom of nickel contained in the nickel carbonyl is combined directly with each of the four bivalent atoms of carbonyl, each of which would saturate two of the eight valencies of nickel, as is shown by this formula—

$$\begin{array}{c} O \\ \colon \\ C \\ \| \\ O:C = \mathbf{Ni} = C:O \\ \| \\ C \\ \colon \\ O \end{array}$$

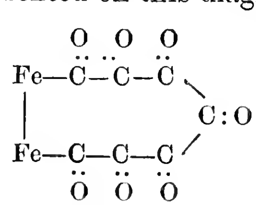
This view seems plausible, and in accordance with the chemical properties of the substance, and I should have no hesitation in accepting it if we had not, in the further pursuit of our work on metallic carbonyls, met with another substance—a liquid compound of iron with carbonic oxide—which in its properties bears so much resemblance to the nickel compound that one cannot assign to it a different constitution, whilst its composition makes the adoption of a similar structural formula next to impossible. It contains, for one equivalent of iron, five equivalents of carbonyl. To assign to it a similar constitution, one would, therefore, have to assume that iron did exercise ten valencies, or two more than any other known element, a view which very few chemists would be prepared to countenance. The atomic refraction of iron in this compound, which Dr. Gladstone has had the kindness to determine, is as unusual as that of the nickel in the nickel compound, and bears about the same ratio to the atomic refraction of iron in other compounds. We have, therefore, to find another explanation for the extraordinarily high atomic refraction of these metals in their compounds with carbon-monoxide, which may possibly modify our present view on this subject. As to the structure of these compounds themselves, we are almost bound to assume that they contain the carbonyl atoms in the form of a chain, as I have represented on this diagram.



Nickel-tetra-carbonyl.



Ferro-penta-carbonyl.



Di-ferro-hepta-carbonyl.

The ferro-carbonyl is prepared in a similar manner to the nickel compound. The iron used is obtained from the oxalate at the very lowest temperature possible, and is in a high degree pyrophoric. It immediately catches fire on coming into contact with air, as I will show you.

This carbonyl forms, however, with such great difficulty, that we overlooked its existence for a long time, and great precautions have to be taken to obtain even a small quantity of it. It forms an amber-coloured liquid, of which I have a small quantity before me. It solidifies below $-21°$ C. to a mass of needle-shaped crystals. It distils completely at 102°. Its specific gravity is 1·466 at 18° C. On heating the vapour to 180° it is completely decomposed into iron and carbonic oxide. The iron mirrors before me have been obtained in this way. Its chemical composition is $Fe(CO)_5$.

It is interesting that, within a short time after we had made known the existence of this body, Sir Henry Roscoe found it in carbonic oxide gas which had stood compressed in an iron cylinder for a considerable time, and expressed the opinion that the red deposit which sometimes forms in ordinary steatite gas-burners is due to the presence of this substance in ordinary illuminating gas. Its presence in compressed gas used for lime-lights has been noticed by Dr. Thorne, whose attention was called to the fact that this gas sometimes will not give a proper light because the incandescent lime becomes covered with oxide of iron.

M. Garnier, in a paper communicated to the French Academy of Science, supposes even that this gas is sometimes formed in large quantities in blast-furnaces when they are working too cold, and refers to some instances in which he found large deposits of oxide of iron in the tubes leading away the gas from these furnaces; but I find it difficult to believe that the temperature of a blast furnace could ever be sufficiently reduced as to give rise to the formation of this compound. On the other hand, it is highly probable that the formation of this compound of iron and carbonic oxide may play an important rôle in that mysterious process by which we are still making, and have been making for ages, the finest qualities of steel, called the cementation process.

The chemical behaviour of the substance towards acids and oxidising agents is exactly the same as that of the nickel compound, but to alkalies it behaves differently. The liquid dissolves without evolution of gas. After a while a greenish precipitate is formed, which contains chiefly hydrated-ferrous oxide, and the solution becomes brown. On exposure to the air it takes up oxygen; the colour changes to a dark red, whilst hydrated-ferric oxide separates out.

We have so far not been able to obtain from this solution any compound fit for analysis, and are still engaged upon unravelling the nature of the reaction that takes place, and of the compounds that are formed.

Although the solution resembles in appearance to some extent the solutions obtained by treating potassium carbonyl with water, it does not give any of the characteristic reactions of the latter. When speaking of potassium carbonyl, I mentioned that by its treatment with water, croconate of potassium was obtained, which has the formula $K_2C_5O_5$.

We have transformed this by double decomposition into ferrous croconic, FeC_5O_5, a salt forming dark crystals of metallic lustre resembling iodine, which is not volatile, and dissolves readily in water, the solution giving all the well-known reaction of iron in croconic acid. You will note how entirely different the properties of this substance are from those of iron carbonyl, which I have described to you; yet, on reference to its composition, you will find that it contains exactly the same number of atoms of iron, carbon, and oxygen, as the latter. This is a very interesting case of isomerism, considering that both compounds contain only iron, carbon, and oxygen. The difference in the properties of these two bodies becomes explainable by comparing the structural formula of the two substances.

I would now call your attention to the great difference in the constitution of the potassium carbonyl and that of the nickel and ferro carbonyl. In the former the metal potassium is combined with the oxygen in the carbonyl; in the latter the metals nickel and iron are combined with the carbon of carbonyl. In the first case we have a benzole ring with its three single and three double bonds; in the second a closed chain with only single bonds. It is evident that the chemical properties of these substances must be widely different.

The ferro-penta-carbonyl remains perfectly unchanged in the dark but if it is exposed to sunlight it is transformed into a solid body of remarkably fine appearance, of gold colour and lustre, as shown by the sample in this tube.

This solid body is not volatile, but on heating it in the absence of air, iron separates out and liquid ferro-carbonyl distils over. If, however, it is heated carefully in a current of carbonic oxide it is reconverted into the ferro-penta-carbonyl and completely volatilised. We have so far found no solvent for this substance, so that we have no means as yet of obtaining it in a perfectly pure state. Several determinations of the iron in different samples of the substance have led to fairly concordant figures, which agree with the formula $Fe_2(CO)_7$, or di-ferro-hepta-carbonyl.

The interesting properties of the substances described have naturally led us "to try," as Lord Kelvin once put it to me so prettily, "to give wings to other heavy metals." We have tried all the well-known and a very large number of the rarer metals; but with the exception of nickel and iron we have so far been entirely unsuccessful. Even cobalt, which is so very like nickel, has not yielded the smallest trace of a carbonyl. This led me to study the question whether, by means of the action of carbonic oxide, the separation on a large scale of nickel from cobalt could not be effected, which has so far been a most complicated metallurgical operation; and subsequently I was led to investigate whether it would not be possible to use carbonic oxide to extract nickel industrially direct from its ores.

For solving these problems within the limits of the resources of a laboratory, we have devised apparatus, the principles of which are shown on this diagram. It consists of a cylinder divided into many compartments, through which the properly prepared ore is passed very slowly by means of stirrers attached to a shaft. On leaving the bottom of this cylinder, the ore passes through a transporting screw, and from this to an elevator, which returns it to the top of the cylinder, so that it passes many times through the cylinder, until all the nickel is volatilised. Into the bottom of this cylinder we pass carbonic oxide, which leaves it at the top charged with nickel carbonyl vapour, and passes through the conduits shown here into tubes set in a furnace and heated to 200° C. Here the nickel separates out from the nickel carbonyl. The carbonic oxide is regenerated and taken back to the cylinder by means of a fan, so that the same gas is made to carry fresh quantities of nickel out of the ore in the cylinder, and to deposit it in these tubes an infinite number of times.

Upon these principles Dr. Langer has constructed a complete plant on a Liliputian scale, which has been at work in my laboratory for a considerable time, and a photograph of which we will now throw on to the screen.

You see here the volatilising cylinder divided into numerous compartments, through which the ore is passing, and subjected to the action of carbonic oxide. At the bottom the ore is delivered into the transporting screw passing through a furnace (for the purpose of heating the ore to about 350° C. whereby its activity is maintained). This screw delivers into an elevator, which returns the ore to the top of the cylinder, so that the ore constantly passes at a slow rate through the cylinder again and again, until the nickel it contains has been taken out. The carbonic oxide gas, prepared in any convenient manner, enters the bottom of the cylinder and comes out again at the top. It then passes through a filter to retain any dust it may carry away, and thence into a series of iron tubes built into a furnace, where they are heated to about 200° C. In these tubes the nickel carbonyl carried off by the carbonic oxide is completely decomposed, and the nickel deposited against the sides of the tubes is from time to time withdrawn, and is thus obtained in the pieces of tubing and the plates which you see on the table.

