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 XIV.
1893—1895.

LONDON:
PRINTED BY WILLIAM CLOWES AND SONS, LIMITED,
STAMFORD STREET AND CHARING CROSS.
1896.

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Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, January 20, 1893.

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

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

Liquid Atmospheric Air.

The prosecution of research at temperatures approaching the zero of absolute temperature is attended with difficulties and dangers of no ordinary kind. Having no recorded experience to guide us in conducting such investigations, the best instruments and methods of working have to be discovered. The necessity of devising some new kind of vessel for storing and manipulating exceedingly volatile fluids like liquid oxygen and liquid air, became apparent when the optical properties of the bodies came under examination. The liquids, being in active ebullition, were in a condition which rendered optical measurements impossible. All attempts at improvement on the principle of using a succession of surrounding glass vessels, the annular space between such vessels having the cool current of the vapour coming from the boiling liquid led through them, proved a failure. Apart altogether from the rapid ebullition interfering with experimental work, the fact that it took place involved a great additional cost in the conduct of experiments on the properties of matter under such exceptional conditions of temperature.

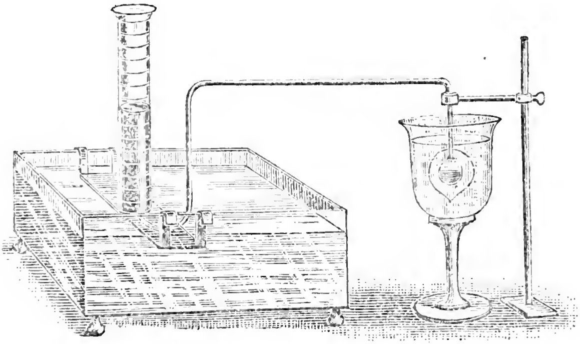
While suffering great anxiety on the question of expenditure, the Goldsmiths' Company came forward with the handsome contribution of 1000*l.* to continue the work with improved apparatus. Personally, I desire to express my grateful thanks to the Goldsmiths' Company for tendering such encouragement and support.

On careful consideration it became apparent that the proper way of attacking the problem was to conduct a series of experiments on the relative amounts of heat conveyed to boiling liquid gases; firstly, by means of the convective transference of heat by the gas particles, and, secondly, by radiation from surrounding bodies. The early experiments of Dulong and Petit on the laws of radiation had proved the very important part played by the gas particles surrounding a body in dissipating heat otherwise than by pure radiation. In the year 1873 I used a highly-exhausted vessel in calorimetric experiments "On the Physical Constants of Hydrogenium" (Trans.

Roy. Soc. Ed., vol. xxvii.) and the subsequent investigations of Crookes, and especially of Bottomley, having confirmed the great importance to be attached to the gas particles in the gain or loss of heat, it naturally occurred to me that the use of high vacua surrounding the vessels containing liquid gases would be advantageous. In order to arrive at definite data, some means of conducting comparative experiments between the amount of convective and radiant heat at such low temperatures had to be devised.

The apparatus shown in Fig. 1 measures the relative volumes of gas distilled in a given time under definite conditions, so that the measure of the gas distilled is proportional to the amount of heat conveyed to the liquid. The experiments are made in the following way: the distilling vessel to the right consists of two concentric spherical chambers, the space between being highly exhausted by a mercurial air-pump. The inner sphere is filled with liquid ethylene, oxygen, or air, and the whole apparatus immersed in water maintained at a constant temperature. Distillation begins immediately and continues at a constant rate provided the liquid in the bulb is maintained at the same level. This is not possible, but a sufficient uniformity is attained by making the observations during the evaporation of the first fourth of the contents. Having measured the volume of gas produced per minute when the sphere is surrounded with a high vacuum, the vessel is taken out of the water, the end nipped off, air allowed to enter, and then again closed. In this condition the inner sphere is filled up again with the liquid and the above experiment repeated. The following results were obtained, using ethylene and oxygen respectively:

Fig. 1.

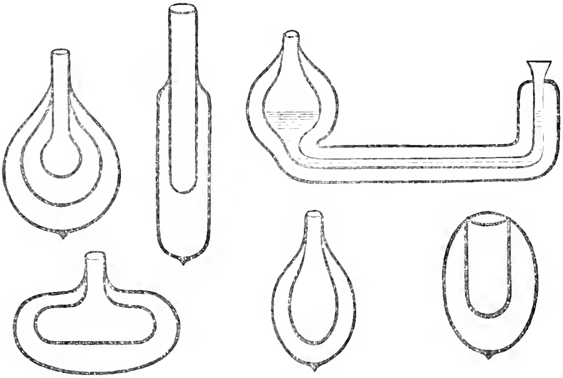


Liquid Oxygen, Vacuum	170 cc. per minute.
" " Air	840 " "
" Ethylene, Vacuum ..	56 " "
" " Air	250 " "

These results prove that under the same conditions a high vacuum diminishes the rate of evaporation to one-fifth part of what it is when the substance is surrounded with air at atmospheric pressure, or, in other words, liquid oxygen or ethylene lasts five times longer when surrounded with a vacuous space.

The next step was to construct a series of glass vessels surrounded by a vacuous space, suitable for various experiments, and such are represented in Fig. 2. In vessels of this kind, if the vacuum is very high, no ice appears on the surface of the outer vessel, even although the walls of the vacuous space are within half an inch of each other, and the liquid oxygen or air evaporates almost solely from the surface, no bubbles of gas being given off throughout the mass of the liquid. So far the convective transference of heat has been stopped by the use of a high vacuum, but if the inner vessel is coated with a bright deposit of silver, then the radiation is diminished also, with the result that the rate of evaporation is further reduced to more than a half. In such vessels liquid oxygen or liquid air can be kept for hours, and the economy and ease of manipulation greatly improved.

Fig. 2.



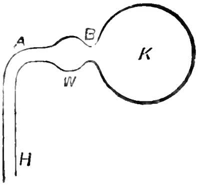
The arrangements represented in Fig. 2 may be employed to study the law of radiation at low temperatures. All that is necessary for the purpose is to immerse the outer vessel in liquids maintained at different temperatures. The following preliminary results have been obtained, using oxygen, which boils at −180° C., in the inner sphere:—

Temperature.	Radiation.
− 115° C.	60 cc.
− 78° C.	120 „
+ 6° C.	300 „
+ 65° C.	600 „

These results show that radiation (along with such convective transference as remains) grows approximately at the rate of the cube of the absolute temperature. Many further experiments must, however, be made before the real law of radiation at low temperatures can be strictly defined.

To produce exceedingly high vacua in vessels used for such purposes as the collection and storage of liquid air, a mercurial vacuum made in the following manner has been found highly satisfactory. Take, as an illustration, a glass vessel shaped like Fig. 3, and after placing in it a quantity of mercury and connecting the pipe H with a good working air-pump, place in an oil or air-bath heated above 200° C., and distil off a good quantity of the mercury. While the distillation is taking place the tube is sealed off at the point A, and the bulb instantly removed from the heated bath to such an extent as to allow condensation to take place in the small chamber marked B W in the figure. As the air-pump can maintain a vacuum of ten millimetres, no difficulty arises in sealing the glass tube during the continuance of the distillation. In some cases the mercury in the vessel has been heated up to near its boiling point in air, and then the air-pump started, causing thereby an almost explosive burst of mercurial vapour which very effectually carries all the air out of the vessel. After cooling, the vessel is removed from the bath, and the excess of mercury brought into the small bulb B W, care having been taken to remove any small globules of mercury, which adhere with great tenacity to the surface of the glass, by heating K while the part B W is kept cool. In this way the vessel K is filled with nothing but mercurial vapour, the pressure of which depends solely on the temperature of the liquid mercury contained in the small enlargement, and as this can, if necessary, be cooled to $-190°$ C. by immersing it in liquid air, we have the means of creating in K a vacuum of inconceivable tenuity. It is sufficient for the production of very good vacua to cool the mercury in B W to $-80°$ C. (using solid carbonic acid as the cooling agent), and while in this condition to seal off the bulb containing all the condensed mercury, so that K is left full of saturated vapour at $-80°$ C. When very high exhaustions are required it is better not to seal off the mercury bulb, as the glass is apt to give off some kind of vapour. A similar mode of proceeding is adopted when other shaped vessels have to be highly exhausted, and no difficulty has arisen in operating with vessels having the capacity of more than a litre.

Fig. 3.



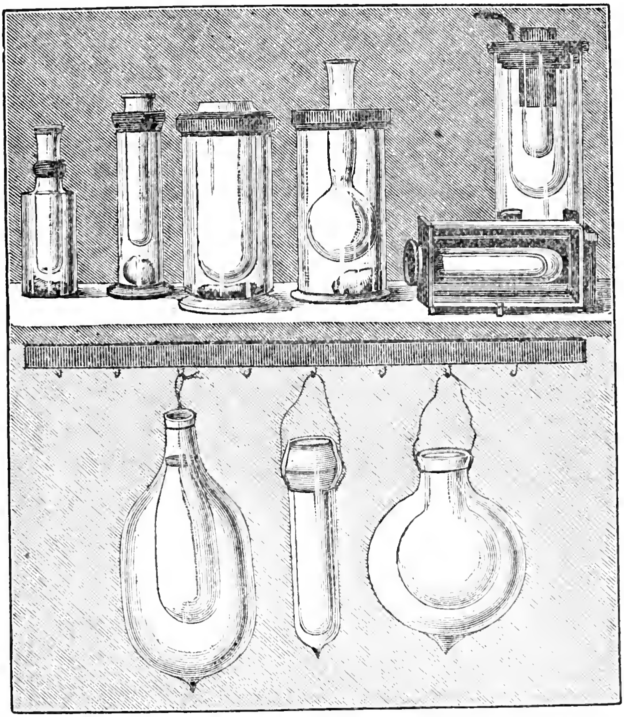
The perfection of the vacuum, assuming that nothing remains but molecules of mercury in the form of vapour, depends upon the temperature to which the subsidiary bulb is cooled. The well-known law which expresses approximately the relation between temperature and pressure in the case of saturated vapours, must be assumed to be applicable to mercury vapour at temperatures where direct measurement becomes impossible. Having calculated the constants of a vapour pressure formula from observed data at high temperatures, it is easy to arrive at a value of the vapour pressure for any assumed lower temperature. Such a formula as the following, $\log. P = 15 \cdot 1151 - 2 \cdot 2931, \log. T - \frac{3665 \cdot 7}{T}$, where P is pressure in millimetres of mercury and T is the absolute temperature, agrees well with the experimental results. This formula gives the vapour pressure at 0° C. as 0·000 18 mill., and at − 80° C. as 0·000,000,003 mill., or respectively, about the sixth and the four hundred thousandth of a millionth of an atmosphere. Such a high vacuum could never be reached by the use of any form of mercurial air-pump. The electric discharge in such vacua produces intense phosphorescence of the glass, giving thereby a continuous spectrum, which makes the detection of the mercury lines difficult.

Consider for a moment the proofs that could be adduced that mercury vapour, even below a millionth of an atmosphere pressure, can behave like an ordinary saturated vapour. The most characteristic property of a space filled with any saturated vapour is that cooling to a lower temperature causes partial condensation of the vapour in the form of liquid or solid. The amount of vapour in a mercurial vacuum at the ordinary temperature would weigh about the tenth of a milligram in the volume of a litre, and to see such an amount of the metal it would require to be concentrated on a small area in the form of a fine metallic film. Experiment has shown that the fifth of a milligram of gold may be made to cover one square centimetre of surface, so that a minute quantity of metal can be observed if properly deposited. This can be easily achieved in such mercurial vacua by cooling a small portion of the surface of the glass to the temperature of −180° C. by the application of a pad of cotton wool saturated with liquid oxygen. In an instant the vapour of mercury deposits in the form of a brilliant mirror, which, on the temperature rising, becomes subdivided into a mass of exceedingly minute spheres of liquid. A repetition of the cooling does not bring down a new mirror, provided the first area is maintained cool, but if the vessel contains excess of liquid mercury any number of mirrors of mercury may be deposited in succession.

Mercury is thus proved to distil at the ordinary temperature when the vapour pressure is under the millionth of an atmosphere. Further, it is easy to prove in this way that the cooling of the liquid mercury in such a subsidiary vessel (which has been described as a temporary part of the vacuum vessel) greatly improves the vacuum. For this purpose it is sufficient to cool the said vessel with some solid carbonic acid, and then to try and reproduce a mirror of mercury in the way previously described. No mercury mirror can be formed so long as the cold bath is maintained. If a piece of blotting paper, cut into any desired shape, be moistened with water, and then applied to the surface of one of the vacuum vessels containing excess of liquid mercury, the local reduction of temperature produced by the evaporation soon causes a rough image of the paper to appear in the form of minute globules of condensed mercury. Such experiments support the view that the laws of saturated vapours are maintained at very low pressures.

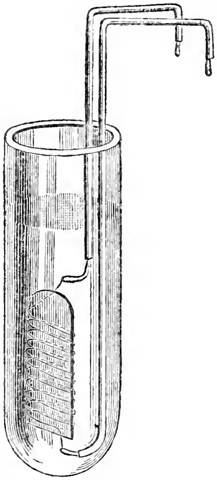
In Fig. 4 specimens of the old and new vessels for collecting and manipulating liquid gases are shown. In each of the old forms it will be noted that a mass of phosphoric anhydride placed in the lower portion is required to absorb traces of water, otherwise the vessels are useless for optical observations. The vacuum receivers get over this difficulty.

Fig. 4.



The perfection of the vacuum in different vessels, all treated in the same way, differs very much, and after use they almost invariably deteriorate. The relative rates of evaporation of liquid oxygen under the same conditions in different vessels is the best test of the vacuum. In many of the large vessels used for the storage of liquid gases, it is convenient and more effective to cause the deposition of a mercury mirror over the surface of the inner vessel (by leaving a little liquid mercury in the lower part of the double-shaped flask), instead of silvering as previously described. Under such conditions the mercury instantly distils and forms a brilliant mirror all over the surface of the inner vessel. The fact that mercury has a very high refractive index and is a bad conductor of heat are factors of importance in retarding the conveyance of heat. After the mercury mirror has been formed any further increase in the thickness of the film can be prevented, and at the same time the vacuum improved by freezing the excess of liquid mercury in the lower part of the vessel. The vacuum vessels described equally retard the loss as well as the gain of heat, and are admirably adapted for all kinds of calorimetric observations. The future use of these vessels in thermal observations will add greatly to the accuracy and ease of conducting investigations. The double spherical form of vacuum vessel is excellent for showing that the elevation or depression of a given volume of air a few feet causes an increase or diminution of volume, due to the small change of atmospheric pressure. The volume of air in the inner sphere is guarded from any sudden change of temperature by the surrounding highly vacuous space. This is only one of the many uses to which such receivers can be put.

FIG 5.



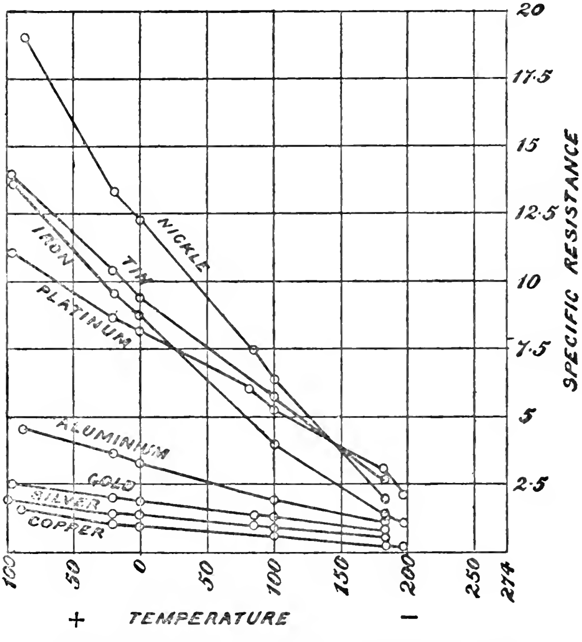
In making vacua, many other substances have been examined along with mercury, but they have not given equally satisfactory results.

Sulphur would occur to any one as a substance that might replace mercury, seeing the density in the form of vapour, and also the latent heat of vaporisation, are nearly identical; and it has the further advantage of being a solid at ordinary temperatures. The sulphur vacua have, however, so far not been an improvement, chiefly because traces of organic matter are decomposed by the sulphur, giving sulphuretted hydrogen and sulphurous acid, gases which are dissolved by and remain in the sulphur.

When the surface of such a sulphur vacuum is cooled with liquid oxygen in the manner previously described, a faint crystalline deposit occurs, only it takes a much longer time to appear than in the case of the mercury vacuum. If a similar vessel is boiled out, using phosphorus as the volatile substance, the application of liquid oxygen to the surface causes instant deposition. Thus it can be proved sulphur and phosphorus distil at ordinary temperatures just like mercury.

An investigation as to the electric conductivity of metals, alloys, and carbon at low temperatures has been undertaken in conjunction with my friend, Professor J. A. Fleming, D.Sc., F.R.S. The experiments are made by means of a resistance coil shown in Fig. 5, consisting of a piece of notched mica coiled with the fine wire to be tested, and of stout insulated copper-rod connections. The coil and connection are immersed in liquid oxygen contained in a vacuum test-tube, and the temperature of −200° C. can be reached by exhausting the oxygen by means of a powerful air-pump. The results point to the conclusion that absolutely pure metals seem to have no resistance near the zero of temperature as indicated by the above curves (Fig. 6) obtained by experiment. With alloys there is little change in resistance, as indicated in the curves (Fig. 7). The conductivity of carbon decreases with low temperatures, and increases with high ones. At the temperature of the electric arc, carbon appears to have no resistance.

Fig. 6.



Electrical Resistance of Metals at Low Temperature.

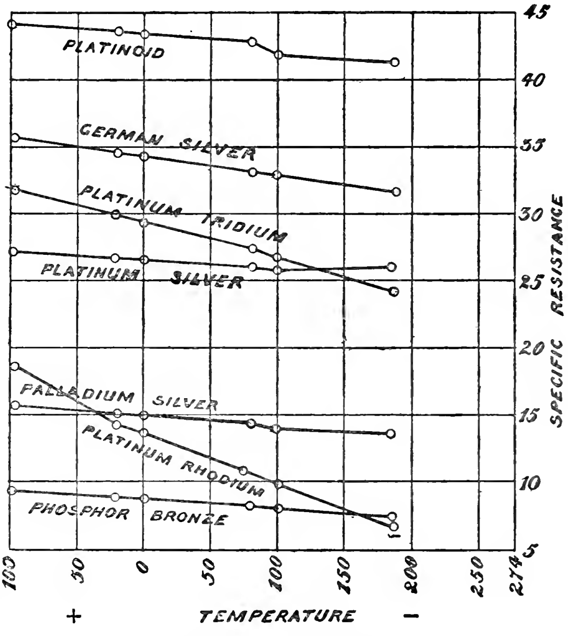
The optical constants of liquid oxygen, ethylene, and nitrous oxide have been so far determined, and in this matter my colleague, Professor Liveing, has been associated with me in the conduct of this work. The results obtained are given in the following table, and tend to confirm the Law of Gladstone as being applicable to such substances:—

Refractive Indices of Liquid Gases.

	Index.	Ref. Constant.	Ref. Molecular.
Oxygen	1·2236	1·989	6·364
Ethylene	13632	0·626	17·528
Nitrous Oxide	13305	0·263	11·587

$$\text{Law of Gladstone } \frac{\mu - 1}{D} = \text{Constant.}$$

Fig. 7.



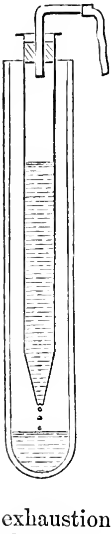
Electrical Resistance of Alloys at Low Temperatures.

The determination of the refractive index of liquid oxygen, at its boiling-point of − 182° C., presented more difficulty than would have been anticipated. The necessity for enclosing the vessel containing the liquid in an outer case to prevent the deposit of a layer of hoar-frost which would scatter all the rays falling on it, rendered manipulation difficult; and hollow prisms with cemented sides cracked with the extreme cold. It was only after repeated attempts, involving the expenditure of a whole litre of liquid oxygen on each experiment, that we succeeded in getting an approximate measure of the refractive index for the D line of sodium.

The mean of several observations gave the minimum deviation with a prism of 59° 15′ to be 15° 11′ 30″, and thence $\mu = 1\cdot2236$. The density of liquid oxygen at its boiling-point of $-182°$ C. is 1·124, and this gives for the refraction-constant, $\frac{\mu-1}{d} = 1\cdot989$, and for the refraction-equivalent 3·182. This corresponds closely with the refraction-equivalent deduced by Landolt from the refractive indices of a number of organic compounds. Also it differs little from the refraction-equivalent for gaseous oxygen, which is 3·0316. This is quite consistent with the supposition that the molecules of oxygen in the liquid state are the same as in the gaseous.

If we take the formula $\frac{\mu^2-1}{(\mu^2+2)d}$ for the refraction-constant we find the value of it for liquid oxygen to be ·1265, and the corresponding refraction-equivalent 2·024. These are exactly the means of the values found by Mascart and Lorenz for gaseous oxygen. The inherent difficulties of manipulation, and the fact that the sides of the hollow prism invariably became coated with a solid deposit, which obscured the image of the source of light, have hitherto prevented our determining the refractive indices for rays other than D.

FIG. 8.

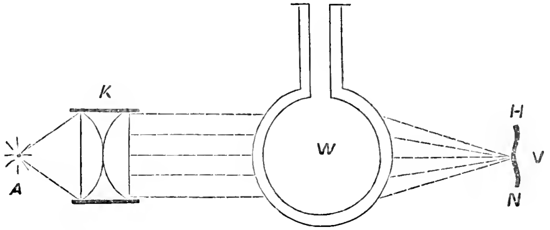


The optical projection of vacuum vessels having the shape of a double test-tube are very suitable for lecture illustration. As the critical point of oxygen is some thirty degrees higher than nitrogen it is easier to liquefy, and, consequently, becomes the most convenient substance to use for the production of temperatures about $-200°$ C. Liquid nitrogen, carbonic oxide, or air can conveniently be made at the ordinary atmospheric pressure, provided they are brought into a vessel cooled by liquid oxygen boiling under the pressure of about half an inch of mercury.

A simple arrangement for this purpose is shown in Fig. 8. The inner tube contains the liquid oxygen under exhaustion, surrounded by a vacuum vessel, the interior space between the inner tube and the vacuum vessel being connected with a receiver containing the gas which is to be liquefied. If the object is to collect liquid air, the inner air space is left quite open, no precautions being needed to free the air from carbonic acid or moisture, because under the conditions such substances are solids, and only cause a slight opalescence in the liquid, which drops continuously from the end of the inner tube and accumulates in the vacuum vessel. If the air supply is forced to bubble through a little strong sulphuric acid, the rate of condensation and the relative volume of gas and liquid can be observed. Liquid air boils at the temperature of $-190°$ C., giving off substantially pure nitrogen.

As the nitrogen boils 10° C. lower than oxygen, after a time the liquid alters its composition and boiling point, finally becoming pure oxygen. During the evaporation the liquid air changes very remarkably in colour, passing from a very faint blue to a much deeper shade. The changes can be traced best by the marked increase in the width of the absorption bands of liquid oxygen. If air, collected in the above manner in a vacuum vessel, is isolated from a rapid heat supply by immersing the vessel in liquid oxygen, and then a powerful air-pump brought to act upon it, after a time it passes into the condition of a clear, transparent, solid ice. Nitrogen solidifies, under such conditions, into a white mass of crystals, but all attempts to solidify oxygen by its own evaporation have failed. Such liquids as air and oxygen, we should anticipate, would be especially transparent to heat radiation, seeing they are very diathermic substances in their gaseous state. The thermal transparency of liquid oxygen can be shown by passing the radiation from the electric arc, as shown in the diagram, through a spherical vacuum vessel filled with clear filtered liquid, thereby concentrating the rays at a focus and igniting a piece of black paper held there.

FIG. 9.



In this experiment the oxygen lens has a temperature of −180° C., yet it does not prevent the concentrated radiation reaching a red heat at the focus. At such low temperatures as boiling oxygen and air all chemical action ceases. If some liquid oxygen is cooled to −200° C., and a glowing piece of wood inserted into the vessel above the liquid, it refuses to burst into flame, because of the low pressure of the vapour. An interesting experiment may be made by immersing an electric pile, composed of carbon and sodium, into liquid oxygen, when almost immediately the electric current ceases. The gaseous oxygen coming from the liquid must be exceedingly pure and dry, and as it has been alleged two chemical substances require the presence of a third one in order that they may combine, it was interesting to ascertain if a substance like sulphur would continue to burn after ignition in such an atmosphere. Sulphur placed in a small platinum vessel that had just been heated to redness, was raised to the boiling point, and in the act of combustion lowered into a vacuum vessel containing liquid oxygen. The combustion continued active, and for a time could be maintained in the middle of the liquid oxygen. This result suggests that oxygen and sulphur can enter into combination in a perfectly dry condition. Some notion of the temperature of liquid air is given by running on to the surface some absolute alcohol, which, after rolling about in the spheroidal state, suddenly solidifies into a hard transparent ice, which rattles on the sides of the vacuum test-tube like a marble. On lifting the solid alcohol out by means of a looped wire the application of the flame of a Bunsen burner will not ignite it. After a time the solid melts and falls from the looped wire like a thick syrup.

It is not the question of the change of state in matter, however interesting, that in our day has special attractions for the chemist, but the means of studying the properties of matter generally under the conditions of such exceptionally low temperatures as are the concomitants of the transition in the case of substances like oxygen and nitrogen. The work of investigation in this field proceeds slowly but surely, and one need not despair (unless on the grounds of expense) in the future of adding further data to our knowledge of the properties of matter near the zero of absolute temperature.

At the commencement of the lecture reference was made to the dangers and difficulties of this kind of research, and it becomes a pleasant duty to acknowledge the great services rendered by my assistants. But for the persistency and determination of Mr. Lennox, coupled with his marked engineering ability, the work would not have made such progress, and he has been ably supported by Mr. Heath.

[J. D.]

WEEKLY EVENING MEETING,

Friday, January 27, 1893.

David Edward Hughes, Esq. F.R.S. Vice-President,
in the Chair.

Francis Galton, Esq. F.R.S. *M.R.I.*

The Just-Perceptible Difference.

We seem to ourselves to belong to two worlds, which are governed by entirely different laws; the world of feeling and the world of matter—the psychical and the physical—whose mutual relations are the subject of the science of Psycho-physics, in which the just-perceptible difference plays a large part.

It will be explained in the first of the two principal divisions of this lecture that the study of just-perceptible differences leads us not only up to, but beyond, the frontier of the mysterious region of mental operations which are not vivid enough to rise above the threshold of consciousness. It will there be shown how important a part is commonly played by the imagination in producing faint sensations, and how its power on those occasions admits of actual measurement.

The last part of the lecture will deal with the limits of the power of optical discrimination, as shown by the smallest number of adjacent dots that suffice to give the appearance of a continuous line, and the feasibility will be explained of transmitting very beautiful outline drawings of a minute size, and larger and rougher plans, maps, and designs of all kinds, by means of telegraphy.

Material objects are measurable by external standards, about which it is sufficient to say that when we speak of a pound, a yard, or an hour, we use terms whose meanings are defined and understood in the same sense by all physicists. The feelings, on the other hand, cannot be measured by external standards, so we are driven to use internal ones, and to adopt a scale of sensation formed by units of just-perceptible differences, rising in the arithmetical order of 1, 2, 3, &c., and by their side a scale of measurements of the stimuli that provoked them. The attempts of those who first experimentalised in Psycho-physics were mainly directed to ascertain the relation between the increase of stimulus and the corresponding increment of sensation.

Their net result has been to confirm, within moderate limits, the trustworthiness of Weber's law, namely, that each successive increment of sensation is caused by the same *percentage* increment of the previous stimulus.

The rate at which a stimulus must be increased in order to give a just-perceptible increment of sensation, has been taken at the average of 1 per cent. for light, 6 per cent. for muscular effort, 33 per cent. for sound and warmth; also 33 per cent. for pressure upon most parts of the body, and as high as 16 per cent. upon the finger tips. But these values must not be trusted too far; they cease to be exact towards the two ends of the scale.

A mechanical arrangement clearly illustrates the consequences of Weber's law. It includes an axle to which is fixed a wheel, a part of a logarithmic spiral, and an index hand. This portion of the machine is carefully balanced, so that it will remain steady in any position in which it is set, while a small force is sufficient to cause it to turn; behind all is a card with equal graduations upon it, over which the index travels. A string, with a scale pan at one end and a counterpoise at the other, is wrapped round the wheel. A string fastened to the axle passes over the logarithmic arm, and a ball is fastened to its free end. The varying weights put in the scale pan will now represent varying amounts of stimulus, and the graduations to which the index points, represent the corresponding variations of sensation.

I exhibit a diagrammatic model of the apparatus, much too rough to give exact indications, but still sufficient for rough explanatory purposes.

Owing to the obvious properties of a spiral, the more the axle to which it is fixed is rotated in the direction of its concave side, the further does the point at which the string is hanging travel away from the axis, and the leverage exerted by the weight of the ball will increase. Whatever be the weight in the scale pan, there is within the working range of the apparatus some position of the beam at which that weight will be counterbalanced by the ball. The property of the logarithmic spiral is that equal degrees of rotation correspond to equal percentage increments of leverage. Hence, when percentage increments of weight are successively placed in the scale pan, the index attached to the beam will successively travel over equal divisions of the scale, in accordance with Weber's formula.

The progressive increase in the effective length of the logarithmic arm is small at first, but is seen soon to augment rapidly, and then to become extravagant. We thus gain a vivid insight through this piece of mechanism into the enormous increase of stimulus, when it is already large, that is required to produce a fresh increment of sensation, and how soon the time must arrive when the organ of sense, like the machine, will break down under the strain rather than admit of being goaded farther.

The result of all this is, that although the senses may perceive very small stimuli, and can endure very large ones without suffering damage, the number of units in the scale of sensation is comparatively small. The hugest increase of good fortune will not make a man who is already well off many degrees happier than before; the utmost torture that can be applied to him will not give much greater pain than he has already sometimes suffered. The experience of a life that we call uneventful usually includes a large share of the utmost possible range of human pleasures and human pains. Thus the physiological law which is expressed by Weber's formula is a great leveller, by preventing the diversities of fortune from creating by any means so great a diversity in human happiness.

The least-perceptible difference varies considerably in different persons, delicacy of perception being a usual criterion of superiority of nature. The sense of pain is curiously blunt in idiots. It varies also in the same person with his health, and extraordinarily so in hysteria and hypnotism, at which times sensitivity is sometimes almost absent, and at other times exceptionally acute. It is somewhat affected by drugs. Thus Dr. Lauder Brunton writes concerning strychnine, that when taken in small doses for a long time, the impressions are felt more keenly and are of longer duration. The sense of touch is rendered more acute; the field of vision is increased, distant objects are more distinct, and the sense of hearing is sharpened. (*Pharmacology*, 1885, p. 888.)

Other drugs or intoxicants may yet be discovered and legitimately used to heighten the sensitivity, or indeed any other faculty during a brief period, in order to perform that which could not otherwise be performed at all, at the cheap price of a subsequent period of fatigue.

Measure of the Imagination.—The first perceptible sensation is seldom due to a solitary stimulus. Internal causes of stimulation are in continual activity, whose effects are usually too faint to be perceived by themselves, but they may combine with minute external stimuli, and so produce a sensation which neither of them could have done singly. I desire now to draw attention to another concurring cause which has hitherto been unduly overlooked, or only partially allowed for under the titles of Expectation and Attention. I mean the Imagination, believing that it should be frankly recognised as a frequent factor in the production of a just-perceptible sensation. Let us reflect for a moment on the frequency with which the imagination produces effects that actually overpass the threshold of consciousness, and give rise to what is indistinguishable from, and mistaken for, a real sensation. Every one has observed instances of it in his own person and in those of others. Illustrations are almost needless; I may, however, mention one as a reminder; it was current in my boyhood, and the incident probably took place not many yards from where I now stand. Sir Humphry Davy had recently discovered the metal potassium, and showed specimens of it to the greedy gaze of a philosophical friend as it lay immersed in a dish of alcohol to shield it from the air, explaining its chemical claim to be considered a metal. All the known metals at that time were of such high specific gravity that weight was commonly considered to be a peculiar characteristic of metals; potassium, however, is lighter than water. The philosopher not being aware of this, but convinced as to its metallic nature by the reasoning of Sir Humphry, fished a piece out of the alcohol, and, weighing it awhile between his finger and thumb, said seriously, as in further confirmation, "How heavy it is!"

In childhood the imagination is peculiarly vivid, and notoriously leads to mistakes, but the discipline of after life is steadily directed to checking its vagaries and to establishing a clear distinction between fancy and fact. Nevertheless, the force of the imagination may endure with extraordinary power and even be cherished by persons of poetic temperament, on which point the experiences of our two latest Poets-Laureate, Wordsworth and Tennyson, are extremely instructive. Wordsworth's famous "Ode to Immortality" contains three lines which long puzzled his readers. They occur after his grand description of the glorious imagery of childhood, and the "perpetual benediction" of its memories, when he suddenly breaks off into—

"Not for these I raise
The song of thanks and praise,
But for those obstinate questionings
Of sense and outward things,
Fallings from us, vanishings," &c.

Why, it was asked, should any sane person be "obstinately" disposed to question the testimony of his senses, and be peculiarly thankful that he had the power to do so? What was meant by the "fallings off and vanishings," for which he raises his "song of thanks and praise"? The explanation is now to be found in a note by Wordsworth himself, prefixed to the ode in Knight's edition. Wordsworth there writes, "I was often unable to think of external things as having external existence, and I communed with all I saw as something not apart from, but inherent in, my own immaterial nature. Many times while going to school have I grasped at a wall or tree to recall myself from this abyss of idealism to the reality. At that time I was afraid of such processes. In later times I have deplored, as we all have reason to do, a subjugation of an opposite character, and have rejoiced over the remembrances, as is expressed in the lines 'Obstinate questionings,' &c."* He then gives those I have just quoted.

It is a remarkable coincidence that a closely similar idea is found in the verses of the successor of Wordsworth, namely, the great poet whose recent loss is mourned by all English-speaking nations, and that a closely similar explanation exists with respect to them. For in Lord Tennyson's "Holy Grail" the aged Sir Percivale, then a monk, recounts to a brother monk the following words of King Arthur:—

"Let visions of the night or of the day
Come, as they will; and many a time they come
Until this earth he walks on seems not earth,
This light that strikes his eyeball is not light,
The air that smites his forehead is not air,
But vision," &c.

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* Knight's edition of Wordsworth, vol. iv. p. 47.

Sir Percivale concludes just as Wordsworth's admirers formerly had done: "I knew not all he meant."

Now, in the *Nineteenth Century* of the present month Mr. Knowles, in his article entitled "Aspects of Tennyson," mentions a conversational incident curiously parallel to Wordsworth's own remarks about himself:—"He [Tennyson] said to me one day, 'Sometimes as I sit alone in this great room I get carried away, out of sense and body, and rapt into mere existence, till the accidental touch or movement of one of my own fingers is like a great shock and blow, and brings the body back with a terrible start.'"

Considering how often the imagination is sufficiently intense to mimic a real sensation, a vastly greater number of cases must exist in which it excites the physiological centres in too feeble a degree for their response to reach to the level of consciousness. So that if the imagination has been anyhow set into motion, it shall, as a rule, originate what may be termed *incomplete* sensations, and whenever one of these concurs with a real sensation of the same kind, it would swell its volume.

This supposition admits of being submitted to experiment by comparing the amount of stimulus required to produce a just-perceptible sensation, under the two conditions of the imagination being either excited or passive.

Several conditions have to be observed in designing suitable experiments. The imagined sensation and the real sensation must be of the same quality; an expected scream and an actual groan could not reinforce one another. Again, the place where the image is localised in the theatre of the imagination must be the same as it is in the real sensation. This condition requires to be more carefully regarded in respect to the visual imagination than to that of the other senses, because the theatre of the visual imagination is described by most persons, though not by all, as internal, whereas the theatre of actual vision is external. The important part played by points of reference in visual illusions is to be explained by the aid they afford in compelling the imaginary figures to externalise themselves, superimposing them on fragments of a reality. Then the visualisation and the actual vision fuse together in some parts, and supplement each other elsewhere.

The theatre of audition is by no means so purely external as that of sight. Certain persuasive tones of voice sink deeply, as it were, into the mind, and even simulate our own original sentiments. The power of localising external sounds, which is almost absent in those who are deaf with one ear, is very imperfect generally, otherwise the illusions of the ventriloquist would be impossible. There was an account in the newspapers a few weeks ago of an Austrian lady of rank who purchased a parrot at a high price, as being able to repeat the Paternoster in seven different languages. She took the bird home, but it was mute. At last it was discovered that the apparent performances of the parrot had been due to the ventriloquism of the dealer. An analogous trick upon the sight could not be performed by a conjuror. Thus he could never make his audience believe that the floor of the room was the ceiling.

As regards the other senses, the theatre of the imagination coincides fairly well with that of the sensations. It is so with taste and smell, also with touch, in so far that an imagined impression or pain is always located in some particular part of the body, then if it be localised in the same place as a real pain it must coalesce with it.

Finally, it is of high importance to success in experiments on Imagination that the object and its associated imagery should be so habitually connected that a critical attitude of the mind shall not easily separate them. Suppose an apparatus arranged to associate the waxing and waning of a light with the rising and falling of a sound, holding means in reserve for privately modifying the illumination at the will of the experimenter, in order that the waxing and waning may be lessened, abolished, or even reversed. It is quite possible that a person who had no idea of the purport of the experiment might be deceived, and be led by his imagination to declare that the light still waxed and waned in unison with the sound after its ups and downs had been reduced to zero. But if the subject of the experiment suspected its object, he would be thrown into a critical mood; his mind would stiffen itself, as it were, and he would be difficult to deceive.

Having made these preliminary remarks, I will mention one only of some experiments I have made and am making from time to time, to measure the force of my own imagination. It happens that although most persons train themselves from childhood upwards to distinguish imagination from fact, there is at least one instance in which we do the exact reverse, namely, in respect to the auditory presentation of the words that are perused by the eye. It would be otherwise impossible to realise the sonorous flow of the passages, whether in prose or poetry, that are read only with the eyes. We all of us value and cultivate this form of auditory imagination, and it commonly grows into a well-developed faculty. I infer that when we are listening to the words of a reader while our eyes are simultaneously perusing a copy of the book from which he is reading, that the effects of the auditory imagination concur with the actual sound, and produce a stronger impression than the latter alone would be able to make.

I have very frequently experimented on myself with success, with the view of analysing this concurrent impression into its constituents, being aided thereto by two helpful conditions, the one is a degree of deafness which prevents me when sitting on a seat in the middle rows from following memoirs that are read in tones suitable to the audience at large; and the other is the accident of belonging to societies in which unrevised copies of the memoirs that are about to be read, usually in a monotonous voice, are obtainable, in order to be perused simultaneously by the eye. Now it sometimes happens that portions of these papers, however valuable they may be in themselves, do not interest me, in which case it has been a never-flagging source of diversion to compare my capabilities of following the reader when I am using my eyes, and when I am not. The result depends somewhat on the quality of the voice; if it be a familiar tone I can imagine what is coming much more accurately than otherwise. It depends much on the phraseology, familiar words being vividly represented. Something also depends on the mood at the time, for imagination is powerfully affected by all forms of emotion. The result is that I frequently find myself in a position in which I hear every word distinctly so long as they accord with those I am perusing, but whenever a word is changed, although the change is perceived, the new word is not recognised. Then, should I raise my eyes from the copy, nothing whatever of the reading can be understood, the overtones by which words are distinguished being too faint to be heard. As a rule, I estimate that I have to approach the reader by about a quarter of the previous distance, before I can distinguish his words by the ear alone. Accepting this rough estimate for the purposes of present calculation, it follows that the potency of my hearing alone is to that of my hearing *plus* imagination as the loudness of the same overtones heard at 3 and at 4 units of distance respectively; that is as about 3^2 to 4^2, or as 9 to 16. Consequently the potency of my auditory imagination is to that of a just-perceptible sound as $16 - 9$ to 16, or as 7 units to 16. So the effect of the imagination in this case reaches nearly half-way to the level of consciousness. If it were a little more than twice as strong it would be able by itself to produce an effect indistinguishable from a real sound.

Two copies of the same newspaper afford easily accessible materials for making this experiment, a few words having been altered here and there in the copy to be read from.

I will conclude this portion of my remarks by suggesting that some of my audience should repeat these experiments on themselves. If they do so, I should be grateful if they would communicate to me their results.

Optical Continuity.—Keenness of sight is measured by the angular distance apart of two dots when they can only just be distinguished as two, and do not become confused together. It is usually reckoned that the normal eye is just able or just unable to distinguish points that lie one minute of a degree asunder. Now, one minute of a degree is the angle subtended by two points, separated by the 300th part of an inch, when they are viewed at the ordinary reading distance of one foot from the eye. If, then, a row of fine dots touching one another, each as small as a bead of one 300th part of an inch in diameter, be arranged on the page of a book, they would appear to the ordinary reader to be an almost invisibly fine and continuous line. If the dots be replaced by short cross strokes, the line would look broader, but its apparent continuity would not be affected. It is impossible to draw any line that shall commend itself to the eye as possessing more regularity than the image of a succession of dots or cross strokes, 300 to the inch, when viewed at the distance of a foot. Every design, however delicate, that can be drawn with a line of uniform thickness by the best machine or the most consummate artist, admits of being mimicked by the coarsest chain, when it is viewed at such a distance that the angular length of each of its links shall not exceed one minute of a degree. One of the apparently smoothest outlines in nature is that of the horizon of the sea during ordinary weather, although it is formed by waves. The slopes of *débris* down the sides of distant mountains appear to sweep in beautifully smooth curves, but on reaching those mountains and climbing up the *débris*, the path may be exceedingly rough.

The members of an audience sit at such various distances from the lecture table and screen that it is not possible to illustrate as well as is desirable the stages through which a row of dots appears to run into a continuous line, as the angular distance between the dots is lessened. I have, however, hung up chains and rows of beads of various degrees of coarseness. Some of these will appear as pure lines to all the audience; others, whose coarseness of structure is obvious to those who sit nearest, will seem to be pure lines when viewed from the farthest seats.

Although 300 dots to the inch are required to give the idea of perfect continuity at the distance of one foot, it will shortly be seen that a much smaller number suffices to suggest it.

The cyclostyle, which is an instrument used for multiple writing, makes about 140 dots to the inch. The style has a minute spur-wheel or roller, instead of a point; the writing is made on stencil paper, whose surface is covered with a brittle glaze. This is perforated by the teeth of the spur-wheel wherever they press against it. The half perforated sheet is then laid on writing paper, and an inked roller is worked over the glaze. The ink passes through the perforations and soaks through them on to the paper below; consequently the impression consists entirely of short and irregular cross bars or dots.

I exhibit on the screen a circular letter summoning a committee, that was written by the cyclostyle. The writing seems beautifully regular when the circular is photographically reduced; when it is enlarged, the discontinuity of the strokes becomes conspicuous. Thus, I have enlarged the word *the* six times; the dots can then be easily seen and counted. There are 42 of them in the long stroke of the letter *h*.

The appearance of the work done by the cyclostyle would be greatly improved if a fault in its mechanism could be removed, which causes it to run with very unequal freedom in different directions. It leaves an ugly, jagged mark wherever the direction of a line changes suddenly.

A much coarser representation of continuous lines is given by embroidery and tapestry, and coarser still by those obsolete school samplers which our ancestresses worked in their girlhood, with an average of about sixteen stitched dots to each letter. Perhaps the coarsest lettering, or rather figuring, that is ever practically employed is used in perforating the books of railway coupons so familiar to travellers. Ten or eleven holes are used for each figure.

A good test of the degree of approximation with which a cyclostyle making 140 perforations to the inch is able to simulate continuous lines, is to use it for drawing outline portraits. I asked the clerk who wrote the circular just exhibited to draw me a few profiles of different sizes, ranging from the smallest scale on which the cyclostyle could produce recognisable features, up to the scale at which it acted fairly well. I submit some specimens of the result. The largest is a portrait of $1\frac{1}{2}$ inches in height, by which facial characteristics are fairly well conveyed; somewhat better than by the rude prints that appear occasionally in the daily papers. It is formed by 366 dots. A medium size is $\frac{3}{4}$ inch high and contains 177 dots, and would be tolerable if it were not for the jagged strokes already spoken of. The smallest sizes are $\frac{1}{3}$ inch high and contain about 90 dots; they are barely passable, on account of the jagged flaws, even for the rudest portraiture.

I made experiments under fairer conditions than those of the cyclostyle, to learn how many dots, discs, or rings per inch were really needed to produce a satisfactory drawing, and also to discover how far the centres of the dots or discs might deviate from a strictly smooth curve without ceasing to produce the effect of a flowing line. It must be recollected that the eye can perceive nothing finer than a minute blur of one 300th part of an inch in angular diameter. If we represent a succession of such blurs by a chain of larger discs, it will be easily recognised that a small want of exactitude in the alignments of the successive discs must be unimportant. If one of them is pushed upwards a trifle and another downwards, so large a part of their respective areas still remains in line, that when the several discs become of only just perceptible magnitude, the projecting portion will be wholly invisible. When the discs are so large as to be plainly perceptible, the alignment has to be proportionately more exact. After a few trials it seemed that if the *bearing* of the centre of each disc from that of its predecessor which touched it, was correctly given to the nearest of the 16 principal points of the compass, N., NNE., NE., &c., it was fairly sufficient. Consequently a simple record of the successive bearings of each of a series of small equidistant steps is enough to define a curve.

The briefest way of writing down these bearings is to assign a separate letter of the alphabet to each of them, *a* for north (the top of the paper counting as north), *b* for north-north-east, *c* for north-east, and so on in order up to *p*. This makes *e* represent east, *i* south, and *m* west.

To test the efficiency of the plan, I enlarged one of the cylostyle profiles, and making a small protractor with a piece of tracing paper, rapidly laid down a series of equidistant points on the above principle, noting at the same time the bearing of each from its predecessor. I thereby obtained a formula for the profile, consisting of 271 letters. Then I put aside the drawing, and set to work to reproduce it solely from the formula. I exhibit the result; it is fairly successful. Emboldened by this first trial, I made a more ambitious attempt, by dealing with the profile of a Greek girl copied from a gem. I was very desirous of learning how far the pure outline of the original admitted of being mimicked in this rough way.

The result is here; a ring has been painted round each dot in order to make its position clearly seen, without obliterating it. The reproduction has been photographically reduced to various different sizes. That which contains only fifty dots to the inch, which is consequently six times as coarse as the theoretical 300 to an inch, is a very creditable production. Many persons to whom this portrait has been shown, failed to notice the difference between it and an ordinary woodcut. The medium size, and much more the smallest size, would deceive anybody who viewed them at the distance of one foot. The protractor used in making them was a square card with a piece cut out of its middle, over which transparent tracing paper was pasted. A small hole of about $\frac{1}{8}$ of an inch in diameter was punched out of the centre of the tracing paper; sixteen minute holes just large enough to allow the entry of the sharp point of a hard lead-pencil were perforated through the tracing paper in a circle round the centre of the hole at a radius of $\frac{1}{4}$ inch. They corresponded to the sixteen principal points of the compass, and had their appropriate letters written by their sides. The outline to be formulated was fixed to a drawing-board, with a T rule laid across it as a guide to the eye in keeping the protractor always parallel to itself. The centre of the small hole was then brought over the beginning of the outline, and a dot was made with the pencil through the perforation nearest to the further course of the outline, and this became the next point of departure. While moving the protractor from the old point to the new one it was stopped on the way, in order that the letter for the bearing might be written through the central hole. These were afterwards copied on a separate piece of paper.

A clear distinction must be made between the proposed plan and that of recording the angle made by each step from the *preceding one.* In the latter case, any error of bearing would falsify the direction of all that followed, like a bend in a wire.

The difficulties of dealing with detached portions of the drawing, such as the eye, were easily surmounted by employing two of the spare letters, R and S, to indicate brackets, and other spare letters to indicate points of reference. The bearings included between an R and an S were taken to signify directive dots, not to be inked in. The points of reference indicated by other letters are those to which the previous bearing leads, and from which the next bearing departs. Here is the formula whence the *eye* was drawn. It includes a very small part of the profile of the brow, and the directive dots leading thence to the eye.

The letters should be read from the left to the right, across the vertical lines. They are broken into groups of five, merely for avoiding confusion and for the convenience of after reference.

The part of the Profile that includes U

&c. iiiilU jiihi &c. &c.

The Eye.

URkkk	kklll	mSVap	ponmn	mmlmm
mlmlm	llmZZ	VnTnn	mnmmm	mmmlm
mmnZZ	Tjjjj	jjkkc	chmmn	mnun
onooZ				

Letters used as Symbols.

R....S = (....). Z = end.
U, V, T are points of reference.

By succeeding in so severe a test case as this Greek outline, it may be justly inferred that rougher designs can be easily dealt with in the same way.

At first sight it may seem to be a silly waste of time and trouble to translate a drawing into a formula, and then, working backwards, to retranslate the formula into a reproduction of the original drawing, but further reflection shows that the process may be of much practical utility. Let us bear two facts in mind, the one is that a very large quantity of telegraphic information is daily published in the papers, anticipating the post by many days or weeks. The other is that pictorial illustrations of current events, of a rude kind, but acceptable to the reader, appear from time to time in the daily papers. We may be sure that the quantity of telegraphic intelligence will steadily increase, and that the art of newspaper illustration will improve and be more resorted to. Important local events frequently occur in far-off regions, of which no description can give an exact idea without the help of pictorial illustration; some catastrophe, or site of a battle, or an exploration, or it may be some design or even some portrait. There is therefore reason to expect a demand for such drawings as these by telegraph, if their expense does not render it impracticable to have them. Let us then go into details of expense, on the basis of the present tariff from America to this country, of one shilling per word, 5 figures counting as one word, cypher letters not being sent at a corresponding rate. It requires two figures to perform each of the operations described above, which were performed by a single letter. So a formula for 5 dots would require 10 figures, which is the telegraphic equivalent of 2 words; therefore the cost for every 5 dots telegraphed from the United States would be 2 shillings, or 2*l.* for every 100 dots or other indications.

In the Greek outline there is a total of 400 indications, including those for directive dots, and for points of reference. The transmission of these to us from the United States would cost 8*l.* I exhibit a map of England made with 248 dots, as a specimen of the amount of work in plans, which could be effected at the cost of 5*l.* It is easy to arrange counters into various patterns or parts of patterns, learning thereby the real power of the process. The expense of pictorial telegraphs to foreign countries would be large in itself, but not large relatively to the present great expenditure by newspapers on telegraphic information, so the process might be expected to be employed whenever it was of obvious utility.

The risk is small of errors of importance arising from mistakes in telegraphy. I inquired into the experience of the Meteorological Office, whose numerous weather telegrams are wholly conveyed by numerical signals. Of the 20,625 figures that were telegraphed this year to the office from continental stations, only 49 seem to have been erroneous, that is two and a third per thousand. At this rate the 800 figures needed to telegraph the Greek profile would have been liable to two mistakes. A mistake in a figure would have exactly the same effect on the outline as a rent in the paper on which a similar outline had been drawn, which had not been pasted together again with perfect precision. The dislocation thereby occasioned would never exceed the thickness of the outline.

The command of 100 figures from 0 to 99, instead of only 26 letters, puts 74 fresh signals at our disposal, which would enable us to use all the 32 points of the compass, instead of 16, and to deal with long lines and curves. I cannot enter into this now, nor into the control of the general accuracy of the picture by means of the distances between the points of triangles each formed by any three points of reference. Neither need I speak of better forms of protractor. There is one on the table by which the ghost of a compass card is thrown on the drawing. It is made of a doubly refracting image of Iceland spar, which throws the so-called "extraordinary" image of the compass card on to the ordinary image of the drawing, and is easy to manipulate. All that I wish now to explain is that this peculiar application of the law of the just-perceptible difference to optical continuity gives us a new power that has practical bearings.

Postscript.—A promising method for practical purposes that I have tried, is to use "sectional" paper; that is, paper ruled into very small squares, or else coarse cloth, and either to make the drawing upon it, or else to lay transparent sectional paper or muslin over the drawing. Dots are to be made at distances not exceeding three spaces apart, along the course of the outline, at those intersections of the ruled lines (or threads) that best accord with the outline. Each dot in succession is to be considered as the *central point*, numbered **44** in the following

11	21	31	41	51	61	71
12	22	32	42	52	62	72
13	23	33	43	53	63	73
14	24	34	**44**	54	64	74
15	25	35	45	55	65	75
16	26	36	46	56	66	76
17	27	37	47	57	67	77

schedule, and the couplet of figures corresponding to the portion of the next dot is to be written with a fine-pointed pencil in the interval between the two dots. These are subsequently copied, and make the formula. By employing 4 for zero, the signs + and − are avoided; 3 standing for −1, 2 for −2, and 1 for −3. The first figure in each couplet defines its horizontal coordinate from zero; the second figure, its vertical one. Thus any one of 49 different points are indicated, corresponding to steps from zero of 0, ±1, ±2, and ±3 intervals, in either direction, horizontal or vertical. Half an hour's practice suffices to learn the numbers. The figures 0, 8, and 9 do not enter into any of the couplets in the schedule, the remaining 51 couplets in the complete series of 100 (ranging from 00 to 99), contain 21 cases in which 0, 8, or 9 forms the first figure only; 21 cases in which one of them forms the second figure only; and 9 cases in which both of the figures are formed by one or other of them. These latter are especially distinctive. This method has five merits—medium, short, or very short steps can be taken according to the character of the lineation at any point; there is no trouble about orientation; the bearings are defined without a protractor, the work can be easily revised, and the correctness of the records may be checked by comparing the sums of the successive small co-ordinates leading to a point of reference, with their total value as read off directly.

A method of signalling is also in use for military purposes, in which positions are fixed by co-ordinates, afterwards to be connected by lines.

[F. G.]

WEEKLY EVENING MEETING,

Friday, February 3, 1893.

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

Alexander Siemens, Esq. M.Inst.C.E. *M.R.I.*

Theory and Practice in Electrical Science.

(With Experimental Illustrations.)

When I was requested to give a Friday evening discourse at this Institution, I felt very much honoured at having an opportunity of speaking to an audience that has listened to so many illustrious men of science. At the same time I felt that instead of selecting a purely scientific subject I should be more likely to interest you if I drew your attention to some illustrations of the way in which science is applied to practice.

The tendency of our century, and especially of the latter half, has been to obliterate ancient distinctions, and to break down barriers which formerly were held to be insurmountable. In this respect I need only remind you that at one time, in chemistry, substances were divided into acids and bases, into metals and metalloids, and that until very lately in physics, some gases were classed by themselves as being permanent, and so on. All of these distinctions have been found untenable in the light of modern research, and in a similar manner the strict divisions maintained for a long time between different branches of science have been more and more abolished, so that nowadays anybody who wishes to excel in any one branch of science ought to possess solid knowledge of the principles of all the others.

One of the most important barriers broken down by the spirit of our times is that formerly held up between science and practice, and the state of mind in which a learned professor once exclaimed about his own particular branch of science: "Thank goodness, there is no practical application of it possible," is more and more forgotten. Instead of that, endeavours are now made on all sides to turn to practical account all scientific investigations.

While quite admitting that it would give much cause for regret if this tendency were developed too far, so as to interfere with the progress of purely scientific researches, it cannot be denied that the application of scientific principles has brought about that immense progress which is characteristic of the last half-century. A conspicuous example of the influence of applied science is furnished by the way the use of electricity has been introduced into our daily life, and several causes have contributed to facilitate the scientific treatment of electrical problems. Not the least among these is the circumstance that at first electricity could not be produced at a cheap rate for general commercial uses. Thus it came about that telegraphy, for which weak currents are sufficient, was for a long time the only practical application, and during this period of comparative quiet a number of the most eminent scientific philosophers devoted their time to discover the characteristic features of this great power in nature, and the laws which it obeys. The consequence has been that at the time when the discovery of the dynamo-electric principle made cheap electricity a possible commodity, the laws on which electric currents act were thoroughly understood, and the development of the introduction of electrical appliances could take place on the firm basis of scientific knowledge.

The obligations that electrical engineers owe to science they have acknowledged in a practical manner in naming the units by which electricity is measured after the learned men who created the science of electricity.

The circumstance that it is possible to reproduce perfectly the exact conditions for which electrical apparatus have been designed, has much facilitated the direct application of laboratory experiments to practical problems. It is, for instance, quite feasible to take a small quantity of ore, to subject it in a laboratory to chemical and electrical treatment, and to judge from the results whether it will be possible to design works for the treatment of such ores in large quantities on the same lines. As far as electricity is concerned it is possible in such cases to predict with absolute accuracy how much energy is wanted in each case to deposit a given quantity of metal in a given time. The electrical engineer is thus enabled to arrive, in a comparatively easy and inexpensive manner, at reliable data, which in other branches of applied science have to be obtained by costly experiments on a large scale.

One of the most striking instances of the direct application of scientific researches to practical purposes has been furnished by Dr. John Hopkinson, who explained in his lecture before the Institution of Civil Engineers in the year 1883, how he had been led by mathematical considerations to infer that alternate-current machines could be run in parallel, and what conditions were necessary to secure success. His conclusions were tried shortly afterwards at the South Foreland Lighthouse, and have proved since to be of the utmost value for central electric lighting stations on the alternate-current system. For the sake of historical accuracy I should mention, perhaps, that Dr. John Hopkinson called attention in the following year to a communication to the Royal Society by Mr. Wilde, who had previously demonstrated the possibility of working alternators in parallel; yet the facts just related are an apt illustration of the point I desired to lay before you.

While science is a safe guide for the engineer, and will warn him of the mistakes and fallacies which ought to be avoided, there are sometimes other considerations which will modify to an important degree conclusions based on scientific principles alone. As an example, the case of heating by electricity may be cited. The scien-

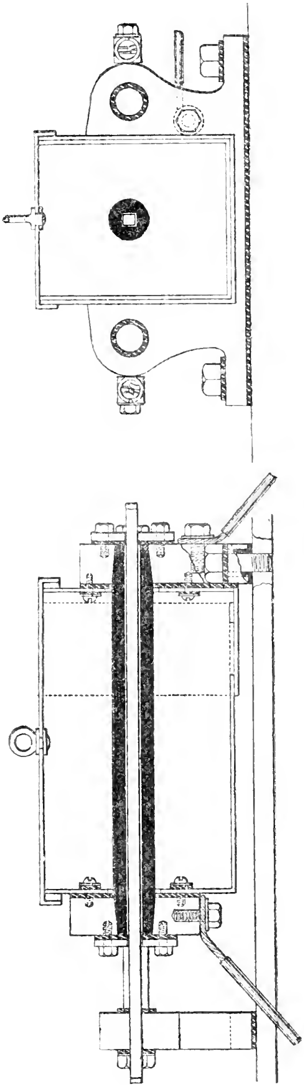


Diagram 1.

Nail-rod Heating.

tific data in connection with this problem are as follows: A kilogram of coal burnt to best advantage will give 8080 calories. The same amount of coal consumed in a boiler will produce steam sufficient for 1 H.P. for 1 hour, and this horse-power can generate electricity at the rate of 660 watts, or about 570 calories, per hour.

Assuming that in heating by burning coal, only a quarter of the theoretical effect is attained, and taking the price of coal at 20*s.* per ton, while the cost of a Board of Trade unit of electricity is 8*d.*, it would appear that a farthingsworth of coal will produce as much heat as 22*d.* worth of electricity.

These figures apply to the conditions of life in London, where fuel is abundant and power comparatively expensive; elsewhere, in Norway for instance, fuel may be expensive, and power, in the shape of waterfalls, cheap. Under such altered conditions electricity may with advantage be employed for heating purposes, by producing it with the aid of water-power, and utilising the heat generated by it, in special appliances.

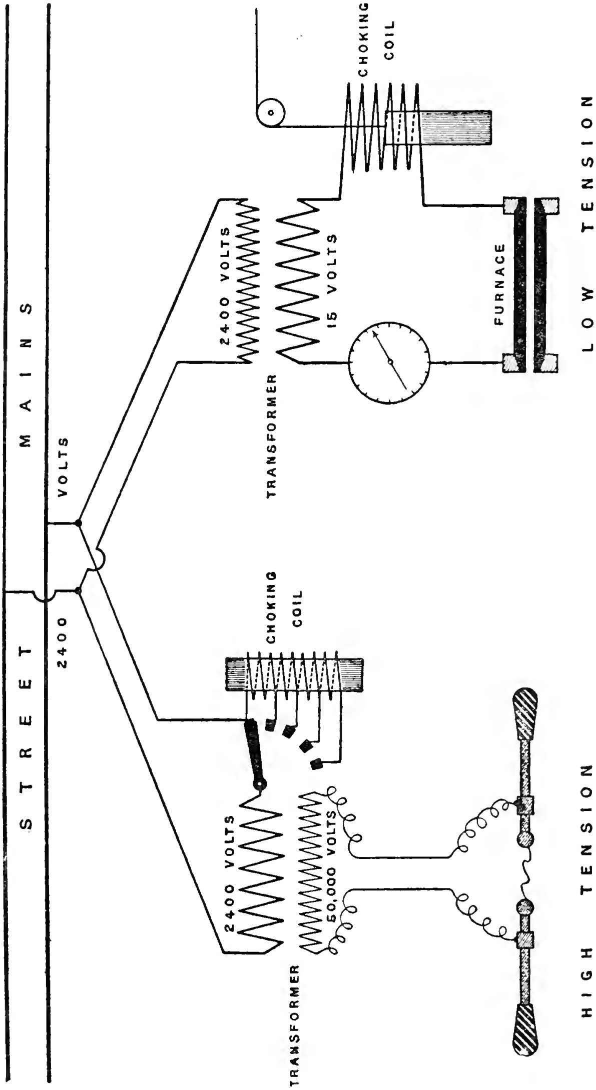
One of the most important industries of Norway is the making of horseshoe nails, for which special machines have been constructed, into which a heated rod of iron has to be fed. For this purpose the rod is passed through a charcoal fire, placed close to the nail-making machine, and a great deal of difficulty is experienced in maintaining the rod at an even and suitable temperature. The apparatus placed in front of you is designed to replace these charcoal fires, and its construction is shown by the diagram on the wall. The essential part of it is a hollow carbon, through which a current of electricity is sent, heating the carbon to any desired temperature. In this apparatus, a current of 400 amperes and 5 volts is used, equal to 2 Board of Trade units per hour, which is supplied from a transformer, the primary circuit of which is connected to the high-pressure mains of the London Electric Supply Company. A diagram of the connections shows that the two wires connected to the supply main are led to a commutator on the table, by which the current can either be sent to the transformer of the heating apparatus, or to another one, which will be mentioned later on. In order to prevent loss of heat by radiation, the carbon is placed in a box filled with sand, and the necessary precautions are taken to let the current pass through the carbon only. After the carbon has become white hot, a rod of iron, in passing through it, is rapidly heated, and the temperature it attains depends on the speed at which it is fed forward.

It would have been very inconvenient to bring a nail-making machine here. With your permission, I will therefore ask Mr. Williamson, who designed the apparatus, to show us how to make spiral steel springs.

By the side of the nail-rod heater stands a similar apparatus for the heating of rivets, which is also illustrated by a diagram, and will be shown in action.

It is obvious that such an apparatus can be used in many places where a coal-fire would be dangerous, and that, considering the waste of fuel in the usual rivet-heating, it probably will be more economical in cost, especially where electric lighting plant is in use.

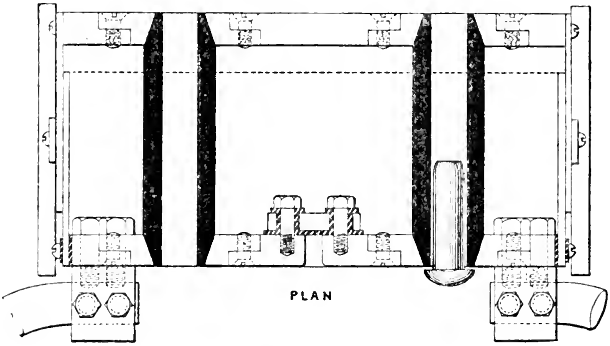
Diagram 2.



Connections in Lecture Hall, &c.

The ingenious way in which Mr. Crompton has utilised the heating effect of electric currents for cooking purposes has no doubt been admired by most of you at the Crystal Palace Exhibition last year; and when we remember that these cooking utensils consume fuel only during the time they are actually in use, and that they can be put in and out of action at a moment's notice, we cannot doubt that these and many other obvious advantages will facilitate their introduction in spite of the figures, as to cost, given by the scientific data.

Diagram 3.



Rivet-heating.

Of late the transmission of power by electricity has occupied a very prominent place in the public interest, and the project of utilising the force of the Niagara Falls at distant towns is as closely discussed as the plan of constructing long railways on which trains are to run at fabulous speeds.

As you will hear a discourse on electric railways in three weeks from to-day, I will not take up your time with this branch of the subject, but will rather draw your attention to the distribution of power by electricity from a central generating station. Before entering further into this, let me remind you that the earliest magneto-electric machines were used nearly sixty years ago for the production of power. I will mention only Jacobi's electric launch of 1835 as an example; it must, therefore, be considered altogether erroneous to ascribe the invention of the transmission of power to an accident at the Vienna Exhibition in 1873, when, it is said, an attendant placed some stray wires into the terminals of a dynamo-machine; it began to turn, and the transmission of power was first demonstrated.

As a matter of fact, Sir William Siemens once informed me that his brother Werner was led to the discovery of the dynamo-electric principle by the consideration that an electro-magnetic machine behaved like a magneto-electric machine, when a current of electricity was sent into it, viz. both turn round and give out power. It was, of course, well known that a magneto-electric machine produces a current of electricity when turned by mechanical power, and Werner concluded that an electro-magnetic machine would behave in the same manner. We all know that he was right, but I relate this circumstance only as a further proof that the generation of power by electric currents had been a well-known fact long previous to the Vienna Exhibition.

Another well-known instance of transmission of power to a distance is furnished by the magneto-electric A B C telegraph instruments, where the motion at the sending end supplies the currents necessary to move the indicator at the receiving station.

As an illustration of the distribution of power by electricity I will briefly describe some radical alterations that have been made at the works of Messrs. Siemens Brothers and Co. by the introduction of electric motors in the place of steam engines. The diagram on the wall shows in outline the various buildings in which work of different kinds is carried on with the help of different machines. Electric motors are supplying the power, sometimes by driving shafting to which a group of tools is connected by belting, and sometimes by being coupled direct to the moving mechanism. Each section of the works has its own meter measuring the energy that is used there, and all of them are connected by underground cables to a central station, where three sets of engines and dynamos generate the electric current for all purposes.

There are two Willans and one Belliss steam engines, each of 300 I.H.P., coupled direct to the dynamos and running at a speed of 350 revolutions per minute.

Room is left for a fourth set; but, including some auxiliary pumps and the switchboards for controlling the dynamos and for distributing the current, the whole space occupied by 1200 horse-power measures only 32 by 42 feet.

Close by are the condensers and three high-pressure boilers, which have replaced some low-pressure ones formerly used for some steam engines driving the machinery in the nearest building.

The advantages that have been secured by the introduction of electric motors may be briefly stated under the following heads:—

1. Various valuable spaces formerly occupied by steam engines and boilers have been made available for the extension of workshops, and these are indicated on the diagram by shading.

2. By abolishing to a great extent the mechanical transmission of power a considerable saving is effected in motive power, which is especially noticeable at times when part only of the machinery is in use.

3. As the electric motors take only as much current as is actually required for the work they are doing, a further saving is effected and

at the same time the facility with which the speed of the motors can be altered without their interfering with each other, presents a feature that is absent from mechanical transmission.

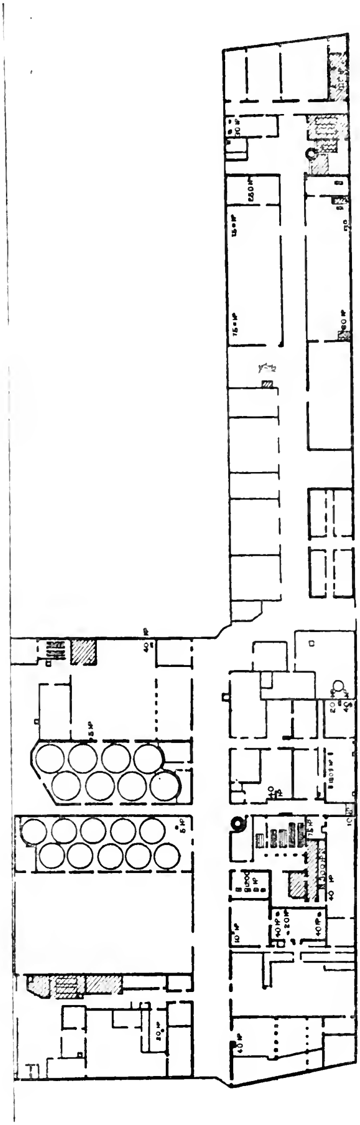


Diagram 4.

Plan of Works.

4. The big steam engines, being compound and condensing, produce a horse-power with a smaller consumption of fuel than the small high-pressure steam engines scattered throughout the works.

5. The numerous attendants of the old steam engines and boilers have mostly been transferred to other work; only a few of them are required at the central station, and one or two men can easily look after all the electric motors used in the various parts of the works.

Elsewhere, equally favourable results have been obtained by the introduction of electrical distribution of power, and in this respect I beg to refer you to a paper read before the German Institution of Civil Engineers by Mr. E. Hartmann in April of last year, and to a paper read by Mr. Castermans before the Society of Engineers in Liège in August last, in which he compares in detail various methods of transmission of power, of which the electrical one was adopted for a new small arms factory.

We may, therefore, take it for granted that the advantages alluded to above have not resulted from local circumstances at Woolwich, but that they can be realised anywhere by the adoption of the electric current for distributing power from a central station. At first sight this result appears to be of interest only to the manufacturer, but the development of this idea may lead to far-reaching consequences when we consider that cheap power is one of the most important requisites for cheap production. You can see on the diagram that the various buildings are separated by roads, and we can easily imagine that in each of them an independent owner carries on work, so that the diagram represents part of a manufacturing town.

While power was generated by steam engines, the cost of producing one horse-power varied a good deal in the different parts, and the various owners could not have obtained their power on equal terms, those possessing the largest steam engines having a distinct advantage. This inequality is done away with altogether when the power is distributed by electricity, as the current can be supplied for large or small powers at the same rate per Board of Trade unit. It is therefore clear that the establishment of central stations for the generation of electricity on a large scale will bring about the possibility of small works competing with large works in quite a number of trades, where cheap power is of the first consideration.

Another circumstance favouring small works is the diminution of capital outlay brought about by the employment of electric motors. Not only are the motors cheaper than boilers and steam-engines of corresponding power would be, but the outlay for belting and shafts is saved, and the structure of the building need not be as substantial as is necessary where belts and shafting have to be supported by it.

A commencement has already been made in this direction by the starting of electric light stations, where the owners do all in their power to encourage the use of the current in motors in order to keep the machinery at their central station more uniformly at work.

The introduction of electricity as motive power will apparently present a strong contrast to the effect steam has had on the development of industries for the reasons already stated; and, in addition, there are many cases where the erection of boilers and steam engines, or even of gas engines, would be inadmissible on account of want of space or of the nuisances that are inseparable from them. Motive power will, therefore, be available in a number of instances where up to the present time no mechanical power could be used, but the work had to be done by manual labour or not at all.

You may have noticed that I have confined my remarks hitherto to the case of distributing electricity over a limited area, but that I have not yet discussed the question of transmitting power to a great distance.

Theoretically we have been told over and over again that the motive power of the future will be supplied by waterfalls, and that their power can be made available over large areas by means of electric currents. As a prominent example, the installation is constantly mentioned by which the power of a turbine at Lauffen was transmitted over a distance of 110 statute miles to the Frankfurt Exhibition with an efficiency of 75 per cent. No doubt this result is very gratifying from a purely scientific point of view, but, unfortunately, in practical life only commercially successful applications of science will have a lasting influence, and in this respect the Lauffen installation left much to be desired.

On the one hand science tells us that the section of the conductor can be diminished as the pressure of electricity is increased, and it appears to be only necessary to construct apparatus for generating electricity at a sufficiently high pressure so as to reduce the cost of a long conductor to reasonable limits. On the other hand, experience shows that at these high potentials the insulation of the electric current becomes a most difficult problem, and for practical purposes difficulty means an increased outlay of money. As an illustration of the difficulties encountered in the employment of high-tension currents, I can demonstrate to you that many of the insulating materials employed with success for low-pressure currents break down under the strain of high-pressure electricity.

For the purpose of these experiments the current of electricity delivered by the street main at a pressure of 2400 volts is diverted to a large transformer placed on the ground-floor, and from there it is led through a twin cable to this room at a pressure which can be increased up to 50,000 volts. This twin cable was used in 1891 at the Frankfurt Exhibition, for conveying a current of 20,000 volts from the main Exhibition to the Exhibition on the Main, and when it was returned to the works, it was found that the insulation was as good as when it was first manufactured. A sample of it lies on the table, and by its side the sample of a concentric cable designed for a current of 2500 volts. A comparison of the two shows in a striking manner how elaborately high-tension cables have to be insulated.

By the first experiment I will try to show you how the space surrounding a conductor connected to a source of high-pressure electricity is, so to speak, filled with electric stresses, that become visible when a vacuum tube is brought near the conductor. This experiment

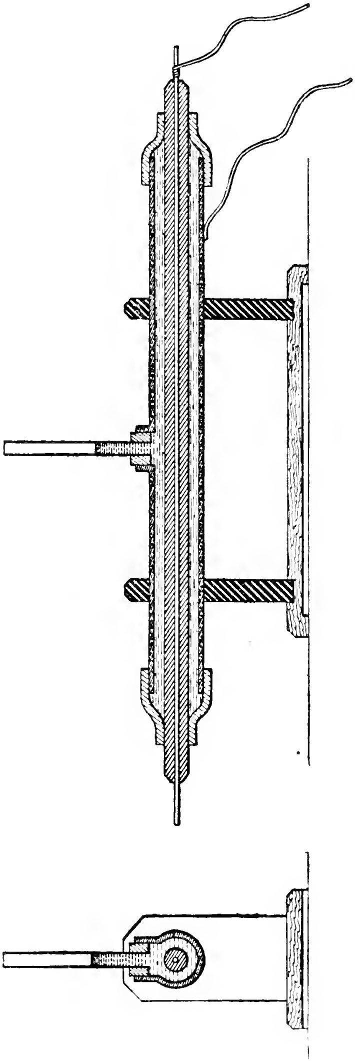


Diagram 5.

Testing of Dielectric.

was shown here by Nikola Tesla in connection with his lecture on alternate currents of high frequency; but I want to show you that high tension and low frequency produce the same effect.

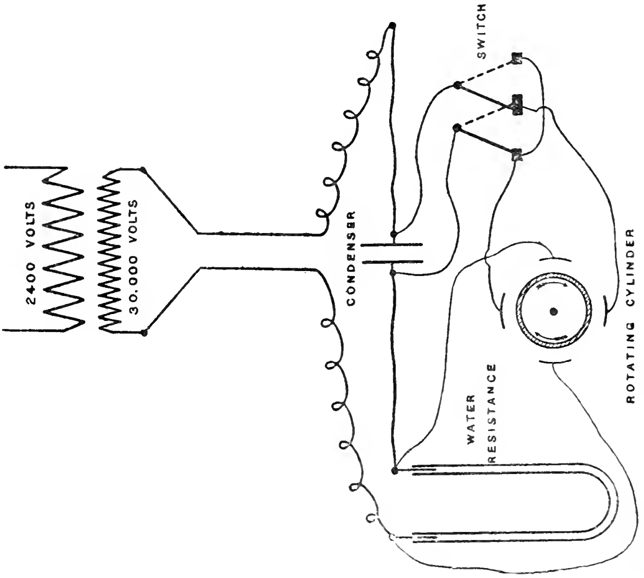
The next experiment was suggested by Dr. Obach, and the apparatus employed in it is shown in Diagram 5. A copper conductor (thickly insulated with indiarubber) is placed in a brass tube, and the annular space between them is filled with coloured water which communicates with a vertical glass tube inserted in the centre of the horizontal brass tube. One conductor from the high-tension transformer is connected to the insulated copper conductor, and the other to the brass tube. Under these conditions no current passes, but the electric stress heats the insulating material, which shows itself by the rise of the coloured liquid in the glass tube.

Werner Siemens called attention to this phenomenon in a paper contributed to Poggendorf's Annalen in 1857, in which he communicated a series of experiments on electrostatic induction, proving, as he expressly stated, the correctness of Faraday's theory of molecular induction.

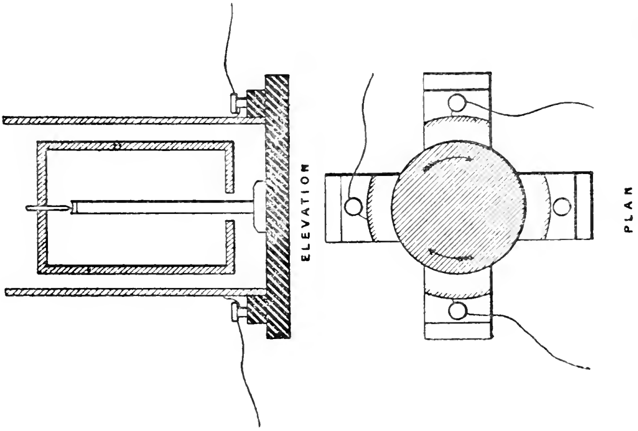
Not very long ago Signor Riccardo Arno showed that a cylinder of insulating material, brought under the influence of a rotary field and suitably suspended, would commence to revolve, thus showing that molecular movement was set up in it. He produced this effect by means of an apparatus, a copy of which you see before you. A hollow cylinder of gutta-percha is suspended on the point of a needle, so that it can be made to turn with very little friction. Around it are placed four vertical metal strips to which the high-tension current is brought, as shown in Diagram 6. Between the two terminals of the high-tension circuit, a connection is made by an inductionless resistance in the shape of a U-tube filled with water and a condenser. Two of the metal strips opposite each other are joined to the ends of the inductionless resistance and the other two strips are connected to the condenser. In this way there is a difference of a quarter of a phase between the two currents, and a rotating field is produced, which causes the cylinder to revolve on account of the electrical hysteresis set up. When the current is reversed the cylinder revolves in the opposite direction. This result also obtains in the case of a glass beaker which is inverted and supported by a pin-point.

Wood, slate, and marble are usually reckoned to be insulating materials, but you will see that they do not offer a protracted resistance to a current of high potential. When the electric spark passes through marble it converts the carbonate of lime into quicklime, as can readily be shown by moistening the broken surface with phenolphthalein, which leaves the carbonate of lime white and colours the quicklime a beautiful pink colour. Even glass is pierced; and we must confess that at present we have no very reliable means of dealing with electricity of very high pressure.

Enough has been shown, however, to prove that by utilising electricity we can extend the employment of natural forces for procuring the necessaries of life; and our experience shows that every time this has been done in the past, the burden of manual



Connections for Rotation of Dielectric.



labour has been lightened, and the comforts of mind and body have been made more accessible to the toiling multitude. In one word, all real and lasting progress is based on the practical application of scientific knowledge.

[A. S.]

GENERAL MONTHLY MEETING,

Monday, February 6, 1893.

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

Frederick Canton, Esq. M.R.C.S.
William Rolle Malcolm, Esq.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned for the following Donations:—

Mrs. Bloomfield Moore	£10
Robert Wilson, Esq.	50
John Bell Sedgwick, Esq.	50

for carrying on investigations on Liquid Oxygen.

The Managers reported, that they had reappointed Professor James Dewar, M.A. L.L.D. F.R.S. as Fullerian Professor of Chemistry.

The following Resolution from the Managers was read:—

Hodgkin's Trust.

"Having regard to the fact that the work of the Institution is devoted to the attainment of truth, and thereby constitutes in itself an investigation of the relations and co-relations existing between man and his Creator,"

Resolved, "That the income of the fund be devoted to that work, and that once in seven years a sum not exceeding 100 guineas be paid to some person, to be selected by the Managers, for writing an Essay showing how the work of this Institution has during the preceding period of seven years furthered the objects of the Trust."

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

FROM

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WEEKLY EVENING MEETING,

Friday, February 10, 1893.

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

Professor Charles Stewart, M.R.C.S. Pres. L.S.

Some Associated Organisms.

[No Abstract.]

WEEKLY EVENING MEETING,

Friday, February 17, 1893.

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

Professor A. H. Church, M.A. F.R.S. *M.R.I.*

Turacin, a remarkable Animal Pigment containing Copper.

The study of natural colouring matters is at once peculiarly fascinating and peculiarly difficult. The nature of the colouring matters in animals and plants, and even in some minerals (ruby, sapphire, emerald and amethyst, for example) is still, in the majority of cases, not completely fathomed.

Animal pigments are generally less easily extracted and are more complex than those of plants. They appear invariably to contain nitrogen—an observation in accord with the comparative richness in that element of animal cells and their contents. Then, too, much of the coloration of animals, being due to microscopic structure, and therefore having a mechanical and not a pigmentary origin, differs essentially from the coloration of plants. Those animal colours which are primarily due to structure do, however, involve the presence of a dark pigment—brown or black—which acts at once as a foil and as an absorbent of those incident rays which are not reflected.

Many spectroscopic examinations of animal pigments have been made. Except in the case of blood- and bile-pigments, very few have been submitted to exhaustive chemical study. Spectral analysis, when uncontrolled by chemical, and when the influence of the solvent employed is not taken into account, is very likely to mislead the investigator. And, unfortunately, the non-crystalline character of many animal pigments, and the difficulty of purifying them by means of the formation of salts and of separations by the use of appropriate solvents, oppose serious obstacles to elucidation. Of blood-red or hæmoglobin it cannot be said that we know the centesimal composition, much less the molecular weight. Even of hæmatin the empirical formula has not yet been firmly established. The group of black and brown pigments to which the various melanins belong still awaits adequate investigation. We know they contain nitrogen ($8\frac{1}{2}$ to 13 per cent.), and sometimes iron, but the analytical results do not warrant the suggestion of empirical formulæ for them. The more nearly they appear to approach purity the freer the majority of them seem from any fixed constituent such as iron or other metal. It is to be regretted that Dr. Krukenberg, to whom we are indebted for much valuable work on several pigments extracted from feathers, has not submitted the interesting substances he has described to quantitative chemical analysis.

I must not, however, dwell further upon these preliminary matters. I have introduced them mainly in order to indicate how little precise information has yet been gathered as to the constitution of the greater number of animal pigments, and how difficult is their study.

And now let me draw your attention to a pigment which I had the good fortune to discover, and to the investigation of which I have devoted I am afraid to say how many years.

It was so long ago as the year 1866 that the solubility in water of the red colouring matter in the wing-feathers of a *plantain-eater* was pointed out to me. [One of these feathers, freed from grease, was shown to yield its pigment to pure water.] I soon found that alkaline liquids were more effective solvents than pure water, and that the pigment could be precipitated from its solution by the addition of an acid. [The pigment was extracted from a feather by very dilute ammonia, and then precipitated by adding excess of hydrochloric acid.] The next step was to filter off the separated colouring matter, and to wash and dry it. The processes of washing and drying are tedious and cannot be shown in a lecture. But the product obtained was a solid of a dark crimson hue, non-crystalline, and having a purple semi-metallic lustre. I named it *turacin* (in a paper published in a now long-defunct periodical 'The Student and Intellectual Observer,' of April, 1868). The name was taken from "*Touraco*," the appellation by which the *plantain-eaters* are known—the most extensive genus of this family of birds being *Turacus*.

From the striking resemblance between the colour of arterial blood and that of the red touraco feathers, I was led to compare their spectra. Two similar absorption bands were present in both cases, but their positions and intensities differed somewhat. Naturally I sought for iron in my new pigment. I burnt a portion, dissolved the ash in hydrochloric acid, and then added sodium acetate and potassium ferrocyanide. To my astonishment I got a precipitate, not of Prussian blue, but of Prussian brown. This indication of the presence of copper in turacin was confirmed by many tests, the metal itself being also obtained by electrolysis. It was obvious that the proportion of copper present in the pigment was very considerable—greatly in excess of that of the iron (less than ·5 per cent.) in the pigment of blood.

Thus far two striking peculiarities of the pigment had been revealed, namely, its easy removal from the web of the feather, and the presence in it of a notable quantity of copper. Both facts remain unique in the history of animal pigments. The solubility was readily admitted on all hands, not so the presence of copper. It was suggested that it was derived from the Bunsen burner used in the incineration, or from some preservative solution applied to the bird-skins. And it was asked "How did the copper get into the feathers?" The doubters might have satisfied themselves as to copper being normally and invariably present by applying a few easy tests and by the expenditure of half-a-crown in acquiring a touraco wing. My results were, however, confirmed (in 1872) by several independent observers, including Mr. W. Crookes, Dr. Gladstone, and Mr. Greville Williams. And in 1873 Mr. Henry Bassett, at the request of the late Mr. J. J. Monteiro, pushed the inquiry somewhat further. I quote from Monteiro's 'Angola and the River Congo,' published in 1875 (vol. ii. pp. 75–77). "I purchased a large bunch of the red wing-feathers in the market at Sierra Leone, with which Mr. H. Bassett has verified Professor Church's results conclusively," &c., &c. Mr. Bassett's results were published in the *Chemical News* in 1873, three years after the appearance of my research in the Phil. Trans. As concentrated hydrochloric acid removes no copper from turacin, even on boiling, the metal present could not have been a mere casual impurity; as the proportion is constant in the turacin obtained from different species of touraco, the existence of a single definite compound is indicated. The presence of traces of copper in a very large number of plants as well as of animals has been incontestably established. And, as I pointed out in 1868, copper can be readily detected in the ash of banana fruits, the favourite food of several species of the "turacin-bearers." The feathers of a single bird contain on the average two grains of turacin, corresponding to ·14 of a grain of metallic copper; or, putting the amount of pigment present at its highest, just one-fifth of a grain. This is not a large amount to be furnished by its food to one of these birds once annually during the season of renewal of its feathers. I am bound, however, to say that in the blood and tissues of one of these birds, which I analysed immediately after death, I could not detect more than faint traces of copper. The particular specimen examined was in full plumage; I conclude that the copper in its food, not being then wanted, was not assimilated.

Let us now look a little more closely at these curious birds themselves. Their nearest allies are the cuckoos, with which they were formerly united by systematists. It has, however, been long conceded that they constitute a family of equal rank with the Cuculidæ. According to the classification adopted in the Natural History Museum, the order Picariæ contains eight sub-orders, the last of which, the Coccyges, consists of two families, the Cuculidæ and the Musophagidæ. To the same order belong the Hoopoes, the Trogons, the Wood-peckers. The plantain-eaters or Musophagidæ are arranged in six genera and comprise 25 species. In three genera—Turacus, Gallirex, and Musophaga—comprising eighteen species, and following one another in zoological sequence, turacin occurs; from three genera (seven species)—Corythæola, Schizorhis, and Gymnoschizorhis—the pigment is absent. [The coloured illustrations to H. Schlegel's Monograph (Amsterdam, 1860) on the Musophagidæ were exhibited]. The family is confined to Africa: 8 of the turacin-bearers are found in the west sub-region, 1 in the south-west, 2 in the south, 2 in the south-east, 4 in the east, 2 in the central, and 2 in the north-east. It is noteworthy that, in all these sub-regions save the south-east, turacin-bearers are found along with those plantain-eaters which do not contain the pigment. Oddly enough two of the latter species, *Schizorhis africana* and *S. zonura*, possess white patches destitute of pigment in those parts of the feathers which in the turacin-bearers are crimson. These birds do not, I will not say cannot, decorate these bare patches with this curiously complex pigment. [Some extracts were here given from the late Mr. Monteiro's book on Angola, vol. ii. pp. 74–79, and from letters by Dr. B. Hinde. These extracts contained references to curious traits of the touracos.]

Usually from 12 to 18 of the primaries or metacarpo-digitals and secondaries or cubitals amongst the wing feathers of the turacin-bearers have the crimson patches in their web. Occasionally the crimson patches are limited to six or seven of the eleven primaries. I have observed this particularly with the violet plantain-eater (*Musophaga violacea*). In these cases the crimson head-feathers, which also owe their colour to turacin, are few in number, as if the bird, otherwise healthy, had been unable to manufacture a sufficiency of the pigment. I may here add that the red tips of the crest feathers of *Turacus meriani* also contain turacin.

In all the birds in which turacin occurs, this pigment is strictly confined to the red parts of the web, and is there unaccompanied by any other colouring matter. It is therefore found that if a single barb from a feather be analysed its black base and its black termination possess no copper, while the intermediate portion gives the blue-green flash of copper when incinerated in the Bunsen flame. [A parti-coloured feather was burnt in the Bunsen flame, with the result indicated.]

Where it occurs, turacin is homogeneously distributed in the barbs, barbicels and crochets of the web, and is not found in granules or corpuscles.

To the natural question "Does turacin occur in any other birds besides the touracos?" a negative answer must at present be given. At least my search for this pigment in scores of birds more or less nearly related to the Musophagidæ has met with no success. In some of the plantain-eaters (species of Turacus and Gallirex) there is, however, a second pigment closely related to turacin. It is of a dull grass-green colour, and was named Turacoverdin by Dr. Krukenberg in 1881. I had obtained this pigment in 1868 by boiling turacin with a solution of caustic soda, and had figured its characteristic absorption band in my first paper (Phil. Trans., vol. clix. 1870, p. 630, fig. 4). My product was, however, mixed with unaltered turacin. But Dr. Krukenberg obtained what certainly seems to be the same pigment from the green feathers of *Turacus corythaix*, by treating them with a 2 per cent. solution of caustic soda. I find, however, that a solution of this strength dissolves, even in the cold, not only a brown pigment associated with turacoverdin, but ultimately the whole substance of the web. By using a much weaker solution of alkali (1 part to a thousand of water) a far better result is obtained. [The characteristic absorption band of turacoverdin, which lies on the less refrangible side of D, was shown; also the absorption bands of various preparations of turacin.] I have refrained from the further investigation of turacoverdin, hoping that Dr. Krukenberg would complete his study of it. At present I can only express my opinion that it is identical with the green pigment into which turacin when moist is converted by long exposure to the air or by ebullition with soda, and which seems to be present in traces in all preparations of *isolated* turacin however carefully prepared.

A few observations may now be introduced on the physical and chemical characters of turacin. It is a colloid of colloids. And it enjoys in a high degree one of the peculiar properties of colloids, that of retaining, when freshly precipitated, an immense proportion of water. Consequently, when its solution in ammonia is precipitated by an acid, the coagulum formed is very voluminous. [The experiment was shown.] One gram of turacin is capable of forming a semi-solid mass with 600 grams of water. Another character which turacin shares with many other colloids is its solubility in pure water and its insolubility in the presence of mere traces of saline matter. It would be tedious to enumerate all the observed properties of turacin, but its deportment on being heated and the action of sulphuric acid upon it demand particular attention.

At 100° C., and at considerably higher temperatures, turacin suffers no change. When, however, it is heated to the boiling-point of mercury it is wholly altered. No vapours are evolved, but the substance becomes black and is no longer soluble in alkaline liquids, nor, when still more strongly heated afterwards, can it be made to yield the purple vapours which unchanged turacin gives off under the same circumstances. This peculiarity of turacin caused great difficulty in its analysis, for these purple vapours contain an organic crystalline compound in which both nitrogen and copper are present, and which resist further decomposition by heat. [Turacin was so heated as to show its purple vapours, and also the green flame with which they burn.] This production of a volatile organic compound of copper is perhaps comparable with the formation of nickel- and ferro-carbonyl.

The action of concentrated sulphuric acid upon turacin presents some remarkable features. The pigment dissolves with a fine crimson colour, and yields a new compound, the spectrum of which presents a very close resemblance to that of hæmatoporphyrin [Turacin was dissolved in oil of vitriol: the spectrum of an ammoniacal solution of the turacoporphyrin thus produced was also shown], the product obtained by the same treatment from hæmatin: in other respects also this new derivative of turacin, which I call turacoporphyrin, reminds one of hæmatoporphyrin. But, unlike this derivative of hæmatin, it seems to retain some of its metallic constituent. The analogy between the two bodies cannot be very close, for if they were so nearly related as might be argued from the spectral observations, hæmatin ought to contain not more but less metal than is found to be present therein.

The percentage composition of turacin is probably—Carbon 53·69, hydrogen 4·6, copper 7·01, nitrogen 6·96, and oxygen 27·74. These numbers correspond pretty nearly to the empirical formula, $C_{82} H_{81} Cu_2 N_9 O_{32}$. But I lay no stress upon this expression.

I have before said that copper is very widely distributed in the Animal Kingdom. Dr. Giunti, of Naples, largely extended (1881) our knowledge on this point. I can hardly doubt that this metal will be found in traces in all animals. But besides turacin only one organic copper compound has been as yet recognised in animals. This is a respiratory, and not a mere decorative, pigment like turacin. Léon Fredericq discovered this substance, called hæmocyanin. It has been observed in several genera of Crustacea, Arachnida, Gastropoda and Cephalopoda. I do not think it has ever been obtained in a state of purity, and I cannot accept for it the fantastic formula—$C_{867} H_{1369} Cu S_4 O_{258}$—which has recently been assigned to it. On the other hand, I do not sympathise with the doubts as to its nature which F. Heim has recently formulated in the *Comptes Rendus*.

It is noteworthy, in connection with the periodic law, that all the essential elements of animal and vegetable organic compounds have rather low atomic weights, iron, manganese and copper representing the superior limit. Perhaps natural organic compounds containing manganese will some day be isolated, but at present such bodies are limited to a few containing iron, and to two, hæmocyanin and turacin, of which copper forms an essential part.

If I have not yet unravelled the whole mystery of the occurrence and properties of this strange pigment, it must be remembered that it is very rare and costly, and withal difficult to prepare in a state of assured purity. It belongs, moreover, to a class of bodies which my late master, Dr. A. W. von Hofmann, quaintly designated as "dirts" (a magnificent dirt truly!)—substances which refuse to crystallize and cannot be distilled. I have experienced likewise, during the course of this investigation, frequent reminders of another definition propounded by the same great chemist, when he described organic research as "a more or less circuitous route to the sink!"

I am very glad to have had the opportunity of sharing with an audience in this Institution the few glimpses I have caught from time to time during the progress of a tedious and still incomplete research into the nature of a pigment which presents physiological and chemical problems of high if not of unique interest.

Let my last word be a word of thanks. I am indebted to several friends for aid in this investigation, and particularly to Dr. MacMunn, of Wolverhampton, the recognised expert in the spectroscopy of animal pigments. [A. H. C.]

WEEKLY EVENING MEETING,

Friday, February 24, 1893.

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

Edward Hopkinson, Esq. M.A. D.Sc.

Electrical Railways.

One of the most striking of the many new departures in the practical application of electrical science, which made the Paris Exhibition of 1881 memorable, was a short tramway laid down under the direction of the late Sir William Siemens, from the Palais de l'Industrie to the Place de la Concorde, upon which a tramcar worked by an electric motor plied up and down with great regularity and success during the period of the Exhibition. Yet few of those who saw in this experiment the possibilities of a great future for a new mode of traction would have ventured to predict that within ten years' time, in the United States alone, over 5000 electric cars would be in operation, travelling 50,000,000 miles annually, and carrying 250,000,000 passengers, or that electrical traction would have solved the problem of better communication in London and other large cities. Two years before the Exhibition in Paris the late Dr. Werner Siemens had exhibited at the Berlin Exhibition in 1879 an experimental electric tramway on a much smaller scale, and his firm had put down in 1881 the first permanent electric railway in the short length of line at Lichterfelde, near Berlin, which, I believe, is still at work. In the same year Dr. William Siemens undertook to work the tramway, then projected, between Portrush and Bushmills, in the North of Ireland, over six miles in length, by electric power, making use of the water-power of the Bush River for the purpose, an undertaking which I had the advantage of carrying out under his direction. It is no part of my object to-night to follow further the history of electric traction, which is so recent that it is familiar to all; but, in alluding to these initial stages of its development, I have desired to recall that it was the foresight and energy of Dr. Werner and Dr. William Siemens, and their skill in applying scientific knowledge to the uses of daily life, which gave the first impulse to the development of the new electrical power.

The problem of electric traction may be naturally considered under three heads:—

(1) The production of the electrical power.
(2) Its distribution along the line.
(3) The reconversion of electrical into mechanical power, in the car motor or locomotive.

The first of these, here in England at any rate, is dependent upon the economical production of steam power, although there are essential points of difference between the conditions under which steam-power is required for electric traction purposes and for electric lighting. But in Scotland and Ireland, and in many countries abroad, there is abundant water power, now only very partially utilised. The Portrush line is worked in part by water and in part by steam-power, but for the Bessbrook and Newry Tramway (of which there is a working model on the table) water-power is exclusively used.

A few experiments will show that the demand for power on the generating plant is greatest at the moment of starting the car or train, when, in addition to the power required to overcome the frictional resistances, power is also required to accelerate the velocity. Thus, if instead of a single car there are a number of trains moving on the one system, and it so happens that several are starting together, the demand made upon the generating plant may at one moment be three or four times as great as that made a few seconds after. This is shown in the diagrams which exhibit the variation of current supplied by the generators on the City and South London Railway, with eight trains running together, the readings being taken every ten seconds. The maxima rise as high as double the mean; thus the generating plant must be capable of instantly responding to a demand double or even treble the average demand upon it.

In electric lighting it is true there is not less variation between the maximum demand and the mean taken during the ordinary hours of lighting, but it is only in the event of sudden fog that the probable demand cannot be accurately gauged beforehand, and provided for by throwing more generators into action. Thus in a lighting station each generator may be kept working approximately at its full load, and therefore under conditions of maximum economy, whereas in a traction station the whole plant must be kept ready to instantaneously respond to the maximum demands which may be made upon it, and must therefore necessarily work with a low load factor, and consequently with diminished economy. So important is the influence on cost of production of the possible demand in relation to average demand, that the Corporation of Manchester, under their order for electric supply, have decided, upon the advice of their engineer, to annually charge a customer 3*l.* per quarter for each unit per hour of maximum supply which he may require, in addition to 2*d.* for each unit actually consumed, i. e. for being ready to supply him with a certain amount of electrical power if required to do so, they charge an additional sum equivalent to the charge for its actual consumption for 1440 hours.

In one respect water-power has an economic advantage over steam-power, because although steam engine and turbine alike work with greatly reduced efficiency at reduced loads, when the turbine gates are partially closed and the water restrained in the reservoir it is not subject to loss of potential energy, whereas the energy of the steam held back by valves of the engine suffers loss through radiation and condensation.

At Bessbrook the turbine and generator dynamo combined yield 60 per cent. of the energy of the water as electrical energy available for work on the line, but when the load is reduced to a third of the full load the efficiency is reduced to 33 per cent. So on the City and South London line, a generator engine and dynamo will yield, when working at their full load, 78 per cent. of the indicated horse-power as useful electrical power, but at half load the efficiency falls to 65 per cent. Notwithstanding these conditions the generator station of the City and South London line is producing electrical energy at a cost of 1·56*d.* per Board of Trade unit, which is less than the annual average cost of production of any electric station in England, with the single exception of Bradford, which has the advantage both of cheap coal and cheap labour. In output it is the largest of any Electric Generating Station in England, the total electrical energy delivered in 1892 being 1,250,000 Board of Trade units, the second on the list being the St. James and Pall Mall with 1,186,826 units.