The carbonic oxide regenerated in these tubes is passed through another filter, thence through a lime purifier, to absorb any carbonic acid which may have been formed through the action of the finely divided nickel upon the carbonic oxide, and is then returned through a small fan into the bottom of the cylinder. The whole of this plant is automatically kept in motion by means of an electric motor, the gearing of which you see here.

By means of this apparatus we have succeeded in extracting the nickel from a great variety of ores, in a time varying, according to the nature of the ore, between a few hours and several days.

Before the end of this year this process is going to be established in Birmingham on a scale that will enable me to place its industrial capacity beyond doubt, so that I feel justified in the expectation that in a few months nickel carbonyl, a substance quite unknown two years ago, and to-day still a great rarity, which has not yet passed out of the chemical laboratory, will be produced in very large quantities, and will play an important rôle in metallurgy.

The process possesses, besides its great simplicity, the additional advantage that it is possible to immediately obtain the nickel in any definite form. If we deposit it in tubes we obtain nickel tubes; if we deposit it in a globe we obtain a globe of nickel; if we deposit it in any heated mould we obtain copies of these moulds in pure, firmly coherent, metallic nickel. A deposit of nickel reproduces the most minute details of the surface of the moulds to fully the same extent as galvanic reproductions, so that all the very numerous objects now produced by galvanic deposition, of which Mr. Swan exhibited here such a large and beautiful variety a fortnight ago, can be produced by this process with the same perfection in pure metallic nickel. It is equally easy to nickel-plate any surface which will withstand the temperature of 180° C. by heating it to that temperature and exposing it to the vapour, or even to a solution of nickel carbonyl, a process which may in many cases have advantages over electro-plating. I have on the table before me specimens of nickel ores we have thus treated, of nickel tubes and plates we have obtained from these ores, and a few specimens of articles of pure nickel and articles plated with nickel which have been prepared in my laboratery. These will give you some idea of the prospects which the process I have described opens out to the metallurgist, upon whom, from day to day, greater demands are made to supply pure nickel in quantities. The most valuable properties of the alloy of nickel and iron, called nickel-steel, which promises to supply us with impenetrable iron-clads, have made an abundant and cheap supply of this metal a question of national importance. The inspection of the few specimens of articles of pure nickel, and of nickel-plated articles, will, I hope, suffice to show you the great facilities the process offers for producing very fine copies, and for making articles of such forms as cannot be produced by hydraulic pressure, the only method hitherto available for manufacturing articles of pure nickel.

I have also here a small coiled pipe made by my process, and kindly lent by Prof. Ramsay, which is interesting as being the first article made in this way for use in a chemical laboratory.

I began my lecture by bringing under your notice an idea of Liebig's, which he published fifty-eight years ago. I have shown you how he himself elaborated this idea, and how it developed, until within recent years it has led to results of the highest scientific importance, and probably of great practical utility.

Had Liebig all these results before his "mind's eye" when he penned those prophetic words I have quoted? This is a question it is impossible to answer. Who will attempt to measure the range of vision of our great men, who from their lofty pinnacle see with eagle eye far into the Land of Science, and reveal to us wonderful sights which we can only realise after toiling slowly along the road they have indicated? Whether Liebig saw all these results or not, it is due to him, and to men like him, that science continues its marvellous advance, dispersing the darkness around us, and ever adding to the scope and exactness of our knowledge, that mighty power for promoting the progress and enhancing the happiness of humanity.

[L. M.]

GENERAL MONTHLY MEETING,

Monday, June 13, 1892.

Sir James Crichton-Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

Harry Stanley Giffard, Esq.
William John Heath, Esq.
Mrs. Lawson,
Alexander Morison, M.D.
The Hon. William Frederick Danvers Smith, M.P.
W. Bezley Thorne, M.D.
Captain R. H. C. Tufnell,

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned for the following Donations:—

Mrs. Bloomfield Moore	£80
Sir David Salomons, Bart.	50
Charles Hawksley, Esq.	50

for carrying on investigations on Liquid Oxygen.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Across the Border, or Pâthan and Biloch. By E. E. Oliver. 8vo. 1890.
The Administration of Warren Hastings, 1772–1785. By G. W. Forrest. 8vo. Calcutta, 1892.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quinta: Rendiconti. Classe di Scienze Fisiche, Matematiche e Naturali. 1° Semestre, Vol. I. Fasc. 6, 7, 8. 8vo. 1892.
Rendiconti, Serie Quinta, Classe di Scienze Morali, Storiche e Filologiche, Vol. I. Fasc. 1, 2. 8vo. 1892.
Asiatic Society of Great Britain, Royal—Journal for January and April, 1892. 8vo.
Astronomical Society, Royal—Monthly Notices, Vol. LII. No. 6. 8vo. 1892.
Bankers, Institute of—Journal, Vol. XIII. Parts 5, 6. 8vo. 1892.
Bashforth, The Rev. F. B. D. (the Author)—A Description of a Machine for Finding the Numerical Roots of Equations. 8vo. 1892.
British Architects, Royal Institute of—Proceedings, 1891–2, Nos. 13–15. 4to.
Brymner, Douglas, Esq. (the Archivist)—Report on Canadian Archives, 1891. 8vo. 1892.
Chemical Industry, Society of—Journal, Vol. XI. No. 4. 8vo. 1892.
Chemical Society—Journal for May, 1892. 8vo.

Chicago Exhibition, 1893, *Royal Commission*—Handbook of Regulations and General Information. 8vo. 1892.
Civil Engineers, Institution of—Minutes of the Proceedings, Vol. CVII. 8vo. 1892.
Cracovie, l'Académie des Sciences—Bulletin, 1892, No. 4. 8vo.
East India Association—Journal, Vol. XXIV. No. 3. 8vo. 1892.
Editors—American Journal of Science for May, 1892. 8vo.
Analyst for May, 1892. 8vo.
Athenæum for May, 1892. 4to.
Brewers' Journal for May, 1892. 4to.
Chemical News for May, 1892. 4to.
Chemist and Druggist for May, 1892. 8vo.
Eastern and Western Review, Vol. II. No. 1. 8vo. 1892.
Educational Review for May. 8vo. 1892.
Electrical Engineer for May, 1892. fol.
Electric Plant for May, 1892. 8vo.
Electricity for May, 1892. 8vo.
Engineer for May, 1892. fol.
Engineering for May, 1892. fol.
Horological Journal for May, 1892. 8vo.
Industries for May, 1892. fol.
Iron for May, 1892. 4to.
Ironmongery for May, 1892. 4to.
Lighting for May, 1892. 8vo.
Nature for May, 1892. 4to.
Open Court for May, 1892. 4to.
Optical Magic Lantern Journal for May, 1892. 8vo.
Photographic Work for May, 1892. 8vo.
Surveyor for May, 1892. 8vo.
Telegraphic Journal for May, 1892. fol.
Zoophilist for May, 1892. 4to.
Electrical Engineers, Institution of—Journal, No. 98. 8vo. 1892.
Ex Libris Society—Journal for May, 1892. 4to.
Florence, Biblioteca Nazionale Centrale—Bolletino, Nos. 153, 154. 8vo. 1892.
Franklin Institute—Journal, No. 797. 8vo. 1892.
Fraser, Colonel A. T. R.E. M.R.I. (the Author)—Land Improvement in India. 8vo. 1892.
Geographical Society, Royal—Proceedings, Vol. XIV. No. 5. 8vo. 1892.
Gladstone, John Hall, Esq. Ph.D. F.R.S. F.C.S. M.R.I.—Tijdschrift van het Nederlandsch Aardrijkskundig Genootschap, Tweede Serie, Deel 1–8. 4to and 8vo. 1881–91.
Harlem, Société Hollandaise des Sciences—Verhandelingen, 3de Verz. Deel 5, 2de Stuk. 4to. 1892.
Johns Hopkins University—American Chemical Journal, Vol. XIV. No. 3. 8vo. 1892.
Leicester Free Libraries—Annual Report, 1891–92. 8vo.
Lewins, R. M.D.—Sadducee and Pharisee. By G. M. McCrie. 8vo. 1892.
Linnean Society—Journal, No. 151. 8vo. 1892.
Manchester Geological Society—Transactions, Vol. XXI. Parts 14–17. 8vo. 1892.
Manchester Literary and Philosophical Society—Memoirs and Proceedings, Vol. V. No. 1. 8vo. 1892.
Manchester Steam Users' Association—Boiler Explosions Act, 1882, Report, Nos. 430–499. 8vo. 1891.
Meteorological Society, Royal—Quarterly Journal, No. 82. 8vo. 1892.
Meteorological Record, No. 42. 8vo. 1892.
Ministry of Public Works, Rome—Giornale del Genio Civile, 1892, Fasc. 2, 3. 8vo. And Designi. fol. 1892.
National Life-Boat Institution, Royal—Annual Report, 1892. 8vo.
New South Wales, Department of Public Instruction—Report, 1890. 8vo. 1891.