Let us pass now to the consideration of the distribution of the electric power along the line. I have equipped the three model tracks before you with three different kinds of conductors. In two of them the rails of the permanent way, which are necessarily uninsulated, are made use of for the return current. This plan, with I believe the almost single exception of the Buda-Pesth Tramway, has been universally adopted with the object of saving the cost of a return conductor; but it is doubtful whether such an arrangement can be considered final, for it must necessarily create differences of potential in the earth, which already in some instances have had disturbing effects upon our observatories, or upon our telegraph and telephone systems. It appears to be probable in the more or less distant future that the use of the earth for the passage of large current will be prohibited by legislation; and that it will be reserved for the more delicate and widely extended operations of telegraphy and telephony. These disturbances may of course be easily avoided by the use of an insulated conductor for the return circuit; and it may be that our legislature, looking forward to a remoter future, when electrical forces may be utilised, compared with which even those involved in our present telegraphs and telephones are inconsiderable, will insist upon all,—tramways, telegraphs and telephones,—using an insulated return; a course which I venture to think would be of present benefit to these services, as well as a safeguard for the interests of the future. In the case of conductors which are in such a position that contact may be made from them to the ground through the body of a horse or some other animal coming into contact with them, there is another strong argument for an insulated return, as many animals, and notably horses, are far more sensitive to electric shock than man. It is not perhaps well known, but still a fact, that a shock of 250 volts is quite sufficient to kill a horse almost instantaneously.

The first model has a single overhead conductor with return by the rails; but in place of a single fishing-rod collector or trolley to take the current from the overhead wire there are fixed on the car two rigid bars, one at each end, which slide along the under surface of the wire and make a rubbing contact against it. This system, devised by Dr. John Hopkinson, has the advantage that there is less difficulty in maintaining contact on uneven roads or on curves, and that the catenaries of the suspended wire may be hung with greater dip, and therefore with less tension. Again, the double contact obviates the frequent breaks and consequent sparking of a single trolley system. The second model shows the system adopted on the City and South London line, and more recently followed on the Liverpool Overhead line, of a conductor of channel steel, upon which collectors fixed to the locomotives make a sliding contact. The third track shows an overhead system like the first, but with an insulated return in place of return by the rails.

The characteristic feature of an electric motor is that it delivers us the mechanical power we require directly in the form of a couple about an axis instead of in the form of a rectilinear force, as is the case with steam, gas, or air engines, which must be reduced to a rotary form by connecting rod and crank. Thus it is possible to sweep away all intermediate gear, and to arrive at once at the simplest of all forms of a traction motor, consisting of but one pair of wheels fixed on a single axle with the armature constructed directly upon it, with its magnets suspended from it and maintained in their position against the magnetic forces acting upon them by their weight. Such a locomotive is shown in the third model before you. So far as I am aware, a locomotive of such simplicity as this has never been constructed for practical work, but on the City and South London line the armatures of the motors are placed directly on the axles, and the magnets suspended partly from the axles and partly from the frame.

The second model is an exact reproduction of the locomotives on the City and South London line, but with a different arrangement of motors. Here both armatures are included in the same magnetic circuit, and both magnets and armatures carried on the frame of the locomotive and not on the axles. The armatures are geared to the axles by diagonal connecting rods, the axle boxes being inclined, so that their rise and fall in the horn blocks is at right angles to the connecting rods. This design, which is due to the late Mr. Lange, of Messrs. Beyer, Peacock & Co., allows of the motor armature being placed on the floor level of the locomotive, and so more easily accessible.

This model will serve to show some of the characteristic features, as well as some of the characteristic defects, of an electric motor as such. But in order to show these clearly I may refer for a moment to the general theory of a motor. It is easily shown that in a series wound motor the couple or turning moment on the axle is a function of the current only, and independent of the speed and electro-motive force. Again, it follows from Ohm's law that the current passing through the motor multiplied by the resistance of the magnet and armature coils is equal to the difference between the electro-motive force at the terminals of the motor and the electro-motive force which would be generated by the motor, if it were working at the same speed as a generator of electricity, that is to say the difference between the electro-motive force at the terminals and what is called the "back" or "counter" electro-motive force of the motor. Hence if the terminals of the motor be coupled direct to the line at the moment of starting when the motor is still at rest, the current will be very great and its power entirely absorbed in the coils of the armature and magnets, but the turning moment will then be a maximum. The motor then begins to move, part of the power being spent in overcoming frictional resistances and part in accelerating the train. A back electro-motive force is then set up, increasing as the speed increases, and causing the current to diminish until finally a position of equilibrium is established, when the speed is such that the back electro-motive force together with the loss of potential in the coils of the motor is equal to the potential of the line. But in practice the mechanical strength of the motor, and the carrying power of its coils, as well as the limited current available from the generators, makes it necessary to introduce resistances in circuit with the motor to throttle the current and to reduce it within proper limits. It is to this point I desire to draw attention, that in traction work when starting the motor resistances must be introduced, which, with the resistance of the motor itself, at the moment of starting, absorb the whole power of the current, reducing the efficiency of the motor to nil, and which continue to absorb a large percentage of the power, until the condition of equilibrium is established. This is the great defect in electric motors for traction work, and its importance can be shown very clearly by reference to the work done on the City and South London line. There the motors when working with their normal current have an efficiency of 90 per cent., but the actual all-round efficiency of the locomotive as a whole is 70 per cent. only, so that the loss in starting is equal to 20 per cent. of the whole power. Of course in some respects the City and South London line is exceptional in that a start is made every two or three minutes. Various devices have been suggested with a view to diminishing this waste of power in starting an electric motor, but none entirely meet the case. Thus, if the locomotive or car has two motors, these can be coupled in series at the start, and subsequently thrown into parallel, thereby doubling the tractive force with a given current, or for the same tractive force reducing the loss of power by three-fourths. When through the increase of speed of the motor the back electromotive force balances the electro-motive force of the line, the speed can be increased by diminishing the magnetic field by reducing the effective coils on the magnets, but this device does not give any assistance at the lower speeds, as the magnets ought to be so wound as to be high on the characteristic curve, or nearly saturated with the normal current, and it is therefore not possible to obtain any increased intensity of field by increasing the convolutions of the magnet coils. If it were possible to use alternate current motors for traction work, the difficulty could at once be met by introducing a transformer in the circuit, and placing the motor in its secondary. The effective convolutions of the secondary circuit on the transformer could then be varied as the speed increases in such wise that the electro-motive force of the line is balanced by the back electro-motive force of the motor and the fall of potential due to the resistance of the motor coils, so avoiding all need for resistances.

The City and South London line has enabled experiments to be made on the efficiency of the railway system as a whole, taking into account the loss of power in the generators, on the line, and in the motors, and in the resistances of the locomotives. The loss in the line is about 11 per cent. of the electrical power generated, and the efficiency of the locomotives as a whole is, as I have shown, 70 per cent.; thus the electrical efficiency of the entire system is 62 per cent. The trains weigh with full load of 100 passengers about 40 tons, and the average speed between stations is 13·5 miles per hour. The cost of working, including all charges, during last half year was 7·1*d.* per train mile, of which 4·7*d.* represents the cost of production of the electric power, and 2·4*d.* the cost of utilising it on the locomotives. It is perhaps hardly a fair comparison to compare the cost of working such a line as the South London line with the cost of steam traction on other lines, inasmuch as steam could not possibly be used in the tunnels, only 10 feet 6 inches diameter, in which this line is constructed, but the comparison is not uninstructive. Take the Mersey Railway, where the gradients and nature of the traffic are similar. On the Mersey Railway the locomotives weigh about 70 tons, and the train, which is capable of carrying about 350 passengers, 150 tons. According to the published returns of the company, the cost of locomotive power is 14*d.* per train mile, i. e. double the cost on the South London line, but for a train weighing between four and five times as much, but capable of carrying only 3½ times the number of passengers; thus the cost of steam traction per ton mile of train is about half that per ton mile of train for electric traction. But it is not on the cost per ton mile that the success of a passenger line depends. The real basis of comparison is the cost per passenger mile, and here electric traction has great advantage over steam, as the dead weight of the electric motor is small compared with the dead weight of steam locomotives of the same power, and with electric motors the trains can be split up into smaller units, at but slightly increased cost, so permitting a more frequent service. We cannot expect, therefore that electric traction with our present knowledge will take the place of steam traction on our trunk lines; but it has its proper function in the working of the underground lines now projected for London, Paris, Berlin, Brussels, and other large towns, and also I think on other urban lines, for example, on the Liverpool Overhead Railway, where trains of large carrying capacity are not required, but a frequent service is essential; and finally, also on those short lines, whether independent or branches of the great trunk lines, where water-power is available. When I undertook the construction of the Bessbrook line it was a condition that the cost of working should be less than the cost of working by steam, a condition which the first six months of working showed to be successfully fulfilled. When Messrs. Mather and Platt undertook the construction of the electric plant for the City and South London Railway, they guaranteed that the cost of traction for a service of 8247 miles per week as actually run should not exceed 6·3*d.* per train mile, exclusive of the drivers' wages. Their anticipations have been more than realised, the actual cost being 5·1*d.* per train mile only. There are, however, other projects, both in America and on the Continent, for electric railways on which the special feature is to be an enormously high speed of travel, speeds of 150 and even 200 miles per hour being promised. With a steam locomotive, involving the reciprocating motion of the piston and connecting rod, such speeds are probably unattainable, but they may be realised in the purely rotary motion of an electric motor. But at such high speeds as these the power required to overcome the air resistance is of special consideration. Probably up to speeds of 750 miles per hour, or even to higher limits still, the ordinary law of air resistance holds good, as the rate of disturbance is still less than the velocity of waves in air, but above these limits we leave the regions of ordinary locomotion and enter rather into the field of projectiles. Assuming, however, that the ordinary laws of air resistance do hold good, I calculate that the power required to propel an ordinary train 200 feet long at 200 miles per hour against the resistance of air alone, apart from the frictional resistances, would not be less than 1700 horse-power. Though there is nothing to prevent the construction of electric locomotives capable of developing this or even greater power, the strength of the materials at present at command will set a limit to the speeds which may be obtained.

In order that the engineer may realise the imperfection of all his works, it is well for him to be constrained from time to time to contemplate the amount of energy involved in his final purpose compared with the energy of the coal with which he starts. I have endeavoured to put before you to-night the losses that occur and the reasons for them, in some steps of the complex machine which constitutes an electric railway; so in conclusion I will draw your attention to the ultimate efficiency of the machine, starting with the coal and ending with the passenger carried through space. The diagram on the wall, starting with the familiar 12,000,000 foot-pounds, the energy of a pound of coal, shows the loss in each step, supposing it made with the most economical appliances known to the engineer, first in the boiler, then in the steam engine, generator dynamo, conductors, locomotives, in the dead weight of the train, till finally we arrive at the energy expended on the passenger himself, which we find to be 133,000 foot-pounds, or but little more than 1 per cent. of the energy with which we started. It is true indeed that transportation is a more economical process than lighting with incandescent lamps, in which the final efficiency is about one-half per cent., but whether in lighting or in traction, when we consider that ninety-nine parts are now wasted for one part saved, we may realise that the future has greater possibilities than anything accomplished in the past.

[E. H.]

WEEKLY EVENING MEETING,

Friday, March 3, 1893.

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

GEORGE SIMONDS, Esq.

Sculpture, considered apart from Archæology.

IT was usual, Mr. Simonds remarked, for lecturers on Sculpture to deal more with Archæology than Art; he did not, however, intend to adopt this principle, but should treat his subject from the practical standpoint of the artist.

He spoke next of the very wide range of the sculptor's art, and said that in metal-work especially a man might find himself called upon to produce a colossal statue to-day and a set of silver tea-spoons to-morrow; after which he spoke of the two opposed principles on which all sculptors' work depends, viz., building up, as in modelling, and cutting down, as in carving, and called attention to the evil results which ensue when either of these processes is applied to a material for which it is unsuited, and gave illustrations of this point in the works of artists of the late seventeenth and of the eighteenth centuries, showing especially that some of the works of Bernini which were executed in marble were really more suitable for bronze.

Faults in this direction, Mr. Simonds stated, were almost always the result of very high technical skill, which tempted the artist to consider it desirable to exhibit a *tour de force*. Even the Greeks themselves were not always free from this somewhat paltry ambition, as was demonstrated by the "Laocöon" and the "Group of the Farnese Bull."

Such splendid misapplication of power was impossible in the early periods of Art, when the technical difficulties sufficed to keep the artist well within the limits prescribed by his material.

The first efforts of sculpture were always purely imitative, and where the imitation was not very successful this was generally due to lack of technical skill rather than to any desire to idealise. This was illustrated by a series of examples of primitive sculpture from various parts of the world. The desire for beauty, however, as understood by the early artist, frequently induced him to exaggerate certain points in his work, often with very grotesque results. Instances were here given of early Etruscan and other sculpture, showing abnormal length of limb and muscular development.

It was a great advance in Art when it was known that harmony of proportion constituted one of the chief elements of beauty. Canons of proportion were established and sculpture became a dignified and beautiful, although, no doubt, to a large extent a conventional art, as was shown by various examples of Egyptian and Etruscan work.

The Assyrians and the Greeks made the further discovery that fresh beauty was to be sought for in rhythm of action and in correctness of construction, as is evidenced in the sculptures of the Temple at Ægina, in the bas-reliefs from the Palace of Korsabad, and others; the Assyrians especially excelling in the rendering of movement, their sculptures of animals, such as lions, horses, mules, &c., being of the very highest artistic merit and beauty.

In all the above works the artist has relied for his effect on proportion, on action and correct construction. He has not concerned himself with the beauty that is to be found in texture and in the mobility of flesh.

Sculpture depends on proportion, construction, action and texture for whatever it may possess of technical excellence.

Conventionalism, however valuable in sculpture, is apt to become wearisome; then comes an artist, bolder than the rest, who forsakes in some degree the ancient tradition, and who endeavours, usually with success, to return more closely to individual Nature; others follow and seek to outstrip him in close imitation of Nature, and for this *texture* is the quality most in demand.

An instance of this in the modern Italian school is Magni's "Reading Girl."

The lecturer then went on to speak of the practice of painting statues, admitting that the Greeks used thus to treat them, but stating that it was not at present possible for us to realise what the effect must have been of a Greek temple with its brilliant colouring and polychrome sculpture; he spoke next of modern attempts to revive the practice by the late John Gibson and others, which, however, had not been successful; although it is a common practice with sculptors to give a slight wash to marble if a warmer tone seems desirable; and, indeed, the warm tints of old marble are often successfully imitated by the Italian dealers in forged antiques, such as are bought by wealthy collectors, and even sometimes find their way into national museums.

The intention of sculpture should be, of course, to place before us a beautiful thought expressed by beautiful form; such is, however, not always the sculptor's only desire; he too often wishes to advertise his own cleverness and to produce work that shall fix the attention of even the most ignorant and careless observer. Thus eccentricity is made to do duty for originality, and the ignorance or neglect of all the rules of harmony of line and composition is supposed to be the triumph of genius over the trammels of conventionality. The result is often very ugly.

The modern sculptor is under many disadvantages compared with the old Greek. We no longer worship physical beauty as they did, nor can we easily get models of sufficient beauty and refinement to be of much use to us in our work. Moreover the profession of artists' model is a very hard one, requiring patience and a strong interest in the work. In ancient Greece the whole nation were both models and connoisseurs. In London there are hardly half a dozen models of either sex that can be considered properly qualified by Nature and by education for their profession; yet London is probably not worse off than other Art centres.

To the modern sculptor, then, only two courses are open; either he must be content passively to follow the ideal types that have been handed down from ages past, in which case his work will certainly be lower in the scale of beauty than that on which his ideal is based, or he must strive to form an ideal for himself, based on a careful and loving study of the most beautiful form that he can find in living nature. In other words he must get the best model he can, and work as closely to Nature as possible, leaving out or passing lightly over such details of form as are blemishes or evidently accidental. By this means we may produce work of great beauty (though perhaps not quite equal to that achieved by the Greeks) and also possessed of the added charms of vitality and individuality. An over-great striving for these two last qualities often results, however, in a tolerance of downright ugliness.

Artists, the lecturer declared, were always to be found anxious to produce whatever the public admired, and if the taste for eccentricity or ugliness prevailed, the supply would be forthcoming until nausea ensued, and then a better taste would prevail. Canova's works were instanced to show how sudden these changes in style and taste often are, and the highly realistic group of "Dædalus and Icarus" was compared with some of his ideal works of a few years later.

The leaders in the revolt from the style of the eighteenth century were Canova, Flaxman and Thorwaldsen, and the movement finally ended with Gibson and his followers in utter conventionalism and graceful insipidity.

Artists no longer try to make imitation antiques but claim the right to look at Nature for themselves; and, while respecting the ancient tradition and teaching of classic art, do not accept these as being of universal application to their own work. Where they transgress them they do so wilfully, and to gain some adequate advantage.

Fashion, it was stated, had considerable influence on Art, and was influenced by it. Thus the artists made beauty fashionable some fifteen years ago, and beautiful women, Greek tableaux and dresses were all the rage with the public; but they soon went out of fashion again, and no one hears of professional beauties at the present day. After this there was a demand for character and individuality; and sculptors were not slow to see that this could be secured by copying the living model with painful accuracy. Coarse knees and angular projecting hips, ill-shaped breasts and bony backs were not spared to us, and the critics sang their praises, and did thereby much injury to the public and to the younger artists, who forthwith adopted the gospel of ugliness.

The lecturer then described some of the processes employed in the production of a work of sculpture, and compared the modern methods with those of the ancients, showing that we now enjoy technical advantages for the production of sculpture in all materials far superior to those of former ages. He explained models of various instruments used in measuring by sculptors when "pointing" their statues, as the roughing-out process is termed, including Kauer's pointing instrument, and Simonds' Iconograph for proportional pointing, and described the uses of various tools and appliances used both for marble and for bronze-work. The principles of bronze-casting were illustrated by means of a working diagram, showing the core inside the mould, the empty space between core and mould to be occupied by the melted bronze, and the mould itself, with the various ducts for the metal, and vents to permit the free exit of air and generated gases.

Yet with all our technical advantages we were yet deficient in *style* compared with the old masters.

All styles, however, have only their day, since there is none so noble but that at some time it has been condemned and cast aside, and none so contemptible but that at some time it has been held to be the only true art.

It is difficult to divest ourselves of prejudice in Art, and many a statue, as for instance, the famous "Esquiline Venus," has had a reputation made for it by some enthusiastic newspaper correspondent who happened to be on the spot when it was discovered, and who has pronounced it to be a Greek work of the very best period. We are apt to forget that there are bad as well as able artists in all periods, and that the work of a really good man in a bad period is perhaps more valuable than a poor thing that chances to belong to the best period of Greek art.

The lecturer then spoke of sculpture as architectural decoration, illustrating his remarks with examples from the Zwinger at Dresden, and the sculpture of the Marmorbad at Cassel, and expressed regret that English architects were so seldom able to induce their clients to expend sufficient money on high-class decorative sculpture, and that even our public buildings were left unbecomingly bare. This was to be ascribed to the fact that few even of the so-called "cultured" people knew anything of sculpture, and it was most common to see in the same house paintings worth thousands of pounds, and close beside them, and regarded by their owner with equal complacency, some wretched cheap bronzes that a sculptor would not give house-room to, but would surely condemn to the melting-pot. Most of the sculptural demand in England is for monumental or portrait work—most of it far from satisfactory; the system of committees and competitions being calculated to produce the worst results. A committee of one—who knows what he wants and applies to a capable artist for it—will always be the most satisfactory.

The lecturer then spoke of the wholesale destruction of monumental statues that had taken place at various dates and in different countries, as, notably, in France, in 1792, and of the motives that induced man to erect and to destroy monuments; and endeavoured to point out that although no monument is so effective as a sculptural one, yet it is by no means proved that a full-sized portrait in bronze of a man and his clothes is the most satisfactory form; and that matters might often be improved by confining the portrait to a bust or a medallion, and allowing allegorical sculpture to complete the story.

Various celebrated equestrian monuments were further shown in illustration, and the lecturer concluded by quoting the words of Professor Tyndall on the power exerted by a noble monumental work over the imagination, it being, he said, "capable of exciting a motive force within the mind which no purely material influence could generate."

[G. S.]

GENERAL MONTHLY MEETING,

Monday, March 6, 1893.

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

Harold A. Des Vœux, M.D. M.R.C.S.
Alfred Spalding Harvey, Esq. B.A.
Louis Makower, Esq.
William Marcet, M.D. F.R.S.
Alfred Mond, Esq.
Mrs. F. W. Mott,
Leslie Pyke, Esq. F.C.S.
Lucas Ralli, Esq.
Mrs. A. Ruffer,
F. Walter Scott, Esq.
Claude Vautin, Esq.
Miss Laura A. Webster,
Rev. W. Allen Whitworth, M.A.

were elected Members of the Royal Institution.

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

JOHN MACDONELL, Esq. LL.D.—Three Lectures on SYMBOLISM IN CEREMONIES, CUSTOMS, AND ART; on Tuesdays, April 11, 18, 25.

PROFESSOR R. K. DOUGLAS.—Three Lectures on MODERN SOCIETY IN CHINA; on Tuesdays, May 2, 9, 16.

E. L. S. HORSBURGH, Esq. M.A.—Three Lectures on NAPOLEON; on Tuesdays, May 23, 30, June 6.

PROFESSOR DEWAR, M.A. LL.D. F.R.S. *M.R.I.*—Five Lectures on THE ATMOSPHERE; on Thursdays, April 13, 20, 27, May 4, 11.

R. BOWDLER SHARPE, Esq. LL.D.—Four Lectures on THE GEOGRAPHICAL DISTRIBUTION OF BIRDS; on Thursdays, May 18, 25, June 1, 8.

JAMES SWINBURNE, Esq. M. Inst. E.E.—Three Lectures on SOME APPLICATIONS OF ELECTRICITY TO CHEMISTRY (The Tyndall Lectures); on Saturdays, April 15, 22, 29.

HENRY CRAIK, Esq. C.B. LL.D.—Three Lectures on I. JOHNSON AND MILTON; II. JOHNSON AND SWIFT; III. JOHNSON AND WESLEY; on Saturdays, May 6, 13, 20.

A. C. MACKENZIE, Esq. MUS. DOC.—Three Lectures on "FALSTAFF." A Lyric Comedy, by Boito and Verdi; on Saturdays, May 27, June 3, 10.

The Managers reported that in accordance with the Acton Endowment Trust Deed they had awarded the Actonian Prize of one hundred guineas to Miss Agnes M. Clerke for her works on "Astronomy," as "illustrative of the Wisdom and Beneficence of the Almighty."

The Special Thanks of the Members were returned to Mr. Frederick Davis for his present of a fine copy of the Spitzer Catalogue (on Japan paper), in six volumes, which was presented by him as a souvenir of the visit of H.R.H. The Prince of Wales, and as commemorative of Professor Dewar's lecture on "Liquid Air," on the 22nd of February last.

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

Mrs. Wigan	£10

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

FROM

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Nature for February, 1893. 4to.
Open Court for February, 1893. 4to.
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WEEKLY EVENING MEETING,

Friday, March 10, 1893.

Sir Douglas Galton, K.C.B. D.C.L. LL.D. F.R.S. Vice-President, in the Chair.

Sir Herbert Maxwell, Bart. M.P.

Early Myth and Late Romance.

(No Abstract.)

WEEKLY EVENING MEETING,

Friday, March 17, 1893.

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

William J. Russell, Esq. Ph.D. F.R.S. *M.R.I.*

Ancient Egyptian Pigments.

The red pigment used by the Egyptians from the earliest times is a native oxide of iron, a hæmatite. Most of the large pieces found by Mr. Petrie are an oolitic hæmatite. One specimen, on analysis, gave 79·11 per cent. and another 81·34 per cent. of ferric oxide. The pieces to be used as pigments were no doubt carefully selected, and the samples that I have examined, mostly from Gurob and Kahun, are very good in colour. All the large pieces were of a singular shape, having one side smooth and curved; and in all cases this side was strongly grooved with striæ, giving somewhat the appearance to the mass of its having been melted, and allowed to cool in a circular vessel. No doubt the explanation of this smooth-curved surface is, that these pieces had actually been in part used to furnish pigments, and having been rubbed with a little water in a large circular vessel, had been ground to this shape. By experiment it was found that these pieces of the native hæmatite yielded, without any further addition by way of medium, a paint which could readily be applied with a brush, as it possesses remarkable adhesive properties, and it resembles exactly, in every particular, the red used in the different kinds of Egyptian paintings. In addition to these samples of the pigments, all of which are native minerals and in their natural conditions, there are other reds, finer in colour and smoother in texture, evidently a superior pigment; these apparently have been made from carefully selected pieces of hæmatite, which have been ground and washed, and dried by exposure to the air. Some of these pieces are very fine in colour, and it would be difficult to match them with any native oxide of iron that is used as a pigment at the present day. There is every reason to believe that this is the earliest red pigment which was used, and it remains to this day the commonest and most important one; it is a body unattacked by acids, unchangeable by heat, and even moisture and sunlight are unable to alter its colour. At the present time many artificial products are used to take the place of this natural pigment.

Yellow pigments.—These, again, are natural products, and by far the most common yellow used by the Egyptians is a native ochre. These ochres consist of about one-quarter of their weight of oxide of iron, from 7 to 10 per cent. of water, and the rest of their substance is clay. When moist they have a greasy feel, and work smoothly and well with the brush. There is no evidence of these bodies having changed colour, but undoubtedly they are chemically not nearly so stable as the red form of oxide of iron. Many of the pieces of this pigment, found at Gurob and at Tel-el-Armarna, are very fine in colour.

Some of the specimens of the very earliest colours of which the exact history is known, appear to be an artificial mixture of these two colours, the red and yellow, thus producing an orange colour. These samples were found on a tomb at Medum, which, according to Professor Flinders Petrie, was built by Nefermat, a high official and remarkable man at the Court of Senefru. Senefru is known to have lived in the fourth dynasty, about 4000 B.C., and to have preceded Khufu, the Cheops of the Greeks, who was the great Pyramid builder. Now, on Nefermat's tomb the characters and figures are incised and filled in with coloured pastes, which I have been able to examine, and it is of interest to know that this use of colour was a special device of Nefermat, for on his tomb it is stated that: "He made this to his gods *in his unspoilable* writing." In this unspoilable writing the figures are all carefully undercut, so that the coloured pastes, so long as they held together, should not be able to drop out. All the pastes used are dull in colour, consisting entirely of natural minerals. Hæmatite, ochre, malachite, carbon, and plaster of Paris appear to be the materials used. Chessylite, as a blue, probably was known even at that date, but the artificial blues seem hardly at this period to have come into use; certainly they are not found in the specimens of the Nefermat colours which I have examined. Another yellow pigment, far brighter in colour, was also often used. It is a sulphide of arsenic, orpiment; it is a bright and powerful yellow, again a body found in nature, but a much rarer body than ochre, and consequently, probably was only used for special purposes, when a brilliant yellow was required. As far as it is known at present, this pigment did not come into use until the eighteenth dynasty. Gold might even be placed among the yellow pigments, for it was largely used, and with wonderfully good effect. Its great tenacity seems to have been fully recognised, for gold is found in very thin sheets, and laid on a yellow ground, exactly as is done at the present day.

These pigments are then simply natural minerals, no doubt carefully selected, and sometimes ground and washed previous to being used; but the blue colour which is so largely used by the Egyptians is an artificial pigment, and consequently has far more interest attached to it than those already mentioned. It is a body requiring considerable care and experience to make, and thus its manufacture enables us to some extent to judge of the knowledge and ability which its producers had of carrying on a chemical manufacture. No doubt the splendid blue of the mineral chessylite was first used, but certainly in the twelfth dynasty—that is, about 2500 B.C.

—these artificial blues were used. They are all an imperfect glass, a frit, made by heating together silica, lime, alkali, and copper ore.* The number of failures which may have occurred, and how much material may have been spoilt, cannot be known, but all the blue frit which I have examined—and it is a considerable amount, some being raw material, lumps as they came from the furnace, and the rest ground pigment—all has been, though differing in grain and quality, well and perfectly made. Now this implies that the materials have been carefully selected, prepared, and mixed, and that definite quantities of each were taken, this necessitating the carefully measuring or weighing of each constituent. An early application of the fundamental law of chemistry, combination in definite proportion. The amount of copper ore added determined the colour; with 2 to 5 per cent. they obtained a light and delicate blue; with 25 to 30 per cent. a dark and rather purple blue; with still more the product would be black; if the alkali was too little in amount, a non-coherent sand resulted; if too much, a hard, stony mass is formed, quite unsuitable for a pigment. The difficulties, however, did not by any means end with the mixture of the materials. For the next process, the heating, is a delicate operation. Unfortunately up to the present time the exact form of furnace in which this operation was carried on is not known. The furnaces were probably, especially after use, very fragile structures, and have passed away. Considerable experience in imitating these frits even when using modern furnaces has taught me that the operation is really a very delicate one; the heat has to be carefully regulated and continued for a considerable length of time, a time varying with the nature of the frit being prepared; and, further, in the rough furnaces used it must have been specially difficult to have prevented unburnt gases from coming in contact with the material; but if they did a blackening of the frit must have taken place. However, all these difficulties were avoided, and a frit was made which exactly answered all the necessary requirements. It had, for instance, the right degree of cohesion, for many of the large pieces which have been found have, like the hæmatite, a smooth, curved striated surface, and on rubbing in a curved vessel with water, easily grind to powder. The powder is naturally much less adhesive than the hæmatite powder, but on adding a little medium, it could at once be used, without other preparation, as a paint. Some of the pieces vary in colour in different parts. This may have

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* A sample of the pale-blue frit gave, on analysis, the following results :—

Silica	88·65
Soda	0·81
Copper oxide	2·09
Lime	7·88
Iron oxide, alumina, &c.	0·57
	100·00

arisen from imperfect mixing, or from some parts of the furnace being hotter than others. It hardly appears to be intentional, possibly some of the dark, purplish-coloured frits were produced by accident; large pieces of it have as yet, I believe, not been found. By means of comparatively small alterations these frits could be obtained of a green colour. One way was by introducing iron. If, for instance, the silica used was a reddish coloured sand, it gave a greenish tinge to the frit; and frit made with some of the ordinary yellowish desert sand was found to give a frit undistinguishable from the most common of the old Egyptian frits. Again, a rather strong green colour is obtained by stopping the heating process at an early stage, this green frit, simply on heating for a longer time, becoming blue. Another way in which even the strong-coloured blue frits have been converted into apparently green pigments is by their being coated over with a transparent but yellowish coloured varnish which has to a remarkable extent retained its transparency, but no doubt become with age more yellow, and although strongly green now, may very likely originally have been nearly colourless, and consequently the frit was then seen in its original blue colour. Even as early as the twelfth dynasty the green frits used were dull in colour, and if by chance a brighter green was required, then they used the mineral malachite. No doubt by far the most brilliant blue used at any time was selected and powdered chessylite, and even down to the twenty-first dynasty they seem to have made use generally of somewhat brilliant coloured frits; but after that time more subdued colours appear to have been used, and even the scarabs were made of a much duller colour than formerly. All these blue frits form a perfectly unfadeable and unchangeable pigment. Neither the sun nor acids are able to destroy or alter their colour.

The only other pigment to which I can refer this evening is the pink colour which, in different shades, was much used. This is again an artificial pigment, and belongs to an entirely different class from any of the foregoing ones, for it is one of vegetable origin. On simply heating it, fumes are given off and the colour is destroyed, but a large white residue remains; this is sulphate of lime. It may here be stated that the white pigments used sometimes were carbonate of lime, but more generally sulphate of lime in form of gypsum, alabaster, &c. This substance is often very white in colour, is very slightly soluble in water, and has a singular smoothness of texture, which makes it work well under the brush; and in addition to these qualities, it is a neutral and very stable compound, so is well fitted for the purpose to which it was applied. It was easily obtained, being found native in many parts of Egypt. It is also interesting to note that there is an efflorescence consisting of this substance which frequently occurs in Egypt, and is of a remarkably pure white colour; probably this was used as a superior white pigment. It was easy to prove then that the pink colour was gypsum stained with organic colouring matter, and to try and imitate the colour appeared to be the most likely way of identifying it. Naturally, madder, which it is known has from the earliest times been used as a dye, was the vegetable colouring substance first tried, and it answered perfectly, giving under very simple treatment the exact shade of colour to the sulphate of lime which the Egyptian pigment had. Essentially the same colouring matter may have been obtained from another source, viz. Munjeet. In the case of madder it is interesting to note that the colour is not manifest in the plant—the *Rubia tinctorum*—for it is obtained from the root, and is even not ready formed there. In the root it exists as a glucoside, and this has to be decomposed before the colour becomes manifest. In this root there exists several colouring matters, which are known as madder-red, madder-purple, madder-orange, and madder-yellow. On breaking up the roots and steeping them in water for some length of time, the colours come out, some sooner than others, so that the tints vary. Again, changes of colour are easily obtained by the addition of very small quantities of iron, lime, alumina, &c., so that in these different ways a considerable range of colours could be obtained, but a delicate pink colour was the one probably generally made. This colour is easily obtained by simply stirring up sulphate of lime in a tolerably strong solution of madder, and adding a little lime, taking care to keep the colouring matter in excess; the colouring matter adheres firmly to the lime salt, and this settles on to the bottom of the vessel; the liquid is then poured off and the solid matter, if necessary, dried, or mixed—probably with a little gum, and used at once without other preparation. That the colouring matter was really madder could also be tested by another method, viz. by means of spectrum analysis. Both the madder-red (alazarin) and the madder-purple (purpurin) give, when the light which they transmit is analysed by the prism, very characteristic absorption bands; the purpurin bands are the ones most easily seen, consequently it became a point of considerable interest to ascertain whether from a specimen of this pigment, some thousands of years old, these absorption bands could be obtained. A small sample of this pink pigment was taken from a cartonage which was exhibited, and by treating it with a solution of alum, the colour was thus transferred to the liquid, and by throwing the absorption spectrum which it gave on the screen, and comparing it with the spectrum from a madder solution, it was clearly seen to be identical.

[W. J. R.]

WEEKLY EVENING MEETING,

Friday, March 24, 1893.

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

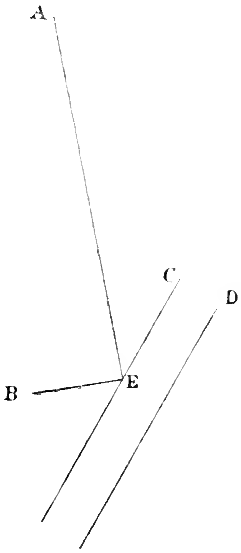
The Right Hon. Lord Rayleigh, M.A. D.C.L. LL.D. F.R.S. *M.R.I.*

Interference Bands and their Applications.

(Abstract.)

The formation of the interference bands, known as Newton's Rings, when two slightly curved glass plates are pressed into contact, was illustrated by an acoustical analogue. A high-pressure flame B (Fig. 1) is sensitive to sounds which reach it in the direction EB, but is insensitive to similar sounds which reach it in the nearly perpendicular direction AB. A is a "bird-call," giving a pure sound (inaudible) of wave-length (λ) equal to about 1 cm.; C and D are reflectors of perforated zinc. If C acts alone the flame is visibly excited by the waves reflected from it, though by far the greater part of the energy is transmitted. If D, held parallel to C, be then brought into action, the result depends upon the interval between the two partial reflectors. The reflected sounds may co-operate, in which case the flame flares vigorously; or they may interfere, so that the flame recovers, and behaves as if no sound at all were falling upon it. The first effect occurs when the reflectors are close together, or are separated by any multiple of $\frac{1}{2}\sqrt{2}.\lambda$; the second when the interval is midway between those of the above-mentioned series, that is, when it coincides with an odd multiple of $\frac{1}{4}\sqrt{2}.\lambda$. The factor $\sqrt{2}$ depends upon the obliquity of the reflection.

Fig. 1.



The coloured rings, as usually formed between glass plates, lose a good deal of their richness by contamination with white light reflected from the exterior surfaces. The reflection from the hindermost surface is easily got rid of by employing an opaque glass, but the reflection from the first surface is less easy to deal with. One plan, used in the lecture, depends upon the use of slightly wedge-shaped glasses (2°)

so combined that the exterior surfaces are parallel to one another, but inclined to the interior operative surfaces. In this arrangement the false light is thrown somewhat to one side, and can be stopped by a screen suitably held at the place where the image of the electric arc is formed.

The formation of colour and the ultimate disappearance of the bands as the interval between the surfaces increases, depends upon the mixed character of white light. For each colour the bands are upon a scale proportional to the wave-length for that colour. If we wish to observe the bands when the interval is considerable—bands of high interference as they are called—the most natural course is to employ approximately homogeneous light, such as that afforded by a soda flame. Unfortunately, this light is hardly bright enough for projection upon a large scale.

A partial escape from this difficulty is afforded by Newton's observations as to what occurs when a ring system is regarded through a prism. In this case the bands upon one side may become approximately achromatic, and are thus visible to a tolerably high order, in spite of the whiteness of the light. Under these circumstances there is, of course, no difficulty in obtaining sufficient illumination; and bands formed in this way were projected upon the screen.*

The bands seen when light from a soda flame falls upon nearly parallel surfaces have often been employed as a test of flatness. Two flat surfaces can be made to fit, and then the bands are few and broad, if not entirely absent; and, however the surfaces may be presented to one another, the bands should be straight, parallel, and equidistant. If this condition be violated, one or other of the surfaces deviates from flatness. In Fig. 2, A and B represents the glasses to be tested, and C is a lens of 2 or 3 feet focal length. Rays diverging from a soda flame at E are rendered parallel by the lens, and after reflection from the surfaces are recombined by the lens at E. To make an observation, the coincidence of the radiant point and its image must be somewhat disturbed, the one being displaced to a position a little beyond, and the other to a position a little in front of, the diagram.

The eye, protected from the flame by a suitable screen, is placed at the image, and being focused upon A B, sees the field traversed by bands. The reflector D is introduced as a matter of convenience to make the line of vision horizontal.

These bands may be photographed. The lens of the camera takes the place of the eye, and should be as close to the flame as possible. With suitable plates, sensitised by cyanin, the exposure required may vary from ten minutes to an hour. To get the best results, the hinder surface of A should be blackened, and the front surface of B should be thrown out of action by the superposition of a wedge-shaped

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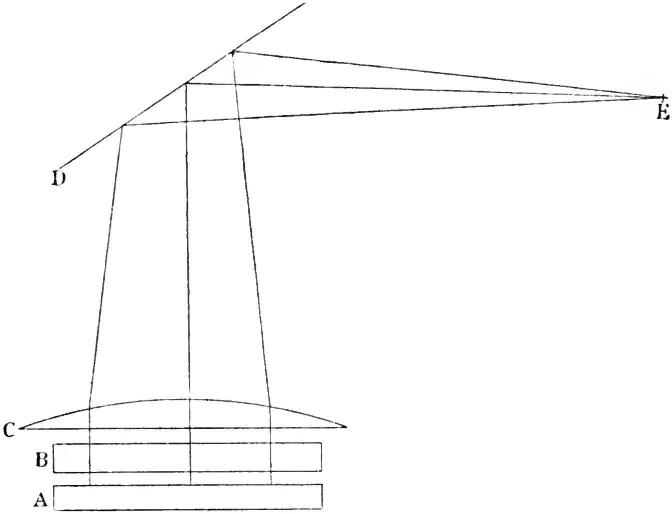
* The theory is given in a paper upon "Achromatic Interference Bands," Phil. Mag. Aug. 1889.

plate of glass, the intervening space being filled with oil of turpentine or other fluid having nearly the same refraction as glass. Moreover, the light should be purified from blue rays by a trough containing solution of bichromate of potash. With these precautions the dark parts of the bands are very black, and the exposure may be prolonged much beyond what would otherwise be admissible.

The lantern slides exhibited showed the elliptical rings indicative of a curvature of the same sign in both directions, the hyperbolic bands corresponding to a saddle-shaped surface, and the approximately parallel system due to the juxtaposition of two telescopic "flats," kindly lent by Mr. Common. On other plates were seen grooves due to rubbing with rouge along a defined track, and depressions, some of considerable regularity, obtained by the action of diluted hydrofluoric acid, which was allowed to stand for some minutes as a drop upon the surface of the glass.

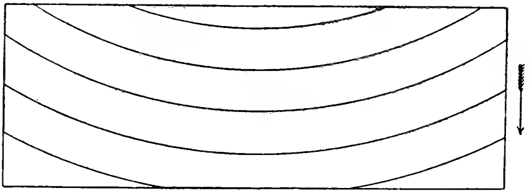
Fig. 2.



By this method it is easy to compare one flat with another, and thus, if the first be known to be free from error, to determine the errors of the second. But how are we to obtain and verify a standard? The plan usually followed is to bring *three* surfaces into comparison. The fact that two surfaces can be made to fit another in all azimuths proves that they are spherical and of equal curvatures, but one convex and the other concave, the case of perfect flatness not being excluded. If A and B fit another, and also A and C, it follows that B and C must be similar. Hence, if B and C also fit one another, all three surfaces must be flat. By an extension of this process the errors of three surfaces which are not flat can be found from a consideration of the interference bands which they present when combined in three pairs.

But although the method just referred to is theoretically complete, its application in practice is extremely tedious, especially when the surfaces are not of revolution. A very simple solution of the difficulty has been found in the use of a free surface of water, which, when protected from tremors and motes, is as flat as can be desired.* In order to avoid all trace of capillary curvature it is desirable to allow a margin of about $1\frac{1}{2}$ inch. The surface to be tested is supported horizontally at a short distance ($\frac{1}{10}$ or $\frac{1}{20}$ inch) below that of the water, and the whole is carried upon a large and massive levelling stand. By the aid of screws the glass surface is brought into approximate parallelism with the water. In practice the principal trouble is in the avoidance of tremors and motes. When the apparatus is set up on the floor of a cellar in the country, the tremors are sufficiently excluded, but care must be taken to protect the surface from the slightest draught. To this end the space over the water must be enclosed almost air-tight. In towns, during the hours of traffic, it would probably require great precaution to avoid the disturbing effects of tremors. In this respect it is advantageous to diminish the thickness of the layer of water; but if the thinning be carried too far, the subsidence of the water surface to equilibrium becomes surprisingly slow, and a doubt may be felt whether after all there may not remain some deviation from flatness due to irregularities of temperature.

Fig. 3.



With the aid of the levelling screws the bands may be made as broad as the nature of the surface admits; but it is usually better so to adjust the level that the field is traversed by five or six approximately parallel bands. Fig. 3 represents bands actually observed from the face of a prism. That these are not straight, parallel, and equidistant is a proof that the surface deviates from flatness. The question next

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* The diameter would need to be 4 feet in order that the depression at the circumference, due to the general curvature of the earth, should amount to $\frac{1}{20}\lambda$.

arising is to determine the direction of the deviation. This may be effected by observing the displacement of the bands due to a known motion of the levelling screws; but a simpler process is open to us. It is evident that if the surface under test were to be moved downwards parallel to itself, so as to increase the thickness of the layer of water, every band would move in a certain direction, viz. *towards* the side where the layer is thinnest. What amounts to the same, the retardation may be increased, without touching the apparatus, by so moving the eye as to *diminish* the obliquity of the reflection. Suppose, for example, in Fig. 3, that the movement in question causes the bands to travel downwards, as indicated by the arrow. The inference is that the surface is concave. More glass must be removed at the ends of the bands than in the middle in order to straighten them. If the object be to correct the errors by local polishing operations upon the surface, the rule is that *the bands, or any parts of them, may be rubbed in the direction of the arrow.*

A good many surfaces have thus been operated upon; and although a fair amount of success has been attained, further experiment is required in order to determine the best procedure. There is a tendency to leave the marginal parts behind; so that the bands though straight over the greater part of their length, remain curved at their extremities. In some cases hydrofluoric acid has been, resorted to, but it appears to be rather difficult to control.

The delicacy of the test is sufficient for every optical purpose. A deviation from straightness amounting to $\frac{1}{10}$ of a band interval could hardly escape the eye, even on simple inspection. This corresponds to a departure from flatness of $\frac{1}{20}$ of a wave-length in water, or about $\frac{1}{30}$ of the wave-length in air. Probably a deviation of $\frac{1}{100}\lambda$ could be made apparent.

For practical purposes a layer of moderate thickness, adjusted so that the two systems of bands corresponding to the duplicity of the soda line do not interfere, is the most suitable. But if we wish to observe bands of high interference, not only must the thickness be increased, but certain precautions become necessary. For instance, the influence of obliquity must be considered. If this element were absolutely constant, it would entail no ill effect. But in consequence of the finite diameter of the pupil of the eye, various obliquities are mixed up together, even if attention be confined to one part of the field. When the thickness of the layer is increased, it becomes necessary to reduce the obliquity to a minimum, and further to diminish the aperture of the eye by the interposition of a suitable slit. The effect of obliquity is shown by the formula

$$2\,t\,(1 - \cos\theta) = n\,\lambda.$$

The necessary parallelism of the operative surfaces may be obtained, as in the above described apparatus, by the aid of levelling. But a much simpler device may be employed, by which the experimental difficulties are greatly reduced. If we superpose a layer of water upon a surface of mercury, the flatness and parallelism of the surfaces take care of themselves. The objection that the two surfaces would reflect very unequally may be obviated by the addition of so much dissolved colouring matter, e.g. soluble aniline blue, to the water as shall equalise the intensities of the two reflected lights. If the adjustments are properly made, the whole field, with the exception of a margin near the sides of the containing vessel, may be brought to one degree of brightness, being in fact all included within a fraction of a band. The width of the margin, within which rings appear, is about one inch, in agreement with calculation founded upon the known values of the capillary constants. During the establishment of equilibrium after a disturbance, bands are seen due to variable thickness, and when the layer is thin, persist for a considerable time.

When the thickness of the layer is increased beyond a certain point, the difficulty above discussed, depending upon obliquity, becomes excessive, and it is advisable to change the manner of observation to that adopted by Michelson. In this case the eye is focused, not, as before, upon the operative surfaces, but upon the flame, or rather upon its image at E (Fig. 2). For this purpose it is only necessary to introduce an eye-piece of low power, which with the lens C (in its second operation) may be regarded as a telescope. The bands now seen depend entirely upon obliquity according to the formula above written, and therefore take the form of circular arcs. Since the thickness of the layer is absolutely constant, there is nothing to interfere with the perfection of the bands except want of homogeneity in the light.

But, as Fizeau found many years ago, the latter difficulty soon becomes serious. At a very moderate thickness it becomes necessary to reduce the supply of soda, and even with a very feeble flame a limit is soon reached. When the thickness was pushed as far as possible, the retardation, calculated from the volume of liquid and the diameter of the vessel, was found to be 50,000 wave-lengths, almost exactly the limit fixed by Fizeau.

To carry the experiment further requires still more homogeneous sources of light. It is well known that Michelson has recently observed interference with retardations previously unheard of, and with the aid of an instrument of ingenious construction has obtained most interesting information with respect to the structure of various spectral lines.

A curious observation respecting the action of hydrofluoric acid upon polished glass surfaces was mentioned in conclusion. After the operation of the acid the surfaces appear to be covered with fine scratches, in a manner which at first suggested the idea that the glass had been left in a specially tender condition, and had become scratched during the subsequent wiping. But it soon appeared that the effect was a *development* of scratches previously existent in a latent state. Thus parallel lines ruled with a knife edge, at first invisible even in a favourable light, became conspicuous after treatment with acid. Perhaps the simplest way of regarding the matter is to consider the case of a furrow with perpendicular sides and a flat bottom. If the acid may be supposed to eat in equally in all directions, the effect will be to *broaden* the furrow, while the depth remains unaltered. It is possible that this method might be employed with advantage to *intensify* (if a photographic term may be permitted) gratings ruled upon glass for the formation of spectra.

GENERAL MONTHLY MEETING,

Monday, April 10, 1893.

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

W. H. Broadbent, M.D.
Henry C. J. Bunbury, Esq.
William Flockhart, Esq.
Francis Gaskell, Esq.
George W. Hemming, Esq. Q.C.
Colin Charles Hood, Esq.
Charles Langdon-Davies, Esq.
B. W. Levy, Esq.
Mrs. W. Rathbone,
Granville R. Ryder, Esq.
Mrs. Sharpe,
F. W. Watkin, Esq. M.A. 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 at low temperatures:—

Alexander Siemens, Esq.	£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

The Secretary of State for India—Archæological Survey of India, New Series, Vol. I. 4to. 1889.
Report on Public Instruction in Bengal, 1891–2. fol. 1892.
The Lords of the Admiralty—Nautical Almanac Circular, No. 14. 8vo. 1893.

Accademia dei Lincei, Reale, Roma—Atti, Serie Quinta: Rendiconti. Classe di Scienze fisiche matematiche e naturali. 1° Semestre, Vol. II. Fasc. 3-5. 8vo. 1893.

Rendiconti, Serie Quinta, Classe di Scienze Morali, Storiche e Filologiche, Vol. II. Fasc. 1. 8vo. 1893.

Agricultural Society of England, Royal—Journal, Third Series, Vol. II. Part 1. 8vo. 1893.

American Association for Advancement of Science—Proceedings, 41st Meeting, Rochester. 8vo. 1892.

Astronomical Society, Royal—Monthly Notices, Vol. LIII. No. 4. 8vo. 1893.

Bankers, Institute of—Journal, Vol. XIV. Part 3 8vo. 1893.

Birmingham Philosophical Society—Proceedings, Vol. VIII. Part 1. 8vo. 1891-2.

British Architects, Royal Institute of—Proceedings, 1892-3, Nos. 11, 12. 4to.

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Carpenter, H. S. Esq. (the Editor)—Bourne's Handy Assurance Manual. 8vo. 1893.

Chemical Industry, Society of—Journal, Vol. XII. No. 2. 8vo. 1893.

Chemical Society—Journal for March, 1893. 8vo.

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Editors—American Journal of Science for March, 1893. 8vo.
Analyst for March, 1893. 8vo.
Athenæum for March, 1893. 4to.
Author for March, 1893.
Chemical News for March, 1893. 4to.
Chemist and Druggist for March, 1893. 8vo.
Electrical Engineer for March, 1893. fol.
Electric Plant for March, 1893. 8vo.
Engineer for March, 1893. fol.
Engineering for March, 1893. fol.
Horological Journal for March, 1893. 8vo.
Industries for March, 1893. fol.
Iron for March, 1893. 4to.
Ironmongery for March, 1893. 4to.
Lightning for March, 1893. 8vo.
Monist for March, 1893. 8vo.
Nature for March, 1893. 4to.
Open Court for March, 1893. 4to.
Photographic Work for March, 1893. 8vo.
Surveyor for March, 1893. 8vo.
Telegraphic Journal for March, 1893. fol.
Transport for March, 1893. fol.
Zoophilist for March, 1893. 4to.

East India Association—Journal, Vol. XXV. No. 1. 8vo. 1893.

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WEEKLY EVENING MEETING,

Friday, April 14, 1893.

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

Sir William H. Flower, K.C.B. D.C.L. LL.D. D.Sc. F.R.S.

Seals.

Sir William Flower began by recalling that about two years ago Lord Salisbury, while taking a comprehensive survey of the general state of international politics, remarked that there were, happily, then, no graver subjects to excite the anger and jealousy of rival nations than seals and lobsters, and it was to the study of the habits of these animals that the energies of diplomatists were mainly directed. In the present lecture it was proposed to speak only of seals, and, taking the common seal as a type, he described its general character and position in the Animal Kingdom. The lecturer then passed in review the distinctive traits and geographical distribution of the various allied species of true seals, and pointed out their economic uses and mode of capture. The next animal treated of was the walrus, and finally, the third group into which the seals are divided—the eared-seals, sea-lions, or sea-bears. These differ from the true seals in possessing small external ears, and in the power of using their hind feet in walking on land like ordinary quadrupeds. It is animals of this group which yield the beautiful fur called "sealskin" in commerce, and the lecture was illustrated by specimens of this fur in various stages of preparation. The wholesale destruction of fur-seals which formerly went on throughout the southern hemisphere was next spoken of, and a more detailed account given of the very remarkable habits of the species from the Behring Sea, which for many years has been the main source of supply of sealskin dresses, and the right of capture of which is now the subject of controversy between Great Britain and the United States. After giving an outline of the questions, as far as they related to the natural history of the animals, to be placed before the arbitrators, he concluded by saying that we can scarcely be too grateful to the statesmen of both nations for having so far agreed as to bring the whole of this difficult and complicated question before such a tribunal as that now sitting at Paris.

[W. H. F.]

WEEKLY EVENING MEETING,

Friday, April 21, 1893.

David Edward Hughes, Esq. F.R.S. Vice-President, in the Chair.

Professor Alex. B. W. Kennedy, F.R.S. *M.R.I.*

Possible and Impossible Economies in the Utilisation of Energy.

The importance of the subject is not difficult to understand if you realise the enormous results—so large as to be even of national interest—which depend upon what can and what can not be done in the way of utilising energy. Economy in energy may mean wealth and prosperity to a nation; waste in energy may mean diminished commerce and general depression.

As an engineer, I am bound to take my stand at once on the firm basis that practically nothing is impossible except perpetual motion. In essence all the things which I shall have to characterise as *impossible* are really perpetual motions, or what is the same thing, attempts to get more out of something than there is in it.

It is familiar to even the least mechanical in this room that there are, in Nature, vast—I dare not say inexhaustible—sources from which we can obtain energy by certain more or less familiar processes. I say "obtain" energy, using the most familiar expression, but perhaps the word is not a very happy one. We require energy to light lamps, to smelt metals, to drive factories, to pull trains, but in no case do we obtain it ready made as we draw water from a well. There is plenty of it in existence, plenty to be had, but to get it in the form in which we want it we have always to transform it from some other form to the required one.

There is in every electric lighting station in this city, for instance, first a transformation of the natural energy of chemical combination into heat, then the transformation of heat energy into mechanical energy in a steam engine, then the transformation of the mechanical energy into electrical energy in the dynamo, and lastly the transformation of electrical energy into light, and also very largely back again into heat, in the lamp.

Often the transformations are not so numerous as in this case, but they always exist to some extent, and all the possibilities and impossibilities of which I have to speak are practically related to some or other of these transformations. About them are three fundamental facts which I will ask you to note.

1. Every transformation of energy is accompanied by some waste of energy.

2. Every transformation occurs in absolute accordance with certain phenomena which reproduce themselves so exactly that they have come to be called *physical laws.*

3. All the quantities which can appear in every transformation are as exactly measurable and have been as exactly measured as could be the temperature of this room, or the breadth of Albemarle Street.

As to the first point, I use the expression "waste" rather than "loss" because the energy cannot well be said to be lost. The amount of energy wasted in a transformation is measured by noting what is called the efficiency of the transformation, which is usually expressed as a percentage. A transformation with only one per cent. loss, for example, would be said to have an efficiency of 99 per cent., and so on. Perpetual motions are simply transformations of 100 per cent. efficiency.

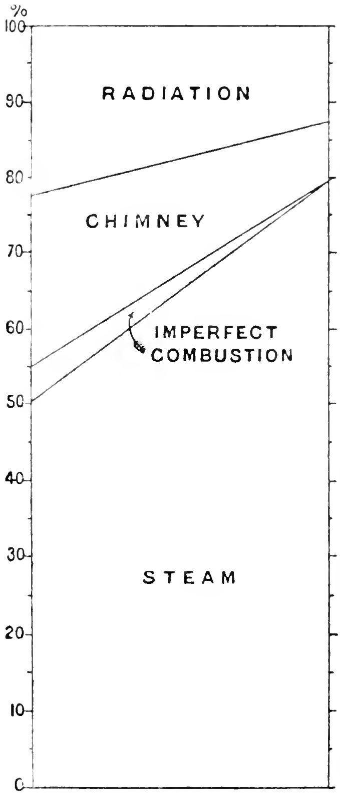
In spite of the great and admitted imperfection of our knowledge of the physical universe, I think it is not impossible to arrive at some understanding as to how far the knowledge which we have can be held to be final, and how far or in what directions it is provisional. I mean, of course, so far as affects our particular object of saying what is, and what is not, possible in one special direction.

It seems perhaps contradictory, but I think it is true, that our superstructure in physical knowledge is much more solid than our foundation. Our knowledge of the ultimate constitution of matter, and particularly of the ultimate constitution of "not-matter," cannot be said to be accurate as yet. But higher up in the scale matters are different, and it is as well that we should remember that there are physico-chemical facts and numbers that are just as fully established, although they refer to phenomena which we cannot entirely follow, as are the figures of the Nautical Almanac.

Let me take the combustion of coal as an example with the external phenomena of which we are all familiar. Chemists have analysed the coal and know of what it consists; so much carbon, so much hydrogen, so much oxygen; they have measured for us that its elements in the combustion enter into various combinations, so that they can tell us exactly the maximum amount of heat energy which can be given out by the burning of a given weight of coal. Yet I have often enough met people who were quite convinced that they possessed the secret by which an amount of heat equal to double this quantity could somehow be obtained. The finality of our knowledge in this respect is quite independent of our ignorance of the ultimate nature of the coal or of the combustion.

I have taken the phenomena of combustion as among those with which every one is fairly familiar. Let us deal with it for a few minutes further in special illustration of my subject. Fig. 1 shows fairly accurately how far we have got in working at the efficiency of this kind of transformation. We start with a given amount of potential energy, known within very narrow limits, represented for us by a pound of coal with a sufficient weight of circumambient atmosphere. Let us represent this known amount of energy, as in the figure, by 100 per cent. What becomes of all this if the coal be burnt in the furnace, say, of an ordinary boiler, and if we endeavour to utilise this combustion energy in the conversion of water into steam? On the left hand of my diagram you may see what often enough happens in every-day careless working; on the right hand you may see what happens in thoroughly good working with real care, but without any special apparatus. You will see that the amount of heat taken up by the steam varies from 50 to 80 per cent. of the whole heat; that a small amount, from 5 per cent. down to nothing, is lost in imperfect combustion, that is, in the formation of carbonic oxide; that a very much larger amount goes up the chimney, having been expended in heating the waste gases; and that finally another large amount is purely wasted, being lost in radiation and otherwise. Now I ask you to notice particularly two things, first, looking at the right hand of the diagram, how much there is still possible. The efficiency of the process is about 80 per cent. The remaining 20 per cent. is all that can be saved. Of this some portion must go in heating the chimney gases. It is the price paid for the draught of the chimney. We must also lose something by radiation; that is inevitable. We cannot look, therefore, to any very astounding increase of economy in boiler work over the best that has been done. Secondly, I ask you to notice what an enormous amount is

Fig. 1.*



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* The blocks illustrating this abstract have been kindly lent by the Editor of the *Electrician.*

possible on the left-hand side of the diagram which is impossible on the right—impossible, that is, because it has already been attained. Half of our possibilities—indeed, far more than half our possibilities—are of improving up to the best; it is infinitely harder to improve up from the best.

When any one tells us that he has invented something, or some method, by which one ton of coal goes as far as two, we may know for certain one of two things; either he has made a mistake (which is possible enough) or else his standard of comparison has been unfortunately chosen. Hundreds of thousands of pounds have been thrown away on elaborate schemes which, at best, could do no more than bring bad and careless practice up to a level passed every day in places where care and common sense have been expended on the same matter. Plenty of room exists for raising the general average efficiency of boiler work; for, if the average working all over the country were brought up to the standard of the best, there would, probably, be one-third less coal used every year than is now actually burnt.

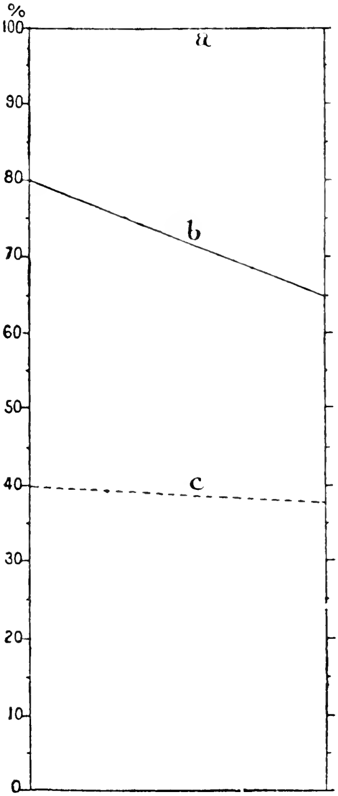
The process which we have discussed is, perhaps, really rather to be called a transference than a transformation; at any rate, it falls into the category of transformation whose theoretical maximum efficiency is 100 per cent., or, allowing for absolutely inevitable losses, perhaps 90 per cent. But with many processes with which we have to do, unfortunately, our maximum theoretical efficiency is only 25 per cent., and instead of attaining 80 per cent. even of this we are often happy enough with half as much.

In the very great majority of instances the mechanical energy which we utilise is originally obtained from the heat of combustion, transferred to some liquid or gaseous body, and by it made to cause certain parts in a machine to move, and to do mechanical work for us.

Now the actual cycle of physical process, isothermal, adiabatic, isodynamic or what not, which the steam or air or gas may go through is often a very complicated one, sometimes so complicated that it is a matter of considerable difficulty to say beforehand exactly what the maximum theoretical efficiency is. We always, however, know two things about it: first, that it is always far less than 100 per cent.; second, that it cannot exceed—and in all practical cases must fall considerably short of—a certain known limiting value much less than 100 per cent. This limiting value is the very familiar one which is written $\frac{T_1 - T_2}{T_1}$. Here T_1 stands for the highest temperature, measured above absolute zero, at which the working fluid receives heat. T_2 stands for the lowest temperature at which the fluid parts with its heat. The difference between the two temperatures, $T_1 - T_2$, is the working difference of temperature. The value of the ratio which I have just given is always much less than unity, and must always be so. In the case of a modern steam engine its value is often about 30 per cent., in the case of a gas engine from 60 to 65 per cent.

In any actually possible steam engine the actual process differs so much from the ideal that not more than 20 or 25 per cent. (out of the 30 per cent. just mentioned) could be attained even if the process were carried out perfectly. But there are very great difficulties in the way of carrying out even this imperfect process at all completely, and so it comes about that, in the final results, only from 5 to perhaps 15 per cent. of the whole heat of the steam is ever turned into work, sometimes a little more, more often a little less.

Fig. 2.



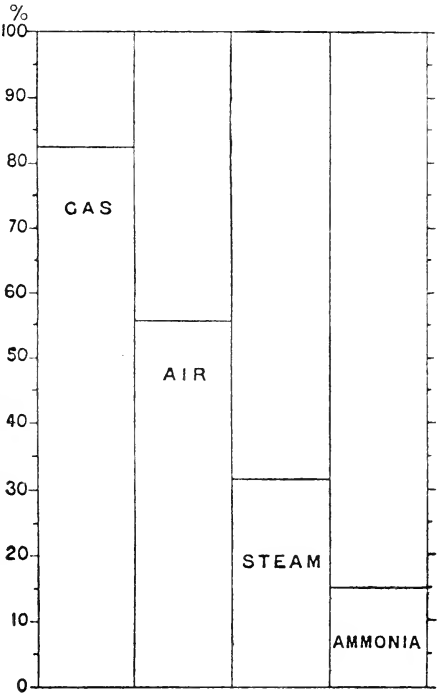
In Fig. 2 I have represented by 100 per cent., not the whole heat given to the steam, but that fraction of it (25 or 30 per cent.) which it is physically conceivable that any actual engine should turn into work.

The space between *a* and *b* shows the losses due to the fact that the engine works in a cycle far inferior to the best possible cycle. The space between *b* and *c* shows the further losses which do actually occur in fact, the area under *c* being all that is utilised.

What possibilities are there of increasing the theoretical maximum efficiency of an engine? The whole matter depends upon whether we can increase the value of the fraction $\frac{T_1 - T_2}{T_1}$. We can do this obviously by either making T_1 higher or T_2 lower, or both. It is not difficult to see how this matter comes out practically. In all heat engines some fluid or other is used as a vehicle for the transformation of heat into work. It may be coal gas or producer gas, steam, air alone, or air mixed with the products of combustion, or even ether or ammonia. With steam we work between the temperatures of, say, 60° or 400° F., or thereabouts. Quite at the other end of the scale come engines worked by gas of different kinds, where combustion actually takes place inside the engine and not in a furnace, and where the highest temperature may, perhaps, be as great as 2000° F. Hot air occupies an intermediate position. With ether or ammonia the temperatures are all very much lower. From our present point of view it is necessary to remember that all these divers working fluids work under the same physical laws and limitations. How, then, do they stand in reference to this problem of economy?

First of all, the mere nature of no one gives it any thermo-dynamic advantage over the others. The fact that steam can be liquefied in a condenser is a great convenience to us, but does not make it, *per se*, one whit worse or better than air, which cannot be liquefied—at least in a condenser. The manner in which the choice of fluid affects the value of the maximum possible thermodynamic efficiency is indicated in Fig. 3. With gas, the highest temperature is the temperature of combustion; the theoretical maximum efficiency is on this account already extremely high. With air, the temperature is only indirectly derived from the temperature of combustion, and the maximum efficiency is smaller, although it is still high. With steam (apart from super-heating) the temperature is dependent upon, and limited by, considerations of pressure, and consequently of safety. With ammonia it is similarly limited; but the engine lies much further down the scale, both as to T_1 and as to T_2.

Fig. 3.



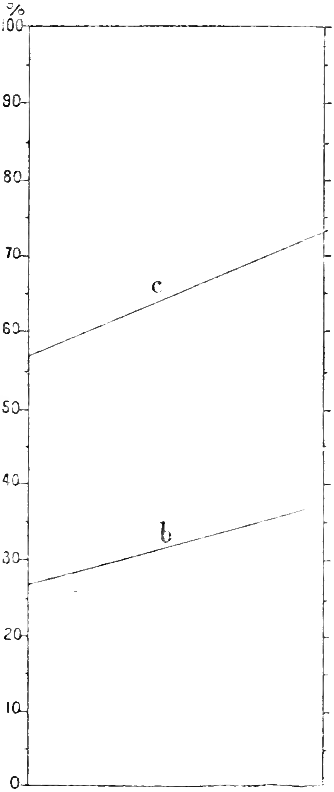
From Fig. 3 we may gather that with gas engines the theoretical efficiency is already so high that we need hardly trouble ourselves about attempting to raise it. With gas, in fact, and to a smaller extent with air, the possibility of improvement lies in bringing the actual up to the theoretical process, and not in attempting to raise the efficiency of the latter. With steam, however, it is different. We want much to raise the theoretical limit of efficiency. But here we are dealing with a material which is liquid at ordinary temperatures and pressures, so that in its working condition it is a vapour and not a gas, and its temperature cannot be raised without at the same time raising its pressure. Considerations of safety and strength of our materials become here very important, but even if we left them out of account altogether, and raised the value of the maximum working pressure of steam engines from its present limit of 10 atmospheres to 20 atmospheres—that is, 100 per cent.—we should have increased the theoretical maximum efficiency only about 10 per cent., a quantity hardly worth considering in such a case.

No doubt the direction in which to seek for improvement is in that of what is called super-heating the steam, or raising its temperature after it has been formed—converting the vapour into gas without increasing its pressure. Theoretically this can be done to any extent, and I have no doubt that within the next coming years it will be very largely done. It is no new idea, although it is only recently that the use of mineral lubricants in engines has made it thoroughly practicable. The losses between *b* and *c* in Fig. 2 are due to many causes, but chiefly to two. The first of these is that the steam is thrown away at too high a pressure—i.e. that it is not expanded sufficiently far in the cylinder. Mechanically this is remediable at once, but only at the cost of making the engine unduly large and costly for its work. This cause of loss is, therefore, likely to remain. The second is one about which there has been much controversy both here and abroad, but which, thanks to the work of such men as Willans and Cotterill and Donkin, is now much cleared up. It is simply this—that as the fresh hot steam is always admitted to a cylinder which has just been emptied of steam having a much lower temperature, a cylinder, moreover, which is made of excellently conducting material, a very large proportion of that steam is at once converted into water on entrance, so that for every cubic foot of steam which leaves the boiler and passes along the pipes perhaps only two-thirds, or even half or less, does work in the cylinder as steam; the rest passes through the engine as water. It is sometimes partially re-evaporated, but never in such fashion or at such time as to be of much real service in doing work. Here truly is a field for economy, and one with very great possibilities.

I have talked long about steam engines. The subject is tempting, at least tempting to me, and, after all, steam is still the working fluid *par excellence.* But I must not forget that my subject has many branches, and must look at some of the others.

The future of gas engines is one which has great possibilities; we have seen that they represent, in fact, the highest existing theoretical maximum efficiency. Up to a certain point their progress was astonishingly rapid; at present, for a few years, they have more or less stood still, although the number of them has continually multiplied. Just now there are signs that the manufacturers—just possibly it may have something to do with the pressure of severe competition—are going to do their best to move forward a little, to make larger and faster running machines, and, in fact, to make an effort to penetrate further into the country where the steam engine has for so long held undisputed sway. About this we must not concern ourselves here, however. The maximum theoretical efficiency of a gas engine is about 80 per cent., or nearly three times as great as in a steam engine. It is obvious that this figure is so high that we need hardly attempt to raise it, especially as we are so far from actually realising it as yet. In Fig. 4 the theoretical maximum efficiency is taken as 100 per cent., in the same way as in Fig. 2, and the line *b* shows how much of this is actually turned into work, the area above *b* representing the lost energy. The greatest cause of loss, that above the line *c*, is represented by the heat taken from the water surrounding the cylinder. The fact is that we are trying to obtain incompatible results. To reach the high efficiency we make the initial temperature very high. But, then, any such temperature would melt up our machines altogether, if the metal were only allowed to reach it. We have, therefore, to adopt the somewhat barbarous expedient of continually keeping the metal cool by a current of water passing through a jacket. This water must of necessity pick up all the heat which can get through the metal and carry it away to waste. Although, therefore, our theoretical maximum efficiency is so much greater than that of a steam engine, our actual efficiency is not nearly so great (comparing the lines *c* in Fig. 2 and *b* in Fig. 4). Notwithstanding this, the actual energy utilised per thermal unit of combustion heat in a gas engine is very considerably greater than in a steam engine. Undoubtedly, very great possibilities for increased economies exist here.

Fig. 4.



I have reserved to the end some discussion of possibilities and impossibilities in connection with some complete industrial processes. Let us take first the generation and distribution of electricity, a matter which is of such keen interest now to so many of us, whether from the point of view of economy in production or economy in our quarter's bills!

The various stages of Fig. 5 tell a tale which, perhaps, may interest you. They represent the gradually degenerative process by which the chemical energy of combustion is converted into electric light. To the left hand of the diagram is represented the boiler process, and the various transformations are represented from left to right, until the lamps are arrived at on the right hand of the figure. Similar letters are used in each section of the figure for similar quantities of energy. In each section the losses of the section before are written off, and the heat actually carried forward is called 100 per cent. K represents the losses in the boiler, which is assumed to have an efficiency of 80 per cent. H represents waste mainly due to condensation in steam pipes, and also to the driving of pumps, and other such losses inevitable in a central station. The third section represents the whole heat which has been received by that portion of the steam which actually found its way to the main engines, and of this, G is the part unavoidably lost from thermodynamic limitations. In Section IV. the heat actually received by the engine is called 100 per cent. Of this, the area F is wasted, and the remainder turned into work. Section V. shows the whole heat turned into work by the engine as 100 per cent., of which the area E represents the energy necessary to drive the engine itself, which, therefore, never reaches the dynamo. In Section VI. the energy received by the dynamo is taken as 100 per cent., and the area D represents the dynamo losses, which are wonderfully small. The energy—now in the form of electrical energy—which leaves the terminals of the dynamo, is represented by 100 per cent. in Section VII. Of this, a certain proportion, sometimes small, sometimes large, but here represented by C, is expended in mains, transformers, or batteries, and never reaches the lamps. Finally, at the right-hand side of the figure we get the energy received by the lamps as 100 per cent., out of which only the small, almost insignificant quantity A is turned into light, the huge remainder B being once more converted into heat. If the size of the area A in Section I. be looked at, it will be seen what an insignificant quantity of the whole heat of combustion is actually and finally turned into its intended purpose.

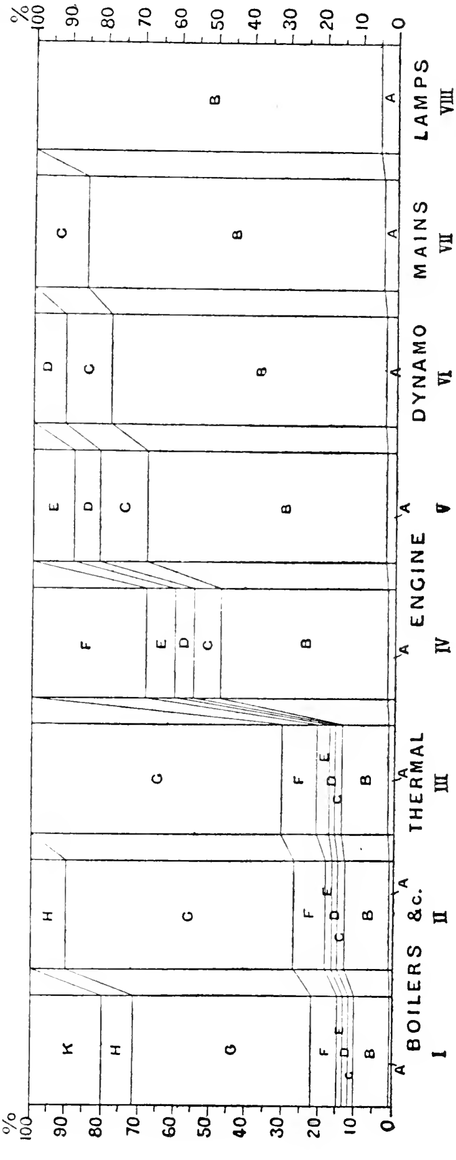
The result savours a little of the ludicrous. Let us go back and review its possibilities. The losses K, I need not deal further with. H represents losses about which we engineers feel very sore, and which sometimes try our temper and our patience greatly, and which are particularly persistent and hard to get rid of. G and F, I have already dealt with. E and D do not look very promising as fields for radical improvement. There are few things

I think more creditable of modern engineers than the smallness of the losses represented by D.

I do not think that the distribution losses C are likely to be reduced to any very sensational extent below the figure at which I have put them. We frequently hear of their almost negligible magnitude, but whenever they get really measured, the sum of a number of things which separately are supposed to be so minute appears to be anything but negligible. No doubt the 15 per cent. loss will be gradually driven back to 10 per cent.; but it will be hard work, and will come about only by degrees and by care and pains in detail, not by any new system of distribution or by any striking invention.

But truly in our last stage we have got one in which improvement is needed and in which we all hope that enormous improvement is possible. Moreover, here it can no longer be said that improvement is a matter of detail and of common sense only. On the contrary, here is a case where we know beforehand that we may any day be surprised by a discovery, on the part of one of the many men who are

Fig. 5.



devoting time and thought to the matter, which may even enable us to multiply many fold the amount of light which we can obtain from a given quantity of energy.

There is yet another direction in which possible economy is to be looked for, a very fascinating one, and by no means an unpromising one. Look for a moment again at Fig. 5. Except only the process at the lamp itself, all the transformations have fair economy, so good that one sees at once that no radical defect exists in them. But cannot some of these be done away with altogether? It is the number of them that tells, and brings down the final result.

If an ordinary gas engine be substituted for a steam engine we cut out one transformation altogether. The boiler losses disappear, or rather, such corresponding losses as exist occur in the gasworks. At the same time we substitute the higher efficiency of the gas engine for the lower of the steam engine, which may be a very important matter. It is practically equivalent to cutting G out altogether.

Of another kind is the possibility at present much talked of, the substitution namely, not only of a gas engine for a steam engine, but at the same time also of a gas producer for a boiler, so that the motor fluid should be producer gas made on the premises and not steam or coal gas from public mains. There seems no doubt that the combination, although it does not much reduce the number of transformations, gives under certain conditions a very high economy of fuel indeed. I do not think the evidence before us is as yet sufficient, although I hope it shortly will be, to enable us to say how far under ordinary working conditions the actual combined efficiency of the whole plant will be distinctly greater than that of existing systems.

[The lecturer dealt with the question of the utilisation of dustbin refuse, of the economic efficiency of electric tramways and of compressed air transmission, illustrating these by diagrams. He discussed also the effect of "load factor" on economic problems. He then concluded as follows:—]

To sum up the whole matter in the way of possibilities and impossibilities, there does not seem to be anything very startling before us in the way of possible economies, except in the two directions of efficiency of lamps as light producers, and of bringing up gas engines to their theoretical maximum. In other respects matters are running along lines which I have endeavoured to indicate, and along which they will doubtless develop more or less rapidly, but always less and less rapidly as they get nearer their limiting efficiency. There is no one point in which we have not some measurements which enable us to set bounds to the possibility of improvement along any known lines, and thus we have means for gauging the value of the pretensions made by each new method, or scheme, or invention, as it appears—not merely guessing at it, but actually estimating its possible value numerically. For those of us who are not born to be inventive geniuses there is always the consoling thought that the difference between good engineering and bad engineering in economy, with the very same materials, is very much greater than the difference between good engineering and any probable improvements upon it. And meantime, we find our hands sufficiently full in trying to keep up to the best existing standards, pending the time when Messieurs the discoverers show us how to get on a little further.

[A. B. W. K.]

WEEKLY EVENING MEETING,

Friday, April 28, 1893.

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

Francis Gotch, Esq. M.A. F.R.S.

The Transmission of a Nervous Impulse.

The lecturer opened with a short account of the present state of our knowledge as to the anatomical structures of nerve-fibres.

He then described and repeated the experiments of Helmholtz, made 50 years ago, with the object of ascertaining the rate at which a nervous impulse was conducted along a nerve-fibre. These experiments form the basis of our more exact knowledge as to that capacity for transmission which is the peculiar vital function of nerves.

The essential feature in the process is the power which each individual part of the living nerve-fibre possesses of awakening in response to a sudden change in its physical environment, this property being expressed by the term "excitability"; in the transmission of the nervous impulse each successive individual part awakens in consequence of the subtle changes present in its aroused neighbours. The awakening thus travels along the nerve as a flame along a fuse.

The power of transmitting a change, and the power of initiating such change in response to a stimulus, in other words, conduction and excitability, are thus brought into correlation.

The lecturer then proceeded to demonstrate and describe experiments carried out by Mr. J. S. Macdonald and himself, in the Physiological Laboratory of University College, Liverpool, upon this subject, these experiments having been made in order to ascertain how far these two properties, (*a*) of responding to an external stimulus, (*b*) of transmitting the nerve impulse started by such a stimulus, could be considered as identical.

In order to ascertain this, an agent was used to modify, on the one hand, the capacity of the nerve to be aroused by physical agencies, and on the other, its power of transmitting an impulse when aroused. The agent employed was a localised alteration in temperature, and experiments were described and demonstrated which showed that whereas cooling to 5° C. tended to block the transmission, such cooling, far from rendering the nerve less responsive to external stimuli, made it more readily affected by the stimulating influence of a large number of physical agencies. Such agencies were shown to be (1) galvanic currents, (2) condenser discharges, (3) mechanical blows, (4) chemical reagents. To all these the nerve responded better when cooled, though it transmitted the nerve impulse produced by such response with greater difficulty. To one agent only did the nerve respond less readily when under the influence of localised cold; this was the induced electrical current.

It thus appears necessary to reconstruct our view of the nature of the process during nerve transmission, for the two events in the nerve, the response to external stimuli and the power to transmit such response, are affected in a diametrically opposed manner by such a simple change as alteration in the nerve's temperature. The favourable influence of localised cold on the response of excitable tissues to external stimulation was further displayed by description and demonstration of the effects produced when muscles, and not nerves, were the objects of experiment. In all cases cold favoured the capacity of the muscle to reply to the stimulus.

Finally, the lecturer brought forward some observations which appeared to show that in addition the transmitting power of a nerve is largely affected by the nature of the agent which started the nerve impulse. We have found it possible to arouse a nerve by a galvanic current in two ways: (1) so that localised cooling of a portion of the conducting path will favour the passage of the impulse (the normal condition), and (2) so that the same localised cooling will block the impulse. It would thus seem that nervous impulses, when started on their journey along nerves, bear throughout that journey some impress of the agent which started them, and hence, that the impulses which are initiated by even slightly different physical agencies, and are then transmitted along nerve-fibres, differ from one another as regards the character of some fundamental quality.

Professor Gotch concluded that these and other recent observations gave experimental proof that the property of transmission possessed by nerves is correlated, not merely with that of excitability, but largely with the source, and thus the nature of the impulse, so that the unknown molecular changes which form the living basis of such transmission in any one nerve-fibre are not the same for all impulses, but change with the source of each.

[F. G.]

ANNUAL MEETING,

Monday, May 1, 1893.

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 1892, testifying to the continued prosperity and efficient management of the Institution, was read and adopted. The Real and Funded Property now amounts to above 104,000*l.* entirely derived from the Contributions and Donations of the Members and of others appreciating the value of the work of the Institution.

Sixty-three new Members were elected in 1892.

Sixty-three Lectures and Twenty Evening Discourses were delivered in 1892.

The Books and Pamphlets presented in 1892 amounted to about 238 volumes, making, with 530 volumes (including Periodicals bound) purchased by the Managers, a total of 768 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. LL.D. F.R.S. M. Inst. C.E.

Managers.

Captain W. de W. Abney, C.B. R.E. D.C.L. F.R.S.
Shelford Bidwell, Esq. M.A. F.R.S.
John Birkett, Esq. F.R.C.S.
Joseph Brown, Esq. C.B. Q.C.
Sir Douglas Galton, K.C.B. D.C.L. LL.D. F.R.S.
David Edward Hughes, Esq. F.R.S.
Alfred Bray Kempe, Esq. M.A. F.R.S.
George Matthey, Esq. F.R.S.
Hugo Müller, Esq. Ph.D. F.R.S.
The Right Hon. Earl Percy, F.S.A.
William Chandler Roberts-Austen, Esq. C.B. F.R.S.
Sir David Salomons, Bart. M.A. F.R.A.S. F.C.S.
Alexander Siemens, Esq. M. Inst. C.E.
Basil Woodd Smith, Esq. F.R.A.S. F.S.A.
Sir Richard Webster, M.P. Q.C. LL.D.

Visitors.

Charles Edward Beevor, M.D. F.R.C.P.
Henry Arthur Blyth, Esq.
Francis Woodhouse Braine, Esq. F.R.C.S.
John Tomlinson Brunner, Esq. M.P.
Michael Carteighe, Esq. F.C.S.
Rookes Evelyn Crompton, Esq. M. Inst. C.E.
James Farmer, Esq. J.P.
Robert Hannah, Esq.
Donald William Charles Hood, M.D. F.R.C.P.
Raphael Meldola, Esq. F.R.S.
Lachlan Mackintosh Rate, Esq. M.A.
Boverton Redwood, Esq. F.C.S.
John Callander Ross, Esq.
John Bell Sedgwick, Esq. J.P. F.R.G.S.
George Andrew Spottiswoode, Esq.

WEEKLY EVENING MEETING,

Friday, May 5, 1893.

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

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

Fogs, Clouds and Lightning.

The air, as every one knows, is composed almost entirely of the two gases, oxygen and nitrogen. It also contains small quantities of other substances, of which the chief are carbonic acid gas and water vapour, and it is the latter of these constituents, water vapour, or "steam" as it is sometimes called, that will principally concern us this evening.

The quantity of invisible water vapour which the air can at any time take up depends upon the temperature; the higher the temperature of the air the more water it can contain. The proportion, however, never exceeds a few grains' weight of water to a cubic foot of air. Air at any temperature, containing as much water as it can possibly hold, is said to be "saturated," while the temperature at which air containing a certain proportion of water becomes saturated is called the "dew point."

The water vapour contained in the atmosphere plays a very important part in many natural phenomena. Among other things, it is the origin of clouds and of fogs. If a body of air containing water in the form of invisible vapour is quickly cooled to a temperature below its dew point, a portion of the vapour becomes condensed into a number of minute liquid particles of water, forming a visible mist, which, when it is suspended in the upper regions of the air, is called a cloud, and when it rests upon the surface of the earth is only too familiarly known as a fog.

The cooling of water-laden air may be brought about in various ways, resulting in the formation of clouds of several distinct characters. [Photographic examples of cumulus, stratus and cirrus clouds were exhibited upon the screen.] For experimental purposes a small body of air may be most conveniently cooled by allowing it to expand. I have here a flask of air which can be connected with the partially exhausted receiver of an air-pump. Inside the flask is an electrical thermometer or thermo-junction, the indications of which can be rendered evident to all present by the movement of a spot of light upon a scale attached to the wall. A deflection of the spot of light to the left indicates cold, to the right, heat. When the stop-cock is opened so that a portion of the air escapes from the flask into the air-pump receiver, you see at once a violent movement of the spot of light to the left, showing that the expansion of the air is accompanied by a fall of temperature. If more air from the room is allowed to enter the flask, the spot moves in the opposite direction.

The large glass globe, upon which the beam from the electric lantern is now directed, contains ordinary air, kept in a state of saturation, or nearly so, by the presence of a little water. You will observe that although heavily laden with water vapour the air is perfectly transparent. If, now, we turn a tap and so connect the globe with the exhausted receiver, the air expands and becomes colder; the space inside the globe is no longer able to hold the same quantity of water as before in the form of vapour, and the excess is precipitated as very finely divided liquid water—water dust it may be called—which fills the globe and is perfectly visible as a cloud or mist. In a few minutes the cloud disappears, partly, no doubt, because some of the particles of water have fallen to the bottom of the vessel, but chiefly because the air becomes in time warmed up to its original temperature (that of the room), and the suspended water is converted back again into invisible vapour.

Now let us repeat the experiment, and before the cloud has time to disperse let us admit some fresh air from outside; the cloud, as you see, vanishes in an instant. The compression of the air raises the temperature above the dew point, and the small floating particles of water are transformed into invisible vapour.

I once more rarefy the air, and admit a fresh supply while holding the flame of a spirit-lamp near the orifice of the inlet pipe, so that some of the burnt air is carried into the interior of the globe. When the air is again expanded a cloud is formed which is, as you observe, far more dense than the others were. It appears on examination that the increased density of this cloud is not due to the condensation of a greater quantity of water. Little, if any, more water is precipitated than before. But the water particles are now much more numerous, their increased number being compensated for by diminished size. Within certain limits, the greater the number of particles into which a given quantity of water is condensed, the greater will be the apparent thickness of the mist produced. A few large drops will not impede and scatter light to the same extent as a great number of small ones, though the actual quantity of condensed water may be the same in each case.

Then comes the question, why should the burnt air from the flame so greatly increase the number of the condensed drops? An answer, though perhaps not quite a complete one, is furnished by some remarkable experiments made by M. Coulier, a French professor, nearly twenty years ago. He believed his experiments pointed to the conclusion that water vapour would not condense at all, even at temperatures far below the dew point, unless there were present in the air a number of material particles to serve as nuclei around which the condensation could take place. All air, he says, contains dust; by which term he does not mean such dust as is rendered evident in this room by the light scattered along the track of the beam issuing from the electric lantern, which consists of comparatively gross lumps of matter, but particles of ultra-microscopical dimensions, "more tenuous than the motes seen in a sunbeam." It is upon such minute specks of matter that water vapour is condensed. Anything that increased the number of dust particles in the air increased the density of the condensation by affording a greater number of nuclei. Air in which a flame had been burnt he supposed to be very highly charged with finely-divided matter, the products of combustion, and thus rendered extraordinarily "active" in bringing about condensation. And that, according to Coulier's view, is the reason why such a dense fog was formed when air which had been contaminated by the spirit flame was admitted to our globe.

On the other hand, air, even burnt air, which has been filtered through tightly packed cotton wool, is found to be perfectly inactive. No cloud or mist will form in it, however highly it may be supersaturated. Coulier explained this fact by supposing that the process of filtration completely removed all dust particles from the air.

On the table before you is a globe containing air which has been thus treated, and which is kept saturated by a little water. When this globe is connected with the exhausted receiver, no trace of any mist is produced: the air remains perfectly clear. We will now admit a little of the ordinary air from outside, and again cool it by expansion. Quite a respectable cloud is thereupon formed in the globe.

The experiments of Coulier were repeated and confirmed by Mascart. The latter also made one additional observation which may very probably turn out to be of great importance. He found that ozone, or rather, strongly ozonised air, was a very active mist producer, and that unlike ordinary air, it was not deprived of its activity by filtration.

Four or five years later, all the facts which had been noticed by Coulier, and others of an allied nature, were independently discovered by Mr. Aitken, who has devoted much time and study to them and made them the foundation of an entirely new branch of meteorology.

Later, perhaps, we may see reason to doubt whether all the conclusions of Coulier and Aitken are quite accurate, especially as regards the action of so-called products of combustion.

What has been said so far applies equally to the generation of clouds and of country fog, for a pure unadulterated fog, such as occurs in rural districts, consists simply of a cloud resting upon the surface of the earth. The fogs, however, which afflict many large towns, and London in a marked degree, appear to possess a character peculiar to themselves. They are distinguished by a well-known colour, which has sometimes been likened to that of pease soup: their density is abnormal, so is their persistence; and they often occur when the temperature of the air is considerably above the dew point. But what renders them especially objectionable is their acrid and corrosive quality, in virtue of which they exert a highly deleterious action upon animal and vegetable life.

The uncleanness of a town fog is of course due to the sooty and tarry matters with which it is charged, and which are derived from the smoke of innumerable fires. Its other and more mischievous specialities are mainly attributable to certain products of the combustion of sulphur, a substance which exists in relatively large proportions (from half to one per cent.) in nearly all varieties of coal.

We may make a sample of London fog in the glass globe by burning a little sulphur near the orifice of the inlet pipe while air is being admitted; and in order to prevent the entrance of any solid particles of sublimed sulphur, we will filter the air through a little cotton wool. The fog formed when the air is expanded far exceeds in density any we have yet seen. The globe appears almost as if it were filled with something that could be cut with a knife.

This is hardly the time or the place to discuss the possible methods by which town fogs might be abolished as such, or rendered as innocuous as those of the country. It is impossible to doubt that year by year they are increasing in virulence, and when the burden of the evil becomes too grievous to be borne, as is likely to be the case before many more winters are past, the remedy will perhaps be found in the compulsory substitution of gas for coal as the ordinary domestic fuel.

Every one has noticed how dense and dark a thundercloud is. It shuts out daylight almost as if it were a solid substance, and the glimmer that penetrates it is often imbued with a lurid or copper-coloured tint.

I had always found it rather difficult to believe that these peculiarities were due simply to the unusual extent and thickness of the clouds, as is commonly supposed to be the case, and it occurred to me about three years ago, that perhaps some clue to the explanation might be afforded by the electrification of a jet of steam. On making the experiment I found that the density and opacity of the jet were greatly increased when an electrical discharge was directed upon it, while its shadow, if cast upon a white screen by a sufficiently strong light, was of a decidedly reddish-brown tint.

As a possible explanation of the effect I suggested that there might occur some action among the little particles of water of a similar nature to that observed by Lord Rayleigh in his experiments upon water jets. Perhaps you will allow me to show his fundamental experiment before further discussing the steam jet.

A jet of water two or three feet long is made to issue in a nearly vertical direction from a small nozzle. At a certain distance above the nozzle the continuous stream is found to break up into separate drops, which collide with one another, and again rebounding, become scattered over a considerable space. But when the jet is exposed to the influence of an electrified substance, such as a rubbed stick of sealing wax, the drops no longer rebound after collision, but coalesce, and the entire stream of water, both ascending and descending, becomes nearly continuous. Look at the shadow of the jet upon the screen and notice what a magical effect the electrified sealing wax produces.

There is one other point to which I wish to direct your particular attention. If the sealing wax, or better, the knob of a charged Leyden jar, is held very close to the jet, so that the electrical influence is stronger, the separate drops do not coalesce as before, but become scattered even more widely than when no electrical influence was operating. They become similarly electrified and, in accordance with the well-known law, repel one another.

We will now remove the water jet, and in its place put a little apparatus for producing a jet of steam. It consists of a half-pint tin bottle, through the cork of which passes a glass tube terminating in a nozzle. When the water in the bottle is made to boil a jet of steam issues from the nozzle, and if we observe the shadow of the steam jet upon the screen we shall see that it is of feeble intensity and of a neutral tint, unaccompanied by any trace of decided colour. A bundle of needles connected by a wire with the electrical machine is placed near the base of the jet, and when the machine is worked electricity is discharged into the steam. A very striking effect instantly follows. The cloud of condensed steam is rendered dense and dark, its shadow at the same time assuming the suggestive yellowish-brown colour.

I at first believed that we had here a repetition, upon a smaller scale, of the phenomenon which occurs in the water jet. The little particles of condensed water must frequently come into collision with one another, and it seemed natural to suppose that, like Lord Rayleigh's larger particles, they rebounded under ordinary circumstances, and coalesced when under the influence of electricity. The great majority of the small particles ordinarily formed consisted, I thought, of perhaps only a few molecules, which were dispersed in the air and again converted into vapour without ever having become visible, while the larger particles formed by their coalescence under electrical action were of such dimensions as to impede the more refrangible waves of light. Hence the brownish-yellow colour.

Other explanations have been proposed. There is the molecular shock theory of the late R. Helmholtz (who, as it turned out, had studied electrified steam jets before I made my own experiments); I shall refer to his speculation later. And there is the dust-nucleus theory, which no doubt appears a very obvious one.

Though I knew that my own hypothesis was not quite free from objection, neither of these alternative ones commended itself to me as preferable; and so the matter rested until a few months ago, when the steam jet phenomenon was discussed anew in a paper communicated to the Royal Society by Mr. Aitken. Mr. Aitken said that he did not agree with my conjecture as to the nature of the effect. This led me to investigate the matter again, and to make some further experiments, the results of which have convinced me that I was clearly in error. At the same time it seems to me that the explanation which Mr. Aitken puts forward is little less controvertible than my own. Mr. Aitken's explanation of the phenomenon is, like mine, based upon Lord Rayleigh's work in connection with water-jets, but, unlike mine, it depends upon the experiment which shows that water particles when strongly electrified are scattered even more widely than when unelectrified. He believes, in short, that electrification produces the effect, not by promoting coalescence of small water particles, but by preventing such coalescence as would naturally occur in the absence of electrical influence. In the electrified jet, he says, the particles are smaller but at the same time more numerous; thus its apparent density is increased.

The chief flaw in my hypothesis lies in the fact that the mere presence of an electrified body like a rubbed stick of sealing wax, which is quite sufficient to cause coalescence of the drops in the water jet, has no action whatever upon the condensation of the steam jet. There must be an actual *discharge* of electricity. But it is by no means essential, as Mr. Aitken assumes, that this discharge should be of such a nature as to electrify, positively or negatively, the particles of water in the jet. If, instead of using a single electrode, we employ two, one positive and the other negative, and let them spark into each other across the jet, dense condensation at once occurs. [Experiment.] So it does if the two discharging points are removed quite outside the jet. [Experiment.] A small induction coil giving sparks an eighth of an inch in length causes dense condensation when the electrodes are more than an inch distant from the nozzle and on the same level. [Experiment.] In one experiment a brass tube two feet long was fixed in an inclined position with its upper end near the steam jet, and its lower end above the electrodes of the induction coil. In about three seconds after the spark was started dense condensation ensued, and it ceased about three seconds after the sparking was stopped. No test was needed, though in point of fact one was made, to show that the steam was not electrified to a potential of a single volt by this operation. And the time required for the influence to take effect showed that whatever this influence might be it was not induction.

The inference clearly is that in some way or other the action is brought about by the air in which an electrical discharge has taken place, and not directly by the electricity itself. The idea has no doubt already occurred to many of you that it is a dust effect. Minute particles of matter may be torn off the electrodes by the discharge, and form nuclei upon which the steam may condense. The experiments of Liveing and Dewar have indeed shown that small particles are certainly thrown off by electrical discharge, and the idea that such particles promote condensation appears to be supported by the fact that if a piece of burning material, such as touch-paper, is held near the jet so that the products of combustion can pass into it, thick condensation is produced. [Experiment.]

From a recent paper by Prof. Barus, published in the 'American Meteorological Journal' for March, it appears that he also is of opinion that such condensation is in all cases due to the action of minute dust particles. Yet it is remarkable that Mr. Aitken, the high priest and chief apostle of the philosophy of dust, gives no countenance to the nucleus theory. He does not even advert to its possibility. I imagine that his experiments have led him, as mine have led me, to the conclusion that it is untenable. And this not only in the case of electrical discharge, but also in the case of burning matter.

If we cause an electrical discharge to take place for some minutes inside a suitably arranged glass bottle, and then, ten or fifteen seconds after the discharge has ceased, blow the air from the bottle into the steam jet, the condensation is not in any way affected. Yet the dust could not have subsided in that time. And again, if we fill another large bottle with dense clouds of smoke by holding a bundle of burning touch-paper inside it, and almost immediately after the touch-paper is withdrawn, force out the smoke-laden air, through a nozzle, upon the jet—you can all see the black shadow of the smoke upon the screen—nothing whatever happens to the jet. Yet a mere scrap of the paper which is actually burning, though the ignited portion may not be larger than a pin's head, at once darkens the jet. Dead smoke (if I may use the term) exerts little or no influence by itself: there must be incandescent matter behind it. The question naturally arises, whether incandescent matter may not be sufficient of itself, without any smoke at all. We can test this by making a piece of platinum wire red hot and then holding it near the jet. It is seen to be quite as effective as the burning touch-paper. Yet here there can be no nuclei formed of products of combustion, for there is no combustion; there is simply ignition or incandescence.

One other point I may mention. It is stated by Barus in the paper above referred to that the fumes given off by a piece of phosphorus constitute a most efficient cause of dense condensation. This is true if they come directly from a piece of phosphorus; but if phosphorus fumes are collected in a bottle and then directed upon the jet, all traces of unoxidised phosphorus being first carefully removed, they are found to be absolutely inoperative. Phosphorus in air can hardly be said to be incandescent, though it is luminous in the dark; but it appears to act in the same manner as if its temperature were high.

All these facts seem to indicate that the several causes mentioned, electrical, chemical and thermal, confer upon the air in which they act some temporary property—certainly not due to mere inert dust—in virtue of which it acquires an abnormal power of promoting aqueous condensation.

I thought that possibly some clue as to the nature of this property might be obtained by observing how some other gases and vapours behaved; but though the experiments I made perhaps tend to narrow the dimensions of the mystery, I cannot say that they have completely solved it. Indeed some of the results only introduce additional perplexities.

One of the most natural things to try is hydrochloric acid, which is known to have a strong affinity for water. If we heat a little of the acid solution in a test-tube, closed with a cork, through which a glass tube is passed, and direct the issuing stream of gas upon the jet, the densest condensation results. [Experiment.] The vapours of sulphuric and nitric acids also cause dense condensation, and I suppose both of these have an affinity for water. But so also, and in an equally powerful degree, does the vapour of acetic acid; yet the affinity of this acid for water, as indicated by the heat evolved when the two are mixed, is very small.

Ammonia gas, when dissolved in water, causes the evolution of much heat. Yet a stream of this gas directed upon the jet has no action. [Experiment.]

Ozonised air, which Mascart found so effective in his experiments with the closed vessel, is quite inoperative with the steam jet. Equally so is the vapour of boiling formic acid, which I believe is chemically a much more active acid than acetic, and has a lower electrical resistance. (See Table.)

CONDENSATION OF STEAM JET.

Active.

Air, oxygen or nitrogen, in which electrical discharge is occurring.
Burning and incandescent substances.
Fumes from phosphorus.
Hydrochloric acid.
Sulphuric acid vapour.
Nitric acid vapour.
Acetic acid vapour.

Inactive.

Air, &c., in which electrical discharge has ceased for about 10 seconds.
Smoke without fire.
Bottled phosphorus fumes.
Ammonia.
Ozone.
Steam.
Alcohol vapour.
Formic acid vapour.
Sulphurous acid.

It seems that we have here a pretty little problem which might, perhaps, be solved without much difficulty by a competent chemist, but which quite baffles me.* Is it possible that the condensing vapours may contain dissociated atoms?

To return to the electrical effect. There are only two kinds of chemical change that I know of which could be brought about in air by an electrical discharge. Either some of the oxygen might be converted into ozone, or the oxygen and nitrogen of the air might be caused to combine, forming nitric acid or some such compound. The former of these would not account for the action of the air upon the jet, because, as we have seen, ozone is inoperative; the latter might. But if the activity of the air is due to the presence in it of a compound of oxygen and nitrogen, then it is clear that an electrical discharge in either nitrogen or oxygen separately would fail to render those gases active.

I arranged a spark bottle, inside which an induction-coil discharge could be made to take place; two bent tubes were passed through the cork, one reaching nearly to the bottom for the ingress of the gas to be tested, the other, a shorter one, for its egress. The open end of the egress tube was fixed near the steam jet, and first common air, then oxygen and then nitrogen were successively forced through the bottle while the coil discharge was going on. All produced dense condensation, but I thought that oxygen appeared to be a little more efficient than common air and nitrogen a little less.