North of England Institute of Mining and Mechanical Engineers—Transactions, Vol. XLI. Part 2. 8vo. 1892.
Payne, Wm. W. Esq. and Hale, Geo. E. Esq. (the Editors)—Astronomy and Astro-Physics for May, 1892. 8vo.
Pharmaceutical Society of Great Britain—Journal for May, 1892. 8vo.
Photographic Society of Great Britain—Journal, Vol. XVI. Nos. 7, 8. 8vo. 1892.
Prince, C. Leeson, Esq. F.R.A.S. F.R. Met. Soc.—Summary of a Meteorological Journal for 1891. fol.
Rayleigh, The Right Hon. Lord, D.C.L. F.R.S. M.R.I.—The Physics of Media. By J. J. Waterston. (Philosophical Transactions, 1892.)
Rio de Janeiro, Observatoire Impérial de—Revista, 1892, No. 1. 8vo.
Roberts, Isaac, Esq. F.R.A.S. F.R.S.—Celestial Photographs, with Notes. fol. 1887–92.
Rochester Academy of Science—Proceedings, Vol. I. Part 2. 8vo. 1891.
Royal Botanic Society of London—Quarterly Record, No. 49. 8vo. 1892.
Royal Institution of Cornwall—Journal, Vol. XI. Part 1. 8vo. 1892.
Seismological Society of Japan—Transactions, Vol. XVI. 8vo. 1892.
Selborne Society—Nature Notes, Vol. III. No. 30. 8vo. 1892.
Smithsonian Institution—Annual Report, 1889–90. 8vo. 1891.
National Museum Report, 1889. 8vo. 1891.
Contributions to North American Ethnology, Vol. VI. 4to. 1890.
Algonquian Bibliography. 8vo. 1891.
Society of Architects—Proceedings, Vol. IV. No. 11. 8vo. 1892.
Society of Arts—Journal for May, 1892. 8vo.
Tacchini, Professor P. Hon. Mem. R.I. (the Author)—Memorie della Società degli Spettroscopisti Italiani, Vol. XXI. Disp. 4ª. 4to. 1892.
United Service Institution, Royal—Journal, No. 171. 8vo. 1892.
United States Department of Agriculture—Monthly Weather Review for January–February, 1892. 4to. 1892.
Meteorological Work for Agricultural Institutions. By M. W. Harrington. 8vo. 1892.
Upsal University—Bulletin de l'Observatoire Météorologique, Vol. XXIII. 4to. 1891–92.
Vincent, Benjamin, Esq. Hon. Lib. R.I.—Report of the Registrar-General for Scotland, 1891. 8vo. 1892.
Zurich Naturforschenden Gesellschaft—Vierteljahrschrift, Jahrgang XXXVII. Heft 3, 4. 8vo. 1891.

GENERAL MONTHLY MEETING,

Monday, July 4, 1892.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

Walter Palmer, Esq. B.Sc.
The Hon. Sir Alfred Wills,

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned for the following Donation:—

Professor Dewar (Grant from Royal Society) ..	£300
Ludwig Mond, Esq.	120
Hugo Müller, Esq.	50

for carrying on investigations on Liquid Oxygen.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Academy of Natural Sciences, Philadelphia—Proceedings, 1892, Part 1. 8vo.
Accademia dei Lincei, Reale, Roma—Classe di Scienze Fisiche, Matematiche e Naturali. Atti, Serie Quinta: Rendiconti. 1° Semestre, Vol. I°. Fasc. 9, 10. 8vo. 1892.
Memorie, Vol. X. 8vo. 1892.
Classe di Scienze Morali, Storiche, etc.: Rendiconti, Serie Quinta, Vol. I. Fasc. 4. 8vo. 1892.
American Philosophical Society—Proceedings, No. 137. 8vo. 1892.
Astronomical Society, Royal—Monthly Notices, Vol. LII. No. 7. 8vo. 1892.
Bavarian Academy of Sciences—Sitzungsberichte, 1892. Heft 1. 8vo.
British Architects. Royal Institute of—Proceedings, 1891–2, Nos. 16, 17. 4to.
Canadian Institute—Transactions, Vol. II. Part 2, No. 4. 8vo. 1892.
The Rectification of Parliament. By S. Fleming, LL.D. 8vo. 1892.
Annual Archæological Report. 8vo. 1891.
Chemical Industry, Society of—Journal, Vol. XI. No. 5. 8vo. 1892.
Chemical Society—Journal for June, 1892. 8vo.
Cornwall Polytechnic Society, Royal—Annual Report for 1891. 8vo.
Cracovie, l'Académie des Sciences—Bulletin, 1892, No. 5. 8vo.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I.—Journal of the Royal Microscopical Society, 1892, Part 3. 8vo.
Dulier, Col. E. C.B. (the Author)—A Natural Process of Dissolution for Smoke and Fogs. 8vo. 1892.
East India Association—Journal, Vol. XXIV. No. 4. 8vo. 1892.
Editors—American Journal of Science for June, 1892. 8vo.
Analyst for June, 1892. 8vo.
Athenæum for June, 1892. 4to.

Editors—continued.
Brewers' Journal for June, 1892. 4to.
Chemical News for June, 1892. 4to.
Chemist and Druggist for June, 1892. 8vo.
Electrical Engineer for June, 1892. fol.
Electricity for June, 1892. 4to.
Electric Plant for June, 1892. 4to.
Engineer for June, 1892. fol.
Engineering for June, 1892. fol.
Engineering Review for June, 1892. 8vo.
Horological Journal for June, 1892. 8vo.
Industries for June, 1892. fol.
Iron for June, 1892. 4to.
Ironmongery for June, 1892. 4to.
Lighting for June, 1892. 4to.
Manufacturers' Engineering and Export Journal for June, 1892. 8vo.
Nature for June, 1892. 4to.
Open Court for June, 1892. 4to.
Photographic News for June, 1892. 8vo.
Photographic Work for June, 1892. 8vo.
Surveyor for June, 1892. 8vo.
Telegraphic Journal for June, 1892. fol.
Zoophilist for June, 1892. 4to.