This last experiment points to a conclusion to which at present I see no alternative. It is that the action on the jet of an electrical discharge is due in some way or other to dissociated atoms of oxygen and nitrogen. There is nothing else left to which it *can* be due.

So far as Robert Helmholtz's explanation coincides with this conclusion I think it must be accepted as correct. As to the precise manner in which he supposed the dissociated atoms to act upon the jet, it is more difficult to agree with him. He thought that the abnormal condensation was a consequence of the molecular shock caused by the violent recombination of the dissociated atoms in the supersaturated air of the jet, the action being analogous to that which occurs when a supersaturated solution of sulphate of soda, for example, is instantly crystallised by a mechanical shock.

To me this hypothesis, ingenious as it is, seems to be more fanciful than probable, but I can only hint very diffidently at an alternative one. To many chemical processes the presence of water is favourable or even essential. Is it possible that the recombination of free atoms may be assisted by water? And is it possible that dissociated atoms in an atmosphere of aqueous vapour may obtain the water needed for their union by condensing it from the vapour?

According to Helmholtz, flames and incandescent substances generally cause dissociation of the molecules of oxygen and nitrogen

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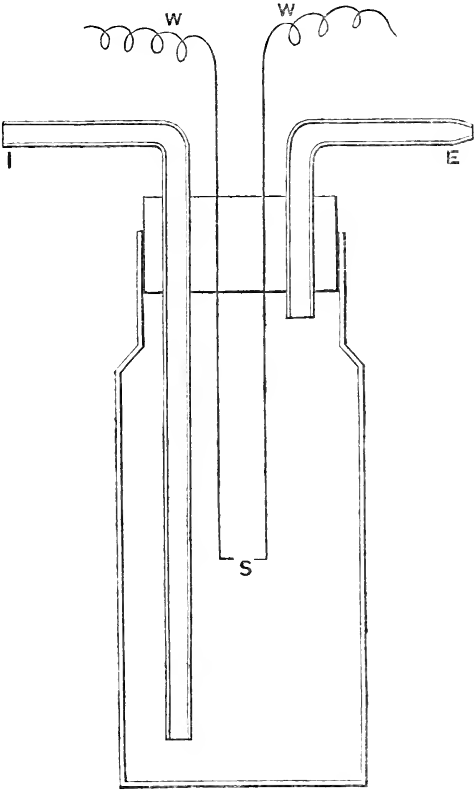
* Two chemists of the highest eminence have been good enough to consider the problem for me, but they are unable to throw any light upon it.

in the surrounding air. This, I believe, is generally admitted. I do not know whether slowly oxidising phosphorus has the same effect.

If it is conceded that the atmospheric gases are dissociated by electrical discharges, and that the presence of such dissociated gases somehow brings about the dense condensation of water vapour, we may still regard the electrified steam jet as affording an illustration of the abnormal darkness of thunder-clouds.



Spark Bottle.

I, Ingress tube; E, egress tube; W, wires to induction coil; S, spark gap.

Perhaps another source of dissociated atoms is to be found in the ozone which is generated by lightning flashes. A molecule of ozone consists of three atoms of atomic oxygen, while one of ordinary oxygen contains only two. Ozone is an unstable kind of material and gradually relapses into ordinary oxygen, the process being that one atom is dropped from the three-atom molecules of ozone, these detached atoms in course of time uniting with one another to form pairs. Thus two molecules of ozone are transformed into three of oxygen. A body of ozone is therefore always attended by a number of dissociated atoms which are looking for partners.

In the steam jet experiment there is not time for the disengagement of a sufficient number of isolated atoms from a blast of ozone to produce any sensible effect. But the case is otherwise when the vapour is confined in a closed vessel, as in Mascart's experiment, or when it occurs in the clouds, where the movement of air and vapour is comparatively slow.

Ozone, it will be remembered, was found by Mascart to produce dense condensation in a closed vessel even after being filtered through cotton wool. Similar filtration seems to entirely deprive the so-called products of combustion of their active property, a fact which has been adduced as affording overwhelming evidence in favour of the dust nucleus theory. Coulier himself, however, detected a weak point in this argument. He produced a flame which could not possibly have contained any products of combustion except steam, by burning pure filtered hydrogen in filtered air; yet this product was found to be perfectly capable of causing dense condensation, and, as in his former experiments, filtration through cotton wool deprived it of its activity.

These anomalies may, I think, be to a great extent cleared up if we assume that the effect of the cotton wool depends, not upon the mere mechanical obstruction it offers to the passage of particles of matter, but upon the moisture which it certainly contains, and which may act by attracting and facilitating the reunion of dissociated atoms before they reach the air inside the vessel. According to this view ozone would remain an active condenser in spite of its filtration, because free atoms would continue to be given off by it after it had passed the cotton wool. The filtration experiment should be tried with perfectly dry cotton wool, which, however, will not be easily procured, and if my suggestion is right, dry wool will be found not to deprive ordinary products of combustion of their condensing power.

To sum up. I think my recent experiments show conclusively that the dense condensation of the steam jet is not due directly either to electrical action or to dust nuclei. The immediate cause is probably to be found in dissociated atoms of atmospheric gases, though as to how these act we can only form a vague guess.

The discourse concluded with some remarks upon atmospheric electricity, and the exhibition of lantern photographs of lightning flashes.

[S. B.]

GENERAL MONTHLY MEETING,

Monday, May 8, 1893.

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 Douglas Galton, K.C.B. D.C.L. LL.D. F.R.S.
David Edward Hughes, Esq. F.R.S.
Hugo Müller, Esq. Ph.D. F.R.S.
The Right Hon. Earl Percy, F.S.A.
Basil Woodd Smith, Esq. F.R.A.S. F.S.A.
Sir Richard Webster, M.P. Q.C. LL.D.
Sir James Crichton-Browne, M.D. LL.D. F.R.S. *Treasurer.*
Sir Frederick Bramwell, Bart. D.C.L. LL.D. F.R.S. *Hon. Secretary.*

The Earl of Leven and Melville,
Mrs. S. F. Beevor,
Herbert C. Newton, Esq.
H. Sylvester Samuel, Esq. F.R.G.S.
Thomas Wrightson, Esq. M.P.
Frederick John Yarrow, Esq.

were elected Members of the Royal Institution.

The Honorary Secretary reported that the late Earl of Derby K.G. *M.R.I.* had bequeathed 2000*l.* to the Royal Institution.

The special thanks of the Members were returned for the following Donation to the Fund for the Promotion of Experimental Research at low temperatures:—

Alfred F. Yarrow, Esq.	£50
Sir David Salomons, Bart.	£50
Henry Vaughan, Esq.	£20

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. XXVI. Part 1. 8vo. 1893.

The Secretary of State for India—Great Trigonometrical Survey of India, Vols. XXVII. XXVIII. XXX. 4to. 1892.

The Lords of the Admiralty—Greenwich Observations for 1890. 4to. 1892.

Greenwich Spectroscopic and Photographic Results, 1890. 4to. 1892.

The Time of Swing of the Indian Invariable Pendulums. 4to. 1891.

Annals of the Cape Observatory, Vol. I. Parts 2–4. 4to. 1881–82.

The New Zealand Government—Results of a Census taken in 1891. 8vo. 1893.

Accademia dei Lincei, Reale, Roma—Classe di Scienze Fisiche, Matematiche e Naturali. Atti, Serie Quinta: Rendiconti. 1° Semestre, Vol. II. Fasc 6. 8vo. 1893.

Classe di Scienze Morali, Storiche, etc.: Rendiconti, Serie Quinta, Vol. II. Fasc. 2. 8vo. 1893.

American Philosophical Society—Proceedings, No. 139. 8vo. 1892.

Aristotelian Society—Proceedings, Vol. II. No. 2, Part 1. 8vo. 1893.

Asiatic Society of Great Britain, Royal—Journal, 1893, Part 2. 8vo.

Astronomical Society, Royal—Monthly Notices, Vol. LIII. No. 5. 8vo. 1893.

Bankers, Institute of—Journal, Vol. XIV. Part 4. 8vo. 1893.

Batavia Observatory—Rainfall in East Indian Archipelago, 1891. 8vo. 1892.

Magnetical and Meteorological Observations, Vol. XIV. fol. 1892.

Binnie, A. R. Esq. M. Inst. C.E. M.R.I. (the Author)—Report on the Flow of the Thames. 8vo. 1892.

British Architects, Royal Institute of—Proceedings, 1893, No. 14. 4to.

British Astronomical Association—Journal, Vol. III. No. 5. 8vo. 1893.

British Museum Trustees—Catalogue of Seals, Vol. II. 8vo. 1892.

Catalogue of Indian Coins. 8vo. 1892.

Catalogue of Oriental Coins, Vol. X. 8vo. 1890.

Catalogue of Chinese Coins. 8vo. 1892.

Ancient Greek Inscriptions, Part IV. Sec. 1. 4to. 1893.

British Museum (Natural History)—Catalogue of British Echinoderms. 8vo. 1892.

Illustrations of Specimens of Lepidoptera Heterocera, Part IX. 4to. 1893.

Guide to Sowerby's Models of British Fungi. 8vo. 1893.

Chemical Industry, Society of—Journal, Vol. XII. No. 3. 8vo. 1893.

Chemical Society—Journal for May, 1893. 8vo.

Clinical Society—Transactions, Supplement to Vol. XXV. 8vo. 1893.

East India Association—Journal, Vol. XXV. No. 2. 8vo. 1893.

Editors—American Journal of Science for April, 1893. 8vo.

Analyst for April, 1893. 8vo.

Athenæum for April, 1893. 4to.

Chemical News for April, 1893. 4to.

Chemist and Druggist for April, 1893. 8vo.

Electrical Engineer for April, 1893. fol.

Electric Plant for April, 1893. 4to.

Engineer for April, 1893. fol.

Engineering for April, 1893. fol.

Engineering Review for April, 1893. 8vo.

Horological Journal for April, 1893. 8vo.

Industries for April, 1893. fol.

Iron for April, 1893. 4to.

Ironmongery for April, 1893. 4to.

Lightning for April, 1893. 4to.

Monist for April, 1893. 8vo.

Nature for April, 1893. 4to.

Open Court for April, 1893. 4to.

Photographic News for April, 1893. 8vo.

Photographic Work for April, 1893. 8vo.

Telegraphic Journal for April, 1893. fol.

Transport for April, 1893.

Zoophilist for April, 1893. 4to.

Electrical Engineers, Institution of—Journal, No. 105. 8vo. 1893.

Florence, Biblioteca Nazionale Centrale—Bolletino, Nos. 175, 176. 8vo. 1893.

Franklin Institute—Journal, No. 808. 8vo. 1893.
Geographical Society, Royal—Geographical Journal, Vol. I. No. 4. 8vo. 1893.
Geographical Society of California—Bulletin, Vol. I. Part 1. 8vo. 1893.
Geological Society—Journal, No. 194. 8vo. 1893.
Harvard University—Bibliographical Contributions, No. 47. 8vo. 1893.
Johns Hopkins University—American Chemical Journal, Vol. XV. No. 4. 8vo. 1893.
University Circulars, No. 104. 4to. 1893.
Keeler, James E. Esq. (the Author)—Observations on the Spectrum of β Lyræ. 8vo. 1893.
Linnean Society—Journal, No. 154. 8vo. 1893.
Manchester Geological Society—Transactions, Vol. XXII. Parts 6, 7. 8vo. 1893.
Massachusetts State Board of Health—Twenty-third Annual Report. 8vo. 1892.
Mechanical Engineers, Institution of—Proceedings, 1892, No. 4. 8vo.
Mendenhall, T. C. Esq. (the Author)—Determinations of Gravity. 8vo. 1892.
Miller, W. J. C. Esq. (the Editor)—The Medical Register for 1893. 8vo.
The Dentists' Register for 1893. 8vo.
Ministry of Public Works, Rome—Giornale del Genio Civile, 1893, Fasc. 1, 2, and Designi. fol. 1893.
Munir Bey, His Excellency—Catalogue of the Library of the late Ahmed Véfyk Pacha. Constantinople. 8vo. 1893.
National Life-Boat Institution, Royal—Annual Report, 1893. 8vo.
North of England Institute of Mining and Mechanical Engineers—Transactions, Vol. XLII. Part 2. 8vo. 1893.
Odontological Society—Transactions, Vol. XXV. Nos. 5, 6. 8vo. 1893.
Payne, W. W. and Hale, G. E. (the Editors)—Astronomy and Astro-Physics for April, 1893. 8vo.
Pharmaceutical Society of Great Britain—Journal for April, 1892. 8vo.
Richardson, B. W. M.D. F.R.S. M.R.I. (the Author)—The Asclepiad, 1893, No. 1. 8vo.
Royal Society of London—Proceedings, No. 320. 8vo. 1893.
Smithsonian Institution—Bureau of Ethnology:
Contributions to North American Ethnology, Vol. VII. 4to. 1890.
Bibliography of the Athapascan Languages. 8vo. 1892.
Annual Report of the Bureau of Ethnology, 1885–86. 4to. 1891.
National Museum Report, 1890. 8vo. 1891.
Society of Architects—Proceedings, Vol. V. Nos. 9, 10. 8vo. 1893.
Society of Arts—Journal for April, 1893. 8vo.
St. Pétersbourg, Académie Impériale des Sciences—Mémoires, Tome XL. No. 2; Tome XLI. No. 1. 4to. 1892–93.
Bulletin, Tome XXXV. No. 3. 8vo. 1893.
Teyler Museum—Archives, Série II. Vol. IV. Part 1. 8vo. 1893.
United Service Institution, Royal—Journal, No. 182. 8vo. 1893.
United States Department of the Interior—Report on Mineral Industries in the U.S. at the Eleventh Census, 1890. 4to. 1892.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1893, Heft 3, 4. 4to.
Victoria Institute—Transactions, No. 103. 8vo. 1893.
Zoological Society of London—Proceedings, 1892, Part 4. 8vo. 1893.
Transactions, Vol. XIII. Part 5. 4to. 1893.
Zurich Naturforschenden Gesellschaft—Vierteljahrschrift, Jahrgang XXXVII. Heft 3, 4. 8vo. 1892.

WEEKLY EVENING MEETING,

Friday, May 12, 1893.

Sir Douglas Galton, K.C.B. D.C.L. LL.D. F.R.S.
Vice-President, in the Chair.

The Right Hon. Lord Kelvin, D.C.L. LL.D. Pres. R.S. *M.R.I.*

Isoperimetrical Problems.

Dido, B.C. 800 or 900.
Horatius Cocles, B.C. 508.
Pappus, Book V., A.D. 390.
John Bernoulli, A.D. 1700.
Euler, A.D. 1744.
Maupertuis (Least Action), b. 1698, d. 1759.
Lagrange (Calculus of Variations), 1759.
Hamilton (Actional Equations of Dynamics), 1834.
Liouville, 1840 to 1860.

The first isoperimetrical problem known in history was practically solved by Dido, a clever Phœnician princess, who left her Tyrian home and emigrated to North Africa, with all her property and a large retinue, because her brother Pygmalion murdered her rich uncle and husband Acerbas, and plotted to defraud her of the money which he left. On landing in a bay about the middle of the north coast of Africa she obtained a grant from Hiarbas, the native chief of the district, of as much land as she could enclose with an ox-hide. She cut the ox-hide into an exceedingly long strip, and succeeded in enclosing between it and the sea a very valuable territory* on which she built Carthage.

The next isoperimetrical problem on record was three or four hundred years later, when Horatius Cocles, after saving his country by defending the bridge until it was destroyed by the Romans behind him, saved his own life and got back into Rome by swimming the Tiber under the broken bridge, and was rewarded by his grateful countrymen with a grant of as much land as he could plough round in a day.

In Dido's problem the greatest value of land was to be enclosed by a line of given length. If the land is all of equal value the general solution of the problem shows that her line of ox-hide should

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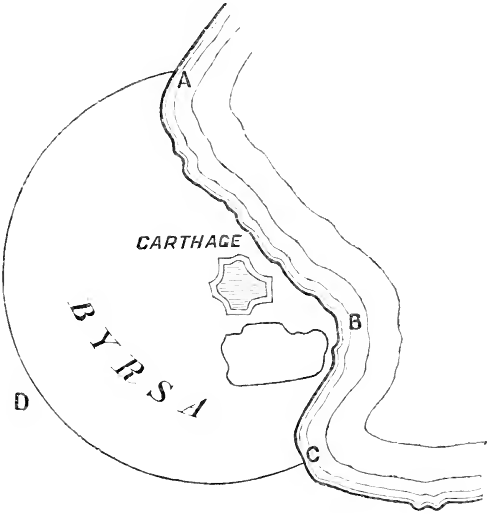
* Called Byrsa, from βύρσα, the hide of a bull. [Smith's 'Dictionary of Greek and Roman Biography and Mythology,' article "Dido."]

be laid down in a circle. It shows also that if the sea is to be part of the boundary, starting, let us say, southward from any *given* point A of the coast, the inland bounding line must at its far end cut the coast line perpendicularly. Here, then, to complete our solution, we have a very curious and interesting, but not at all easy, geometrical question to answer:—What must be the radius of a circular arc A D C, of given length, and in what direction must it leave the point A, in order that it may cut a given curve A B C perpendicularly at some unknown point C? I don't believe Dido could have passed an examination on the subject, but no doubt she gave a very good practical solution, and better than she would have found if she had just mathematics enough to make her fancy the boundary ought to be a circle. No doubt she gave it different curvature in different parts to bring in as much as possible of the more valuable parts of the land offered to her, even though difference of curvature in different parts would cause the total area enclosed to be less than it would be with a circular boundary of the same length.



The Roman reward to Horatius Cocles brings in quite a new idea, now well known in the general subject of isoperimetrics: the greater or less speed attainable according to the nature of the country through which the line travelled over passes. If it had been equally easy to plough the furrow in all parts of the area offered for enclosure, and if the value of the land per acre was equal throughout, Cocles would certainly have ploughed as nearly in a circle as he could, and would only have deviated from a single circular path if he found that he had misjudged its proper curvature. Thus, he might find that he had begun on too large a circle, and, in order to get back to the starting point and complete the enclosure before night-fall, he must deviate from it on the concave side; or he would deviate from it on the other side if he found that he had begun on too small a circle, and that he had still time to spare for a wider sweep. But, in reality, he must also have considered the character of the ground he had to plough through, which cannot but have been very unequal in different parts, and he would naturally vary the curvature of his path to avoid places where his ploughing must be very slow, and to choose those where it would be most rapid.

He must also have had, as Dido had, to consider the different value of the land in different parts, and thus he had a very complex problem to practically solve. He had to be guided both by the value of the land to be enclosed and the speed at which he could plough according to the path chosen; and he had a very brain-trying task to judge what line he must follow to get the largest value of land enclosed before night.

These two very ancient stories, whether severe critics will call them mythical or allow them to be historic, are nevertheless full of scientific interest. Each of them expresses a perfectly definite case of the great isoperimetrical problem to which the whole of dynamics is reduced by the modern mathematical methods of Euler, Lagrange, Hamilton and Liouville (Liouville's Journal, 1840–1850). In Dido's and Horatius Cocles' problems, we find perfect illustrations of all the fundamental principles and details of the generalised treatment of dynamics which we have learned from these great mathematicians of the eighteenth and nineteenth centuries.

Nine hundred years after the time of Horatius Cocles we find, in the fifth Book of the collected Mathematical and Physical Papers of Pappus of Alexandria, still another idea belonging to isoperimetrics —the economy of valuable material used for building a wall; which, however, is virtually the same as the time per yard of furrow in Cocles' ploughing. In this new case the economist is not a clever princess, nor a patriot soldier; but a humble bee who is praised in the introduction to the book not only for his admirable obedience to the Authorities of his Republic, for the neat and tidy manner in which he collects honey, and for his prudent thoughtfulness in arranging for its storage and preservation for future use, but also for his knowledge of the geometrical truth that a "hexagon can enclose more honey than a square or a triangle with equal quantities of building material in the walls," and for his choosing on this account the hexagonal form for his cells. Pappus, concluding his introduction with the remark that bees only know as much of geometry as is practically useful to them, proceeds to apply what he calls his own superior human intelligence to investigation of useless knowledge, and gives results in his Book V., which consists of fifty-five theorems and fifty-seven propositions on the areas of various plane figures having equal circumferences. In this Book, written originally in Greek, we find (Theorem IX. Proposition X.) the expression "isoperimetrical figures," which is, so far as I know, the first use of the adjective "isoperimetrical" in geometry; and we may, I believe, justly regard Pappus as the originator, for mathematics, of *isoperimetrical problems,* the designation technically given in the nineteenth century* to that large province of mathematical and engineering science in which different figures having equal circumferences, or different paths between two given points, or between some two points on two given curves, or on one given curve, are compared in connection with definite questions of greatest efficiency and smallest cost.

In the modern engineering of railways an isoperimetrical problem of continual recurrence is the laying out of a line between two towns along which a railway may be made at the smallest prime cost. If this were to be done irrespectively of all other considerations, the requisite datum for its solution would be simply the cost per yard of making the railway in any part of the country between the two towns. Practically the solution would be found in the engineers' drawing office by laying down two or three trial lines to begin with, and calculating the cost of each, and choosing the one of which the cost is least. In practice various other considerations than very slight differences in the cost of construction will decide the ultimate choice of the exact line to be taken, but if the problem were put before a capable engineer to find very exactly the line of minimum total cost, with an absolutely definite statement of the cost per yard in every part of the country, he or his draughtsmen would know perfectly how to find the solution. Having found something near the true line by a few rough trials they would try small deviations from the rough approximation, and calculate differences of cost for different lines differing very little from one another. From their drawings and calculations they would judge by eye which way they must deviate from the best line already found to find one still better. At last they would find two lines for which their calculation shows no difference of cost. Either of these might be chosen; or, according to judgment, a line midway between them, or somewhere between them, or even not between them but near to one of them, might be chosen, as the best approximation to the exact solution of the mathematical problem which they care to take the labour of trying for. But it is clear that if the price per yard of the line were accurately given (however determined or assumed) there would be an absolutely definite solution of the problem, and we can easily understand that the skill available in a good engineer's drawing-office would suffice to find the solution with any degree of accuracy that might be prescribed;

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* Example, Woodhouse's 'Isoperimetrical Problems,' Cambridge, 1810.

the minuter the accuracy to be attained the greater the labour, of course. You must not imagine that I suggest, as a thing of practical engineering, the attainment of minute accuracy in the solution of a problem thus arbitrarily proposed; but it is interesting to know that there is no limit to the accuracy to which this ideal problem may be worked out by the methods which are actually used every day by engineers in their calculations and drawings.

The modern method of the "calculus of variations," brought into the perfect and beautiful analytical form in which we now have it by Lagrange, gives for this particular problem a theorem which would be very valuable to the draughtsman if he were required to produce an exceedingly accurate drawing of the required curve. The curvature of the curve at any point is convex towards the side on which the price per unit length of line is less, and is numerically equal to the rate per mile perpendicular to the line at which the Neperian logarithm of the price per unit length of the line varies. This statement would give the radius of curvature in fraction of a mile. If we wish to have it in yards we must take the rate per yard at which the Neperian logarithm of the price per unit length of the line varies. I commend the Neperian logarithm of price in pounds, shillings and pence to our Honorary Secretary, to whom no doubt it will present a perfectly clear idea; but less powerful men would prefer to reckon the price in pence, or in pounds and decimals of a pound. In every possible case of its subject the "calculus of variations" gives a theorem of curvature less simple in all other cases than in that very simple case of the railway line of minimum first cost, but always interpretable and intelligible according to the same principles.

Thus in Dido's problem we find by the calculus of variations that the curvature of the enclosing line varies in simple proportion to the value of the land at the places through which it passes; and the curvature at any one place is determined by the condition that the whole length of the ox-hide just completes the enclosure.

The problem of Horatius Cocles combines the railway problem with that of Dido. In it the curvature of the boundary is the sum of two parts; one, as in the railway, equal to the rate of variation perpendicular to the line, of the Neperian logarithm of the cost in time per yard of the furrow (instead of cost in money per yard of the railway); the other varying proportionally to the value of the land as in Dido's problem, but now divided by the cost per yard of the line, which is constant in Dido's case. The first of these parts, added to the ratio of the money-value per square yard of the land to the money-cost per lineal yard of the boundary (a wall suppose), is the curvature of the boundary when the problem is simply to make the most you can of a grant of as much land as you please to take provided you build a proper and sufficient stone wall round it at your own expense. This problem, unless wall-building is so costly that no part of the offered land will pay for the wall round it, has clearly a determinate finite solution if the offered land is an oasis surrounded by valueless desert. It has also a determinate finite solution even though the land be nowhere valueless, if the wall is sufficiently more and more expensive at greater and greater distances from some place where there are quarries, or habitations for the builders.

The simplified case of this problem, in which all equal areas of the land are equally valuable, is identical with the old well-known Cambridge dynamical plane problem of finding the motion of a particle relatively to a line of reference revolving uniformly in a plane: to which belongs that considerable part of the "Lunar Theory" in which any possible motion of the moon is calculated on the supposition that the centre of gravity of the earth and moon moves uniformly in a circle round the sun, and that the motions of the earth and moon are exactly in this plane. The rule for curvature which I have given you expresses in words the essence of the calculation, and suggests a graphic method for finding solutions by which not uninteresting approximations * to the cusped and looped orbits of G. F. Hill † and Poincare ‡ can be obtained without disproportionately great labour.

In the dynamical problem, the angular velocity of the revolving line of reference is numerically equal to half the value of the land per square yard; and the relative velocity of the moving particle is numerically equal to the cost of the wall per lineal yard in the land question.

But now as to the proper theorem of curvature for each case; both Dido and Horatius Cocles no doubt felt it instinctively and were guided by it, though they could not put it into words, still less prove it by the "calculus of variations." It was useless knowledge to the bees, and, therefore, they did not know it; because they had only to do with straight lines. But as you are not bees I advise you all, even though you have no interest in acquiring as much property as you can enclose by a wall of given length, to try Dido's problem for yourselves, simplifying it, however, by doing away with the rugged coast line for part of your boundary, and completing the enclosure by the wall itself. Take forty inches of thin soft black thread with its ends knotted together and let it represent the wall; lay it down on a large sheet of white paper and try to enclose the greatest area with it you can. You will feel that you must stretch it in a circle to do this, and then, perhaps, you will like to read Pappus (Liber V. Theorema II. Propositio II.) to find mathematical demonstration that you have judged rightly for the case of all equal areas of the enclosed land equally valuable. Next try a case in

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* Kelvin, "On Graphic Solution of Dynamical Problems." 'Phil. Mag.' 1892 (2nd half-year).

† Hill, "Researches in the Lunar Theory," Part 3. National Academy of Sciences, 1887.

‡ 'Méthodes Nouvelles de la Mécanique Céleste,' p. 109 (1892).

which the land is of different value in different parts. Take a square foot of white paper and divide it into 144 square inches to represent square miles, your forty inches of endless thread representing a forty miles wall to enclose the area you are to acquire. Write on each square the value of that particular square mile of land, and place your endless thread upon the paper, stretched round a large number of smooth pins stuck through the paper into a drawing-board below it, so as to enclose as much value as you can, judging first roughly by eye and then correcting according to the sum of the values of complete squares and proportional values of parts of squares enclosed by it. In a very short time you will find with practical accuracy the proper shape of the wall to enclose the greatest value of the land that can be enclosed by forty miles of wall. When you have done this you will understand exactly the subject of the calculus of variations, and those of you who are mathematical students may be inclined to read Lagrange, Woodhouse, and other modern writers on the subject. The problem of Horatius Cocles, when not only the different values of the land in different places but also the different speed of the plough according to the nature of the ground through which the furrow is cut are taken into consideration, though more complex and difficult, is still quite practicable by the ordinary graphic method of trial and error. The analytical method of the calculus of variations, of which I have told you the result, gives simply the proper curvature for the furrow in any particular direction through any particular place. It gives this and it cannot give anything but this, for any plane isoperimetrical problem whatever, or for any isoperimetrical problem on a given curved surface of any kind.

Beautiful, simple, and clear as isoperimetrics is in geometry, its greatest interest, to my mind, is in its dynamical applications. The great theorem of least action, somewhat mystically and vaguely propounded by Maupertuis, was magnificently developed by Lagrange and Hamilton, and by them demonstrated to be not only true throughout the whole material world, but also a sufficient foundation for the whole of dynamical science.

It would require nearly another hour if I were to explain to you fully this grand generalisation for any number of bodies moving freely, such as the planets and satellites of the solar system, or any number of bodies connected by cords, links, or mutual pressures between hard surfaces, as in a spinning-wheel, or lathe and treadle, or a steam engine, or a crane, or a machine of any kind; but even if it were convenient to you to remain here an hour longer, I fear that two hours of pure mathematics and dynamics might be too fatiguing. I must, therefore, perforce limit myself to the two-dimensional, but otherwise wholly comprehensive, problems of Dido and Horatius Cocles. Going back to the simpler included case of the railway of minimum cost between two towns, the dynamical analogue is this:—For price per unit length of the line substitute the velocity of a point moving in a plane under the influence of a given conservative system of forces, that is to say, such a system that when material particles not mutually influencing one another are projected from one and the same point in different directions, but with equal velocities, the subsequent velocity of each is calculable from its position at any instant, and all have equal velocities in travelling through the same place whatever may be their directions. The theorem of curvature, of which I told you in connection with the railway engineering problem, is now simply the well-known elementary law of relation between curvature and centrifugal force of the motion of a particle.

The motion of a particle in a plane is, as Liouville has proved, a case to which every possible problem of dynamics involving just two freedoms to move can be reduced. But to bring you to see clearly its relation to isoperimetrics, I must tell you of another admirable theorem of Liouville's, reducing to a still simpler case the most general dynamics of two-freedoms motion. Though not all mathematical experts, I am sure you can all perfectly understand the simplicity of the problem of drawing the shortest line on any given convex surface, such as the surface of this block of wood (shaped to illustrate Newton's dynamical theory of the elliptic motion of a planet round the sun) which you see on the table before you. I solve the problem practically by stretching a thin cord between the two points, and pressing it a little this way or that way with my fingers till I see and feel that it lies along the shortest distance between them. And now, when I tell you that Liouville has reduced to this splendidly simple problem of drawing a shortest line (geodetic line it is called) on any given curved surface every conceivable problem of dynamics involving only two freedoms to move, I am sure you will understand sufficiently to admire the great beauty of this theorem.

The doctrine of isoperimetrical problems in its relation to dynamics is very valuable in helping to theoretical investigation of an exceedingly important subject for astronomy and physics—the stability of motion, regarding which, however, I can only this evening venture to show you some experimental illustrations.

The lecture was concluded with experiments illustrating—

1. Rigid bodies (teetotums, boys' tops, ovals, oblates, &c.) placed on a horizontal plane, and caused to spin round on a vertical axis, and found to be thus rendered stable or unstable according as the equilibrium without spinning is unstable or stable.

2. The stability or instability of a simple pendulum whose point of support is caused to vibrate up and down in a vertical line, investigated mathematically by Lord Rayleigh.

3. The crispations of a liquid supported on a vibrating plate, investigated experimentally by Faraday; and the instability of a liquid in a glass jar, vibrating up and down in a vertical line, demonstrated mathematically by Lord Rayleigh.

4. The instability of water in a prolate hollow vessel, and its

stability in an oblate hollow vessel, each caused to rotate rapidly round its axis of figure,* which were announced to Section A of the British Association at its Glasgow meeting in 1876 as results of an investigation not then published, and which has not been published up to the present time.

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* 'Nature,' 1877, vol. 15, p. 297, 'On the Precessional Motion of a Liquid.'

WEEKLY EVENING MEETING,

Friday May 19, 1893.

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

Alfred Austin, Esq.

Poetry and Pessimism.

[No Abstract.]

WEEKLY EVENING MEETING,

Friday, May 26, 1893.

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

Herbert Beerbohm Tree, Esq.

The Imaginative Faculty.

When the gift of the Imagination was conferred upon mankind, a double-edged sword covered with flowers was thrust into its baby-hands. Just as the highest joys which are known to us are those of the imagination, so also are our deepest sorrows—the sorrows of our fantasy. Love, ambition, heroism, the sense of beauty, virtue itself, become intensified by the imagination, until they reach that acute and passionate expression which renders them potent factors for good or evil in individuals. Even so has the imagination ever been the strongest power in fostering the aspirations, in shaping the destinations of nations. It is the vision through whose lens we see the realities of life, either in the convex or in the concave, diabolically distorted or divinely out of drawing. . . .

"Can acting be taught?" is a question which has been theoretically propounded in many a magazine article, and has vexed the spirit of countless debating societies. It is answered in practice on the stage, and, I think, triumphantly answered in the negative. Acting, in fact, is purely an affair of the imagination—the actor more than any other artist may be said to be the "passion-winged minister of thought." Children are born actors. They lose the faculty only when the wings of their imagination are weighted by self-consciousness. It is not every one to whom is given the capacity of always remaining a child. It is this blessed gift of receptive sensibility which it should be the endeavour (the unconscious endeavour, perhaps) of every artist to cultivate and to retain. There are those who would have us believe that technique is the end and aim of art. There are those who would persuade us that the art of acting is subject to certain mathematical laws, forgetting that these laws are but the footnotes of adroit commentators, and in no sense the well-springs of art. What I venture to assert is that all that is most essential, most luminous, in acting may be traced to the imaginative faculty. It is this that makes the actor's calling at once the most simple and the most complex of all the arts. It is this very simplicity which has caused many to deny to acting a place among the arts, and which has so often baffled those who would appraise the art of acting as a precise science, and measure it by the yard-measure of unimaginative criticism. Yet in another sense no art is more complex than the dramatic art in its highest expression, for in none is demanded of its exponent a more delicate poise, a subtler instinct; none is more dependent on that acute state of the imagination, on that divine insanity which we call genius. The actor may be said to rank with, if after, the poet. He, like the poet, is independent of recognised laws. The histrionic art is indeed essentially a self-governed one. Its laws are the unwritten laws of the book of nature, illuminated by the imagination. But if the actor can claim exemption from academic training, it would be idle to affirm that he is independent of personal attributes, or that he can reach any degree of eminence without those accomplishments which the strenuous exercise of art alone can give. His Pegasus, however, should be tamed in the broad arena of the stage rather than in the enervating stable of the academy. In acting, in fact, there is an infinity to learn, but infinitely little that can be taught. The actor must be capable, of course, of pronouncing his native language, and of having a reasonable control over the movements of his limbs, but thus equipped, his technical education is practically complete. He is his own "stock-in-trade." The painter has his pigments, the poet his pen, the sculptor his clay, the musician his lute; the actor is limited to his personality—he plays upon himself. To give free range to the imaginative quality is the highest accomplishment of the actor. He whose imagination is most untrammelled is he who is most likely to touch the imagination of an audience. To arrive at this emancipation of the mind is his ultimate and highest achievement. The development of this sensitive or receptive condition depends largely on the surrounding influences of life. A general knowledge of men and things is, of course, the first essential; but I doubt whether education, in its accepted sense, is so necessary or indeed desirable in an artistic career as it is in what I may call the more concrete walks of life. What is meat to one is often poison to the other. The midwife of science is sometimes the undertaker of art. I have touched upon what, in its restricting influence on the imaginative faculty, I have called the pernicious habit of reading books—a practice which in its too free indulgence may tend to fetter the exercise of that imagination and that observation of life which are so essential to the development of the artist. Some people are educated by their memories, others by observation, aided by the imagination. One man will be able by a look at a picture, or by the scanning of an old manuscript, to project himself into any period of history; while another will by laborious unimaginative study acquire no more artistic inspiration than can be obtained by learning the 'Encyclopædia Britannica' by heart. I have often noticed that those who devote their spare energies to indiscriminate reading acquire a habit of thinking by memory, and thus gradually lose the faculty which the spontaneous observation of life tends to quicken. Their thought becomes artificial—they think by machinery—originality loses its muscle; the memory is developed at the expense of the imagination. Take any incident of everyday life—to the man who is not in the habit of exercising his imagination it will appear as a vulgar fact; to him who sees the same incident with the dramatic, the imaginative eye, it will give birth to an original thought, which is often more vital than a quotation.

The education of the artist, then, should be directed rather to the development of the imagination than to the storage of facts. For purposes of immediate information the British Museum is always open to him; the judges of the land are ever ready to set him right on points of law into a misapprehension of which a too lively imagination may have led him. I am so bold as to think that an university education, which is so propitious to success in other callings may be a source of danger to the artist. The point of view is apt to become academic, the academic to degenerate into the didactic, for all cliques, even the most illustrious, have a narrowing tendency. The development of those qualities which are so favourable to distinction in other callings may tend to check in the artist that originality which is so essential to the exercise of our fascinating, if fantastic, calling. The very social advantages which an university career brings may tend to inculcate a conventional regard for the "good form" of a "set," and to divert the current of youthful enthusiasm into an undue sense of the importance of boot-varnish. I maintain that such surroundings, and the influences of a too prosperous society, may tend to hinder rather than to foster the growth of this sensitive plant, which will often flourish in the rude winds of adversity and perish in the scent-laden *salons* of fashion. To argue that the artist should shut himself off from the world, and wrap himself round with a mantle of dignified ignorance, would of course be absurd. I have already said that a knowledge of men and things is essential to him, and this knowledge is manifestly impossible unless he is in sympathetic touch with his generation, for we cannot give out what we have not taken in. His should be the bird's-eye view. But the allurements of society should never be allowed to absorb or enslave him—lest in sipping its enervating narcotic he should drift from the broad stream of life into the backwater of self-indulgence. The poet, like the soldier, may "caper nimbly in a lady's chamber to the lascivious pleasing of a lute," but if he dances a too frequent attendance in the antechamber of fashion, the jealous muse deserts him, and the poet's song henceforth finds utterance in the lisping treble of the "*vers de Société*," and a fitful inspiration in the chronicling of an illustrious birth or a serene demise. It takes a genius to survive being made Poet-Laureate—indeed this official reward might often be conferred only on the poet when he is dead, to benefit his family and to point out the beauties of his works to an otherwise indifferent posterity.

Of all the fetters which cramp the imagination, none is so frequent as self-consciousness. With many of us this failing becomes a disease. The actor is more liable to its attacks than any other artist, since he cannot separate his personality from his work. This is the necessary condition under which he works; he cannot, like the poet or the painter, choose his mood—he is the slave of the moment. Under what disadvantages would a painter work if his patron were standing at his elbow watching each stroke of his brush.

It is only when the mind of the actor is emancipated from the trammels of his surroundings that his imagination is allowed full play. The nervousness which afflicts him in his first performance of a new *rôle* will often paralyse his imagination; though it is true that the dependence on this imaginative faculty varies in individuals. . . .

I have endeavoured to show how the imaginative faculty in acting may be cramped by self-consciousness, and how susceptible it is to social and other influences which surround the life of the artist. In the same way it is also susceptible of infinite cultivation if left to its own devices. I am willing to admit that every artist works according to his own method; but I maintain that that art is likely to produce the greatest effect which is least reliant on what are called the canons of art, that is to say, that art which springs spontaneously from the yielding up of the artist to his imagination. I have known actors who frequently arrive at many of their best effects through patient study; indeed, I believe, great actors have been known to study each gesture before a looking-glass. This seems to me, nevertheless, a mistaken system, and one certainly which would be destructive to the effects of those who prefer to rely on the mood of the moment. Another aspect of our art which has of late been much debated is, whether it is desirable that the actor should or should not sink his individuality in the part he is playing; whether, in fact, the actor should be absorbed in his work, or the work be absorbed in the actor. It seems to me, in spite of all that certain writers are never tired of dinning into our ears, that the higher aim of the artist is to so project his imagination into the character he is playing that his own individuality becomes merged in his assumption. This indeed seems to me the very essence of the art of acting. I remember that when I first went upon the stage, I was told that to obtain any popular success, an actor must be always himself, that the public even like to recognise the familiar voice before he appears on the scene, that he should, if possible, confine himself to what was called "one line of business," and that he should seek to cultivate a certain mannerism which should be the badge of his individuality. It seems to me that this is an entirely erroneous and mischievous doctrine Indeed, I will go so far as to maintain that the highest expression in every branch of art has always been the impersonal. The greatest artist that ever lived was the most impersonal, he was the most impersonal because the most imaginative. I mean our own Shakespeare. Where do we find him in his work? The spirit, the style everywhere—but the man? nowhere—except in the sense *le style c'est l'homme.* Take 'Othello,' for instance, the finest perhaps of all his stage-plays. If we think we have found him in the noble outbursts of the Moor, in the over-mastering passion of the simple-minded warrior, we lose him immediately in the intellectual sympathy which he seems to lavish on the brutal cynicism of the subtle and brilliant Iago. In one moment he soars to the very heights of poetic ecstasy, in the next he descends with equal ease and apparent zest into the depths of sottish animalism. We find him in the melodious wail of Hamlet, we lose him in the hoggish grunts of Falstaff. What sort of a man Shakespeare was we none of us know. We are led to believe that he was an excellent business man, with a taste for agriculture. In his work he becomes effaced—his spirit is like a Will-o'-the-wisp. His mind is like the Irishman's flea—"you no sooner put your finger upon him, but he isn't there." His was essentially a plastic mind—he was capable of entering into the thoughts of all men, and made their point of view his own. Nowhere did he insist on his personal predilections—he was, in fact, the artist—the creator—he looked upon mankind with all the impartiality of a god, he laid their hearts bare with the imperturbability of an inspired vivisectionist. The abiding hold which the play of 'Hamlet' has exercised over so many successive generations is mainly due to its wondrous mystery which holds the imagination of an audience enthralled, for, in the conventional sense, it cannot be said to be a pattern stage-play. In what a masterful fashion is the key-note of mystery struck in the very first scene on the ramparts; from the moment when the solitary soldier calls through the night, "Who's there?" the imagination of the audience is held spellbound; with such marvellous power is it played upon by the dramatist that from the first scene a modern sceptical audience accepts the supernatural basis of the play. Probably more inspired nonsense has been written on the subject of 'Hamlet' by the unimaginative commentator than on any subject within the scope of literature. Yet to him who will approach Shakespeare's masterpiece in the right spirit, it will be seen to have that simplicity which is characteristic of all great works. The finest poems which have ever been penned, the greatest pictures which have ever been painted, the greatest inventions which have been given to the world have been distinguished by this quality of simplicity. I have noticed this same characteristic in great men. It is only when we do not yield ourselves up to our imagination that the simple appears incomprehensible. Nearly all the mad doctors have diagnosed Hamlet's case, and nearly all claim him as their own. This is the tendency of the specialist. It is rather a question, I think, as to the sanity of Hamlet's commentators. An astounding instance of this super-subtlety—(in itself a symptom of madness)—is shown in the comments of some of the German critics. One of these gravely informs us that the passage, "You know sometimes he walks for hours here in the lobby," proves beyond a doubt that Hamlet was really a fat man, for, in order to reduce his obesity, he took four hours' regular exercise in the lobby; but, perhaps, our German friend was a specialist in Banting. Another critic, Leo by name, supplies a still more marvellous instance of painstaking misunderstanding of the obvious in his elucidation of

Hamlet's hysterical outburst at the conclusion of the play-scene. In this, some actors use the words peacock, and others pajock, signifying toad. But our critic throws a new light upon the passage which may commend itself to some realistic Hamlet of the future. The word in dispute was, says Leo, really "hiccup," which was intended as a stage direction. Our genial critic argues that Hamlet intended to call the King an *ass*, and ass certainly rhymes with "was." The passage, he contends, should read thus:—

> "For thou dost know, oh Damon dear,
> This realm dismantled was
> Of Jove himself, and now reigns here
> A very—very—(*hiccups*)."

Hamlet's indignation is apparently too deep for words—the very height of tragic emotion finds expression in a hiccup! The unimaginativeness of the critic is in this case absolutely monumental. In 'Macbeth' we have another instance of the astounding imaginativeness of Shakespeare. The test of the greatness of a work is that it is not only great in itself, but that it is the cause of greatness in others. A very striking instance of this suggestive fecundity of the poet was told me of Mrs. Siddons in her playing the sleep-walking scene. At the words "All the perfumes of Arabia will not sweeten this little hand," the conscience-stricken woman sees with her mind's eye a stain upon her hand, and, raising it to her mouth, desperately sucks the imaginary blood from it, spitting it out as she does so. The daring of this piece of realism, which might strike the common-place as vulgar, was in reality a stroke of imaginative genius, and, I am told, produced an electrical effect upon the audience. In dramatic literature that work is highest which is most suggestive, which gives to the artist as to the spectator most opportunities of weaving round the work of the poet the embroidery of his own imagination. If I may instance a modern play, I should say that this quality is displayed in an eminent degree in Ibsen's latest work, 'The Master Builder.' We know that this play is condemned by some as a flagrant outrage of conventional form, while others dismiss it as a commonplace presentation of a commonplace theme. I must confess that, judged by Ibsen's plays, Scandinavia, in its sordid Suburbanism, seems to me an undesirable abiding-place. All the more wonderful is it that the magician should have been able to conjure up from this dank soil, which would appear congenial only to mushroom-growths, such wondrous and variegated plants. In witnessing this play we are moved by its power, we are fascinated by its originality. Few fail to feel the thud of its pulse. Each weaves his own version of its message. The master has gained his end; he has stirred the imagination of his audience; he alone remains sphinx-like, unexplained; he is the artist—wise master!

In using Shakespeare as an illustration of the highest development of the imaginative artist, and in claiming for his work that impersonality which I hold to be the distinguished mark of his genius, I am far from denying that many of our greatest writers, many of our greatest painters and actors, have been those whose personality is most resonant in their work, but I say that the intrusion of that personality is not the merit of their work, but rather its limitation. No doubt a more easily won popularity is awarded by that large public which demands an exhibition of individuality rather than of characterisation, of personality rather than of impersonation; yet it is better to strive for the higher, even if we miss it, than to clutch at the lower, even if it is within easy reach. The adroit actor should be able at will to adapt his individuality to the character he is pourtraying. By the aid of his imagination, he becomes the man, and behaves unconsciously as the man would or should behave; this he does instinctively rather than from any conscious study, for what does not come spontaneously may as well not come at all. Even the physical man will appear transformed. If he imagines himself a tall man, he will appear so to the audience—how often have we not heard people exclaim that an orator appeared to grow in height as his speech became eloquent? If the actor imagines himself a fat man, he will appear fat to the spectator. There is a kind of artistic conspiracy between the actor and his audience. It is not the outer covering, which is called the "make up," which causes this impression, it is the inner man—who talks fat, walks fat, and thinks fat. As in the planet, it is only when the internal fire ceases that the body becomes hardened and unpliable. The actor, even though he be peasant born, will be able by the power of his imagination to acquire the rare gift of distinction. He will be able, by the aid of his imagination, to become a king—that is to say, not the accidental king, who in actual life may lack dignity, but the king of our imagination. In this connection it is on record that Napoleon the First once administered a rebuke to Talma, with whom he had a dramatic affinity. The actor, it seems, in playing a Roman emperor, used violent gestures. Napoleon, criticising this exuberance, said, "Why use these unnecessary flourishes?—When I give an order, I require nothing to enforce it—my word is enough. This is no way to behave as an emperor." The first Napoleon was a great actor—and his dramatic instinct was not the least formidable among those qualities which made him such a power in the world's history. As on the stage, so it is in real life, we are not what we are, we become what we imagine ourselves to be. A man is not always what he appears to his valet. He often finds his truest expression in his work. A great man will often appear uninteresting and commonplace in real life. Who has not felt that disappointment? The real man is to be found in his work. It is this personality which is often obliterated by his biographer—for detraction is the only tribute which mediocrity can pay to the great. This literary autopsy adds a new terror to death. A man might at least be permitted to leave his reputation to his critics, as he would leave his brains to a hospital.

But I am forgetting Napoleon—he was able to imagine himself an emperor, and, circumstances conspiring with him, he became one. His enemies thought they were belittling him by calling him an actor, and the Pope. whom he hurled from the Papal throne, could only retort "*Comediante*"; but the comedian continued to play his part of emperor while the Pope was in exile. The artistic methods of the first Napoleon are brought into strong relief when contrasted with those of his less imaginative nephew. Indeed, the difference between the imaginative and the unimaginative actor is well exemplified in these two. Had Napoleon the Third possessed the true dramatic instinct, he would not have been guilty of the Boulogne fiasco. On that occasion, in order to impress the populace with a supernatural significance of his mission, he had recourse to the stagey device of a tame eagle, which, as the emblem of empire, was at a given cue to alight upon him. But the bird, which had been trained to perch upon his top-hat, disdained his crown. Here we have an illustration of the futility of unimaginative stage-management.

The imagination is the mind's eye. To him who has it not, life presents itself as a picture possessing all the merits of a photograph, and none of the blemishes of a work of art. He who does not treasure it, will lose its use. In a burst of scientific fantasy, I once propounded the theory that the soft place on the top of a baby's head was really intended by beneficent Nature to enable us, through this yet open channel, to destroy by electricity, or what not, those tissues of the brain which go to make the vicious portions of our nature. In unfolding my discovery to a scientific friend, I learned, however, that this particular part of our brain was really a primitive eye, and was no doubt used by our prehistoric ancestors for the purpose of seeing objects overhead. The Cyclops was probably a throwback of this species. In certain lower forms of animals, I am told, in lizards, for instance, this eye is infinitely more developed than it is in the higher animals, in whom, from disuse, it has become practically extinct. Even so will the imagination, this third eye of the mind, looking heavenward, lose its function unless it is exercised. The waning of the imagination is, next to the loss of his childish faith, the most tragic thing in a man's life. I can conceive no fate more terrible than that which befalls the artist in watching with still undiminished powers of self-observation, the slow ebbing of the imaginative faculty, to see it drifting out to sea in the twilight of life. Better be deprived of sight than to feel that the world has lost its beauty—for the blind are happier than the blear-eyed. . . .

It would be interesting to know whether the cultivation of the æsthetic faculties would have strengthened or weakened in Darwin those other forces which have made him such a shining figure in the history of science. It may be that what was a loss to the man was a gain to humanity, for to every one is only vouchsafed a limited power of concentration. Nor must it be supposed that Science and Art are separate and opposing forces; they are rather two mighty currents springing from one parent source. The greatest victories which mind has achieved over matter have been due to the soaring flights of the imagination rather than to a mere crawling research along the surface of facts. This hall, where Faraday, Huxley, and Tyndall have spoken, has witnessed displays of the imagination equal to those of the highest poetry. As the diver dives for pearls into the depths of the sea, so does science project itself on the wings of the imagination into the mists which shroud the vast unexplained, snatching in its flight the secrets which solve the mysteries of the universe, and which point out to mankind the invisible stepping-stones connecting the known with the unknown.

It was in this hall that Professor Dewar summoned the elusive and invisible atmosphere, which since all time has enveloped the earth, and with the wand of science compelled it to appear before you in a palpable and visible form. Even so does the imagination distil from the elemental ether of thought and truth the liquid air of art. I have endeavoured to show that, just as the highest achievement of science is that which we owe to the imagination, so also is the highest achievement of art that which carries us out of the sordid surroundings of every-day life into the realms of idealised truth. Its loftiest mission is to preserve for us, amid the din and clash of life, those illusions which are its better part—to epitomise for us the aspirations of mankind, to stifle its sobs, to nurse its wounds, to requite its unrequited love, to sing its lullaby of death. It is the unwept tear of the criminal, it is the ode of the agnostic to immortality, it is the toy of childhood, the fairyland of the mature, and gilds old age with the afterglow of youth.

[H. B. T.]

WEEKLY EVENING MEETING,

Friday, June 2, 1893.

Sir Douglas Galton, K.C.B. D.C.L. LL.D. F.R.S. Vice-President, in the Chair.

Professor Osborne Reynolds, M.A. LL.D. F.R.S.

Study of Fluid Motion by means of Coloured Bands.

In his charming story of 'The Purloined Letter,' Edgar Allan Poe tells how all the efforts and artifices of the Paris police to obtain possession of a certain letter, known to be in a particular room, were completely baffled for months by the simple plan of leaving the letter in an unsealed envelope in a letter-rack, and so destroying all *curiosity* as to its contents; and how the letter was at last found there by a young man who was not a professional member of the force. Closely analogous to this is the story I have to set before you to-night—how certain mysteries of fluid motion, which have resisted all attempts to penetrate them are at last explained by the simplest means and in the most obvious manner.

This indeed is no new story in science. The method adopted by the minister, *D.*, to secrete his letter appears to be the favourite of Nature in keeping her secrets, and the history of science teems with instances in which keys, after being long sought amongst the grander phenomena, have been found at last not hidden with care, but scattered about, almost openly, in the most commonplace incidents of every-day life which have excited no curiosity.

This was the case in physical astronomy—to which I shall return after having reminded you that the motion of matter in the universe naturally divides itself into three classes.

1. The motion of bodies as a whole—as a grand illustration of which we have the heavenly bodies, or more humble, but not less effective, the motion of a pendulum, or a falling body.

2. The relative motion of the different parts of the same fluid or elastic body—for the illustration of which we may go to the grand phenomena presented by the tide, the whirlwind, or the transmission of sound, but which is equally well illustrated by the oscillatory motion of the wave, as shown by the motion of its surface, and by the motion of this jelly, which, although the most homely illustration, affords by far the best illustration of the properties of an elastic solid.

3. The inter-motions of a number of bodies amongst each other —to which class belong the motions of the molecules of matter

resulting from heat, as the motions of the molecules of a gas, in illustration of which I may mention the motions of individuals in a crowd, and illustrate by the motion of the grains in this bottle when it is shaken, during which the white grains at the top gradually mingle with the black ones at the bottom—which interdiffusion takes an important part in the method of coloured bands.

Now of these three classes of motion that of the individual body is incomparably the simplest. Yet, as presented in the phenomena of the heavens, which have ever excited the greatest curiosity of mankind, it defied the attempts of all philosophers for thousands of years, until Galileo discovered the laws of motion of mundane matter. It was not until he had done this and applied these laws to the heavenly bodies that their motions received a rational explanation. Then Newton, taking up Galileo's parable and completing it, found that its strict application to the heavenly bodies revealed the law of gravitation, and developed the theory of dynamics.

Next to the motions of the heavenly bodies, the wave, the whirlwinds, and the motions of clouds, had excited the philosophical curiosity of mankind from the earliest time. Both Galileo and Newton, as well as their followers, attempted to explain these by the laws of motion, but although the results so obtained have been of the utmost importance in the development of the theory of dynamics it was not till this century that any considerable advance was made in the application of this theory to the explanation of fluid phenomena, and although during the last fifty years splendid work has been done, work which, in respect of the mental effort involved, or the scientific importance of the results, goes beyond that which resulted in the discovery of Neptune, yet the circumstances of fluid motion are so obscure and complex that the theory has yet been interpreted only in the simplest cases.

To illustrate the difference between the interpretation of the theory of the heavenly bodies and that of fluid motion, I would call your attention to the fact that solid bodies, on the behaviour of which the theory of the motion of the planets is founded, move as one piece, so that their motion is exactly represented by the motion of their surfaces; that they are not affected with any internal disorder which may affect their general motion. So surely is this the case, that even those who have never heard of dynamics can predict with certainty how any ordinary body will behave under any ordinary circumstances, so much so that any departure is a matter of surprise. Thus I have here a cube of wood, to one side of which a string is attached. Now hold it on one side, and holding the string you naturally suppose that when I let go it will turn down so as to hang with the string vertical; it does not do so, that is a matter of surprise; I place it on the other side and it still remains as I place it. If I swing it as a pendulum it does not behave like one.

Would Galileo have discovered the laws of motion had his pendulum behaved like this? Why is its motion peculiar? There is internal motion. Of what sort? Well, I think my illustration may carry more weight if I do not tell you; you can all, I have no doubt, form a good idea. It is not fluid motion or I should feel bound to explain it. You have here an ordinary looking object which behaves in an extraordinary manner, which is yet very decided and clear, to judge by the motion of its surface, and from the manner of the motion I wish you to judge of the cause of the observed motion.

This is the problem presented by fluids, in which there may be internal motion which has to be taken into account before the motion of the surface can be explained. You can see no more of what the motion is within a homogeneous fluid, however opaque or clear, than you can see what is going on within the box. Thus, without colour bands the only visual clue to what is going on within the fluids is the motion of their bounding surfaces. Nor is this all; in most cases the surfaces which bound the fluid are immovable.

In the case of the wave on water the motion of the surface shows that there is motion, but because the surface shows no wave it does not do to infer that the fluid is at rest.

The only surfaces of the air within this room are the surfaces of the floor, walls, and objects within it. By moving the objects we move the air, but how far the air is at rest you cannot tell unless it is something familiar to you.

Now I will ask you to look at these balloons. They are familiar objects enough, and yet they are most sensitive anemometers, more sensitive than anything else in the room; but even they do not show any motion; each of them forms an internal bounding surface of the air. I send an *aerial messenger* to them, and a small but energetic motion is seen by which it acknowledges the message, and the same message travels through the rest, as if a *ghost* touched them. It is a wave that moves them. You do not feel it, and, but for the surfaces of the air formed by the balloons, would have no notion of its existence.

In this tank of beautifully clear distilled water, I project a heavy ball in from the end, and it shows the existence of the water by stopping almost dead within two feet. The fact that it is stopped by the water, being familiar, does not raise the question, Why does it stop?—a question to which, even at the present day, a complete answer is not forthcoming. The question is, however, suggested, and forcibly suggested, when it appears that with no greater or other evidence of its existence, I can project a disturbance through the water which will drive this small disc the whole length of the tank.

I have now shown instances of fluid motion of which the manner is in no way evident without colour bands, and were revealed by colour bands, as I showed in this room sixteen years ago. At that time I was occupied in setting before you the manners of motion revealed, and I could only incidentally notice the means by which this revelation was accomplished.

Amongst the ordinary phenomena of motion there are many which render evident the internal motion of fluids. Small objects suspended in the fluid are important, and that their importance has long been recognised is shown by the proverb—straws show which way the wind blows. Bubbles in water, smoke and clouds, afford the most striking phenomena, and it is doubtless these that have furnished philosophers with such clues as they have had. But the indications furnished by these phenomena are imperfect, and, what is more important, they only occur casually, and in general only under circumstances of such extreme complexity that any deduction as to the elementary motions involved is impossible. They afford indication of commotion, and perhaps of the general direction in which the commotion is tending, but this is about all.

For example, the different types of clouds; these have always been noticed and are all named. And it is certain that each type of clouds is an indication of a particular type of motion in the air; but no deductions as to what definite manner of motion is indicated by each type of cloud have ever been published.

Before this can be done it is necessary to reverse the problem, and find to what particular type of cloud a particular manner of motion would give rise. Now a cloud, as we see it, does not directly indicate the internal motion of which it is the result. As we look at clouds, it is not in general their motion that we notice, but their figure. It is hard to see that this figure changes while we are watching a cloud, though such a change is continually going on, but is apparently very slow on account of the great distance of the cloud and its great size. However, types of clouds are determined by their figure, not by their motion. Now what their figure shows is not motion, but is the history or result of the motion of particular strata of the air in and through surrounding strata. Hence, to interpret the figures of the clouds we must study the changes in shape of fluid masses, surrounded by fluid, which result from particular motions.

The ideal in the method of colour bands is to render streaks or lines in definite position in the fluid visible, without in any way otherwise interfering with these properties as part of the homogeneous fluid. If we could by a wish create coloured lines in the water these would be ideal colour bands. We cannot do this, nor can we exactly paint lines in the air or water.

I take this ladle full of highly coloured water, lower it slowly into the surface of the surrounding water till that within is level with that without; then turn the ladle carefully round the coloured water; the mass of coloured water will remain where placed.

I distribute the colour slowly. It does not mix with the clear water, and although the lines are irregular they stand out very beautifully. Their edges are sharp here. But in this large sphere, which was coloured before the lecture, although the coloured lines have generally kept their places, they have, as it were, swollen out and become merged in the surrounding water in consequence of molecular motion. The sphere shows, however, one of the rarest phenomena in Nature—the internal state in almost absolute internal rest. The forms resemble nothing so much as stratus clouds, as seen on a summer day, though the continuity of the colour bands is more marked. A mass of coloured water once introduced is never broken. The discontinuity of clouds is thus seen to be due to other causes than mere motion.

Now, having called your attention to the rarity of water at rest, I will call your attention to what is apt to be a very striking phenomenon, namely, that when water is contained, like this, in a spherical vessel of which you cannot alter the shape, it is impossible by moving the vessel suddenly to set up relative motion in the interior of the water. I may swing this vessel about and turn it, but the colour band in the middle remains as it was, and when I stop shows the water to be at rest.

This is not so if the water has a free surface, or if the fluid is of unequal density. Then a motion of the vessel sets up waves, and the colour band shows at once the beautifully lawful character of the internal motion. The colour bands move backwards and forwards, showing how the water is distorted like a jelly, and as the wave dies out the colour bands remain as they were to begin with.

This illustrates one of the two classes of internal motion of water or fluid. Wherever fluid is not in contact with surfaces over which it has to glide, or which surfaces fold on themselves, the internal motions are of this purely wave character. The colour bands, however much they may be distorted, cannot be relatively displaced, twisted, or curled up, and in this case motion in water once set up continues almost without resistance. That wave motion in water with a free surface, is one of the most difficult things to stop is directly connected with the difficulty of setting still water in motion; in either case the influence must come through the surfaces. Thus it is that waves once set up will traverse thousands of miles, establishing communication between the shores of Europe and America. Wave motion in water is subject to enormously less resistance than any other form of material motion.

In wave motion, if the colour bands are across the wave they show the motion of the water; nevertheless, their chief indication is of the change of shape while the fluid is in motion.

This is illustrated in this long bottle, with the coloured water less heavy than the clear water. If I lay it down in order to establish equilibrium, the blue water has to leave the upper end of the bottle and spread itself over the clear water, while the clear water runs under the coloured. This sets up wave motion, which continues after the bottle has come to rest. But as the colour bands are parallel with the direction of motion of the waves, the motion only becomes evident in thickening and bending of the colour bands.

The waves are entirely between the two fluids, there being no motion in the outer surfaces of the bottle, which is everywhere glass.

They are owing to the slight differences in the density of the fluids, as is indicated by the extreme slowness of the motion. Of such kind are the waves in the air, that cause the clouds which make the mackerel sky, the vapour in the tops of the waves being condensed and evaporated again as it descends, showing the results of the motion.

The distortional motions, such as alone occur in simple wave motion, or where the surfaces of the fluid do not fold in on themselves, or wind in, are the same as occur in any homogeneous continuous material which completely fills the space between the surfaces.

If plastic material is homogeneous in colour it shows nothing as to the internal motion; but if I take a lump built of plates, blue and white, say a square, then I can change the surfaces to any shape without folding or turning the lump, and the coloured bands which extend throughout the lump show the internal changes. Now the first point to illustrate is that, however I change its shape, if I bring it back to the original shape the colour bands will all come back to their original positions, and there is no limit to the extent of the change that may thus be effected. I may roll this out to any length, or draw it out, and the diminution in thickness of the colour bands shows the extent of the distortion. This is the first and simplest class of motion to which fluids are susceptible. By this motion alone the elements of the fluid may be, and are, drawn out to an indefinitely fine line, or spread out in an indefinitely thin sheet, but they will remain of the same general figure.

By reversing the process they change back again to the original form. No colour band can ever be broken, even if the outer surface be punched in till the punch head comes down on the table; still all the colour bands are continuous under the punch, and there is no folding or lapping of the colour bands unless the external surface is folded.

The general idea of mixture is so familiar to us that the vast generalisation to which these ideas afford the key, remains unnoticed. That continued mixing results in uniformity, and that uniformity is only to be obtained by mixing, will be generally acknowledged, but how deeply and universally this enters into all the arts can but rarely have been apprehended. Does it ever occur to any one that the beautiful uniformity of our textile fabrics has only been obtained by the development of processes of mixing the fibres. Or, again, the uniformity in our construction of metals; has it ever occurred to any one that the inventions of Arkwright and Cort were but the application of the long-known processes by which mixing is effected in culinary operations? Arkwright applied the draw-rollers to uniformly extend the length of the cotton sliver at the expense of the thickness; Cort applied the rolling-mill to extend the length of the iron bloom at the expense of its breadth; but who invented the rolling-pin by which the pastry-cook extends the length at the expense of the thickness of the dough for the pie-crust?

In all these processes the object, too, is the same throughout—to obtain some particular shape, but chiefly to obtain a uniform texture. To obtain this nicety of texture it is necessary to mix up the material, and to accomplish this it is necessary to attenuate the material, so that the different parts may be brought together.

The readiness with which fluids are mixed and uniformity obtained is a by-word; but it is only when we come to see the colour bands that we realise that the process by which this is attained is essentially the same as that so laboriously discovered for the arts—as depending first on the attenuation of each element of the fluid—as I have illustrated by distortion.

In fluids, no less than in cooking, spinning and rolling—this attenuation is only the first step in the process of mixing—all involve the second process, that of folding, piling, or wrapping, by which the attenuated layers are brought together. This does not occur in the pure wave motion of water, and constitutes the second of the two classes of motion. If a wave on water is driven beyond a certain height it leaps or breaks, folding in its surface. Or, if I but move a solid surface through the water it introduces tangential motion, which enables the fluid to wind its elements round an axis. In these ways, and only in these ways, we are released from the restriction of not turning or lapping. And in our illustration, we may fold up our dough, or lap it—roll it out again and lap it again; cut up our iron bar, pile it, and roll it out again, or bring as many as we please of the attenuated fibres of cotton together to be further drawn. It may be thought that this attenuation and wrapping will never make perfect admixture, for however thin each element will preserve its characteristic, the coloured layers will be there, however often I double and roll out the dough. This is true. But in the case of some fluids, and only in the case of some fluids, the physical process of diffusion completes the admixture. These colour bands have remained in this water, swelling but still distinct; this shows the slowness of diffusion. Yet such is the facility with which the fluid will go through the process of attenuating its elements and enfolding them, that by simply stirring with a spoon these colour bands can be drawn and folded so fine that the diffusion will be instantaneous, and the fluid become uniformly tinted. All internal fluid motion other than simple distortion, as in wave motion, is a process of mixing, and it is thus from the arts we get the clue to the elementary forms and processes of fluid motion.

When I put the spoon in and mixed the fluid you could not see what went on—it was too quick. To make this clear, it is necessary that the motion should be very slow. The motion should also be in planes, at right angles to the direction in which you are looking. Such is the instability of fluid that to accomplish this at first appeared to be difficult. At last, however, as the result of much thought, I found a simple process which I will now show you, in what I think is a novel experiment, and you will see, what I think has never been seen before by any one but Mr. Foster and myself, namely, the complete process of the formation of a cylindrical vortex sheet resulting from the motion of a solid surface. To make it visible to all I am obliged to limit the colour band to one section of the sheet, otherwise only those immediately in front would be able to see between the convolutions of the spiral. But you will understand that what is seen is a section, a similar state of motion extending right across the tank. From the surface you see the plane vane extending half-way down right across the tank; this is attached to a float.

I now institute a colour band on the right of the vane out of the tube. There is no motion in the water, and the colour descends slowly from the tube. I now give a small impulse to the float to move it to the right, and at once the spiral form is seen from the tube. Similar spirals would be formed all across the tank if there were colours. The float has moved out of the way, leaving the revolving spiral with its centre stationary, showing the horizontal axis of the spiral is half-way between the bottom and surface of the tank, in which the water is now simply revolving round this axis.

This is the vortex in its simplest and rarest form (for a vortex cannot exist with its ends exposed). Like an army it must have its flanks protected; hence a straight vortex can only exist where it has two surfaces to cover its flanks, and parallel vertical surfaces are not common in nature. The vortex can bend, and, as with a horse-shoe axis, can rest both its flanks on the same surface, as this piece of clay, or with a ring axis, which is its commonest form, as in the smoke ring. In both these cases the vortex will be in motion through the fluid, and less easy to observe.

These vortices have no motion beyond the rotation because they are half-way down the tank. If the vane were shorter they would follow the vane; if it were longer they would leave it.

In the same way, if instead of one vortex there were two vortices, with their axes parallel, extending right across, the one above another, they would move together along the tank.

I replace the float by another which has a vane suspended from it, so that the water can pass both above and below the vane extending right across the middle portion of the tank. In this case I institute two colour bands, one to pass over the top, the other underneath, the vane, which colour bands will render visible a section of each vortex just as in the last case. I now set the float in motion and the two vortices turn towards each other in opposite directions. They are formed by the water moving over the surface of the vane, downwards to get under it, upwards to get over it, so that the rotation in the upper vortex is opposite to that in the lower. All this is just the same as before, but that instead of these vortices standing still as before they follow at a definite distance from the vane, which continues its motion along the tank without resistance.

Now this experiment shows, in the simplest form, the *modus* *operandi* by which internal waves can exist in fluid without any motion in the external boundary. Not only is this plate moving flatwise through the water, but it is followed by all the water, coloured and uncoloured, enclosed in these cylindrical vortices. Now, although there is no absolute surface visible, yet there is a definite surface which encloses these moving vortices, and separates them from the water which moves out of their way. This surface will be rendered visible in another experiment I shall show you. Thus the water which has only wave motion is bounded by a definite surface, the motion of which corresponds to the wave; but inside this closed surface there is also water, so that we cannot see the surface, and this water inside is moving round and round, but so that its motion at the bounding surface is everywhere the same as that of the outside water.

The two masses of water do not mix. That outside moves out of the way of and past the vortices over the bounding surface, while the vortices move round and round inside the surface in such a way that it is moving in exactly the same manner at the surface as the wave surface outside.

This is the key to the internal motion of water. You cannot have a pure wave motion inside a mass of fluid with its boundaries at rest, but you have a compound motion, a wave motion outside, and a vortex within, which fulfils the condition that there shall be no sliding of the fluid over fluid at the boundary.

A means which I hope may make the essential conditions of this motion clearer occurred to me while preparing this lecture, and to this I will now ask your attention. I have here a number of layers of cotton-wool (wadding). Now I can force any body along between these layers of wadding. They yield, as by a wave, and let it go through; but the wadding must slide over the surface of the body so moving through it. And this it must *not* do if it illustrate the conditions of fluid motion. Now there is one way, and only one way, in which material can be got through between the sheets of wadding without slipping. It must roll through; but this is not enough, because if it rolls on the under surface it will be slipping on the upper. But if we have two rollers, one on the top of the other, between the sheets, then the lower roller rolls on the bottom sheet, the upper roller rolls against the upper sheet, so that there is no slipping between the rollers or the wadding, and, equally important, there is no slipping between the rollers, as they roll on each other. I have only to place a sheet of canvas between the rollers and draw it through; both the flannel rollers roll on the canvas and on the wadding, which they pass through without slipping, causing the wadding to move in a wave outside them, and affording a complete parable of the vortex motion.

I will now show by colour bands some of the more striking phenomena of internal motion, as presented by Nature's favourite form of vortex, the vortex ring, which may be described as two horseshoe vortices with their ends founded on each other.

To show the surface separating the water moving with the vortex from that which gives way outside, I discharge from this orifice a mass of coloured water, which has a vortex ring in it formed by the surface as already described. You see the beautifully defined mass moving on slowly through the fluid, with the proper vortex ring motion, but very slow. It will not go far before a change takes place, owing to the diffusion of the vortex motion across the bounding surface; then the coloured surface will be wound into the ring which will appear. The mass approaches the disc in front. It cannot pass, but will come up and carry the disc forward; but the disc, although it does not destroy the ring, disturbs the motion.

If I send a more energetic ring it will explain the phenomenon I showed you at the beginning of this lecture; it carries the disc forward as if struck with a hammer. This blow is not simply the weight of the coloured ring, but of the whole moving mass and the wave outside. The ring cannot pass the disc without destruction with the attendant wave.

Not only can a ring follow a disc, but as with the plane vane so with the disc, if we start a disc we must start a ring behind it.

I will now fulfil my promise to reveal the silent messenger I sent to those balloons. The messenger appears in the form of a large smoke ring, which is a vortex ring in air rendered visible by smoke instead of colour. The origination of these rings has been carefully set so that the balloons are beyond the surface which separates the moving mass of water from the wave, so that they are subject to the wave motion only. If they are within this surface they will disturb the direction of the ring, if they do not break it up.

These are, if I may say so, the phenomenal instances of internal motion of fluids. Phenomenal in their simplicity they are of intense interest, like the pendulum, as furnishing the clue to the more complex. It is by the light we gather from their study that we can hope to interpret the parable of the vortex wrapped up in the wave, as applied to the wind of heaven, and the grand phenomenon of the clouds, as well as those things which directly concern us, such as the resistance of our ships.

[O. R.]

GENERAL MONTHLY MEETING,

Monday, June 5, 1893.

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

George Matthews Arnold, Esq.
Charles Claude Carpenter, Esq.
Frederick Henry Cheesewright, Esq. M. Inst. C.E.
Ernest Prescot Hill, Esq.
Henry Kemp, Esq. M. Inst. C.E.
The Right Hon. Stuart Knill (Lord Mayor),
Mrs. Lucas,
Alexander Campbell Mackenzie, Esq. Mus. Doc.
Carl Edward Melchers, Esq.
Phineas Phillip, Esq.
William Cuthbert Quilter, Esq. M.P.
John Robbins, Esq. F.C.S.
John Gorges Robinson, Esq. J.P.
Thomas Thornton, Esq.
George White, Esq. B.A. LL.B.

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 at low temperatures:—

Messrs. Crossley Bros.	£50
James Mansergh, Esq.	£21
Sir Henry Doulton	£50
Captain A. Noble	£50

The special thanks of the Members were returned to Messrs. Ducretet and Lejeune (of Paris) for their present of an Electric Furnace.

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—A Dictionary of the Economic Products of India. Vols. I.-VI. 8vo. 1889-92.
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Boston Public Library—Annual Report, 1892. 8vo. 1893.
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  Analyst for May, 1893. 8vo.
  Athenæum for May, 1893. 4to.
  Author for May, 1893.
  Chemical News for May, 1893. 4to.
  Chemist and Druggist for May, 1893. 8vo.
  Electrical Engineer for May, 1893. fol.
  Electric Plant for May, 1893. 8vo.
  Engineer for May, 1893. fol.
  Engineering for May, 1893. fol.
  Horological Journal for May, 1893. 8vo.
  Industries for May, 1893. fol.
  Iron for May, 1893. 4to.
  Ironmongery for May, 1893. 4to.
  Journal of Geology, Vol. I. Nos. 1, 2. 8vo. 1893.
  Machinery Market for May, 1893. 8vo.
  Monist for May, 1893. 8vo.
  Nature for May, 1893. 4to.
  Open Court for May, 1893. 4to.
  Photographic Work for May, 1893. 8vo.
  Telegraphic Journal for May, 1893. fol.
  Transport for May, 1893. fol.
  Tropical Agriculturist, 1893. 8vo.
  World's Fair Electrical Engineering for May, 1893. 8vo.
  Zoophilist for May, 1893. 4to.
Florence Biblioteca Nazionale Centrale—Bolletino, Nos. 177, 178. 8vo. 1893.
Franklin Institute—Journal, No. 809. 8vo. 1893.
  Meteorological Observations of the Pennsylvania State Weather Service. fol. 1893.
Geographical Society, Royal—Geographical Journal, Vol. I. No. 5. 8vo. 1893.
Geological Institute, Imperial, Vienna—Verhandlungen, 1893, Nos. 2–5. 8vo.
Harvard University—Bulletin for May, 1893. 8vo.
Institute of Brewing—Transactions, Vol. VI. No. 6. 8vo. 1893.
Johns Hopkins University—American Chemical Journal, Vol. XV. No. 5. 8vo. 1893.
  American Journal of Philology, Vol. XIV. No. 1. 8vo. 1893.
  University Circular, No. 105. 4to. 1893.
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Manchester Geological Society—Transactions, Vol. XXII. Part 8. 8vo. 1893.
Meteorological Society, Royal—Meteorological Record, No. 47. 8vo. 1893.
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Payne, Wm. W. Esq. and Hale, Geo. E. Esq. (the Editors)—Astronomy and Astro-Physics for May, 1893. 8vo.

Place, Walter, Esq. (the Author)—Notes on Natal. 8vo. 1893.

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Royal Society of London—Proceedings, No. 321. 8vo. 1893.

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Abhandlungen, Band XIII. No. 6. 8vo.
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Smithsonian Institution—Contributions to Knowledge, No. 842. 8vo. 1892.

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Wagner Free Institute of Science, Philadelphia—Transactions, Vol. III. Part 2. 4to. 1892.

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WEEKLY EVENING MEETING.

Friday, June 9, 1893.

HUGO MÜLLER, Esq. Ph.D. F.R.S. Vice-President, in the Chair.

Professor T. E. THORPE, D.Sc. F.R.S. *M.R.I.*

The Recent Solar Eclipse.[*]

MOST people who take any interest in those larger problems with which men of science are nowadays concerned are aware that there are certain questions relating to the chemistry and physics of the sun which, at present, can only be solved by observations made during the fleeting moments of the total phase of a solar eclipse. Thanks to the action of the *Nautical Almanac* office in this country, and of similar institutions in other countries, we have not only ample warning of the advent of an eclipse of the sun, but we are furnished with such details concerning the time of its occurrence, the direction of the path of the moon's shadow on the earth, and the duration of the various phases, that we are enabled to decide whether it is expedient to attempt to seize the precious seconds during which the sun is obscured by the moon, in order to get further light on those questions which, as has been said, can only be at present solved, or at least studied, at such times.

During the eclipse of last April the moon's shadow swept over a considerable expanse of land. It touched the coast of Chili in latitude 29° S. at about 8.15 A.M. of local time, passed over the highlands of that country, across the borders of Argentina and Paraguay, and over the vast plains and forests of Central Brazil, emerging, at about noon of local time, at a short distance to the north-west of Ceara on the North Atlantic seaboard. Crossing the Atlantic, at about its narrowest part, it struck the coast of Africa north of the river Gambia, and finally disappeared somewhere in the Sahara. It would seem, therefore, there was ample choice in the selection of stations. But all situations were not equally good or equally available. There were, indeed, special reasons why every effort should be put forth to observe this eclipse as completely as possible. To begin with, it had an unusually long totality—upwards of four minutes at places at or near the central line of the shadow. Next, it occurred at about a period of maximum of solar energy, and hence we had an

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[*] A full report of the discourse is given in the 'Fortnightly Review,' July 1893.

opportunity of solving certain questions as to the connection between the character of the corona and the solar cycle. Further, it was hoped that by multiplying the stations along the path of the eclipse, and therefore by making observations at considerable intervals of time, the photographic records might decide upon the possibility of changes in the form and internal disposition of the corona—a question of the greatest importance in regard to the physical nature of this solar appendage.

After careful consideration of sites, and of the various suggestions which were made as to the nature of the work to be undertaken, a committee, representing the Royal Society, the Royal Astronomical Society, and the Solar Physics Committee of the Science and Art Department, decided to send two observers to Para Curu, in the province of Ceara in Brazil; and four observers to some station in Senegal, preferably Fundium, on the Salum river. Substantially the same scheme of work was arranged for the two parties. Spectroscopic observations with the Prismatic Camera and a series of photographs with what is now known as the Duplex Coronagraph were to be taken at each station. In the case of the African station, it was further decided that photometric measurements of the coronal light should be made by the method adopted by Captain Abney and myself on the occasion of the West Indian eclipse of 1886.

It will be understood that the work of both parties was entirely confined to the study of the corona. In the first place, photographic records of its form, its extension and internal structure were to be made according to a uniform plan at both stations. The apparatus to be used consisted of a sort of double camera, in one compartment of which was placed a 4-inch lens of 60 inches focus, belonging to Captain Abney, which has already seen much service in eclipse photography. It was employed in Egypt in 1882, in the Caroline Islands in 1883, in the West Indies in 1886, and in the Salut Isles, in French Guiana, where that veteran eclipse observer, Father Perry, lost his life, in 1889. One special reason for using this lens was that the continuity of the series of photographs which have been obtained by it might be maintained. It gives pictures on the scale of about half an inch to the moon's diameter. In the other compartment was a 4-inch Dallmeyer photo-heliograph lens mounted in combination with a 2½-inch Dallmeyer negative lens of 8-inch negative focus, giving with the total length of 68 inches pictures on the scale of over 1½ inch to the moon's diameter. This double camera was fitted with special plate-carriers, enabling two plates to be exposed at the same time, one to each lens, so that by one operation of changing and exposing, two pictures of the eclipsed sun could be simultaneously obtained. The times of exposure were so arranged that the longest exposed picture with the enlarging combination should receive the same photographic effect as the shortest exposed picture with the Abney lens. The whole arrangement was equatorially mounted, so that the plates were kept in a constant position with respect to the sun during the times of exposure.

Three different methods were employed to obtain photographic records of the spectrum of the corona. In the first, which was suggested by Professor Norman Lockyer more than twenty years ago, the eclipsed sun was to be photographed through a prism attached to a telescope of 6-inch aperture. In this manner an image of the corona would be obtained corresponding with each kind of light emitted by it. Thus, if the corona consisted entirely of glowing hydrogen, there would be an image in the position occupied by each of the lines in the hydrogen spectrum. If, as may be expected, the materials composing the corona are different in different regions, the images obtained will not exactly resemble each other, but the form of each image will depend upon the distribution of that particular spectral line through the corona. The complete spectrum of every part of the corona which is bright enough to be photographed will, therefore, be obtained with a single exposure.

The other method of studying the spectrum of the corona is by means of the ordinary slit spectroscopes. The arrangement, employed by Captain Hills, consisted of two spectroscopes, each provided with a condensing lens and camera, mounted on an equatorial stand. The spectroscopes were of different dispersive power, one having two prisms, and the other one. The slits were placed parallel to each other, and were so arranged as to cut across opposite limbs of the sun at right angles to the sun's equator. An image of the sun is thrown on the slit by the condensing lens, and the slit is long enough to cover the whole width of the corona. The resulting photographs ought then to show at least three different spectra: a continuous spectrum over the dark body of the moon, on either side of which will appear the prominence spectrum, and outside of which again will be the true corona spectrum, which may or may not be broken up into bands by the occurrence of rifts or dark spaces in the corona. This method has the great advantage of discriminating between the different spectra of every portion of the corona along the line of the slit; the main difficulty connected with it is the want of light, which makes it almost impossible to give a sufficient exposure unless the slit is opened rather wide. It was decided, therefore, to make only one exposure with each spectroscope; this was to last as nearly as possible the whole time of totality, and the most rapid photographic plates procurable were to be used.

The measurement of the visual brightness of the coronal light was to be effected by the following arrangement. An image of the corona is accurately focused on a white screen by means of an equatorial of 6-inch aperture and 78-inch focal length, and the intensity of the light from different portions of the corona at definite distances from the limb is compared with that of a standard glow-lamp by means of an arrangement constructed on the principle of the Bunsen photometer, the light from the standard glow-lamp being varied by introducing a variable amount of resistance into the current and measuring the current strength at the moment of comparison.

In order to ascertain the total intensity of the coronal light Mr. Forbes employed a similar contrivance, his screen, however, having only one large translucent spot or disc, as in the ordinary Bunsen photometer. Concurrently with these observations it was arranged that the photographic intensity of the coronal light, as distinct from the visual intensity, should also be measured by a method devised by Captain Abney, which consists in impressing standard intensity scales along the edges of the photographic plates to be exposed in the coronagraph, these being developed at the same time as the coronal pictures. The photographic plates to be used in the split spectroscope were also provided, in like manner, with standard scales, with a view of measuring the comparative luminosity of different portions of the coronal spectrum, a point which has an important bearing on the question of the possibility of photographing the corona in ordinary sunlight.

On the day preceding that of the eclipse the French gunboat *Brandon* came up the river, bringing with her the Governor of Senegal. His Excellency M. de la Mothe, together with the administrator of the district, M. Allys, to whom the expedition is indebted for many courtesies, paid a visit to the English camp at Fundium and witnessed the final rehearsal of our operations. They arranged for a guard to protect the enclosure during the time of the eclipse, and gave orders that all chanting, screaming, or beating of tom-toms in the village was to be forbidden.

On Sunday, the 16th, the day of the eclipse, although the morning was bright and clear, the effects of the comparatively moist winds from the sea were to be seen in the changed colour of the sky and the prevalence of thin haze. Still the sky was almost cloudless, save for a few thin wispy cirri, which floated almost motionless near the horizon. A gentle air from the west made scarce a ripple on the yellow waters of the Salum. As the day advanced, the sky became even lighter in colour, and there was a perceptible haze in the neighbourhood of the sun; the wind almost died away, and everything betokened that we should have to face—as indeed we fervently hoped might be the case—the pitiless glare of that fiercest of all suns—the African sun at noon. At 12.30 our party went ashore, the huts were uncovered, the equatorials adjusted, clocks wound, and the instruments set running on the sun. Shortly before 2 P.M. the officers and men from the *Alecto*, bringing their lanterns, came to the camp and took up their several positions. As the light waned there was a distinct feeling of chilliness in the air, and the wind suddenly rose in short sharp gusts. The few natives who had congregated round the stockade began to show signs of trepidation, but no sound of distress or fear was heard save the plaintive cry of a tethered goat near the administrator's house. There was a great hush as the last gleam of sunlight died away. The corona seemed almost to flash into existence, so suddenly did its light grow in intensity. Faint indications of its appearance could, indeed, be perceived on the photometer screen some seconds before the last trace of the yellow crescent disappeared. The phenomenon known as "Baily's beads" was plainly visible. The lower corona was wonderfully bright, and a whole row of prominences started into view. The panaches, sheafs, and other evidences of "structure" were distinctly marked on the white screen. The general sky illumination was so great that only some five or six stars were visible. The gloom, indeed, was nothing like so intense as I had seen in previous eclipses, and there would have been little difficulty in reading the second-hand of the chronometer or the scales of the ammeters without the aid of the lighted lanterns. And now the oft-repeated programme was being gone through for the last time, with a quickened sense and a concentrated earnestness springing from the consciousness that the veritable four minutes—the 240 and odd seconds—on which our thoughts for months past had been dwelling, were now speeding away, and that, with the first rush of sunlight on the other side of the black disc of the moon, our opportunities would be gone for ever. The silence was most impressive; it was broken by the stentorian voice of the quartermaster as he told us at intervals, by the aid of his log-glass, the number of seconds that still remained to us. Now and again, too, one heard from the adjoining huts the command to expose, and the sharp click of the carriers as slide after slide was inserted and withdrawn. Thanks to the repeated drills, everything went with the smoothness and regularity of clockwork. There was no hitch or stoppage, and no undue haste on the part of anybody. Sergeant Kearney secured ten out of the twelve corona pictures that he had been instructed to make. Mr. Fowler, in all, made thirty exposures in the prismatic camera, including a number taken during the five minutes before and after totality; and Captain Hills obtained both his slit-spectroscope photographs. Mr. Gray and I made twenty photometric measurements of the light from different parts of the corona, and Mr. Forbes obtained eleven concordant observations of its total intensity. The full measure of our success was not yet known to us, but every man had the certain knowledge that he had secured enough to make the eclipse of April 16, 1893, take its place as one of the best observed eclipses of recent times, and that his work, done at the sacrifice of much personal comfort, and under the trying circumstances of a fierce temperature and an unhealthy climate, would contribute towards the solution of one of the most profoundly interesting of all physical problems.

After a short rest the command, "Down huts," was given, and in a few hours the *Alecto*, with all our cases once more packed and safely stowed, was groping her way amongst the shallows and banks of the Salum down to the sea. The memory of our green-canvassed structures and of the strange instruments of brass and iron with which we English sought to shoot the moon for trying to eat up the sun has now doubtless become one of the traditions of the Wolofs and Sereres of Fundium.

M. Deslandres, I am happy to say, was not less successful. In a communication which he has just made to the French Academy he gives a brief account of some of the main results he has gathered from the photographs which he was able to take. His instrumental equipment enabled him to obtain photographs of the corona, to study its spectrum, to examine the coronal light in the most refrangible part of the ultra-violet, and to measure the rotation of the corona by the method of displacement of the lines in its spectrum. His coronal photographs showed luminous jets of a length equal to twice the diameter of the sun, while the general form was similar to that usually observed at times of maximum sun-spot frequency. The spectrum photographs have revealed the existence of at least fifteen new coronal and chromospheric lines. But the most novel of M. Deslandres's observations relate to the rotation of the corona. His negatives showed the spectra of two points on exactly opposite sides of the corona, situated in the equatorial plane of the sun, at a distance equal to two-thirds of his diameter. The lines in the spectra indicated large displacements, and from the measurements, M. Deslandres concludes that the corona must travel nearly with the disc in its motion, and thus be subject to its periodical rotational movement.

M. Bigourdan, who had been stationed at Joal, on the coast of Senegal, since December last, for the purpose of observing southern nebulæ and making pendulum observations, was commissioned by the Bureau des Longitudes to search during the eclipse for the inter-mercurial planet which Leverrier assumed to exist, and which he named Vulcan. M. Bigourdan was also requested to make careful determinations of all the four contacts, with a view of obtaining additional data for correcting the tables of the motion of the sun and moon.

As regards Vulcan, M. Bigourdan was not more successful than his predecessors, but he determined with great accuracy the time of the total phase at Joal, which he found to be 4 min. 1 sec. My own observations at Fundium, which is about as much to the south of the probable central line as Joal is to the north, gave 4 min. 3 sec. as the time of totality, which is in very fair accord with M. Bigourdan's determination. M. Coculesco, a young Roumanian astronomer, who volunteered to accompany M. Deslandres to Fundium, found 4 min. 11 sec.

As yet we have only meagre information of the results obtained by other observers. In spite of the many chances against them, Mr. Taylor and Mr. Shackleton were successful at Para Curu.

Although large portions of the sky were covered with cumuli, the sun was not clouded over at the period of totality; the atmosphere, of course, was nearly saturated with aqueous vapour, but no haze or precipitation of moisture seems to have occurred, and in consequence of the remarkable transparency of the air the photographs are certain to be of exceptional interest.

The Americans, who were mainly stationed in Chili, were equally fortunate. At Minas Aris, the Harvard College station, the atmospheric conditions are said to have been all that could have been wished for; there was no passing cloud or haze to mar the observations. The corona is reported by Professor Pickering to resemble that of 1857, as portrayed by Liais, and that of 1871, as observed by Captain Tupman. There were four streamers, two of which had a length exceeding the sun's radius, or stretching out more than 435,000 miles. Several dark rifts were visible, extending outwards from the moon's limb to the utmost limit of the corona. No rapid movement was observed within the streamers. The moon appeared of almost inky blackness, while from behind it, streamed out on all sides radiant filaments, beams, and sheets of pearly light. The inner corona was of dazzling brightness, but still more dazzling were the eruptive prominences which blazed through it, to use the words of Professor Young, like carbuncles. Generally, the inner corona had a uniform altitude, forming a ring of four minutes of arc in width, but separated with more or less definiteness from the outer corona, which projected to a far greater distance, and was much more irregular in shape. The outer corona seems to have been much larger than in 1879 or 1889, as, indeed, might have been expected at a period of maximum solar energy. The party seems to have been successful in photographing for the first time the "reversing layer" of the solar atmosphere.

Professor Schaeberle, from the Lick Observatory, who observed at Mina Bronces, in the Desert of Atacama, reports that the corona was similar to that of 1883. He obtained in all fifty photographs, eight of which are ten by twenty inches in size, and one of which shows an image of the sun four inches in diameter, the corona covering a plate eighteen by twenty-two inches—a truly "record" result. The photographs are said to afford strong presumptive evidence of the truth of the mechanical theory of the corona which is associated with Professor Schaeberle's name.

I cannot close without some reference to the debt of gratitude we are under to Captain Lang and his officers, for the readiness, zeal, and intelligence with which they co-operated in our work. Indeed, the whole crew of the gunboat did all in their power, often under circumstances of no little personal hardship, to minister to our success, and to contribute to our comfort. The best-laid schemes of astronomers, as of other men, "gang aft a-gley." There is a spanner to make, or a bit of soldering to be done, or a piece of woodwork to be altered. Assistance of this kind was always most cheerfully and promptly rendered. Lastly, it remains to be said, the recollection of the hospitality of H.M.S. *Alecto* and of H.M.S. *Blonde*, which took us away from the fever-stricken coast, will ever remain one of the pleasantest of the associations connected with the successful expedition of the African eclipse party.

[T. E. T.]

GENERAL MONTHLY MEETING,

Monday, July 3, 1893.

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

His Royal Highness The Duke of Connaught, K.G.

was elected an Honorary Member of the Royal Institution.

Alfred Walter Soward, Esq. F.C.S.

was elected a Member 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 at low temperatures:—

Ludwig Mond, Esq.	£500
Robert Hannah, Esq.	50
Sir Walter Gilbey, Bart.	21
Henry Arthur Blyth, Esq.	21
James Blyth, Esq.	21

The Special Thanks of the Members were returned to William Schooling, Esq. *M.R.I.* for his present of Portraits of Sir George B. Airy, Professor J. Crouch Adams, Professor Cayley, and Dr. William Huggins.

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, 1893, Part 1. 8vo.
Accademia dei Lincei, Reale, Roma—Classe di Scienze Fisiche, Matematiche e Naturali. Atti, Serie Quinta: Rendiconti. 1° Semestre, Vol. II. Fasc. 8, 9. 8vo. 1893.
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Astronomical Society, Royal—Monthly Notices, Vol. LIII. No. 7. 8vo. 1893.
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Brymner, Douglas, Esq. (the Archivist)—Report on Canadian Archives, 1892. 8vo. 1893.

Chemical Industry, Society of—Journal, Vol. XII. No. 5. 8vo. 1893.
Chemical Society—Journal for June, 1893. 8vo.
Clerke, Miss Agnes M. (the Author)—History of Astronomy during the Nineteenth Century. 3rd edition. 8vo. 1893.
Cornwall Polytechnic Society, Royal—Annual Report for 1892. 8vo.
Cracovie. L'Académie des Sciences—Bulletin, 1893, No. 5. 8vo.
East India Association—Journal, Vol. XXV. No. 6. 8vo. 1893.
Editors—American Journal of Science for June, 1893. 8vo.
Analyst for June, 1893. 8vo.
Athenæum for June, 1893. 4to.
Brewers' Journal for June, 1893. 8vo.
Chemical News for June, 1893. 4to.
Chemist and Druggist for June, 1893. 8vo.
Electrical Engineer for June, 1893. fol.
Electric Plant for June, 1893. 4to.
Engineer for June, 1893. fol.
Engineering for June, 1893. fol.
Engineering Review for June, 1893. 8vo.
Horological Journal for June, 1893. 8vo.
Industries for June, 1893. fol.
Iron for June, 1893. 4to.
Ironmongery for June, 1893. 4to.
Machinery Market for June, 1893. 8vo.
Nature for June, 1893. 4to.
Open Court for June. 1893. 4to.
Photographic News for June, 1893. 8vo.
Photographic Work for June, 1893. 8vo.
Telegraphic Journal for June, 1893. fol.
Transport for June, 1893.
Zoophilist for June. 1893. 4to.
Electrical Engineers, Institution of—Journal, No. 106. 8vo. 1893.
Florence Biblioteca Nazionale Centrale—Bolletino, Nos. 179, 180. 8vo. 1893.
Franklin Institute—Journal, No. 810. 8vo. 1893.
Geographical Society, Royal—Geographical Journal, Vol. I. No. 6. 8vo. 1893.
Institute of Brewing—Transactions, Vol. VI. No. 7. 8vo. 1893.
Johns Hopkins University—Studies in Historical and Political Science, Eleventh Series, Nos. 5, 6. 8vo. 1893.
University Circulars, No. 106. 4to. 1893.
Leeds Literary and Philosophical Society—Annual Report, 1892–3. 8vo.
Madras Government –Madras Meridian Circle Observations, Vol. VI. 1877–79. 8vo. 1893.
Hourly Meteorological Observations, 1856-61. 8vo. 1893.
Maillot, H. Esq. (the Author)—Dissertation sur les systèmes des poids et mesures et de numération. 8vo. 1892.
Massachusetts Institute of Technology—Vol. V. No. 4. 8vo. 1892.
Meriden Scientific Association—Addresses. 8vo. 1892.
Microscopical Society, Royal—Journal, 1893, Part 3. 8vo.
Ministry of Public Works, Rome—Giornale del Genio Civile, 1893, Fasc. 3, 4, and Designi. fol. 1893.
Odontological Society—Transactions, Vol. XXV. No. 8. 8vo. 1893.
Payne, W. W. and Hale, G. E. (the Editors)—Astronomy and Astro-Physics for June, 1893. 8vo.
Pharmaceutical Society of Great Britain—Journal for June, 1893. 8vo.
Philadelphia Geographical Club—Bulletin, Vol. I. No. 1. 8vo. 1893.
Royal Institution of Cornwall—Journal, Vol. XI. Part 2. 8vo, 1893.
Royal Society of London—Proceedings, Nos. 322, 323. 8vo. 1893.
Salomons, Sir David, Bart. M.A. M.R.I. (the Author)—The Management of Accumulators. 7th edition. 8vo. 1893.
Selborne Society—Nature Notes for July, 1893. 8vo.

Society of Architects—Proceedings, Vol. V. No. 12. 8vo. 1893.
Society of Arts—Journal for June, 1893. 8vo.
Tacchini, Prof. P. Hon. Mem. R.I. (the Author)—Memorie della Società degli Spettroscopisti Italiani, Vol. XXII. Disp. 5ª. 4to. 1893.
United Service Institution, Royal—Journal, No. 184. 8vo. 1893.
United States Department of Agriculture—Monthly Weather Review for March, 1893. 4to.
Bulletin, No. 3. 8vo. 1893.
The Hawks and Owls of the U.S. By A. K. Fisher. 8vo. 1893.
Vogel, E. Esq. (the Author)—The Atomic Weights are, under Atmospheric Pressure, not identical with the specific gravities. 8vo. 1893.
Zoological Society of London—Proceedings, 1893, Part 1. 8vo. 1893.
Transactions, Vol. XIII. Part 6. 4to. 1893.

GENERAL MONTHLY MEETING,

Monday, November 6, 1893.

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

John Astley Bloxam, Esq. F.R.C.S.
Matthew Wilks Geary, Esq. F.R.G.S.
James Sidney Hargrove, Esq.
Gordon Donaldson Peters, Esq.
Jean Paul Richter, Esq. Ph.D.
Sir Richard Henry Wyatt, D.L. J.P.

were elected Members of the Royal Institution.

The Managers reported, That at their Meeting held this day they had elected Charles Stewart, Esq. M.R.C.S. Fullerian Professor of Physiology for three years (the appointment dating from January 13, 1894).

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

Lord Armstrong	£100

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. XXVI. Parts 2, 3. 8vo. 1893.
The Secretary of State for India—South Indian Inscriptions. Edited by E. Hultzsch. Vol. II. Part 2. 8vo. 1892.
The Lords of the Admiralty—Nautical Almanac Circular, No. 15.
The Meteorological Office—Hourly Means, 1890. 8vo. 1893.
British Museum (Natural History)—Catalogue of Birds, Vol. XXI. 8vo. 1893.
Catalogue of Snakes, Vol. I. 8vo. 1893.
Catalogue of Madreporian Corals, Vol. I. 4to. 1893.
The French Government—Documents Inédits sur l'Histoire de France: Lettres du Cardinal Mazarin, Tome VII. 8vo. 1893.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quinta: Rendiconti. Classe di Scienze fisiche, matematiche e naturali. 1° Semestre, Vol. II. Fasc. 10–12. 2° Semestre, Vol. II. Fasc. 1–8. 8vo. 1893.
Classe di Scienze Morali, Storiche e Filologiche, Serie Quinta, Vol. II. Fasc. 3–7. 8vo. 1893.
Agricultural Society of England, Royal—Journal, 3rd Series, Vol. IV. Parts 2, 3. 8vo. 1893.
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Proceedings, New Series, Vol. XIX. 8vo. 1893.