Electrical Engineers, Institution of—Journal, No. 99. 8vo. 1892.
Ex-Libris Society—Journal for June, 1892. 4to.
Fleming, J. A. Esq. M.A. F.R.S. M.R.I. (the Author)—The Alternate Current Transformer, Vol. II. 8vo. 1892.
Florence, Biblioteca Nazionale Centrale—Bolletino, Nos. 155, 156. 8vo. 1892.
Franklin Institute—Journal, No. 798. 8vo. 1892.
Geographical Society, Royal—Proceedings, Vol. XIV. No. 6. 8vo. 1892.
Harlem Société Hollandaise des Sciences—Archives Néerlandaises, Tome XXVI. Livraison 1. 8vo. 1892.
Institute of Brewing—Transactions, Vol. V. No. 6. 8vo. 1892.
Iowa, Laboratories of Natural History—Bulletin, Vol. II. No. 2. 8vo. 1892.
Johns Hopkins University—University Circulars, Nos. 98, 99. 4to. 1892.
American Chemical Journal, Vol. XIV. No. 4. 8vo. 1892.
Lawes, Sir J. B. and Gilbert, Dr. J. H. (the Authors)—Field and other Experiments at Rothamsted. fol. 1892.
Linnean Society—Journal, No. 201. 8vo. 1892.
Manchester Geological Society—Transactions, Vol. XXI. Parts 18, 19. 8vo. 1892.
Massachusetts, State Board of Health—Report on Water Supply and Sewerage, Two Parts. 8vo. 1890.
Niblett, J. T. Esq. (the Author)—Secondary Batteries. 8vo. 1892.
Odontological Society—Transactions, Vol. XXIV. No. 7. 8vo. 1892.
Payne, W. W. and Hale, G. E. (the Editors)—Astronomy and Astro-Physics for June, 1892. 8vo.
Pharmaceutical Society of Great Britain—Journal, June, 1892. 8vo.
Radcliffe Library—Catalogue of Books added to Library during 1891. 4to. 1892.
Richardson, B. W. M.D. F.R.S. M.R.I. (the Author)—The Asclepiad, Vol. IX. No. 2. 8vo. 1892.
Royal Irish Academy—Proceedings, Third Series, Vol. II. No. 2. 8vo. 1892.
Royal Society of London—Proceedings, Nos. 308, 309. 8vo. 1892.
Royal Society of New South Wales—Journal and Proceedings, Vol. XXV. 8vo. 1891.
Sanitary Institute—Transactions, Vol. XII. 8vo. 1892.
Saxon Society of Sciences, Royal—Mathematisch-physischen Classe, Berichte, 1892, No. 1. 8vo. 1892.
Abhandlungen, Band XVIII. Nos. 5, 6. 4to. 1892.
Philologisch-historischen Classe, Band XIII. No. 4. 8vo. 1892.

Selborne Society—Nature Notes, Vol. III. No. 31. 8vo. 1892.
Society of Arts—Journal for June, 1892. 8vo.
United Service Institution, Royal—Journal, No. 172. 8vo. 1892.
United States Department of Agriculture—Monthly Weather Review for March, 1892. 4to.
Report of Chief of Weather Bureau, 1891. 8vo.
University of London—Calendar, 1892–93. 8vo.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1892: Heft 5. 4to.
Victoria Institute—Transactions, No. 98. 8vo. 1892.
Zoological Society of London—Procedings, 1892, Part 1. 8vo. 1892.

GENERAL MONTHLY MEETING,

Monday, November 7, 1892.

Sir James Crichton-Browne, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

Miss Emily Drummond,
J. J. Duveen, Esq.
Edward Johnson, Esq.
George Blundell Longstaff, M.D. F.R.C.P.
Robert Dobie Wilson, Esq.

were elected Members of the Royal Institution.

The Sincere Thanks of the Members were returned to Mr. Thomas G. Hodgkins, of Brambletye Farm, Setauket, Long Island, New York, for his munificent donation of $100,000 for "the investigation of the relations and co-relations existing between man and his Creator"; and the high appreciation of the Members was expressed of the example that he has thus set to those possessed of means and interested in the promotion of scientific investigation.

The Sincere Thanks of the Members were returned to the Goldsmiths' Company for their liberal donation of £1000 "for the continuation and development of the valuable original research which the "Society is engaged in carrying on; and especially for the prosecution "of investigations on the properties of matter at temperatures approaching that of the zero of absolute temperature"; and the peculiar gratification of the Members was expressed that this gift should have been made by a Company who, by the great share they have taken in the founding and development of the City and Guilds of London Institute for the Promotion of Technical Education, and by their more recent establishment and endowment of the Goldsmiths' Company's Technical and Recreative Institute at New Cross, have evinced their appreciation of the application of science to industrial purposes, and now by this donation to the Royal Institution show they recognise the value of the initiative stage—that of purely scientific research.

The Special Thanks of the Members were returned for the following Donation:—

Mr. F. D. Mocatta £50

for carrying on investigations on Liquid Oxygen.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Lords of the Admiralty—Greenwich Observations for 1889. 8vo. 1891.
The Governor-General of India—Geological Survey of India. Records, Vol. XXV. Parts 2, 3. 8vo. 1892.
Palæontologia Indica. Index of Genera and Species. 4to. 1892.
Memoirs. Index to first 20 volumes 1859–83. 8vo. 1892.
The Secretary of State for India—Papers relating to the Archæology of Burma, by E. Forchhammer. fol. 1884–1891.
Persia, by George N. Curzon. 2 vols. 8vo. 1892.
Archæological Survey of India. New Series. Vol. II. 4to. 1891.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quinta: Rendiconti. Classe di Scienze Fisiche, Matematiche e Naturali. 1° Semestre, Vol. I. Fasc. 11-12, 2° Semestre, Vol. I. Fasc. 1-7. 8vo. 1892.
Rendiconti, Serie Quinta, Classe di Scienze Morali, Storiche e Filologiche, Vol. I. Fasc. 5-7. 8vo. 1892.
Memorie, Vol. X. 4to. 1892.
Agricultural Society of England, Royal. Journal. 3rd Series. Vol. III. Parts, 2, 3. 8vo 1892.
American Academy of Arts and Sciences. Proceedings. New Series. Vol. XVIII. 8vo. 1891.
Memorial of Joseph Lovering. 8vo. 1892.
American Geographical Society—Bulletin. Vol. XXIV. Nos. 2, 3. 8vo. 1892.
American Philosophical Society—Transactions. Vol. XVII. Parts 1, 2. 4to. 1892.
Proceedings. No. 138. 8vo. 1892.
Antiquaries, Society of—Proceedings, Vol. XIV. No. 1. 8vo. 1892.
Archæologia. Second Series. Vol. III. 4to. 1892.
Asiatic Society of Bengal—Proceedings. 1891, Nos. 7–10. 1892, Nos. 1–3. 8vo.
Journal. Vol. XL. Part 1, Nos. 2–3. Part 2, Nos. 2–6. Vol. XLI. Part 1, No. 1. Part 2, No. 1. 8vo. 1891–2.
Asiatic Society of Great Britain, Royal—Journal for July and October, 1892. 8vo.
Astronomical Society, Royal—Memoirs, Vol. L. 4to. 1892.
Monthly Notices. Vol. LII. Nos. 8-9. 8vo. 1892.
Bankers, Institute of—Journal, Vol. XIII. Part 7. 8vo. 1892.
Bavarian Academy of Sciences—Sitzungsberichte. 1892. Heft 2. 8vo.
Binnie, A. R. Esq. M.Inst.C.E. M.R.I. (*the Author*)—Average Annual Rainfall. 8vo. 1892.
Birmingham Philosophical Society—Proceedings, Vol. VII. Part 2. 8vo. 1892.
British Architects, Royal Institute of—Proceedings, 1891-2, Nos. 18-20. 1892–93. No. 1. 4to.
Calendar 1892–3. 8vo.
British Museum (Natural History). Catalogue of Birds. Vols. XVI. XVII. 8vo. 1892.
California, University of—Publications, 1891–92. 8vo.
Cambridge Philosophical Society—Proceedings, Vol. VII. Part 6. 8vo. 1892.
Transactions, Vol. XV. Part 3. 4to. 1892.
Canada, Geological and Natural History Survey of—Catalogue of Canadian Plants, Part IV. 8vo. 1892.
Contributions to Canadian Micro-Palaeontology, Part IV. 8vo. 1892.
Report 1889, Part D, Nos. 1-9, Part N, Nos. 1–3. fol.
Chemical Industry, Society of—Journal, Vol. XI. Nos. 6–9. 8vo. 1892.
Chemical Society—Journal for July to October, 1892. 8vo.
City of London College—Calendar, 1892–3. 8vo. 1892.