American Geographical Society—Bulletin, Vol. XXV. No. 2. 8vo. 1893.
American Philosophical Society—Proceedings, No. 141. 8vo. 1893.
American Society of Civil Engineers—Reference Map of the United States. 8vo. 1893.
Asiatic Society of Great Britain, Royal—Journal, 1893, Parts 3, 4. 8vo.
Asiatic Society of Bengal—Journal, Vol. LXII. Part 1, Nos. 1, 2; Part 2, Nos. 1, 2. 8vo. 1893.
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Asiatic Society, Royal (China Branch)—Journal, Vol. XXV. 8vo. 1893.
Asiatic Society, Royal (Bombay Branch)—Journal, Vol. XVIII. No. 49. 8vo. 1893.
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Bandsept, A. Esq. (the Author)—Sur certains phénomènes observés avec la combustion rationnelle du gaz. 8vo. 1893.
Bankers, Institute of—Journal, Vol. XIV. Parts 5–7. 8vo. 1893.
Basel Naturforschenden Gesellschaft—Verhandlungen, Band X. Heft 1. 8vo. 1892.
Bavarian Academy of Sciences, Royal—Sitzungsberichte, 1889, Heft 2, 3; 1893, Heft 2. 8vo.
Behr, F. B. Esq. (the Author)—Lightning Express Railway Service. 4to. 1893.
Belgium Royal Academy of Sciences—Mémoires, Tome XLVIII. XLIX. L. Part 1. 8vo. 1892-3.
Annuaire, 1892–3. 8vo.
Bulletins, 3^e Série, Tomes XXII.–XXV. 8vo. 1891–3.
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Partial Impact. 8vo. 1879.
Boston Public Library—Bulletin, Vol. IV. Nos. 1, 2. 8vo. 1893.
Botanic Society of London, Royal—Quarterly Record, No. 54. 8vo. 1893.
Bouglon, Baron R. de (the Author)—Les Reclus de Toulouse sous la Terreur. Premier Fascicule. 8vo. 1893.
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Calendar, 1893–4. 8vo. 1893.
British Astronomical Association—Memoirs, Vol. I. Parts 1–4, 6. 8vo. 1893.
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Campbell, Walter Scott, Esq. F.L.S. (the Author)—Report on Silk Culture in New South Wales. 8vo. 1893.
Canada, Geological Survey of—Catalogue of Minerals in the Museum. 8vo. 1893.
Chemical Industry, Society of—Journal, Vol. XII. Nos. 6–8. 8vo. 1893.
Chemical Society—Journal for July-Oct. 1893. 8vo.
City of London College—Calendar, 1893–4. 8vo.
Civil Engineers, Institution of—Minutes of the Proceedings, Vols. CXII.–CXIV. 8vo. 1893.
Clinical Society—Transactions, Vol. XXVI. 8vo. 1893.
Cowles, Wm. S. Esq. U.S.N.—The Columbian Naval Review of 1893. 8vo. 1893.
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Dax, Société de Borda—Bulletin, Dix-Huitième Année, 1° Trimestre. 8vo. 1893.
Devonshire Association—Report and Transactions, Vol. XXV. 8vo. 1893.
East India Association—Journal, Vol. XXV. Nos. 7, 8. 8vo. 1893.
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Electrical Engineer for July-Oct. 1893. fol.

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Lightning for July, 1893. 8vo.
Machinery Market for July-Oct. 1893. 8vo.
Monist for July-Oct. 1893. 8vo.
Nature for July-Oct. 1893. 4to.
Open Court for July-Oct. 1893. 4to.
Photographic Work for July-Oct. 1893. 8vo.
Transport for July-Oct. 1893. fol.
Tropical Agriculturist for July-Oct. 1893. 8vo.
World's Fair Electrical Engineering for July-Oct. 1893. 8vo.
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Franklin Institute—Journal, Nos. 811-814. 8vo. 1893.

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Georgofili Reale Accademia—Atti, Quinta Serie, Vol. XVI. Disp. 2ª. 8vo. 1893.

Griffith, Rev. Alexander (the Author)—The Cycle System of Measuring Time. 8vo. 1892.

Hales, John W. Esq. M.A. (the Author)—Folia Literaria. 8vo. 1893.

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Hildebardson, H. H. Esq. and Hagström, K. L. Esq. (the Authors)—Des Principales Méthodes employées pour observer et mesurer les nuages. 8vo. 1893.

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Iron and Steel Institute—Journal, 1893, No. 1. 8vo.

Johns Hopkins University—American Chemical Journal, Vol. XV. No. 6. 8vo. 1893.
American Journal of Philology, Vol. XIV. No. 2. 8vo. 1893.
University Circular, No. 107. 4to. 1893.
Studies in Historical and Political Science, Eleventh Series, Nos. 9, 10. 8vo. 1893.

Kennedy, Professor A. B. W. F.R.S. M.R.I. (the Author)—Possible and Impossible Economies in the Utilization of Energy. 8vo. 1893.

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Transactions: Botany, Vol. III. Part 8; Zoology, Vol. V. Parts 8-10. 4to 1892-3.

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Macnab, W. Esq. F.C.S. M.R.I. (the Translator)—Berthelot's Explosives and their power. Translated by C. N. Hake and W. Macnab. 8vo. 1893.

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Manchester Geological Society—Transactions, Vol. XXII. Parts 9-11. 8vo. 1893.

Manchester Literary and Philosophical Society—Memoirs and Proceedings, Vol. VII. Nos. 2, 3. 8vo. 1892-3.

Mancini, Professor Diocleziano (the Translator)—Translations fromShelley. 8vo. 1893.

Maryland State Weather Service—Monthly Report, Vol. III. No. 1. 8vo. 1893.
Massachusetts Institute of Technolgy—Technology Quarterly, Vol. VI. No. 1. 8vo. 1893.
Mechanical Engineers, Institution of—Proceedings, 1893, Nos. 1, 2. 8vo.
Medical and Chirurgical Society, Royal—Transactions, Vol. LXXV. 8vo. 1892.
Meteorological Society, Royal—Meteorological Record, No. 48. 8vo. 1893.
Quarterly Journal, No. 87. 8vo. 1893.
Microscopical Society, Royal—Journal, 1893, Parts 4, 5. 8vo.
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Musical Association—Proceedings, 19th Session, 1892–3. 8vo. 1893.
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New York Academy of Sciences—Annals, Vol. VII. Nos. 1–5. 8vo. 1893.
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Numismatic Society—Chronicle and Journal, 1893, Parts 2, 3. 8vo.
Payne, Wm. W. Esq. and Hale, Geo. E. Esq. (the Editors)—Astronomy and Astro-Physics for July–Oct. 1893. 8vo.
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Photographic Society of Great Britain—Catalogue of the Library and Museum. 8vo. 1893.
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Prince, C. Leeson, Esq. F.R.A.S. (the Author)—The Great Drought of 1893. 8vo. 1893.
Radcliffe Library—Catalogue of Books added to Library during 1892. 4to. 1893.
Raffard, N. J. Esq. (the Author)—Considérations sur le Régulateur de Watt. 8vo. 1893.
Renwick, the Hon. Arthur, M.D.—An Australian Language as spoken by the Awabakal. By L. E. Threlkeld. 8vo. 1892.
Richardson, Sir B. W. M.D. F.R.S. M.R.I. (the Author)—The Asclepiad, 1893, No. 2. 8vo.
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GENERAL MONTHLY MEETING,

Monday, December 4, 1893.

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

Henry Jackson Wells Dam, Esq. Ph.B.
His Excellency John Duncan, M.D.
James Mortimer Garrard, Esq.
Edward Lancelot Holland, Esq. M.A.
Surgeon-General E. M. Sinclair, M.D.
Alfred H. Tarleton, Esq.
Julius Wallack, Esq.

were elected Members of the Royal Institution.

The following Lecture Arrangements were announced:—

Professor Dewar, M.A. LL.D. F.R.S. *M.R.I.* Fullerian Professor of Chemistry, R.I. Six Lectures (adapted to a Juvenile Auditory) on Air: Gaseous and Liquid. On Dec. 28 (*Thursday*), Dec. 30, 1893; Jan. 2, 4, 6, 9, 1894.

Professor Charles Stewart, M.R.C.S. Pres. L.S. Fullerian Professor of Physiology, R.I. Nine Lectures on Locomotion and Fixation in Plants and Animals. On *Tuesdays*, Jan. 16, 23, 30, Feb. 6, 13, 20, 27, March 6, 13.

The Rev. Canon Ainger, M.A. LL.D. Three Lectures on The Life and Genius of Swift. On *Thursdays*, Jan. 18, 25, Feb. 1.

W. Martin Conway, Esq. M.A. F.S.A. F.R.G.S. Three Lectures on The Past and Future of Mountain Exploration. On *Thursdays*, Feb. 8, 15, 22.

Professor Max Müller, M.A. LL.D. Three Lectures on The Vedânta Philosophy. On *Thursdays*, March 1, 8, 15.

Professor W. H. Cummings, F.S.A. Hon. R.A.M. Three Lectures on English Schools of Musical Composition (with Musical Illustrations). On *Saturdays*, Jan. 20, 27, Feb. 3.

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 Light, with special Reference to the Optical Discoveries of Newton. On *Saturdays*, Feb. 10, 17, 24, March 3, 10, 17.

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

FROM

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Brewers' Journal for November, 1893. 8vo.
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Horological Journal for November, 1893. 8vo.
Industries and Iron for November, 1893. fol.
Ironmongery for November, 1893. 4to.
Machinery Market for November, 1893. 8vo.
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Photographic News for November, 1893. 8vo.
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Tropical Agriculturist for October, 1893.
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Ex Libris Society—Journal for December, 1893. 8vo.
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Hood, Donald W. C. M.D. M.R.I.—Hospitals and Asylums of the World, by H. C. Burdett, Vols. III.-IV. and Portfolio of Plans. 4to. 1893.
The Amphioxus and its treatment, by Dr. B. Hatschek. 8vo. 1893.
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Niblett, J. T. Esq. (the Author)—Portative Electricity. 8vo. 1893.
Nisbet, John, Esq. (the Author)—Essays on Sylvicultural Subjects. 8vo. 1893.
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SPECIAL GENERAL MEETING,

Friday, December 15, 1893.

The Honorary Secretary (Sir Frederick Bramwell) reported that by order of His Grace the Duke of Northumberland, K.G. (the President), this Special General Meeting had been summoned to pass a vote of sympathy and condolence with Mrs. Tyndall on the occasion of the death (on December 4th) of Dr. Tyndall, Honorary Professor of Natural Philosophy of the Institution.

The Honorary Secretary read a letter from the President expressing regret at his inability to be present, and thereupon

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

There were also present:—Sir John Lubbock, Sir Richard Webster, Sir Joseph Lister, Sir Frederick Abel, Lord Grimthorpe, Sir Douglas Galton, Sir Philip Magnus, Dr. Edward Frankland, Dr. J. H. Gladstone, Dr. Thomas Buzzard, and Professor Dewar.

The Honorary Secretary read letters and telegrams of regret from many Members unable to be present, and expressing sympathy with the object of the meeting:—among these were Lord Kelvin, Lord Rayleigh, the Comte de Paris, Sir James N. Douglass, Sir William O. Priestley, the Rev. Canon Jenkins, Mr. Joseph Brown, and Dr. Anderson.

Sir James Crichton-Browne (the Chairman) said: I think I may venture to say that it is good for us to be here this afternoon—to withdraw ourselves, for a brief period, from business pursuits or pleasures, to assemble together in a place hallowed by the life-work of a great man who has just passed from amongst us—to build an altar to his memory, and to burn thereon the incense of our gratitude and admiration.

And nowhere more fittingly than in the Royal Institution of Great Britain can a tribute to the memory of the late Professor Tyndall be paid, for not only was his scientific career co-extensive with his connection with this Institution, but he found in it the opportunities which made that career possible, and he gave back to it the first and best fruits of his scientific labours. Tyndall contributed many papers to the Royal Society and British Association;

he published many books and pamphlets; he lectured in many places; but it was in the Royal Institution that his days were spent; it was in its laboratories that his well-devised, skilfully-executed, and far-reaching researches were carried out; it was in this theatre that, in many fascinating chapters, the story of his work was told. So intimate was his connection with this Institution, so thoroughly did he become its *genius loci*, that there must be many of you here this afternoon who, moved by your grief for his death, and touched by the associations of the scene, can almost fancy you see his lithe, nervous figure standing where I now stand, and his keen, mobile countenance, lighted up by the thick-coming conceptions of his nimble brain—can almost fancy you hear his ringing voice making again clear to all the dark and difficult sayings and discoveries of science.

Impelled by the energy of Bunsen, and drawn by the magnetism of Faraday, Tyndall may be said to have formally dedicated himself to science when he delivered his first Friday evening discourse here on "The Influence of Material Aggregation on the Manifestations of Force," on the 11th of February, 1853. His last, his valedictory discourse, the last public lecture he ever gave, on "Thomas Young and the Wave Theory," was delivered here on the 22nd of January, 1886, and in the interval between these dates, covering more than a generation, he poured forth an almost continuous stream of lectures and discourses, marking the progress of those branches of science to which he devoted himself, and to the advancement of which he so largely contributed by his researches, marking also the expansions of science as a whole, and the mutations of speculative thought, and distinguished—always distinguished—by a rare lucidity of style and a rich drapery of language, which, however, never concealed, but only gracefully expressed thought. The late Professor Goodsir's constant monition to his assistant in their anatomical researches was, "Let us have God's truth, Mr. Arthur; nothing but God's truth," and Tyndall was not less loyal than Goodsir in his allegiance to the eternal verities. He never allowed himself to be seduced by the sunny glades of verbiage by as much as one hair's breadth from the path of strict accuracy, and his lectures will always be remarkable for the degree in which they combine solid scientific information with appropriate literary adornments.

Tyndall delivered, in all, in this Institution 51 Friday evening discourses, 307 afternoon lectures, and 12 Christmas courses, comprising 72 lectures, and no one who has not himself delivered a discourse or lecture here is in a position to estimate what an enormous expenditure of mental energy such a record implies—especially in the case of a lecturer like Tyndall who had often to sum up in a few words the results of weeks and even months of patient original investigation, and who before each lecture had to make elaborate preparation for experiments. He was no idler in the field, and if he sometimes reaped what others had sown, it was his happy faculty to be able to convert the crop at once into wholesome and palatable bread for general consumption. It is as a popular expounder of science that Tyndall stands pre-eminent. It is as a translator of texts of nature that but for him must long have remained hieroglyphic that he will be best remembered. It is as an evangelist of science, primarily to the intelligent and educated classes, but ultimately to all, that his greatest influence has been exerted. He was one of the Apostles of a new dispensation. His teachings altered the very spirit of the times, and created a tolerance of scientific truth which it had not before enjoyed. He might have been a martyr to science had he been less amply endowed with weapons of defence and attack.

Professor Tyndall was wont, I believe, to trace his descent from William Tyndale, one of the first English translators of the Scriptures, and the argument from heredity ought, I think, to compensate for some gaps in the genealogy, which, I understand, existed, for the two men, if we may judge by their character and work, seem to have been of the same stock and blood. William Tyndale's aim was to place an open Bible within the reach of every ploughboy; it was John Tyndall's aim to make another revelation accessible to all. William Tyndale was a fearless controversialist; John Tyndall was not less so. William Tyndale's writings were remarkable for their perspicuity, noble simplicity, propriety of idiom and purity of style. John Tyndall's writings are notable for the selfsame characteristics. William Tyndale's last words, before he was strangled and burnt, were, "Lord, open the King of England's eyes!" John Tyndall's life-long aspiration was to open the people of England's eyes.

We who had our eyes opened by Tyndall more or less, who were led by him to look deeper, to discern more clearly—we who profited by his prelections, will, I am sure, always feel grateful for the enlightenment he brought. He was a brilliant expounder of scientific truth; he was an unrivalled experimentalist; he communicated to his auditors his own vivid convictions. Always earnest in manner, in his latter days he became almost vehement, as if he laboured under a pressing obligation to deliver himself of his message before his weary vigils closed in that long, sad, silent sleep on which he has now fallen.

Professor Tyndall's juvenile lectures here deserve special mention because of their popularity and educational utility. The first course of these, on "Light," was delivered in 1862, and after that, year after year, with only two exceptions, up to 1884, a Christmas course was forthcoming; each successive course being listened to by large numbers of boys and girls, and by children of an older growth, with the profoundest attention and delight. These lectures were veritably "the fairy tales of science," but they were imbued with "the long result of Time," and it seems to me that we are today drawing no small advantage from them. Many of those who now take an interest in this Institution, who support it and promote its objects, were baptised into science at Tyndall's Christmas lectures.

It is not for me, in the presence of some of the greatest living masters of physical science, to attempt to define the significance or estimate the value of Tyndall's contributions to natural philosophy. That his researches in magnetism, light, heat, electricity, on ice and water, on the cleavage of rocks, and on the atmosphere as a vehicle of sound, were of great and permanent value I do not doubt, and I trust something will be said about them by those who are well able to appraise them. But I may perhaps be permitted to point out that in that department of science with which I am myself most familiar —namely, biology—Tyndall did signal service. Not only did he expose and snuff out some erroneous observations and fallacious theories as to the spontaneous origin of life, which but for his interference might have done much mischief and retarded progress; not only did he supply novel illustrations of the truths already demonstrated by Pasteur, but he himself furnished original bacteriological observations which must always be thankfully remembered and do infinite credit to his sagacity. Having found that boiling, which completely sterilised infusions in his earlier experiments, subsequently failed to do so, it occurred to him that this might be owing to the presence in the air of his laboratory of bacteria differing from those which he had first encountered in being possessed of spores, capable of resisting the temperature of boiling water—and this view, be it observed, suggested itself to him at a time when the existence of spores in bacteria had not been proved. Then the truly luminous idea struck him that if his surmise were correct, thorough sterilisation might be effectually secured if the infusions, instead of being subjected to a single boiling, however prolonged, were submitted to a series of boilings of short duration on successive days. He calculated that the first boiling would prove fatal to the bodies of the full-grown adult and reproductive bacteria, although it might not prove destructive of the more resisting spores, and he further calculated that the products of the germination of these spores would not have time in a single day to grow to reproductive maturity and produce spores of their own, so that the second boiling would destroy all the bacteria that had resulted from the sprouting of the original spores in the interval. And, further, he surmised that although all the spores present at the outset might not germinate in the first twenty-four hours, yet all might be expected to have done so within a few days, and thus, that in no long time, the daily boilings would have abolished the entire generation of bacteria, and rendered the solution absolutely and permanently sterile. The result corresponded exactly with Tyndall's anticipations, and so he not only added to the natural history of bacteria by giving grounds for believing them to form spores, but devised the simple and effectual method of sterilising liquids now universally employed by bacteriologists.

I have the authority of Sir Joseph Lister for saying that Tyndall's labours greatly facilitated the study of micro-organisms and advanced the knowledge of septic diseases in their various forms, and thus promoted the progress of that antiseptic surgery for which Sir Joseph Lister himself is mainly responsible, and to which we owe the saving of thousands, nay, of tens of thousands of lives and the abolition of purgatories of human sufferings.

I have spoken of Tyndall as a Professor of this Institution, as a teacher, as a writer, as an original investigator; let me say one word about him as a man, and I have done. And only one word about him in this aspect is needful, for, as a man he was simplicity itself. There was no subtilty about him, no complexity of nature requiring unravelment, no organic incongruity inviting analysis. He was almost boyish in his directness and transparency, almost womanly in his tenderness; but a true, strong man in all that does become a man. He had a photosphere of charm around him, made up of emanations of intellect and kindly courtesy. Impulsive he was, if you will, but without impulse learning is apt to be pedantic, and science frigid and repellant. In a sordid age, he was untainted by wealth-worship, and amidst many temptations he never turned his talents to mercantile account. It was his chivalrous devotion to truth and justice that made him combative, and it was the simplicity of his tastes and the frugality of his life that enabled him to indulge his lavish generosity. A little incident that has come to my knowledge illustrates his benevolence. Many years ago a servant of this Institution had the misfortune to lose his little daughter by scarlet fever. He had only recently joined the staff and thought he was scarcely known to Professor Tyndall, but he immediately received from him a letter full of consoling sympathy and enclosing a five-pound note.

Perhaps one touch of humour might have saved Tyndall from some extravagances into which he ran, but it might also have made him tamer and less picturesque than he was; and we are content to take him as he came to us, with all his human frailties, with all his divine gifts. Of him it cannot be said, to use his own words, that he was as "a streak of morning" vanishing "into the infinite azure of the past," for he has become a bright particular star in the firmament of genius, shining through the darkness as long as the world shall last. The clods of Haslemere may claim his ashes, but his memory is the heritage of all mankind.

The Chairman moved—"That the members of the Royal Institution of Great Britain, in special general meeting assembled, hereby record their deep regret at the death of Dr. John Tyndall, D.C.L. LL.D. F.R.S., who was for forty years connected with the Institution as Lecturer, Professor and Honorary Professor of Natural Philosophy; and who, by his brilliant abilities and laborious researches, nobly promoted the objects of the Institution, and conspicuously enhanced its reputation, while at the same time he extended scientific truth, and rendered many new additions to natural knowledge practically available for the service of mankind; and that the members of the Royal Institution further desire to convey to Mrs. Tyndall an expression of their sincere sympathy and condolence with her in the bereavement she has sustained in the loss of her gifted and distinguished husband."

Sir RICHARD WEBSTER, in seconding the resolution, said: Ladies and gentlemen,—I possess no qualifications specially qualifying me to second the resolution which has been proposed in such eloquent and touching language by Sir James Crichton-Browne. After listening to his speech, my own feeling would be to content myself with simply seconding the proposition without further words; but it is fitting that on such occasion one who may be said to represent the general public should join in the expression of sympathy and sorrow. Our first thoughts to-day must be for her that is left behind, who has with such wonderful tenderness watched over Tyndall's life; no words can too fully express our feeling for her. Of Tyndall I may be permitted to say that no one could hear him lecture without catching something of the spirit which he threw into everything which he undertook, making each subject that he handled appear to be peculiarly his own. Others, who follow me, will speak as I cannot venture to do of his work, but I well remember when I was quite a boy attending lectures given by Tyndall in this building, and I was always impressed by the extraordinary facility which he had of making people understand subjects of which, when they entered the room, they were absolutely ignorant. Tyndall had an extraordinary power of generalisation, which he seemed almost by instinct to have acquired, so that he was able not only from his own researches but from the careful study of the researches of others, to state a proposition in such a way that it defied criticism, and at the same time proved an invaluable guide to others. He also had an extraordinary power of clear exposition, which enabled even the ignorant and unlearned to feel that he was imparting to them some portion of his great knowledge. Perhaps his greatest characteristic was that earnest power for work, which he not only displayed himself but infused into all those who worked with him, and which was, perhaps, not the least valued part of that heritage which he has left to this Institution.

Sir JOHN LUBBOCK said: I am not competent to express an opinion as to the value of Prof. Tyndall's scientific work, but no doubt you have called on me as one of his very oldest friends. It was my privilege to make many excursions with him, and I look back upon them as among the greatest privileges of my life. His was a noble and generous nature, and his conversation was not only full of instruction, but what is better still, inspired one with a love of science.

I shall never enter this Institution, and particularly this theatre, without thinking of him, and the recollection of his friendship will always be one of my most cherished memories. With regard to Mrs. Tyndall, I fear that nothing we can say can prove any consolation to her now; but I hope the time may come when the sympathy of so many devoted friends may bring some comfort to her.

Dr. **Edward Frankland** said: After a knowledge of, and a friendship with Tyndall extending over nearly half a century, I have, of course, formed very definite opinions of his character. I first met Tyndall in 1847, at Queenwood College, Hampshire, where, for the first time in any school in England, experimental science was taught. It was this which attracted both of us to that Institution, and led Tyndall, in the intervals of work, to embrace all possible opportunities of acquiring some knowledge of chemistry. He was bright, sociable, original and greatly in earnest, with a fund of enthusiasm and humour. His individuality was so strong that I have never known any one in the least like him. Fond of literature and a devoted student of Carlyle and Emerson, also of Fichte and Goethe through translations, his mind was then almost a complete blank concerning physical science in any form. He worked in my laboratory and I gave him lessons in chemistry; in return he taught me mathematics. We attended each other's lectures and became fast friends. Eighteen months later we migrated to the University of Marburg in Hesse Cassel to study chemistry under Bunsen, and here Tyndall acquired a fair knowledge of qualitative and quantitative analysis; but the advent of the young and enthusiastic Knoblauch, as professor extraordinary of physics at Marburg in 1849, diverted his attention from chemistry to physics, in which science he found more scope for his mathematical knowledge. From Knoblauch's laboratory he passed to that of Magnus in Berlin. On returning to England in 1852, he made the acquaintance of Bence Jones and Faraday, and became professor of natural philosophy in this Institution in the following year. You, Sir, have so lucidly and graphically described his subsequent career that there is no room for any observations of mine; except perhaps this, that, commencing the elementary study of experimental science at the age of 27, Tyndall stands almost, if not quite alone as regards the number and importance of his original investigations, and the brilliant position which he achieved in the course of the following forty years as an expounder of scientific truths.

Professor **Dewar** also paid a touching tribute to the memory of Dr. Tyndall, who had been for many years his personal friend, and after referring to Tyndall's private life, he said: I have never seen such abject idolatry of life, sympathy, and care expended by any human being as that of Mrs. Tyndall for her husband. However sad the disaster which caused the death of the Professor, it ought to be some consolation to her to think how many times she had been the means of prolonging his life. She is the noblest woman I have ever known.

The Resolution was spoken to by others, including Sir JOSEPH LISTER, Sir FREDERICK ABEL, LORD GRIMTHORPE, and Dr. J. H. GLADSTONE.

Upon the Chairman putting the Resolution the whole of the company rose, and the motion was adopted unanimously.

Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, January 26, 1894.

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

Alfred Perceval Graves, Esq. M.A.

Old Irish Song.

In the dim morning twilight of Ancient Erin, legend describes her Fileas, or musical bards, as constant attendants upon the king and chieftain.

As Mr. Alfred M. Williams, the American critic, picturesquely puts the tradition, "Surrounded by the Orsidigh, or instrumental musicians, who fulfilled the function of a modern military band, they watched his progress in battle for the purpose of describing his feats in arms, composed birthday odes and epithalamia, aroused the spirits of clansmen with war songs, and lamented the dead in the caoines, or keens, which are still heard in the wilder and more primitive regions of Ireland."

We must, of course, discount much of the legendary colour which enthusiasts like Walker take on trust. But this is the picture of the early Irish bard presented to us by the chroniclers. Amongst other privileges, he wore a tartan with only one shade of colour less than that upon the king's robe, and his assassination involved a blood-penalty inferior only to the royal eric. But before attaining such high honours, he had to satisfy the moral requirements of "purity of hand, bright without wounding, purity of mouth without poisonous satire, purity of learning without reproach, purity as a husband in wedlock."

He had, moreover, to pass through a decidedly arduous courtship of the Muse before he was entitled to claim her favour; indeed, some writers go so far as to say that, like the patriarch, he had to serve seven years for her, committing to memory an almost incredible number of earlier compositions, and giving the closest study to the laws of verse, before he could become a poet on his own account.

When it is added that these laws of Irish verse, as finally formulated by the early Celtic professors, were the most complicated ever invented—not only limiting the sense within the stanza, but fixing the amount of alliteration and the number of syllables in each line, to say nothing of their assonantal requirements—we may well understand that although Early Irish verse may be granted, according to Professor Atkinson, to be the most perfectly harmonious combinations of sounds that the world has ever known, it must also be conceded that Irish "direct metre" was the most difficult kind of verse under the sun—the despairing opinion of another leading Irish philologist.

Dr. Whitley Stoke's comment is "that in almost all the ancient Celtic poetry, substance is ruthlessly sacrificed to form, and the observance of the rigorous rules of metre seems regarded as an end in itself." The consequence of such an artificial system, combined with the high privileges of the bardic caste, resulted in the multiplication of minor parts to a degree which would have paralysed all Mr. Traill's efforts to keep pace with them, had he been a contemporary critic.

O'Curry quotes this droll account of their pecuniary dealings: "At this time we are told that the poets became more troublesome and importunate than ever. They were in the habit of travelling about the country in companies of thirty, composed of pupils and teachers, and each company had a silver pot, called 'the Pot of Avarice,' having chains of bronze attached to it by golden hooks. It was suspended from the points of the spears of nine of the company, which were thrust through the links at the other end of the chains. The reason that the pot was called the Pot of Avarice was because it was into it that whatever of gold or silver they received was put, and, whilst the poem was being chanted, the best nine musicians in the company played music round the pot. If their minstrelsy was well received, and adequately paid for, they left their blessing behind them in verse; if it was not, they satirised their audience in the most virulent terms of which their poetical vocabulary was capable; and, be it observed, that to the satire of an Irish bard, to whom there still clung in the popular belief the mystical attributes of the druid, there attached a fatal malignity."

At the time of the conversion of Ireland to the Christian faith, the bards were said to number a third of the male population, and in 590 A.D. a Synod was held at Drumkeat, by Aed, king of Ulster, which greatly reduced their forces. Indeed, such was the popular irritation against them, that had it not been for the friendly intervention of the statesman-poet, St. Columbkille, they would probably have been banished altogether.

The Filea, or bard, no doubt was a minstrel as well as a poet, in the first instance, but in the course of time there would appear to have been a further bardic differentiation, and we learn that perfection in the three Musical Feats, or three styles of playing, gave the dignity of Ollamh, or Doctor of Music, to the professors of the harp. Now what were the three Musical Feats? Here they are, well described in a weird old folk tale.

Lugh (the Tuatha da Danann king), and the Daghda (their great chief and druid), and Ogma (their bravest champion), followed the Fomorians and their leader from the battlefield of Moyturah, because they had carried off the Daghda's harper, Uaithne by name. The pursuers reached the banqueting house of the Fomorian chiefs, and there found Breas, the son of Elathan, and Elathan the son of

Delbath, and also the Daghda's harp hanging upon the wall. This was the harp in which the music was spell-bound, so that it would not answer, when called forth, until the Daghda evoked it, when he said: "Come, Durdabla; come, Coircethairchuir (the two names of the harp). Come, Samhan; come, Camh, from the mouths of harps and pouches and pipes. The harp came forth from the wall then, and killed nine persons in its passage; and it came to the Daghda, and he played for them the three musical feats which give distinction to a harper, viz., the Suantree (which, from its deep murmuring, caused sleep), the Gauntree (which, from its merriment, caused laughter), and the Golltree (which, from its melting plaintiveness, caused crying). He played them the Golltree, until their women cried tears; and he played them the Gauntree, until their women and youths burst into laughter; he played them the Suantree, until the entire host fell asleep. It was through that sleep that they (the three champions) escaped from those Fomorians who were desirous to slay them."

This passage is of threefold interest. It indicates the popular belief in the introduction of music into Ireland by the Tuatha da Danann, a mysterious race, by some regarded as an offshoot of the Danai, whom tradition declares to have conquered and civilised the country and then to have disappeared from it into fairyland. Again, it contains the first reference in Irish literature to the harp or cruit, destined to become our national instrument. Lastly, it describes three styles of Irish music, of each of which we have characteristic examples that have descended to the present day. For the Gauntree, which was provocation of mirth and frolic and excited spirit, is represented by the jigs, reels, planxties, and quick-step marches; the Golltree, or the sorrowful music, still lingers in the keens or lamentations, and some of our superb marches of the wilder and sadder type; and the Suantree survives in many a beautiful Irish hush song.

The Irish sleep-compelling airs have not attracted the notice they deserve. Moore ignored them altogether, but Dr. Petrie prints many of them, and points out their resemblance to the slumber-tunes still in vogue in India and elsewhere in the East. They certainly support the tradition of the oriental affinities of the Early Irish.

The first period of Irish bardic literature may roughly be said to be that of epic poetry interspersed with songs. Fine exemplifications of these are to be found in the 'Silva Gadelica,' a recent translation of a series of Early Irish tales by Mr. Standish Hayes O'Grady. The music to which they were sung has perished or become dissociated from these lyrics, but some of their measures are identical with those of rustic Irish folk tunes. We now come to the bardic period, thus described by the poet Spenser in 'A View of the State of Ireland':—

"*Iren.*—There is amongst the Irish a certain kind of people called Bardes, which are to them instead of Poets, whose Profession is to set forth the Praises or Dispraises of men in their Poems or Rithmes; the which are had in so high Regard and Estimation amongst them, that none dare displease them for fear to run into Reproach through their offence, and to be made infamous in the mouths of all men.

"For their verses are taken up with a general applause, and usually sung at all feasts and meetings by certain other persons, whose proper function that is, who also receive for the same great rewards and reputation amongst them."

It would appear that the poet Spenser made a study of the Irish poetry of his day, and a music-book of the sixteenth century, misnamed 'Queen Elizabeth's Virginal Book,' contains three Irish airs, one of which, Callino Casturame, is evidently alluded to by Pistol in Shakespere's 'Henry V.,' who, on meeting a French soldier, cries, "Quality! Caleno custure me"—clearly, "Colleen oge astore."

Here it may be well to give, in full, the famous passage in Spenser's 'View of the State of Ireland' relating to the character of the bardic lyrics of his day.

"*Eudoxus.*—But tell me (I pray you) have they any art in their compositions? or be they any thing witty or well-favoured, as poems should be?

"*Irenæus.*—Yea, truly, I have caused divers of them to be translated unto me, that I might understand them; and surely they were favoured of sweet wit, and good invention, but skilled not of the goodly ornaments of poetry; yet were they sprinkled with some pretty flowers of their natural device, which gave good grace and comeliness unto them; the which it is a great pity to see so abused, to the gracing of wickedness and vice, which with good usage would serve to adorn and beautify Virtue.

"As of a most notorious thief and wicked outlaw, which had lived all his lifetime of spoils and robberies, one of their Bardes in his praise will say, That he was none of the idle milksops that was brought up by the fireside, but that most of his days he spent in arms and valiant enterprizes; that he did never eat his meat, before he had won it with his sword; that he lay not all night slugging in a cabbin under his mantle, but used commonly to keep others waking to defend their lives; and did light his candle at the flames of their houses, to lead him in the Darkness; that the day was his night, and the night his day, that he loved not to be long wooing of wenches to yield to him, but where he came he took by force the spoil of other men's love, and left but lamentation to their lovers; that his musick was not the harp, nor lays of love, but the cries of people, and clashings of armour: and finally that he died not bewailed of many, but made many wail when he died, that dearly bought his death.

"Do you not think (Eudoxus) that many of these praises might be applied to men of best deserts, yet are they all yielded to a most notable traitor, and amongst some of the Irish not finally accounted of. For the song, when it was first made and sung to a person of high degree there, was bought (as their manner is) for forty Crowns."

The lyrical epoch alluded to by Spenser is the second era of bardic poetry in Ireland. It embraces the period of the English struggle for supremacy in the country—that terrible time of internecine war which alike brutalised the Saxon and the Celt. In times such as these it was impossible to compose long narrative poems. As Mr. Williams well puts it, "The inspiration of the bards was turned to more direct appeals for war, rejoicings for victory, and lamentations for misfortune and defeat. The poetry took a more lyric form, and became an ode instead of an epic."

Irish Music and Song had now fallen on evil days. The downfall of the great Celtic families, and many of the great Anglo-Irish ones who had espoused their quarrel with successive English Governments, forced our national bards, for want of better support, to wander from castle to castle, instead of remaining as leading figures in the great households.

Turlough O'Carolan was the most remarkable of these wandering lyrists. Born in the year 1670, he early lost his sight through small-pox, but solaced himself for this deprivation by the study of music, in which he made astonishing progress. The 'Irish Monthly Review' gives this instance of his wonderful musical memory, and his extraordinary power of musical improvisation. At the house of an Irish nobleman, where Geminiani was present, Carolan challenged that eminent composer to a trial of skill. The musician played over on his violin the Fifth Concerto of Vivaldi, and it was instantly repeated by Carolan on his harp, although he had never heard it before. The surprise of the company was increased when Carolan asserted that he would compose a concerto, himself, upon the spot; and he did then and there invent a piece that has since gone by his name. He composed upon the buttons of his coat, the buttons serving for the purpose of the lines, and the intervals between them for the spaces.

Carolan did not adhere entirely to the Irish style of composition, and his musical pieces show a considerable Italian influence; yet, as Mr. Bunting writes, he felt the full excellence of the ancient music of his country. He was a most prolific composer. One harper at the beginning of this century was alone acquainted with about a hundred of his tunes, and many were at that time believed to have been lost.

Passing over the period of 1798, which does not furnish many lyrics of first-rate quality, we now come to that important epoch in Irish lyric literature—the Granard and Belfast meetings of harpers, promoted with the object of reviving the taste for Irish music, which had begun to decay. These meetings, which took place about the year 1792, were very successful, and awoke in the distinguished Belfast musician, Mr. Bunting, such an enthusiasm for Irish music, that he thenceforth devoted his main efforts to its collection and publication. Of the Belfast meeting he writes thus vividly:—

"All the best of the old class of harpers, a race of men then nearly extinct, and now gone for ever, were present—Hempson, O'Neill, Fanning, and seven others, the least able of whom has not left his equal behind. Hempson realised the antique pictures drawn by Cambrensis and Galilei, for he played with long crooked nails, and in his performance the tinkling of the small wires under the deep notes of the bass were particularly thrilling. He was the only one who played the very old music of the country, and this in a style of such finished excellence as persuaded me that the praises of the old Irish harp in Cambrensis, Fuller, and others, were no more than just to that admirable instrument and its then professors. But more than anything else the conversation of Arthur O'Neill—who, although not so absolute a harper as Hempson, was of gentle blood, and a man of the world, who had travelled over all parts of Ireland—won and delighted me. All that the genius of later poets and romance writers has feigned of the wandering minstrel was realised in this man. There was no house of any note in the north of Ireland, as far as Meath on the one hand, and Sligo on the other, in which he was not well-known and eagerly sought after."

What are our grounds for believing that many of the airs played at the harp meetings are very ancient?

First, the testimony of the harpers, most of them very old men, at the Belfast meetings one hundred years ago, who smiled on being interrogated by Bunting as to the antiquity of the so-called ancient airs, and answered—"They are more ancient than any to which our popular tradition extends." Moreover, Bunting informs us that "though coming from different parts of Ireland, and the pupils of different masters, the harpers played these ancient tunes in the same key, with the same kind of expression, and without a single variation in any essential passage, or even in any note." He adds, "This circumstance seemed the more extraordinary when it was discovered that the most ancient tunes were in this respect the most perfect, admitting of the addition of a bass with more facility than such as were less ancient. Hence we may conclude that their authors must necessarily have been excellent performers, versed in the scientific part of their profession, and that they had originally a view to the addition of harmony in the composition of their pieces.

"It is remarkable that the performers all tuned their instruments upon the same principle, totally ignorant of the principle itself, and without being able to assign any reason either for their mode of tuning, or of their playing the bass." And here it may be mentioned that the ancient Irish harps had commonly thirty strings, and were tuned in the key of G, and that the Irish airs supposed to be the oldest are in the ordinary major scale of G, and were played in this key. But, for the sake of variety, the harpers played tunes in other scales, and melodies were composed in the scale of A, but with the tuning of the harp unchanged.

But the strongest proof of the skill of the Irish harpers of the thirteenth century is the testimony of Gerald Barry, best known as

Giraldus Cambrensis, an inveterate opponent of everything else Irish—"They are incomparably more skilful than any other nation I have ever seen. For their manner of playing on these instruments, unlike that of the Britons or (Welsh) to which I am accustomed, is not slow and harsh, but lively and rapid, while the melody is both sweet and sprightly. It is astonishing that in so complex and swift a movement of the fingers the musical proportions as to tune can be preserved; and that throughout the difficult modulations on their various instruments, the harmony is completed with such a sweet rapidity. They enter into a movement and conclude it in so delicate a manner, and tinkle the little strings so sportively under the deeper tones of the bass strings—they delight so delicately, and soothe with such gentleness, that the perfection of their art appears in the concealment of Art."

John of Salisbury (twelfth century) is equally eulogistic, and Fuller says, "Yea, we might well think that all the Concert of Christendom in this war and the Crusade conducted by Godfrey of Boulogne would have made no music if the Irish harp had been wanting." There is indeed a continued record of praise (British and Continental) of the Irish music and its professors from the twelfth to the seventeenth centuries, which we may conclude by Drayton's stanza in his Polyolbion :—

"The Irish I admire,
And still cleave to that lyre
As to our Muse's mother;
And think, till I expire,
Apollo's such another."

The antiquity of individual airs has distinct historical confirmation by Bunting and others. The tune called 'Thugamar fein a sowra lin' was sung to welcome the landing of the Duke of Ormond by a band of Virgins who went out to meet him from Dublin. Again, the ancient Irish air 'Summer is coming' is the same song practically as 'Summer is a comin in,' which is reputed as the first piece of music set in score in Great Britain. Bunting claims that air, therefore, for Ireland on the ground of the extreme improbability of its having been borrowed by the ancient Irish from a country that has no national music of its own (the Welsh excepted). "Their ignorance of the English language," he adds, "and their rooted aversion to their invaders, were effectual bars to any such plagiarism or adoption."

Besides the remarkable similarity between our lullabies and those of the East, already touched on, there is a marked correspondence between some of the early Norse and ancient Irish tunes. The distinguished Swedish harpist, Sjöden, who visited Dublin on the occasion of the Moore Centenary, showed me that some of our old Irish airs—for instance—the 'Cruiskeen Lawn'—were almost identical with early Norse ones; the question for settlement of course being, whether the Irish got them from the Danes or the Danes from the Irish, though the musical reputation of our ancestors, amongst whom the Danes formed maritime settlements at Dublin, Waterford, Limerick, and elsewhere, points to the latter conclusion. Then there is the strong internal evidence of extreme antiquity from the old-world characters of such airs as the 'March from Fingal.'

To what poetical measures were these old airs sung? We have, fortunately, some clue to this, not only in the modern Irish words to them published by Dr. Joyce, but in the important fact that we have Irish poems, as early as the ninth century, which will sing to some of the ancient airs; for example, an invocation for God's protection upon his coracle, by Cormac Mac Cullinane, King and Bishop of Cashel, who died in 903. This measure is identical with that of Shenstone's lines:—

> "My banks they are furnished with bees,
> Whose murmurs invite us to sleep,
> My grottoes are shaded with trees
> And my hills are white over with sheep."

Professor O'Curry puts the case very strongly, but not, I think, too strongly, when he says, "Those verses of King Cormac McCullinane, now almost one thousand years old, which sing to the air of 'For Ireland I would not tell who she is,' is adduced as an interesting fact, proving that a fragment of a lyric poem, ascribed to a writer of the ninth century, and actually preserved in a MS. book so old as the year 1150, presents a peculiar structure of rhythm, exactly corresponding with that of certain ancient musical compositions still popular and well known, and, according to tradition, of the highest antiquity.

"I believe such a fact is unknown in the musical history of any other nation of Europe; and yet in ours very many such instances could be adduced of ancient lyric music still in existence, in minutely exact agreement with forms of lyric poetry used not only in but peculiar to the most ancient periods of our native literature."

A large proportion of the Irish airs are in eight-line measures, consisting of two quatrains; though originally it would appear that the verses consisted of four lines only, in which event the range of the air was very limited. But, as time went on, the strain appears to have been repeated, with a variation, and then added to by means of a strain of different character, the final musical measure being a repetition of the first strain.

Stanzas built up to suit such airs largely consist of sixteen lines, which are quaintly called "curving eight-lined verses"—the meaning of the word curved referring to the second part of eight lines, which are added to the first eight to fill up the curve turn, or second part of the tune.

Finally, it may be worth while to state the case of the Scotch claim to Irish airs, and the Irish claim to Scotch melodies. A pedantic attempt has been made to specify certain Irish musical characteristics, the absence of which will prove one of the airs in dispute to be Scottish. But Sir Robert Stewart justly points out that the so-called unfailing characteristic of Irish, as of Chinese, melody to omit the fourth and seventh of the scale, is by no means a sure test. In many Irish airs these intervals are wanting, in others they both exist. In some they are omitted in the first strain and are present in the second part of the air. Again, the presence of the submediant or sixth of the scale, supposed to be a never-failing test of an Irish air, is equally emphatic in the Scottish air 'Auld Lang Syne,' and many other Scottish tunes.

The Scotch airs may be roughly classed as Highland tunes and Lowland tunes. The first class have a close affinity with the Irish music, and no wonder, for not only are the Highland Scotch of North Irish descent, but the Scotch of the West coast were for centuries closely connected with their kinsfolk across the North Channel, and a constant exchange of minstrelsy must have therefore gone on between them. The Lowland Scotch tunes form a large and distinct body of national melodies, composed by national musicians, and not found in Irish collections. In Ireland there is a much larger body of airs acknowledged on all hands to be purely Irish and *not* found in Scotch collections.

Outside these airs there is a large number common to and claimed by both countries. As Dr. Joyce pithily puts it, "In regard to a considerable proportion of them it is now impossible to determine whether they were originally Irish or Scotch. A few are claimed in Ireland that are certainly Scotch, but a very large number claimed by Scotland are really Irish, of which the well-known air 'Eileen Aroon' or 'Robin Adair' is an example. From the earliest times it was a common practice among the Irish harpers to travel in Scotland. How close was the musical connection between the two countries is hinted by the Four Masters when, in recording the death of Mac Carroll, they call him the chief minstrel of Ireland and Scotland! and there is abundant evidence to show that this connection was kept up till towards the end of the last century." Ireland was long the school for Scottish Harpers, as it was for those of Wales: "Till within the memory of persons still living, the school for Highland poetry and music was Ireland, and thither professional men were sent to be accomplished in these arts." Such facts as these sufficiently explain why so many Irish airs have become naturalised in Scotland.

"It is not correct to separate and contrast the music of Ireland and that of Scotland as if they belonged to two different races. They are in reality an emanation direct from the heart of one Celtic people; and they form a body of national melody superior to that of any other nation of the world."

[A. P. G.]

WEEKLY EVENING MEETING,

Friday February 2, 1894.

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

T. J. Cobden-Sanderson, Esq.

Bookbinding: its Processes and Ideal.

Bookbinding is in itself a comparatively simple matter and is easily described; but it is associated with great and interesting conditions of society, and at its highest rises into disinterested admiration by such means of expression as are within its reach of what is most beautiful and wonderful in human achievement, the written and printed speech of man. Binding, moreover, like every other handicraft, is on its ideal side a discipline and a type of life. I propose, therefore, to explain indeed how a book is bound, and how, when bound, it may be tooled. But I propose also throughout to set the craft into imaginative sympathy with the thought it would perpetuate; to touch upon its origin, its history and its patrons, to characterise the styles of the great periods of tooled decoration; to insist upon the need of some new departure in the invention and development of pattern; and finally, leaving the special objects of the binder's craft, to find in the intuition of the harmony of the universe an outline of the ideal of the craftsman and of the artist.

Speaking generally, binding has its origin in the desire to perpetuate thought. Before the discovery or invention of pliable portable material suitable for writing upon, "binding" was sought for and found in imperishable natural objects, stones, tablets, columns, ready to hand, upon which the thought was permanently incised. In this case the binding may be said to have preceded the writing. It was only when writing was made upon separate pieces or sheets of a pliable and perishable material, that binding proper was invented to hold the pieces or sheets together and to give strength to them, and protection and beauty.

But here again a distinction must be made. The pliable written sheet may be either *rolled* or *folded*, each giving rise to a form of binding peculiar to itself.

The rolled sheet is bound by fastening each sheet to the other *sideways*, and rolling the whole laterally from end to end, the last sheet serving as a cover to all the rest.

The folded sheet, on the other hand, is bound by simply sewing or otherwise fastening the parts of the sheet to one another at the back crease or fold. And a number of folded sheets or of sections, as they are called, are bound by fastening each of them at the back to some common support, so that when all are sewn or otherwise fastened *at the back*, they may yet be free to open and shut *at the front*, or fore-edge.

The invention of the folded sheet thus gave rise to the invention of modern binding, which in its essence is the union at the back of the folded sheets, which together constitute the folded book, or as I might say, despite the latent contradiction, the folded volume.

Throughout the long period which has elapsed since the invention of the folded sheet—it is said to have been invented in the third century before Christ—binding must have undergone many and important changes. But of these changes few records remain. Speaking generally of the binding of the middle and later ages, we may say that at each successive epoch the form of the binding adapted itself to the state of literature at the time. When books were few and large and stationary, the binding was correspondingly large and bossy and heavy; and when books became numerous and lighter and portable, the binding adapted itself to the new conditions, and, dropping the oak boards, the brass fittings, clasps, bosses and chains, became itself light and portable and beautiful. And thus wood, and silk, and velvet and leather, iron and brass, and silver and gold, and precious stones, were all used by the artificers of the middle and earlier ages, in the protection and embellishment of the world's written wealth. The invention of printing, however, and the multiplication of books, gave the victory to leather and to gold tooling, and with the invention of printing, binding passed into its modern phase, and became ultimately a craft apart, the craft of the bookbinder.

To the renown of bookbinding many countries and cities and patrons have contributed, as well as the artists and craftsmen whose work it has been. Singularly enough the names of very few bookbinders are known, but it is well known that to Grolier and to France is mainly due the gold tooling which is still the chief means of making the bound book beautiful. This tooling, of obscure origin, was practised first in Europe in Italy, but was soon after introduced into France by Grolier, and the French schools of the sixteenth and seventeenth centuries are still the great schools of design in that decorative method.

Deserving of mention or of allusion in this connection, even in the shortest account of bookbinding, are the innumerable crafts—crafts for the production of materials and crafts for the production of tools—upon which the binder's own craft depends. For this collaboration of crafts is a fact of capital importance and should always be borne in mind, that the solidarity of all industries may be understood and the dignity of each be appreciated.

It is to be regretted, however, that at this moment the craftsmen immediately concerned in making a book, the paper-maker, the printer, and the binder, are not in possession of ideas bearing and operative upon the book as a whole, and controlling their several crafts to the one common end of the book beautiful, and the binder is in the unfortunate position of coming last, to inherit all, and be helpless under, the mistakes of his predecessors the paper maker, printer and publisher.

Modern binding may be divided into two main divisions:

1. Bindings for use.
2. Bindings for beauty's sake.

I do not say that the divisions can be precisely defined or that the useful may not be beautiful, or that the beautiful may not be useful. I mean only that of a certain class the utility of the binding is the main characteristic, and that of a certain other class not the utility of the binding but the beauty of the decoration is the prominent and delightful feature. All bindings may be, and most bindings are, decorated in some form or other, but I would deprecate the decoration in gold of cloth or paper bindings: the material is too poor and the kind of binding is unsuitable for elaborate invention. Decoration should be reserved for cases in which a permanent pleasure is aimed at, and decoration in all its affluence exclusively for bindings of the best kind, and for books that are in themselves, apart from their apparel, beautiful and worthy of conspicuous honour.