Civil Engineers, Institution of—Minutes of the Proceedings, Vols. CVIII. CIX. CX. 8vo. 1892.
Clinical Society—Transactions, Vol. XXV. 8vo. 1892.
Colonial Institute, Royal—Proceedings, Vol. XXIII. 8vo. 1892.
Cotgreave, A. Esq. F.R.H.S. (the Compiler)—Catalogue of the Guille-Allès Library, Guernsey. 8vo. 1891.
Cracovie, l'Académie des Sciences—Bulletin, 1892, Nos. 6, 7. 8vo.
Crisp, Frank, Esq. LL.B. F.L.S. M.R.I.—Journal of the Royal Microscopical Society, 1892, Parts 4–5. 8vo.
Dallmeyer, Thomas, R. Esq. F.R.A.S. M.R.I. (the Author)—The Telephotographic Lens. 8vo. 1892.
Dax, Société de Borda—Bulletin, Seizième Année. 4e Trimestre. Dix-septième Année. 1er 2e Trimestre. 8vo. 1891–2.
East India Association—Journal, Vol. XXIV. Nos. 5–7. 8vo. 1892.
Editors—American Journal of Science for July–Oct. 1892. 8vo.
Analyst for July–Oct. 1892. 8vo.
Athenæum for July-Oct. 1892. 4to.
Brewers' Journal for July–Oct. 1892. 4to.
Chemical News for July–Oct. 1892. 4to.
Chemist and Druggist for July–Oct. 1892. 8vo.
Electrical Engineer for July–Oct. 1892. fol.
Electric Plant for July–Oct. 1892. 8vo.
Electricity for July–Oct. 1892. 8vo.
Engineer for July–Oct. 1892. fol.
Engineering for July–Oct. 1892. fol.
Horological Journal for July–Oct. 1892. 8vo.
Industries for July–Oct. 1892. fol.
Iron for July-Oct. 1892. 4to.
Iron and Coal Trades Review, July. 4to.
Ironmongery for July–Oct. 1892. 4to.
Lightning for July–Oct. 1892. 8vo.
Monist, July–Oct. 8vo.
Nature for July–Oct. 1892. 4to.
Open Court for July–Oct. 1892. 4to.
Photographic Work for July–Oct. 1892. 8vo.
Surveyor for July-Oct. 1892. 8vo.
Telegraphic Journal for July–Oct. 1892. fol.
Transport for July–Oct. fol.
Zoophilist for July–Oct. 1892. 4to.
Electrical Engineers, Institution of—Journal, No. 100. 8vo. 1892.
Ex Libris Society—Journal for July to October, 1892. 4to.
Florence, Biblioteca Nazionale Centrale—Bolletino, Nos. 158–164. 8vo. 1892.
Franklin Institute—Journal, Nos. 799–801. 8vo. 1892.
Geographical Society, Royal—Proceedings, Vol. XIV. Nos. 7, 8. 8vo. 1892.
Geological Institute, Imperial, Vienna—Jahrbuch, Band XLI. Heft 2, 3. Band XLII. Heft 1. 8vo. 1892.
Abhandlungen, Band XVII. Heft 1–2. 4to. 1892.
Verhandlungen, 1892, Nos. 6–10. 8vo.
Geological Society—Quarterly Journal, No. 191, 192. 8vo. 1892.
Georgofili, Reale Accademia—Atti, Quarta Serie, Vol. XV. Disp. 2a. 8vo. 1892.
Harlem, Société Hollandaise des Sciences—Archives Neerlandaises, Tome XXV. Liv. 5. Tome XXVI. Liv. 2. 8vo. 1892.
Harris and Haddon, Messrs. (the Publishers)—Atomic Consciousness. By J. Bathurst. 8vo. 1892.
Horticultural Society, Royal—Journal, Vol. XIV. 8vo. 1892.
Imperial Institute—Year Book. 8vo. 1892.
Institute of Brewing—Transactions, Vol. V. No. 7. 8vo. 1892.
Iron and Steel Institute—Proceedings in America in 1890. 8vo. 1892.
Journal, 1892, No. 1. 8vo.

Johns Hopkins University—American Chemical Journal, Vol. XIV. Nos. 5–6. 8vo. 1892.
American Journal of Philology, Vol. XIII. Nos. 1, 2. 8vo. 1892.
Studies in Historical and Political Science, Tenth Series, Nos. 7–9. 8vo. 1892.
University Circular—No. 100. 4to. 1892.
Kennedy, T. S. Esq. J.P. M.R.I.—Sucre de Betterave. By Ch. Bardy. 8vo. 1881.
Alcoolisme. By Ch. Bardy. 8vo. 1888.
Leeds, Philosophical and Literary Society—Annual Report, 1891–2. 8vo.
Lewins, R. M.D.—Miss Naden's World Scheme. By G. M'Crie. 8vo. 1892.
Linnean Society—Journal, No. 152–3. 8vo. 1892.
Transactions. Botany. Vol. III. Parts 4–7. 4to. 1891–92.
Madras Government—Madras Meridian Circle Observations, 1874–76. 8vo. 1892.
Madras Government Central Museum—Report, 1891–92. fol. 1892.
Geological Map of the Madras Presidency. fol. 1892.
Manchester Geological Society—Transactions, Vol. XXI. Part 20. 8vo. 1892.
Manchester Literary and Philosophical Society—Memoirs and Proceedings, Vol. V. No. 2. 8vo. 1892.
Mechanical Engineers, Institution of—Proceedings, 1892, No. 2. 8vo.
Meteorological Society, Royal—Quarterly Journal, No. 83. 8vo. 1892.
Meteorological Record, No. 43. 8vo. 1892.
Ministry of Public Works, Rome—Giornale del Genio Civile, 1892, Fasc. 4–7. 8vo. And Designi. fol. 1892.
Mitchell C. Pitfield, Esq. M.R.I. (the Author)—Enlargement of the Sphere of Women. 8vo. 1892.
Montpellier Académie des Sciences—Mémoires, Tome XI. No. 2. 4to. 1891.
Musical Association—Proceedings, Eighteenth Session, 1891–92. 8vo. 1892.
National Life-Boat Institution, Royal—Journal, No. 165. 8vo. 1892.
New South Wales, Agent-General—Annual Report of the Department of Mines (N.S.W.) for 1891. fol. 1892.
New York Academy of Sciences—Transactions, Vol. XI. Nos. 1–5, 7–8. 8vo. 1891–2.
Annals, Vol. VI. Nos. 1–6. 8vo. 1891–92.
North of England Institute of Mining and Mechanical Engineers—Transactions, Vol. XLI. Part 3. 8vo. 1892.
Nova Scotian Institute of Science—Proceedings and Transactions, 2nd Series, Vol. I. Part 1. 8vo. 1891.
Numismatic Society—Chronicle and Journal, 1892, Part 2. 8vo.
Odontological Society—Transactions, Vol. XXIV. No. 8. 8vo. 1892.
Payne, Wm. W. Esq. and Hale, Geo. E. Esq. (the Editors)—Astronomy and Astro-Physics for July–Oct. 1892. 8vo.
Pennsylvania Geological Survey—Atlases A.A. Parts 5–6. 8vo. 1891.
Pharmaceutical Society of Great Britain—Journal for July–Oct. 1892. 8vo.
Photographic Society of Great Britain—Journal, Vol. XVI. No. 9, Vol. XVII. No. 1. 8vo. 1892.
Physical Society of London—Proceedings, Vol. XI. Part 4. 8vo. 1892.
Pitt-Rivers, Lieut.-General, D.C.L. F.R.S. M.R.I. (the Author)—Excavations in Bokerby Dyke and Wansdyke. Privately Printed. Vol. III. 4to. 1892.
Excavations in Cranborne Chase. Vol. II. 4to. 1888.
Powers, E. Esq. (the Author)—Should the Rainfall Experiments be Continued? 8vo. 1892.
Preussische Akademie der Wissenschaften—Sitzungsberichte, Nos. 1–40. 8vo. 1892.
Radcliffe Observatory—Observations, Vol. XLV. 8vo. 1891.
Reynolds, Miss K. M.—Lantern Slides of Faraday Apparatus, 1892.
Richardson, B. W. M.D. F.R.S. M.R.I. (the Author)—The Asclepiad, Vol. IX. Part 3. 8vo. 1892.
Royal Botanic Society of London—Quarterly Record Nos. 50, 51. 8vo. 1892.

Royal College of Physicians, Edinburgh—Reports from the Laboratory, Vol. IV. 8vo. 1892.
Royal College of Surgeons of England—Calendar, 1892. 8vo.
Royal Dublin Society—Proceedings, Vol. VII. New Series, Parts 3–4. 8vo. 1892.
Transactions, Vol. IV. Series II. Parts 9–13. 4to. 1891.
Royal Historical Society—Transactions, New Series, Vol. VI. 8vo. 1892.
Royal Irish Academy—Cunningham Memoirs, No. 7. 8vo. 1892.
Proceedings, Series II. Vol. IV. 8vo. 1884–88.
Transactions, Vol. XXIX. Parts 18–19. 8vo. 1892.
Royal Society of Canada—Proceedings and Transactions, Vol. IX. 4to. 1892.
Royal Society of Edinburgh—Transactions, Vol. XXXVI. Parts 2–3. Vol. XXXVII. Part 1. 4to. 1892.
Proceedings, Vol. XVIII. 8vo. 1892.
Royal Society of London—Proceedings, Nos. 307–315. 8vo. 1892.
Saxon Society of Sciences, Royal—Mathematische Physischen Classe: Abhandlungen, Band XVIII. No. 7. 4to. 1892.
Berichte, 1892, No. 2. 8vo. 1892.
Selborne Society—Nature Notes, Vol. III. Nos. 32–35. 8vo. 1892.
Siemens, Dr. Werner Von (the Author)—Scientific and Technical Papers, Vol. I. 8vo. 1892.
Smith, Basil Woodd, Esq. F.S.A. M.R.I.—Middlesex County Records, Vol. IV. 8vo. 1892.
Smithsonian Institution—Contributions to Knowledge, Vol. XXVIII. fol. 1892.
Société Archæologique du Midi de la France—Bulletin, Nos. 8–9. 8vo. 1891–92.
Society of Architects—Proceedings, Vol. IV. No. 12. 8vo. 1892.
Society of Arts—Journal for July–Oct. 1892. 8vo.
St. Pétersbourg, Académie Impériale des Sciences—Mémoires, Tome XXXVIII. Nos. 9–13. 4to. 1892.
Statistical Society, Royal—Journal, Vol. LV. Parts 2–3. 8vo. 1892.
Swanwick, Miss Anna, M.R.I. (the Author)—Poets, the Interpreters of their Age. 8vo. 1892.
Tacchini, Professor P. Hon. Mem. R.I. (the Author)—Memorie della Società degli Spettroscopisti Italiani, Vol. XXI. Disp. 5ª–8ª. 4to. 1892.
Tasmania, Royal Society of—Proceedings for 1891. 8vo. 1892.
Toronto Meteorological Office—Report of Meteorological Service of Canada, by Charles Carpmael. 8vo. 1892.
United Service Institution, Royal—Journal, Nos. 173–176. 8vo. 1892.
United States Department of Agriculture—Monthly Weather Review for March to June, 1892. 4to. 1892.
Weather Bureau, Bulletin, Nos. 1–4. 8vo. 1892.
University College, London—Report on the Bentham MSS. in the College. 8vo. 1892.
Veneto, l'Ateneo—Revista, Serie XV. Vol. II. Fasc. 1-6. 8vo. 1891.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1892, Heft 6–8. 4to. 1892.
Victoria Institute—Transactions, Nos. 99-100. 8vo. 1892.
Wright & Co. Messrs. John (the Publishers)—Golden Rules of Surgical Practice, by E. H. Fenwick. 12mo. 1892.
Ptomaines and other Animal Alkaloids, by A. C. Farquharson. 8vo. 1892.
Yorkshire Philosophical Society—Annual Report for 1892. 8vo.
Zoological Society of London—Proceedings, 1892, Parts 2, 3. 8vo. 1892.
Zurich Naturforschenden Gesellschaft—Vierteljahrschrift, Jahrgang XXXVII. Heft 1, 2. 8vo. 1892.

GENERAL MONTHLY MEETING,

Monday, December 5, 1892.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

Mrs. A. R. Binnie,
William Scott Fox, Esq.
Shrimant Sampatrao Gaikwad,
Lieut. Edmond Herbert Hills, R.E.
Mrs. Harry Jonas,
Nikola Tesla, Esq.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned for the following donation:—

Ludwig Mond, Esq. .. .. 200*l.*

for carrying on investigations on Liquid Oxygen.

The decease of Mr. Thomas G. Hodgkins, of Brambletye Farm, Setauket, Long Island, New York, was announced.

The following Lecture Arrangements were announced:—

SIR ROBERT STAWELL BALL, M.A. LL.D. F.R.S. Lowndean Professor of Astronomy and Geometry in the University of Cambridge. Six Lectures (adapted to a Juvenile Auditory) on ASTRONOMY. On Dec. 27 (*Tuesday*), Dec. 29, 31, 1892; Jan. 3, 5, 7, 1893.

PROFESSOR VICTOR HORSLEY, F.R.S. F.R.C.S. *M.R.I.* Fullerian Professor of Physiology, R.I. Ten Lectures on THE STRUCTURE AND FUNCTIONS OF THE NERVOUS SYSTEM—THE FUNCTIONS OF THE CEREBELLUM, AND THE ELEMENTARY PRINCIPLES OF PSYCHO-PHYSIOLOGY. On *Tuesdays*, Jan. 17, 24, 31, Feb. 7, 14, 21, 28, March 7, 14, 21.

THE REV. CANON AINGER, M.A. LL.D. Three Lectures on TENNYSON. On *Thursdays*, Jan. 19, 26, Feb. 2.

PROFESSOR PATRICK GEDDES. Four Lectures on THE FACTORS OF ORGANIC EVOLUTION. On *Thursdays*, Feb. 9, 16, 23, March 2.

THE REV. AUGUSTUS JESSOPP, D.D. Three Lectures on THE GREAT REVIVAL—A STUDY IN MEDIÆVAL HISTORY. On *Thursdays*, March 9, 16, 23.

PROFESSOR C. HUBERT H. PARRY, Mus. Doc. M.A. Professor of Musical History and Composition at the Royal College of Music. Four Lectures on EXPRESSION AND DESIGN IN MUSIC (with Musical Illustrations). On *Saturdays*, Jan. 21, 28, Feb. 4, 11.

THE RIGHT HON. LORD RAYLEIGH, M.A. D.C.L. LL.D. F.R.S. *M.R.I.* Professor of Natural Philosophy, R.I. Six Lectures on SOUND AND VIBRATIONS. On *Saturdays*, Feb. 18, 25, March 4, 11, 18, 25.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Great Trigonometrical Survey of India, Vols. XXV.-XXVI. 4to. 1891-92.

The New Zealand Government—The New Zealand Official Handbook for 1892. 8vo.

Accademia dei Lincei, Reale, Roma—Classe di Scienze Fisiche, Matematiche e Naturali. Atti, Serie Quinta: Rendiconti. 2° Semestre, Vol. I°. Fasc. 8, 9. 8vo. 1892.
Memoire, Vol. VI. 8vo. 1890.
Atti, Anno 44, Sess. 7ª. 4to. 1891.
Atti, Serie Quarto, Anno CCLXXXVI.-CCLXXXVII. 4to. 1890-91.
Classe di Scienze Morali Storiche, etc.: Rendiconti, Serie Quinta, Vol. I. Fasc. 9. 8vo. 1892.
American Association for the Advancement of Science.—Proceedings, 40th Meeting. Washington, 1891. 8vo. 1892.
Asiatic Society of Bengal—Journal, Vol. LXI., Part 1, No. 2; Part 2, No. 2. 8vo, 1892.
Proceedings, Nos. 4-5. 8vo. 1892.
Aubertin, J. J. Esq. M.R.I. (the Author)—Wanderings and Wonderings. 8vo. 1892.
Australian Museum, Sydney—Annual Report of the Trustees for 1891. 8vo. 1892.
Bankers, Institute of—Journal, Vol. XIII. Part 8. 8vo. 1892.
Boyle, R. Esq. (the Publisher)—A Sanitary Crusade through the East and Australasia. 8vo. 1892.
British Architects, Royal Institute of—Proceedings, 1892-3, No. 23. 4to.
Chemical Industry, Society of—Journal, Vol. XI. No. 10. 8vo. 1892.
Chemical Society—Journal for November, 1892. 8vo.
Chicago Exhibition, 1893—World's Congress Auxiliary. Preliminary Publications. 8vo. 1892.
Cracovie, l'Académie des Sciences—Bulletin, 1892, No. 8. 8vo.
Devonshire Association for the Advancement of Science, Literature and Art—Report and Transactions, Vol. XXIV. 8vo. 1892.
Devonshire Domesday, Part 9. 8vo. 1892.
Dupré, A. Esq. Ph.D. F.R.S. F.C.S. (the Author)—A Short Manual of Inorganic Chemistry, by Dupré and Hake. 8vo. 1892.
Editor—American Journal of Science for November, 1892. 8vo.
Analyst for November, 1892. 8vo.
Athenæum for November, 1892. 4to.
Brewers' Journal for November, 1892. 4to.
Chemical News for November, 1892. 4to.
Chemist and Druggist for November, 1892. 8vo.
Electrical Engineer for November, 1892. fol.
Electricity for November, 1892. 4to.
Electric Plant for November, 1892. 4to.
Engineer for November, 1892. fol.
Engineering for November, 1892. fol.
Engineering Review for November, 1892. 8vo.
Horological Journal for November, 1892. 8vo.
Industries for November, 1892. fol.
Iron for November, 1892. 4to.
Ironmongery for November, 1892. 4to.
Lightning for November, 1892. 4to.
Manufacturers' Engineering and Export Journal for November, 1892. 8vo.
Nature for November, 1892. 4to.
Open Court for November, 1892. 4to.
Photographic News for November, 1892. 8vo.
Photographic Work for November, 1892. 8vo.
Surveyor for November, 1892. 8vo.
Telegraphic Journal for November, 1892. fol.
Transport for November, 1892.
Zoophilist for November, 1892. 4to.
Florence Biblioteca Nazionale Centrale—Bolletino, Nos. 165-166. 8vo. 1892.
Franklin Institute—Journal, No. 803. 8vo. 1892.
Gaikwad, Shrimant Sampatrao K. (the Founder)—Catalogue of English Books in the Shri Sazaji Library. 8vo. 1891.
Rules of the Shri Sazaji Library. 8vo. 1892.

Geographical Society, Royal—Proceedings, Vol. XIV. No. 12. 8vo. 1892.
Grant, Robert, Esq. M.A. F.R.S. (the Author)—Second Glasgow Catalogue of 2156 Stars, for the Epoch 1890. 4to. 1892.
Harlem, Société Hollandaise des Sciences—Archives Néerlandaises, Tome XXVI. Livraison 3. 8vo. 1892.
Institute of Brewing—Transactions, Vol. VI. No. 1. 8vo. 1892.
Johns Hopkins University—University Circular, No. 101. 4to. 1891.
American Chemical Journal, Vol. XIV. No. 7. 8vo. 1892.
American Journal of Philology, Vol. XIII. No. 3. 8vo. 1892.
Johnston, Thomas C. Esq. (the Author)—Did the Phœnicians discover America? 8vo. 1892.
M'Intosh, W. C. M.D. F.R.S. (the Author)—A Brief History of the Scottish Fisheries, 1882–92. 8vo. 1892.
Manchester Geological Society—Transactions, Vol. XXII. Part 1. 8vo. 1892.
Manchester Public Free Libraries—Annual Report, 1891–92. 8vo.
Mancini, Prof. Diocleziano (the Author)—P. B. Shelley: Biographical Note, and Translations. 8vo. 1892.
Meteorological Society, Royal—Quarterly Journal, No. 84. 8vo. 1892.
Meteorological Record, No. 44. 8vo. 1892.
Ministry of Public Works, Rome—Giornale del Genio Civile, 1892, Fasc. 8, and Designi. fol. 1892.
North of England Institute of Mining and Mechanical Engineers—Transactions, Vol. XXXIX. Part 3; Vol. XL. Part 5; Vol. XLI. Part 5. 8vo. 1892.
Numismatic Society—Chronicle and Journal, 1892, Part 3. 8vo. 1892.
Odontological Society—Transactions, Vol. XXV. No. 1. 8vo. 1892.
Payne, W. W. and Hale, G. E. (the Editors)—Astronomy and Astro-Physics for November, 1892. 8vo.
Pharmaceutical Society of Great Britain—Journal, November, 1892. 8vo.
Royal Irish Academy—Transactions, Vol. XXX. Parts 1–2. 4to. 1892.
Royal Society of London—Proceedings, No. 316. 8vo. 1892.
Saxon Society of Sciences, Royal—Mathematisch-physischen Classe, Berichte, 1892. No. 3. 8vo. 1892.
Abhandlungen, Band XVIII. No. 8. 4to. 1892.
Selborne Society—Nature Notes, No. 36. 8vo. 1892.
Sidgreaves, the Rev. W. F.R.A.S. (the Author)—The Nova of 1892. 8vo.
Society of Architects—Proceedings, Vol. V. Nos. 1–2. 8vo. 1892.
Society of Arts—Journal for November, 1892. 8vo.
Stewart, Alexander, Esq. F.R.C.S. Edin. (the Author)—Our Temperaments. 2nd Edition. 8vo. 1892.
Tacchini, Prof. P. Hon. Mem. R.I.—Memorie della Società degli Spettroscopisti Italiani. Vol. XXI. Disp, 9ª. 4to. 1892.
United Service Institution, Royal—Journal, No. 177. 8vo. 1892.
United States Department of Agriculture—Monthly Weather Review for August, 1892. 4to.
United States Geological Survey—Mineral Resources of the United States, 1889–90. 8vo. 1892.
United States Internal Revenue—Report on Glucose. 8vo. 1884.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1892, Heft 9. 4to.
Victoria Institute—Transactions, No. 101. 8vo. 1892.
Vincent, Benjamin, Esq. Hon. Lib. R.I. (the Editor)—Haydn's Dictionary of Dates. 20th Edition. 8vo. 1892.
Yorkshire Archæological and Topographical Association—Journal, Part 46, 8vo. 1892.

WEEKLY EVENING MEETING,

Friday, June 10, 1892.

THE RIGHT HON. LORD KELVIN, D.C.L. LL.D. Pres. R.S. Vice-President, in the Chair.

PROFESSOR DEWAR, M.A. LL.D. F.R.S. *M.R.I.*

Magnetic Properties of Liquid Oxygen.

(*Abstract.*)

AFTER alluding to the generous aid which he had received both from the Royal Institution and from others in connection with his researches on the properties of liquid oxygen, and to the untiring assistance rendered him by his co-workers in the laboratory, Prof. Dewar said that on the occasion of the commemoration last year of the centenary of the birth of Michael Faraday he had demonstrated some of the properties of liquid oxygen. He hoped that evening to go several steps further, and to show liquid air, and to render visible some of its more extraordinary properties.

The apparatus employed consisted of the gas-engine downstairs, which was driving two compressors. The chamber containing the oxygen to be liquefied was surrounded by two circuits, one traversed by ethylene, the other by nitrous oxide. Some liquid ethylene was admitted to the chamber belonging to its circuit, and there evaporated. It was then returned to the compressor as gas and liquefied, and thence, again, into the chamber as required. A similar cycle of operations was carried out with the nitrous oxide. There was a hundredweight of liquid ethylene prepared for the experiment. Ethylene was obtained from alcohol by the action of strong sulphuric acid. Its manufacture was exceedingly difficult, because dangerous, and as the efficiency of the process only amounted to 15 or 20 per cent. the preparation of a hundredweight of liquid was no light task. The cycle of operations, which, for want of time, was not fully explained, was the same as that commonly employed in refrigerating machinery working with ether or ammonia.

The lecturer then exhibited to the audience a pint of liquid oxygen, which by its cloudy appearance showed that it contained traces of impurity. The oxygen was filtered, and then appeared as a clear transparent liquid with a slightly blue tinge. The density of oxygen gas at $-182°$ C. is normal, and the latent heat of volatilisation of the liquid is about 80 units. The capillarity of liquid oxygen at its boiling-point was about one-sixth that of water. The temperature of liquid oxygen at atmospheric pressure, determined by the specific heat method, using platinum and silver, was $-180°$ C.

Reference was then made to a remarkable experimental corroboration of the correctness for exceedingly low temperatures of Lord

Kelvin and Prof. Tait's thermo-electric diagram. If the lines of copper and platinum were prolonged in the direction of negative temperature, they would intersect at $-95°$ C. Similarly, the copper and palladium lines would cut one another at $-170°$ C. Now, if this diagram were correct, the E.M.F. of the thermo-electric junctions of these two pairs of metals should reverse at these points. A Cu—Pt junction connected to a reflecting galvanometer was then placed in oxygen vapour and cooled down. At $-100°$ C. the spot of light stopped and reversed. A Cu—Pd junction was afterwards placed in a tube containing liquid oxygen, and a similar reversal took place at about $-170°$ C.

Liquid oxygen is a non-conductor of electricity: a spark, taken from an induction coil, one millimetre long in the liquid requires a potential equal to a striking distance in air of 25 millimetres. It gave a flash now and then, when a bubble of the oxygen vapour in the boiling liquid came between the terminals. Thus liquid oxygen is a high insulator. When the spark is taken from a Wimshurst machine the oxygen appears to allow the passage of a discharge to take place with much greater ease. The spectrum of the spark taken in the liquid is a continuous one, showing all the absorption bands.

As to its absorption spectrum, the lines A and B of the solar spectrum are due to oxygen, and they came out strongly when the liquid was interposed in the path of the rays from the electric lamp. Both the liquid and the highly compressed gas show a series of five absorption bands, situated respectively in the orange, yellow, green and blue of the spectrum.

Experiments prove that gaseous and liquid oxygen have substantially the same absorption spectra. This is a very noteworthy conclusion considering that no compound of oxygen, so far as is known, gives the absorptions of oxygen. The persistency of the absorption through the stages of gaseous condensation towards complete liquidity implies a persistency of molecular constitution which we should hardly have expected. The absorptions of the class to which A and B belong must be those most easily assumed by the diatomic molecules (O_2) of ordinary oxygen; whereas the diffuse bands above referred to, seeing they have intensities proportional to the square of the density of the gas, must depend on a change produced by compression. This may be brought about in two ways, either by the formation of more complex molecules, or by the constraint to which the molecules are subjected during their encounters with one another.

When the evaporation of liquid oxygen is accelerated by the action of a high expansion pump and an open test-tube is inserted into it, the tube begins to fill up with liquid atmospheric air, produced at the ordinary barometric pressure.

Dr. Janssen had recently been making prolonged and careful experiments on Mont Blanc, and he found that these oxygen lines disappeared more and more from the solar spectrum as he reached higher altitudes. The lines at all elevations come out more strongly when the sun is low, because the rays then have to traverse greater thicknesses of the earth s atmosphere.

Michael Faraday's experiments made in 1849 on the action of magnetism on gases opened up a new field of investigation. The following table, in which + means "magnetic" and – means "negative," summarises the results of Faraday's experiments.

MAGNETIC RELATIONS OF GASES (FARADAY).

	In Air.	In Carbonic Acid.	In Hydrogen.	In Coal Gas.
Air	0	+	+ weak	+
Nitrogen	–	–	– strong	–
Oxygen	+	+	+ strong	+ strong
Carbonic acid	–	0	–	– weak
Carbonic oxide ..	–	+	–	– weak
Nitric oxide	– weak	+	+	..
Ethylene	–	–	–	– weak
Ammonia	–	–	–	..
Hydrochloric acid ..	–	–	– weak	..

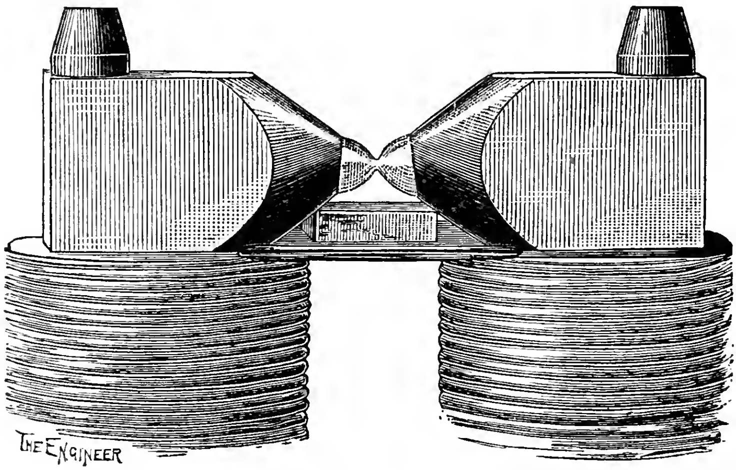
Becquerel was before Faraday in experimenting upon this subject. Becquerel allowed charcoal to absorb gases, and then examined the properties of such charcoal in the magnetic field. He thus discovered the magnetic properties of oxygen to be strong, even in relation to a solution of ferrous chloride, as set forth in the following table:—

SPECIFIC MAGNETISM, EQUAL WEIGHTS (BECQUEREL).

Iron	+	1,000,000
Oxygen	+	377
Ferrous chloride solution, sp. gr. 1·4334 ..	+	140
Air	+	88
Water	–	3

The lecturer took a cup made of rock salt, and put in it some liquid oxygen. The liquid did not wet rock salt, but remained in a spheroidal state. The cup and its contents were placed between and a little below the poles of an electro-magnet. Whenever the circuit was completed, the liquid oxygen rose from the cup and connected the two poles, as represented in the cut, which is copied from a photograph of the phenomenon. Then it boiled away, sometimes more on one pole than the other, and when the circuit was broken it fell off the pole in drops back into the cup. He also showed that the magnet would draw up liquid oxygen out of a tube. A test-tube containing liquid oxygen when placed in the Hughes balance produced no disturbing effect. The magnetic moment of liquid oxygen is about 1000 when the magnetic moment of iron is taken as 1,000,000. On cooling some bodies increased in magnetic power. Cotton wool, moistened with liquid oxygen, was strongly attracted by the magnet, and the liquid oxygen was actually sucked out of it on to the poles. A crystal of ferrous sulphate, similarly cooled, stuck to one of the poles.

The lecturer remarked that fluorine is so much like oxygen in its properties, that he ventured to predict that it will turn out to be a magnetic gas.



Magnetic Attraction of Liquid Oxygen.

Nitrogen liquefies at a lower temperature than oxygen, and one would expect the oxygen to come down before the nitrogen when air is liquefied, as stated in some text-books, but unfortunately it is not true. They liquefy together. In evaporating, however, the nitrogen boils off before the oxygen. He poured two or three ounces of liquid air into a large test-tube, and a smouldering splinter of wood dipped into the mouth of the tube was not re-ignited; the bulk of the nitrogen was nearly five minutes in boiling off, after which a smouldering splinter dipped into the mouth of the test-tube burst into flame.

Between the poles of the magnet all the liquefied air went to the poles; there was no separation of the oxygen and nitrogen. Liquid air has the same high insulating power as liquid oxygen. The phenomena presented by liquefied gases present an unlimited field for investigation. At $-200°$ C. the molecules of oxygen had only one-half of their ordinary velocity and had lost three-fourths of their energy. At such low temperatures they seemed to be drawing near what might be called "the death of matter," so far as chemical action was concerned; liquid oxygen, for instance, had no action upon a piece of phosphorus and potassium or sodium dropped into it; and once he thought and publicly stated, that at such temperatures all chemical action ceased. That statement required some qualification because a photographic plate placed in liquid oxygen, could be acted upon by radiant energy, and at a temperature of − 200° C. was still sensitive to light.

Prof. M'Kendrick had tried the effect of these low temperatures upon the spores of microbic organisms, by submitting in sealed glass tubes blood, milk, flesh, and such-like substances, for one hour to a temperature of − 182° C., and subsequently keeping them at blood heat for some days. The tubes on being opened were all putrid. Seeds also withstood the action of a similar amount of cold. He thought, therefore, that this experiment had proved the possibility of Lord Kelvin's suggestion, that life might have been brought to the newly-cooled earth upon a seed-bearing meteorite.

In concluding, the lecturer heartily thanked his two assistants, Mr. R. N. Lennox and Mr. J. W. Heath, for the arduous work they had had in preparing such elaborate demonstrations.

[J. D.]

INDEX TO VOLUME XIII.

END OF VOL. XIII.

LONDON: PRINTED BY WILLIAM CLOWES AND SONS, LIMITED,
STAMFORD STREET AND CHARING CROSS.