The binding of a book, to come closer to our subject, is a series of processes too numerous to be entered upon in detail, in so short an account of bookbinding as the present, but the main operations are as follows:

1. The sheets are folded so that the headlines of each page shall, if possible, be at a uniform height throughout the book.
2. The sections are then sewn to cords, set and held at equal distances from one another in a frame, and at right angles to the sections.
3. The ends of the cords are frayed out and laced into and fastened to rectangular pieces of millboard (called boards), cut to the size of the sides of the book, which they protect.
4. The boards and back are then covered with leather or other suitable material, and the last and first sheets of the book (added to the book proper for the purpose) are pasted down upon the inside of the boards.

The book so treated is completely "forwarded," as it is called, and ready to pass into the hands of the "finisher" to be tooled or decorated, or "finished." The decoration in gold on the surface of a bound book is wrought out bit by bit by means of small engraved brass stamps called "tools." The steps of the process are shortly as follows:

1. The pattern is first worked out with the tools blackened in the smoke of a candle or lamp, upon a piece of paper cut to the exact size of the portion of the book to be decorated.
2. The piece of paper with the pattern upon it is then applied

to the surface to be decorated, and the pattern is reimpressed on the paper, and so through on to the surface of the book.

3. The paper is now removed, and the pattern on the book is reimpressed with hot tools to make the impression crisp and distinct.

4. At this stage a different process begins. The surface of the cover, with the pattern impressed upon it as described, is taken bit by bit and treated as follows:

1. First it is moistened with water or vinegar.

2. Then the pattern is pencilled over with "glaire," which is a liquid composed of the white of an egg beaten up and drained off.

3. Then, when the glaire is dry, the surface is lightly touched with oil or grease to give a hold to the gold leaf next to be applied.

4. Then the gold leaf, cut to the size and shape of the portion of the cover to be operated on, is applied by a flat brush called a "tip," and pressed down by a pad of cotton-wool to reveal the pattern underneath.

5. Then, and finally, the pattern with the gold upon it is gone over again with the hot tools, and the gold is impressed into it. The rest of the gold is rubbed away with an oiled rag, and the pattern is now displayed permanently in gold and "finished."

The description is easy—how easy!—but the craft is difficult. Gold cannot be persuaded to stick as a friend may be persuaded to stay; it must be *made* to stick, i.e. all the conditions upon which successful gold tooling depends must *in all cases* be observed, and there is the rub! What in each case—and the circumstances are never quite the same—*are* the conditions? How divine them? A little more, or a little less, makes so much difference. How dry may the leather be, or how damp must it be? How much glaire? How hot must the tools be? When is the moment to begin? Then how difficult it is correctly to manipulate the tools, to keep them even upon the leather. How difficult, finally, to keep the leather, throughout all the long and difficult operation, perfectly clean and the gold brilliant! What patience, what natural aptitude, what acquired skill, what fortitude! "The city sparkles like a grain of salt." Shall I ever succeed? the apprentice may well ask himself. Shall I ever attain to such skill, to such consciousness of power, that I shall not even know *how* to fail? In this difficulty, too, and in the effort and ambition to overcome it, lies a further difficulty, the snare of the art, the temptation of the finisher. He becomes engrossed in it—the finisher in mere finishing. He pursues it positively, and not in subordination to design. And he achieves victory at last, only to find that what he should have achieved, the thing beautiful, has escaped him. He can tool, but he cannot design; and he has so magnified execution that when completely successful, when completely triumphant, he is then most conspicuously a failure. The tremulous outline of design—and design appeals to the imagination, to the inner eye of the soul as well as to the outer eye of sense—the tremulous outline of design has perished in the too great exactitude of his accomplished execution. Wholly to achieve victory, indeed, in the binder's craft, to forget no end in the prosecution of the means, to exaggerate no feature from long practice and perfect skill, to permit no craft of hand to overcome the judgment of the head, is, in bookbinding, as in all crafts, an exceedingly difficult task, and we have in the very development of a craft the cause of its ultimate decay. But what an education the prosecution of a craft is for the soul of a man! The silent matter, which is the craftsman's material, is wholly in his hands, it hears and makes no reproaches, but it never forgives and it has no mercy. Sunrise after sunrise lights the craftsman to his task, sunset after sunset leaves him to his regret. Shall the sun ever rise upon victory or set upon contentment? It is a great struggle. He only knows how great the struggle is, who knows what the aim of craft rising into the ideal is, and who tolerates, between him and it, no cloud of self-illusion, no splendour of popular praise to blind or to darken his gaze. And so through the work of his hand man may rise indeed to his soul's height. But the victory itself is withdrawn behind the veil. The world may not know it when it is achieved, and the artist himself may sometimes see it achieved, as he thinks, when to reach it he has yet to traverse the entire way of truth.

"Sown in a wrinkle of the monstrous hill,
The city—sparkles still, a grain of salt."

The great schools of design for the decoration of bound books are the great schools of France of the sixteenth and seventeenth centuries.

1. The first great school—the school of Grolier as it may be called—is characterised mainly by the simple motives of straightness and curvature. Straight and curved bands or straps, and straight or curved lines are interwoven one with the other and distributed on a more or less simple or intricate, but always symmetrical plan over the sides and back and sometimes the edges of a book.

2. The second great school—the school of the Eves—is characterised by the symmetrical distribution over the side of the cover of symmetrically drawn compartments or panels, and the union of them all into one organic whole by the intermediation of twisted or interwoven bands. This is its main and for its earlier years almost its only characteristic. But the school attained its maturity by the combination with it of an independent contemporary style, which consisted in the use of a number of branches, spreading from each corner of the cover towards the centre, the unity of the whole being enhanced by a semis, simple or alternate, of some simple tools over the whole of the side. The combination was effected under the direction, if not by the hands, of the great binders Nicholas and Clovis Eve, and consisted in the enrichment of the interspaces of the first style by means of the sprays and branches of the second. When mature the school was characterised by compartments symmetrically

distributed and connected, filled with dainty devices or with the severer tools of the Grolier pattern, and supported and enriched in the interspaces by foliated branches and sprays.

3. The third great school—the school of Le Gascon—and perhaps the last, was characterised by the combination with the geometrical framework of the preceding school of a new motive, borrowed, I think, from the contemporary lace, or perhaps filigree work, and used, ultimately, to fill in both the compartments or panels and the space between them. The motive is an exceedingly simple one, a small spiral of dots, but the close repetition of it has a singularly rich if somewhat bewildering effect. The school, however, in what specially characterised it, has dropped the tradition of form and is content with the glitter of gold. The repetition of the spiral is not always organic in its construction. The spirals are placed side by side, they do not grow the one out of the other. And I submit that all patterns, to be good, must be organic in the relation of their details and organic in the method of their development.

The great schools of design which I have thus attempted to characterise are historical, and they are closed. The future, as I have elsewhere had occasion to remark, is not, in my opinion, with them or their developments or repetition, however much the present may occupy itself with their corrected iteration.

Design is invention and development, and when development has reached a certain point the invention is exhausted and some new departure must be taken. No new departure, however, of any importance has taken place since the close of the great schools of France of the sixteenth and seventeenth centuries, and the decoration of bound books is still an open problem awaiting solution at the hands of genius.

But though the problem awaits solution the conditions of the problem may, I think, be stated shortly in general terms. In the first place, then, there must be in any design a scheme or framework of distribution. The area to be covered must be covered according to some symmetrical plan. In the second place, the scheme or framework of distribution must itself be covered by the orderly repetition, and if need be, modification and development of some primary element of decoration. In the great French schools which I have attempted to describe, the *motifs* were primarily curved or straight bands or lines, and compartments composed of the same, the whole pattern of the first school becoming, in principle, the *motifs* of the second and third.

Before leaving this subject of design I may be permitted to prophesy that in the infinite inventions of Nature herself will, in the future as in the past, be found the suggestions of design, and that in seeking them there the craftsman artist will enter again into that vital communion with her which is the condition at once of his own happiness and of his own imaginative growth. But the prophecy must be accompanied by this caution—design cannot, in my opinion, be taught. It is as distinctly a gift of imaginative genius as the power of poetical vision and expression. To the conditions of the problem, then, must be added the genius suitable for its solution.

I have now, in conclusion, to say what, in my opinion, the craft of the binder is, and in what relation it stands to the supreme art and craft of life itself.

All this universe of light and shade and sound, which at all moments surrounds us, and constitutes the supreme object of man's thoughts, his intranscendant inner and outer self, may be looked upon as itself a work of art in progress, and man's life through the ages as an attempt, ever renewed, to apprehend it in its entirety, and to reduce it to something appreciable by his imagination and his affections. This is not the moment to dwell at length upon this attempt, or to show how, with increasing knowledge of his environment, his previous conceptions of it have perished to give birth to higher and wider appreciations; but I may allege that, in my opinion, all the religions which have figured upon the stage of history, as well as all philosophical and scientific systems, are attempts at this reduction of the universe and of man as a part of it, to an entirety harmonious within itself, and fit to be the dwelling place of the imaginative soul of mankind. They are attempts, and for some of us they have ceased to be adequate. For myself, I see only unbounded space and infinite time, and within those illimitables, a finite world obedient to law, unfolding to unknown ends; and though I cannot grasp that world in its entirety, yet I can divine the amplitude of its rhythm, be sensitive to its adaptations and to the balance of its parts, and, in the spirit of the infinitely great, work at the infinitely little, and feel the two akin in their adjustments, balance and rhythm.

It is in this intuition of the harmony of the universe that the ideal of the work of the hand resides. It is itself an adjustment, at once beautiful and serviceable. It is a dedication of man's powers to an end not beyond man's reach; it develops invention and the imaginative faculties; it distracts the mind from the vexed question, never wholly to be put aside, of man's own ultimate destiny; it gives him rest; it gives him hope, that even as from the work of his own hands here there arise things of beauty and of use, so from his whole life's work there may arise in the "hereafter," which in some sense may be only another form of the "present," a something of even greater use and greater beauty still.

It is in this wise that I commend to you all the life of the workman, of the workman working in little in the spirit of the whole.

[T. J. C.-S.]

GENERAL MONTHLY MEETING,

Monday, February 5, 1894.

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

His Grace The Duke of Bedford,
The Right Hon. The Earl of Derby, G.C.B.
The Right Hon. Lord Justice Sir Horace Davey,
Lord Greenock,
Mrs. Fleming Baxter,

were elected Members of the Royal Institution.

The Honorary Secretary read the following letter from Mrs. Tyndall:—

Hindhead House, Haslemere,
December 18, 1893.

Dear Friends,

I thank you from my heart for your kind sympathy in my unspeakable grief.

(Signed) Louisa C. Tyndall.

To Sir Frederick Bramwell, Bart. and
the 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 Lords of the Admiralty—Nautical Almanac for 1897. 8vo.

The New Zealand Government—Statistics of the Colony of New Zealand, 1892. fol. 1893.

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  Athenæum for Dec. 1893 and Jan. 1894. 4to.
  Author for Dec. 1893 and Jan. 1894.
  Brewers' Journal for Dec. 1893 and Jan. 1894. 4to.
  Chemical News for Dec. 1893 and Jan. 1894. 4to.
  Chemist and Druggist for Dec. 1893 and Jan. 1894. 8vo.
  Electrical Engineer for Dec. 1893 and Jan. 1894. fol.
  Electrical Engineering for Dec. 1893 and Jan. 1894.
  Electrical Review for Dec. 1893 and Jan. 1894.
  Electricity for Dec. 1893 and Jan. 1894. 8vo.
  Electric Plant for Dec. 1893 and Jan. 1894. 8vo.
  Engineer for Dec. 1893 and Jan. 1894. fol.
  Engineering for Dec. 1893 and Jan. 1894. fol.
  Engineering Review for Jan. 1894. 8vo.
  Horological Journal for Dec. 1893 and Jan. 1894. 8vo.
  Industries and Iron for Dec. 1893 and Jan. 1894. fol.
  Ironmongery for Dec. 1893 and Jan. 1894. 4to.
  Iron and Coal Trades Review for Dec. 1893 and Jan. 1894. 8vo.
  Iron, Steel and Coal Times for Jan. 1894.
  Lightning for Dec. 1893 and Jan. 1894. 8vo.
  Machinery Market for Dec. 1893 and Jan. 1894. 8vo.
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  Nature for Dec. 1893 and Jan. 1894. 4to.
  Open Court for Dec. 1893 and Jan. 1894. 4to.
  Photographic Work for Dec. 1893 and Jan. 1894. 8vo.
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Victoria Institute—Transactions, Vol. XXVI. No. 105. 8vo. 1893.

WEEKLY EVENING MEETING,

Friday, February 9, 1894.

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

Professor W. F. R. Weldon, M.A. F.R.S.

Fortuitous Variation in Animals.

[No Abstract.]

WEEKLY EVENING MEETING,

Friday, February 16, 1894.

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

Professor Nichol, M.A. LL.D.

Bacon's Key to Nature.

No English writer of equal eminence is so inadequately known by the mass of his countrymen as Lord Bacon. Every man and woman of culture is more or less familiar with the Essays: few have turned ten pages of 'Organum,' glanced at the 'De Sapientia Veterum,' or ventured into one grove of the 'Sylva Sylvarum.' Save as a writer of fine sentences, Bacon has been generally judged by Pope's lampooning line, Lord Campbell's shallow summary, and the farthest adrift of Lord Macaulay's estimates. The moral charges brought against him are at least plausible, though often gross, exaggerations, but the popular travesty of his system set forth by his most eloquent English critic is ludicrous. The writer's diffuseness of illustration permits the whole gist of fifty bright and delusive pages to be given in two. "Bacon," says Macaulay, "was neither a philosopher nor a logician, but a reformer. The peculiarity of his work lay in the fact of its object being altogether different from any which his predecessors had proposed. They had wasted their dialectic on labours like those spent on a treadmill. He strove not to solve enigmas, but to enrich man's estate. The ancient philosophy disdained to be useful, and was content to be stationary. The reviewer finds on the tree which Socrates planted and Plato watered, flowers but no fruit. To the revolving questions of the Schools he answers, "Cur quis non prandeat hoc est." "The aim of Plato," he concludes, "was to exalt man into a god; that of Bacon, to supply him with what he wants as a man. The one drew a good bow, but shot at the stars; the other fixed his eye on a common target, and hit it in the white. While the world was resounding with doctrinal disputes, the traitorous friend and pure philanthropist (!), leaving the windy war to those who liked it, was content to add to the sum of human happiness." Macaulay often talks of the absolute originality of the 'Novum Organum,' of the new era it opened up, but, in flat contradiction of himself, he asserts in his essay that the author's benevolent aim was the sum and substance of his philosophy. The notion that he invented a new method of arriving at truth by *induction* he takes to be about as well founded as that of the people who, in the middle ages, imagined that Virgil was a great conjuror.

Induction has been practised since the beginning of the world, by every human being. The Inductive method had been long before analysed by Aristotle; but it is an analysis of that which we are all doing from morning till night. The man who infers that mince pies have disagreed with him, because he was ill when he ate them, well when he ate them not, most ill when he ate most, and least ill when he ate least, has employed all the Tables of the 'Novum Organum.'

In the always superficial, generally second-hand view of Bacon's work thus condensed, there are two flagrant defects :—

1. It is an impudent travesty of the Greek schools.

2. Stating, only with partial accuracy, what Bacon actually achieved, it leaves us with hardly any conception of what he intended to do. He was no mere Empiric; an observer and experimentalist, he was also a Philosopher, animated by a spirit far less removed from that of the ancient thinkers than Macaulay imagines it to have been.

I.

On the theme of Bacon's relation to the "old age" which he calls "the youth of the world," we can afford space but for a few explanatory paragraphs. He was born in an era of mental revolution and shared to the full its enthusiasms, its inspiration and its excesses. The great political liberal CONSERVATIVE of his generation, he was on matters more purely speculative the great INNOVATOR. In his youthful tractates he arraigns the most illustrious of his predecessors with an audacity which he himself compares to that of Alexander, and in language which recalls Milton's railing at Salmasius. No part of Bacon's plan is more definite than his determination to break with the Past, though no part of it was more incompletely fulfilled. To the last, while dreaming dreams of a Utopia almost as ideal as Plato's Republic, he yet himself was more than half an Aristotelian, and constructed the world out of Categories to which he gave the name of Forms.

The motto prefixed to the 'Instauratio,' "Multi pertransibunt et augebitur scientia," with the suggestion on its frontispiece of Columbus and Drake, is as far as possible removed from the τέλος τέλειον of a Sophoclean play or an Aristotelian State. But he himself stood on the confines of two worlds, and while consciously pressing toward the future, was still unconsciously influenced by the spirit of the past. His Ethics and Politics were largely those of the Greeks, and none among his predecessors was more satisfied of having solved a problem and found a key. The Greeks and their mediæval copyists had mixed Physics and Metaphysics. Bacon failed to disentangle them, confused *a priori* and *a posteriori* truths, and hoped to deal with them by a single method. The injustice of his criticisms would therefore seem to be doubled; but he lived in an uncritical age, an age of creation rather than analysis, of conquest more than appreciation, and his aim made it impossible for him to realise that of the earlier thinkers even had they come down to him less obscured by the mists of commentary. A Declaration of Rights against the greatest thinkers of Greece was the signal of sixteenth century independence. The modern student of philosophy sees a chain of thought in all the schools: they lean on those that went before, and make some advance as they roll, like tides, after one another; but this advance is unconscious. Each new philosopher seems to have found the secret of the earth, and proclaimed it with a religious zeal.

How little has modern Europe added to the pure speculation of the Greeks! How little of physical discovery has Greece bequeathed to modern Europe! There is no greater contrast in history than such sterility and such luxuriance. The ancients had no instruments to work with but language and logical forms. They had hardly any notion of what we mean by a law of nature, and only thought of law as an idea. The same causes that retarded progress on one side, during the ages of antiquity, retarded it on every side during those succeeding, in which the world seemed to re-enact its childhood. Scholasticism had turned away from Nature: to the study of which Bacon first, with sufficient eloquence, recalled men's minds. He himself had, in a sense, less claim to the title of a physical philosopher than Roger Bacon, who is said to have made gunpowder, while Francis thought the courage of soldiers might be increased by eating it. He calls Copernicus that cabman who drives the earth about, and expresses a wish that the Italian astronomers would give up their stargazing.

II.

The mass of Analysis and Criticism accumulated about the Baconian philosophy still leaves room for difference of opinion as to its degree of inaccuracy in detail and failure in direct result, for doubt regarding many of the beliefs of its author, recorded with frequent inconsistency and some confusion in so many various tentative forms; but there is no room for rational difference as to its design, which was to explore the Universe, and under constant protest of reverence for the mysteries of Faith, to make men its masters.

Bacon's audacity was native to the times in which he was born. All the features, bright and dark, of our Elizabethan age—its splendour, the wearisomeness of its intrigue, in chief its *daring*—are conspicuously reflected in his career. The hundred years preceding had all over Europe been rife in changes, changes in moment hardly approached during the interval since the assertion of Greek independence. So much was going out, so much coming in, that the previously established order of things seemed like an unsubstantial pageant. Men's minds were dazzled and their fancies inflamed at the opening of the gates of the modern world. The age was adventuring in more paths than even the author of the 'Instauratio Magna' was ready to approve; but he was the first fully to recognise its increasing purpose. It was an age at once of scepticism and of credulity. When a new world of fact, so full of authentic marvels, had been so suddenly revealed, to doubt regarding a new marvel seemed as unnatural as it would now seem to accept it. The image in the 'De Augmentis' of the child emerging from Plato's cave applies, in a way scarce intended, to the great actor of the time, and to the great thinker himself. When the memory of the Incas was fresh, what wonder that the former was lured from his manor and home fields to seek the Eldorado, whose battlements seem to gleam through the arch of experience; or that the other rose from the study of Paracelsus and Agricola to lay the foundations of a new Atlantis, where the coarser metals might be transmuted into gold "by superinducing the 'forms' of the precious ore. Similarly, while modern History and Science were yet in infancy, long ere *Specialism* had made havoc of "the grand style," it was by an equally characteristic and natural audacity that the one essayed to bring together the records of all the nations, while the other aspired to catalogue the "Phænomena Universi" and to supply an Organum "to storm and occupy the castles and strongholds of the Nature of Things."

During the last thirty years of the sixteenth century, the Renaissance and the Reformation met and were blended in the writings of Sidney, Hooker, Spenser, Marlow, Shakespeare, Raleigh and Bacon. Liberty was still restrained within jealously guarded bounds, but there was emancipation enough to bring with it the feeling of a freer atmosphere; after a feverish night men breathed the morning, and social peace was the more secure that it was the calm of a sky cleared by storms. There was time to look before and after, to read Drayton's poetical antiquities, and the well-languaged Daniel, to weave a more gorgeous web out of the cycle of Arthurian Romance, and lay down a new scheme for interpreting Nature.

What then was Bacon's central Idea, and how far was it capable of realisation? To these questions we must now confine ourselves. He has been accepted as an often incisive critic and a keen observer. His own idea of his position was that of a discoverer of a *mundus alter et idem*, a new world of more moment to mankind than the Indies of Columbus. He would have received with indignation the verdict that his work was mainly negative, that he would be known to the future by his incidental wisdom and commended by rhetoricians for his popular aims. He arraigned the thinkers of the Past because he fancied himself to have found what they had missed. He assailed their love of system because he had a supplanting system. He despised their *a priori* views because, by the exercise of an imagination almost Shakespearian in its daring, he conceived to have banished from the future of the physical drama all need for further imaginings. Nowhere do we find a more exalted conception of Nature than in his pages, but he holds it as a cardinal doctrine that she is finite, that the time is at hand when all essential knowledge may be grasped, the world well won, and the age of the Garden before the Fall restored.

Bacon insists that we must enter the Kingdom of Nature *sub personâ infantis*, but he has himself the air of one taking possession of a throne. He had little of the submissive spirit which led Newton to confess himself "a child gathering pebbles on the shore of the infinite sea," or that of the modern poet, "moving about in worlds not realised." His always proud humility lay in his acceptance of the dictum of the 'Parmenides' that the least of Nature's manifestations is worthy of our note: but his aspirations as a thinker dwarfed his ambition as a statesman. By every image at command, of a fancy among the masters of prose equalled by Plato alone, he impresses us with his belief in his possession of a *clue*, a KEY, a *secret*, that had come to him by a sort of inspiration. He had unlocked the door barred alike to Aristotle and Aquinas; learned the "Open, Sesame" where Paracelsus had been calling "wheat and rye." He had grazed the beach of the "New Atlantis" though he might only live to blow the clarion for colonising generations; he had realised the magic of which the Magi only dreamt. The gods had answered his prayer as that of Pygmalion; he knew the tune of the 'Winter's Tale' to call the marble statues of the old Philosophy down from their pedestals to take life and colour, and move fostering, gladdening and restoring among men.

Bacon's "Interpretation of Nature" receives some light from the crude Pre-Socratic speculations on the one side, and on the other from the modern "Correlation of Forces" and conjectures as those of Leibnitz and Boscowitch on the borderland where physics seem to merge into metaphysic. Like the earliest recorded thinkers of Greece, Bacon founded his *Unity* on Examination of the External world rather than on a Mental Analysis. He accepts the conclusions of neither Thales nor Heraclitus; but he holds that in looking behind appearances to some physical basis into which the shows of the Universe may be resolved, they were on a path more fruitful than the impossible attempt to separate non-existent substances from attributes, or paradigms from realities. He is as ready as any Greek or German to admit that "things are not what they seem"; but, setting aside the inscrutable truths of religion, he has no faith in anything that is not physical. His "noumena" *are* "phenomena" interrogated and explained; Proteus grappled with through every alias till he returns to his proper shape; Heat confessing itself to be an *expansive motion.* In dealing as with the ultimate Nature of things Bacon suggests rather than dogmatises; hazarding the view that all the assumed *elements* may be reduced to one, as has been imagined by chemists like Samuel Brown, who have tried to establish what the alchemists vaguely guessed. Bacon is nowhere bold enough to assert with Pythagoras, that all apparent varieties of quality are resolvable into arrangements of form; but he feels confident in having gone far towards Unity in his simplification of apparently complex natures.

Nature with him is a mighty conjuress, who plays a myriad tricks with a few cards; man as "interpres" has to detect, and, as "minister" to replay those tricks, and so by mocking to become her master. Think how many colours may be made out of various combinations of the three called primary. May it not be possible to resolve even these last by processes analogous to those which analyse the secondary? Look at the combinations in a laboratory—the manufacture of an apparent diamond out of a block of coal. How many shapes may result from the arrangement of, say, six solid factors! How many more if these factors are fluid? Is there anything strange in the belief that everything that strikes any of our senses may be resolved into the action and reaction of a limited number of "simple" irresolvable "natures"? To discover these is Bacon's prime quest: for as in formal reasoning the conclusion follows from the premisses, so if we have once caught hold of the "motifs" or αρχαὶ of Nature we shall be able to reproduce her results. The "unseen universe" by which we are surrounded, is thus at once the "garment of God" and the heritage of man. Man is, with Bacon, "the roof and crown of things," and his view of the relation of the chief of creatures to the rest of creation is expressed in the verse of his friend and coadjutor in translation, George Herbert,—

> "For us the winds do blow,
> The earth doth rest, heaven move and fountains flow.
>
> . . . .
>
> Man is one world and hath
> Another to attend him."

In the same spirit Bacon protests against the old preference of Passive to Active Life, of the βίος θεωρητικὸς to the βίος πρακτικὸς, as in the famous passage about Pompey saying, "It is necessary for me to serve the State while I live, not for me to live longer," and the speech of Pythagoras to Hiero, "At the Olympian games there are mere spectators, but in this theatre of man's life it is reserved only for God and Angels to be lookers on." This is Bacon's Philanthropia: like Socrates he thought it his prime duty to inquire into the agencies which most affect human life and happiness; but he wished to find *general* and not *special* agencies. Against nothing does Bacon record more frequent and strenuous protests than the mere Empirical Utilitarianism with which he has been calumniously credited. "It is a corrupt judgment," he exclaims, "to think that there are no true differences of things, but according to utility." He perpetually set his "Experimenta Lucifera" above the Experimenta Fructifera. He conjured down Philosophy from Heaven to Earth, but with her aureole on.

The first book of the 'Advancement of Learning' is, next to the Essays, the most familiar of the author's works. There could be no more adequate prelude to the "Great Instauration" than the exaltation of "the Dignity of Knowledge," in language rivalled only by the advocacy of Freedom of Speech in the 'Areopagitica.' Nowhere does Bacon, in the forefront of his age, more suggest the thought that while morning broke on all statues alike, Memnon alone made music in reply. Nowhere does he assert himself as an orator of science more persuasive if not greater than Leonardo or Galileo; nowhere has he given more conclusive answers to the imputation of narrow if not sordid utilitarianism, preferred against his name by those who have taken it to their market without more than a glance at his work.

1. The 'Advancement of Learning,' expanded into the 'De Augmentis,' fulfils as adequately as was possible for one man to do at that age, the promise of the first part of his Instauration, the "Partitiones Scientiarum." They constitute a Diorama of Science, practical and speculative, as known up to his time. With numerous errors, they abound in wise reflections and countless suggestions, and argument for endless comment, on which we cannot here touch.

2. In logical if not actual order, the Second Part was meant to be a Catalogue of all the facts of Nature under the title of "Phænomena Universi," which Bacon thought might be made complete, but to which he was only enabled in the 'Sylva Sylvarum' and elsewhere to make a few isolated contributions.

3. Thirdly we have the 'Novum Organum' itself.

4 and 5. Two other fragmentary parts give examples of its working.

6. To the last, the Philosophy itself, the future progress of Science has been making gradual contributions.

I can only say a few words about the Organum.

After some introductory aphorisms, Bacon dwells on the defects inherent in man's own nature, errors of sense and of the understanding. He then proceeds to arrange the prepossessions or mental disturbing causes under the famous four heads of *Idola.* These being so well known to Macaulay's schoolboy, merely call for enumeration. On the threshold of inquiry we encounter those phantoms of the mind, *Theatri*, reverence for Aristotle and the other misled and misleading schools which he again in detail arraigns; *Fori*, common talk and public opinion; *Specus*, a lawyer's or politician's bias which he had to encounter in dealing with Coke and Cecil; last, *Tribus*, the infusion of the passions and the affections that coloured the dry light in his own as in all minds. These are the Idola, which are to Science as formal fallacies are to Logic, the prima facie pitfalls in the way, the duties of omission in Natural Philosophy. It is evident that they may either act together or separately in the same mind and in reference to the same thing. If I say the sun moves round the earth because my eyes tell me so, it is an Idolum *tribus;* if because language takes it for granted, it is an Idolum *fori;* because Ptolemy says so, it is an Idolum *theatri;* because that view agrees with other theories of my own, it is an Idolum *specus*. When those spectres are laid the way is clear for unbiassed investigation.

Having got all the facts that *are*, about the object of my research, I have next to range them under three Tables: (1) of *Affirmatives* containing a collection of all the known instances that agree in having the same quality, e.g. to take Bacon's example, of all bodies that give forth *heat*. (2) of *Negatives*, a collection of examples of bodies otherwise similar (else the list would be endless) which do not agree in the same nature, i.e. which do not give out *heat*. A main use of this table is to discover the nature sought by observing qualities absent in the analogous nature. Thus boiling water is hot, ice is cold, living bodies are hot, dead bodies are cold; but in boiling water and in living bodies there is motion of parts, in ice and dead bodies they are fixed. Does it not seem, therefore, that motion of parts is of the nature of heat? The stress Bacon lays on Negative Instances is the first clear assertion of the principle "Audiatur et altera pars" in Philosophy, and he uses it quaintly of superstitions, in the question, Where are the tablets hung of those that perished with their vows? The employment of this table along with the first corresponds to the "Joint Method of Agreement and Difference" in modern Logic. (3) While the use of the third, that of Majus and Minus, more or less intensity of the quality, is Mill's "Method of concomitant Variations."

So far all is plain. The use of these Tables is in the main modern Induction. There is wanting only the recognition of Theory, of Hypotheses as a motive and principle of arrangement; nor is Bacon without a vague idea of the value of shrewd conjecture, which he expresses under the name of Vindemiatio Prima or Permissio Intellectus, i.e. an indulgence to the understanding to gather early grapes.

Meanwhile he throws into his *Table of Exclusions everything* about Heat which is not present in the affirmatives, present in the negative, everything which increases when the phenomenon decreases and *vice versâ*. The conclusion drawn from this as regards Heat, not so far astray as critics, to whom detail is everything, have assumed, is yet partly a guess. Bacon had worked up to the modern canon of Residues, but he failed properly to apply it, for of his Prerogative Instances, meant as severer tests, he has only given "a few illustrations." He was hurried away by the very impatience, misled by the same love of uniformity which in his predecessors he had denounced. Above all he had set before himself an impracticable design. He thought he could put labels on the whole of Nature. He did not know by how many parts her subtilty passes the subtilty of the human mind, or how many new instruments were needed to wring her secrets from her tenacious grasp; and his lists are often a jumble of things great and small; his instances and conclusions, medleys of shrewd suggestion and almost childish fancies. Like Galileo, he maintained that Nature must be interpreted like a book, that we must learn the alphabet—though far longer than he knew—before arranging the facts of an Historia Naturalis, but the *Induction* which would have given to the Table of Exclusions their conclusive force, was impossible; and his *Forms* were evasive.

A few words only on these two heads, before a closing summary of his work and its results. It goes without saying that Macaulay's travesty and that of his Philistine centurions is a consummate *ignoratio elenchi*. Like the chamberlain in Tennyson's "Day Dream," he dallied with his golden chime and "smiling put the question by." To confound the instinctive induction of all ages with the inductive process is to contend that because the burnt dog dreads the fire, it is idle to inquire into the nature of heat, or that mensuration and trigonometry are useless because for practical purposes a man can divine the height of most maypoles with his eyes and the breadth of most fields with his legs.

As irrelevant is the criticism that Bacon's Method was latent in his Age. So no doubt the law of gravity was latent in the age of Newton and the steam engine in the age of Watt. The characteristic of originality is to be first born in the new thought.

Yet more absurd is the contention that Bacon had been forestalled. We might as well assert that Newton was anticipated by the ancients because they recorded the phenomenon of attraction. The Induction of Socrates was a mere process of leading the mind to moral convictions by the use of Example and Analogy. Plato's view comes nearer to our own; but his progress is from thought to higher thought, not from Observation to Law. Aristotle's *Induction* is a mere summary reasserting of the whole that which had previously been asserted of all the parts. Bacon's deals with facts so as to elicit a law out of them: it is a process of discovery and yet in a sense geometrical, as it would give to the old inductive syllogism an immense and in point of fact impossible extension.

The heat in *a b c* is an expansive motion.
" " all heat.
All heat is an expansive motion.

Where the minor is unattainable. The process of his exclusion fails because the list of negatives can never be complete. It was something, as he asserts, which had never been attempted before, but it was also something which will never be attempted again. Recent induction—that of Mill, Whewell, Herschel, Faraday and Darwin—is the means by which the great sequences of Nature, called Laws, are investigated by the aid of apt conjecture, and by careful verification established. But Bacon thought to accomplish more than this. By aid of a Method, which, from its exhaustiveness, he held to be as certain in its results as a demonstration of Euclid, so mechanical that when once understood all men might employ it, yet so startling that it was to be a new sun to the borrowed beams of stars, he aspired to penetrate into the inner nature of things and so hold them in command.

I can only say a few words about those *Forms* which are the supreme and ultimate objects of his research. They are, he says, related to effects when permanent qualities, as efficient causes are to events, and as aids to their discovery he introduces his conception of the *latent Schematism* or invisible structure of bodies as crystals revealed by cleavage, and the *latent* or secret *process* by which changes are brought about, as the process which takes place in a seed or an egg, before the appearance of the plant or chick.

Bacon himself approaches the subject of his Forms, with the same sort of deference as Plato does his Idea of Good, and endeavours to illustrate his conception with all sorts of Imagery. It is neither mere shape nor an abstract idea, nor a modern law of Nature, which is merely the register of a great fact. As regards the name, he says "It seemeth best to keep way with antiquity 'usque ad aras' and therefore to retain the ancient terms, though I often alter the uses." He does not perceive that in retaining the old names he drags along with them a part of the old conceptions. His Form is to the shows of things as *inner* to outer, it is the "very thing," having the same relation to the so-called primary qualities as they have to the secondary, the hidden *nature* arrived at from the concrete manifestation. He regards every complex body as a turma or congeries of *simple* qualities which we can ascertain by analysis, as it were breaking down the less known species into a better known genus and differentia. As *Heat* is resolved into a specific sort of motion, so he holds it is possible to reduce all phenomena to combinations of simple elements, which we may recombine and superinduce on various substances, and so become the minister as well as the interpreter of Nature.

Bacon constantly approaches great discoveries as with a divining rod, and then passes them by. Astronomers, he says, in the 'De Augmentis,' "have brought a beautiful hide but stuffed with straw." They have arranged skilful systems to resolve the visible phenomena into circular movements, but they have neglected to ask the *cause* of the phenomena. The interior of the ox (namely the physical reason) is wanting; out of which (with the help of hypotheses) a theory might be devised, which would not merely satisfy the phenomena, but would set forth the substance, motion and influence of the heavenly bodies as they really are. *This is precisely what Newton, working on the data of Kepler, really did;* and, in a subsequent paragraph, Bacon, suggesting that the discovery is to be made by obtaining information of heavenly things from those seen amongst ourselves, comes still nearer in his anticipation. But he rarely "dips into the future," without immediately reverting to the past. He recedes from his guess as to the method by which the law of gravity was finally established, to the impossible Tables of Speusippus at the base and an Ixion-like embrace of the golden clouds which shrouded the apex of his unaccomplished pyramid. He sought to solve mysteries which Nature by no torture or binding of Proteus has ever been forced to reveal. To ask the meaning of her primitive qualities is to batter at the last gate of Spenser's Busiris, bearing the inscription

"Be not too bold," and his audacity contrasts with the modest temperance of more practically successful men of science, who owed their triumphs in large measure to the self-restraint which led men first to ascertain facts without any inquiry after their cause. Bacon's Tables including those of moral as well as physical conditions, tables of Anger, Hate, Love, as well as of Density, Heat, Cold, exceed the error of such of his partial successors as Comte and Buckle, in ignoring the peculiarly complex action, if not the absolute freedom, of the human will. Knowledge of the mental powers is a sort of experience; History an observation on mankind; Education an experiment; but the uniformity of Nature does not hold good in the same degree in the Moral as in the Physical world.

Bacon overestimated the precision of his Method, in comparing it to a pair of compasses, so that, once in possession of it, all intellects may work alike, and consequently fell into the error of supposing that it must proceed by a mechanical accumulation, ignoring the fact that in Astronomy the great guesses, to be afterwards verified, saved centuries of the work of ants. Macaulay's criticism is here just. No method will bridge the gulf between a dunce and a man of genius. His numerous minor errors have been sufficiently indicated. They are no more frequent than might have been expected from the wide grasp of his mind, his antithetical temper and combative iconoclasm, and from the abnormal faculty of finding analogies which led him to snatch unripe apples from the tree of wisdom. No man can leap beyond his own shadow; hardly one beyond the shadow of his age.

When the criticism of pure logicians, who retort on Bacon's attacks on the syllogism by reasserting its analytic value; of *specialists*—the modern Schoolmen—who resent his insufficient view of their little worlds; of mere physicists, who dislike his metaphysical side and dwell, as Baron Liebig does, with acrimonious exclusiveness on his defects; of German metaphysicians who have treated him as hardly in their blame as the majority of German critics have dealt with Shakespeare in their praise; when their cavils have done their worst, he has received from scientific and literary men of larger grasp the crown of the great anticipator of the results, the first organiser of the methods of modern science, the prophet of things that Newton revealed. He is commended by Descartes as the man who knew best how to make experience useful; by Gassendi as the originator of the Logic from which we may expect the development of a new philosophy. Leibnitz conceived his own Monadism to be akin to the doctrine of the "De Principiis," and asserted, "We do well to think highly of Verulam, for his hard sayings have in them a deep meaning." Kant alludes to Bacon's work as that of one of the greatest physicists of modern times. Laplace refers to him as "the brightest man of his bright century." Playfair says, "It is easier to find new Galileos than new Bacons." Herschel compares him to "the star that announces the day." Mackintosh declares "his authority will have no end." Mill, marking his lacunæ, reveres his name. Whewell, who sets it on the forefront of his comprehensive survey, and the lamented Tyndall, with literary graces only less unique than those of his master, are at one with historians like Hallam in proclaiming him the first mover in a mighty impulse.

Bacon's self-criticism is sound, "Fungar vice cotis acutum reddere quæ ferrum valet expers ipsa secandi." He sharpened the instruments for others to use; he pointed the path which he could not follow, to the walls of the citadel he failed to storm. His claim to have moved the intellects that move the world does not rest alone on his forecasts of discovery. He opened a way to "unpathed waters, undreamed shores," by training his contemporaries to habits of observation which he first set on a rank of equal dignity with abstract thought. He invented nothing, but he called the Sciences back to their sources, and so, in the phrase of Rémusat, "threw out a thought full of the future." His predecessors spoke in lower tones. It was only Bacon's enthusiasm, through half a century maintained, his dauntless tenacity and his splendid powers of speech that first gave to modern science wings to make way through the minds of men.

Nor Leonardo nor Galileo had his far-ranging view of the Unity of Nature and of Science, or of the ultimate consilience of knowledge and practical power. His rubric was "all things by scale to unity." His perception of analogies, however "portentous," led him right in tracing a nexus in the scheme of things. Bacon reflects and repeats the old vague efforts in the same direction, from Heraclitus's finer fire, the start and goal of the way up and down, to Plato's Triads; from the speculations on phenomena and noumena that ran through the period from Xenocrates to Zeno, to the metaphysical paradigms of the mediæval realists, physically realised in *Owen's archetypal skeleton*. But, with all its uncritical want of precision, his own view is no mere summary: it is a real, though sometimes shadowy premonition of the later discoveries that have linked together, under the conception of the "Correlation of Forces," the polarity of magnetism, the spark of electricity, the affinity of chemical elements and of crystalline poles, the unification of heat, light and picture-rendering rays as undulations of the universal air.

Similar conceptions are embodied in Schelling's "Harmonies of Nature" and the comprehensive anticipation of Hegel, "Magnetism is the universal act of investing multiplicity with unity"; but they are nowhere clad in such imaginative reality as in Bacon's extension of the world by the revelation of an unseen universe, a Fairyland of Science in which "we are citizens of no mean city."

The epochs of Comte revolve, but in widening circles, as the positive again merges in the religious. The Greeks followed a mirage of the land they never reached. The forces of Nature address the child in images and myths; Heaven lies about him, because his fancies do not transcend the dome of blue; and he sees in the twilight the celestial gates. The stars to him are gods, and make a sphery chime. Later, "the intellectual power through words and things pursues its dim and perilous way" to the same goal, and nature once more appears as the garment of divinity. The world is one; "one law, one element" is the first utterance and the last, the Alpha and Omega of philosophy; but at the close the fictitious has been exchanged for the real, when Faith, Fancy and Truth are blended in a higher metaphysics.

These ideas are common to Bacon with other theorists. He stands by himself in his belief in being able to make them live. His philosophy is no *θεωρία* or dream; but a ministration to the wants of the mankind he loved with a philanthropy often inconsistent with personal devotion. With him knowledge alone had no satiety; in age, when the Loves are changed into the Graces, he ran the race as in the heyday, never feeling the weariness of Faust, and only at times the *suave mari magno*. His philosophy has its concrete presentation in the 'New Atlantis,' that rises from the sea in our memories, like Prospero's Isle, the most practical and among the most poetic of the anticipations of the future. It is an allegory of his fragmentary work: and, in closing the records of his varied life, we linger on the sound of the sea rippling by the beach of its richly coloured shore. Its details may be faulty, its design is prophetic; nor in Plato or Augustine, nor in More or Sidney, Campanella or Milton is there so much sympathy with our increasing purpose, combined with so much sense of its limitations. Bacon never soars away from life, he realises its complexity, its temptations and the indefinite range of its aggregate power. Like Shakespeare, he "puts a girdle round the world," and has left a name not to "point a moral or adorn a tale," but to be a beacon as well as a warning from one who, in a sense of the infanti perduti, has also been among the eternal benefactors of his race.

[J. N.]

WEEKLY EVENING MEETING,

Friday, February 23, 1894.

Professor David Edward Hughes, F.R.S. Vice-President, in the Chair.

Professor Silvanus P. Thompson, D.Sc. F.R.S. *M.R.I.*

Transformations of Electric Currents.

[Abstract deferred.]

WEEKLY EVENING MEETING,

Friday, March 2, 1894.

Sir Douglas Galton, K.C.B. D.C.L. LL.D. F.R.S. Vice-President, in the Chair.

Professor John G. McKendrick, M.D. LL.D. F.R.S.

The Theory of the Cochlea and the Inner Ear.

[No Abstract.]

GENERAL MONTHLY MEETING,

Monday, March 5, 1894.

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

Edward Henry Cardwell, Esq.
James Childs, Esq.
J. Dundas Grant, M.D. F.R.C.S.
Sir Alfred Seale Haslam,
Alexander T. Hollingsworth, Esq.
Robert Trefusis Mallet, Esq. M.Inst.C.E.
Sidney Ashmore Stewart Maud, Esq.
Mrs. Hermina Melchers,
Percy Alport Molteno, Esq. B.A. LL.B.
Dr. Eugen Obach,
Charles E. S. Phillips, Esq.
The Hon. Lionel Walter Rothschild,
Miss Harriet Russell,
J. Cranefield Scholey, Esq.
Miss Isla Stewart,
Arthur Talbot, Esq.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to Mrs. Rae for her present of a Portrait of the late Dr. John Rae, *M.R.I.*

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

Professor J. A. Fleming, M.A. D.Sc. F.R.S. *M.R.I.* Professor of Electrical Engineering in University College, London.—Four Lectures on Electric Illumination; on *Tuesdays*, April 3, 10, 17, 24.

Professor J. W. Judd, F.R.S. V.P.G.S.—Three Lectures on Rubies: their Nature, Origin, and Metamorphoses; on *Tuesdays*, May 1, 8, 15.

The Rev. W. H. Dallinger, LL.D. Sc.D. F.R.S.—Three Lectures on The Modern Microscope; an Instrument for Recreation and Research; on *Tuesdays*, May 22, 29, June 5.

Francis Seymour Haden, Esq. President of the Royal Society of Painter Etchers.—Two Lectures on The Etching Revival; on *Thursdays*, April 5, 12.

Professor J. F. Bridge, Mus. Doc. Organist of Westminster Abbey, and Gresham Professor of Music.—Two Lectures on Music: 1. Musical Gestures; 2. Mozart as a Teacher (with Musical Illustrations); on *Thursdays*, April 19, 26.

Professor Dewar, M.A. LL.D. F.R.S. *M.R.I.* Fullerian Professor of Chemistry, R.I.—Three Lectures on The Solid and Liquid States of Matter; on *Thursdays*, May 3, 10, 17.

PROFESSOR W. M. FLINDERS PETRIE, D.C.L. Professor of Egyptology in University College, London.—Three Lectures on EGYPTIAN DECORATIVE ART; on *Thursdays*, May 24, 31, June 7.

JOHN ALFRED GRAY, Esq. M.R.C.S.—Two Lectures on LIFE AMONG THE AFGHANS; on *Saturdays*, April 7, 14.

H. D. TRAILL, Esq. D.C.L.—Two Lectures on LITERATURE AND JOURNALISM; on *Saturdays*, April 21, 28.

CAPTAIN ABNEY, C.B. D.C.L. F.R.S. *M.R.I.*—Three Lectures on COLOUR VISION (THE TYNDALL LECTURES); on *Saturdays*, May 5, 12, 19.

ROBERT W. LOWE, Esq. Author of "Bibliographical Account of English Theatrical Literature," "Thomas Betterton," &c.—Three Lectures on THE STAGE AND SOCIETY; on *Saturdays*, May 26, June 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

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  Atti, Serie Quarta. Anno CCLXXXVIII.-CCLXXXIX. 4to. 1892-93.
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  Proceedings, 1893, Nos. 8, 9. 8vo.
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  Brewers' Journal for February, 1894. 8vo.
  Chemical News for February, 1894. 4to.
  Chemist and Druggist for February, 1894. 8vo.
  Electrical Engineer for February, 1894. fol.
  Electrical Literature for January, 1894. 8vo.
  Electrical Review for February, 1894 8vo.

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Electric Plant for February, 1894. 4to.
Engineer for February, 1894. fol.
Engineering for February, 1894. fol.
Engineering Review for February, 1894. 8vo.
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Ironmongery for February, 1894. 4to.
Machinery Market for February, 1894. 8vo.
Nature for February, 1894. 4to.
Open Court for February, 1894. 4to.
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Transport for February, 1894.
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Zoophilist for February, 1894. 4to.

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Payne, W. W. and Hale, G. E. (the Editors)—Astronomy and Astro-Physics for February, 1894. 8vo.

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Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1894: Heft 1. 4to. 1894.

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WEEKLY EVENING MEETING,

Friday, March 9, 1894.

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

William H. White, Esq. C.B. LL.D. F.R.S. *M.R.I.*

The Making of a Modern Fleet.

The special programme of war shipbuilding embodied in the Naval Defence Act of 1889 is now approaching its completion. Of the seventy ships therein provided for, all except eight or nine will be completed and ready for service at the end of this month, when the five years' period contemplated in the Act will terminate. The few remaining ships will then be far advanced, and in the Navy Estimates for 1894–5 less than 300,000*l.* will have to be provided for their completion. What has been done constitutes an unprecedented feat, whether it be considered on the basis of expenditure, or in the addition made in a comparatively short time to the naval strength of the empire. No other country in the world could rival this performance, which furnishes an object lesson, on a large scale, of what has to be done whenever the making of a modern fleet is undertaken.

Regarding the transaction from this point of view, the principal steps may be summarised as follows:—

1. The selection of types, and the numbers of ships of each type to be built.
2. The preparation of designs for each type, fulfilling the conditions laid down for offensive and defensive powers, speed and coal endurance.
3. The making of estimates of cost; these estimates including the unit costs for each type; the aggregate cost of the whole scheme; and the incidence of expenditure on each year of the period of construction.
4. The allocation of orders, so that the actual construction of ships, machinery, and armaments may be completed within the stipulated period.

For ships of the Royal Navy, the Board of Admiralty is the responsible authority in the selection of types, and determination of the numbers to be built of each type. Since actual experience in modern naval warfare is almost entirely wanting, differences of opinion necessarily arise respecting the relative values of different types, the best methods of protection, the most suitable armaments, and other features of construction. In the ancient fleet of unarmoured sailing vessels, long-continued experience in actual war, associated with practical stagnation in the construction, armament and propulsion of war-ships, made the selection of types an easy matter. Now the progress of invention is rapid, and change follows fast upon change; so that the decision of fighting and sea-keeping qualities is a difficult undertaking. Whatever is done is certain to be challenged or criticised.

The Admiralty has many advantages in its action as a "Committee on Designs." On the Board are a number of experienced and distinguished naval officers. The largest war-fleet in the world is under its orders, and from the service afloat come many reports, suggestions, and records of experiment. What is being done at home and abroad in the construction and armament of ships; the improvement of ordnance, ammunition, armour, torpedoes and other matters of importance, is well known and carefully considered. Use is made of the best engineering talent of the country in devising improved types of propelling machinery, auxiliary mechanical appliances, gun-mountings and other portions of the equipment. When considered desirable, distinguished naval and professional men are called into council. But the final decision as to the characteristics and qualities of each of Her Majesty's ships necessarily rests with the Admiralty.

Universal experience in all navies and at all periods shows that there must be a considerable variety of types in any fleet. No single type can be trusted to perform all the services required at a given moment. Progress in invention and consequent change in type necessarily introduces further variety. Iron and steel-hulled ships have great durability. On the "Effective List" of the Royal Navy still remain specimens of the earliest sea-going ironclads, now over thirty years old; and examples of successive types which during that long period have made their appearance as first-class ships, only to pass gradually into lower classes and finally into the Reserve. Obsolete as many of these vessels are in engines, guns and armour, they are practically as strong as they ever were. Should a war take place, and serious engagements happen between the more modern ships on each side, it is quite conceivable that the so-called "obsolete" ships of the Reserve may play an important part in the final stages of the conflict.

Apart from the variety of type produced by lapse of time, there is the variety arising from the necessities of service. By common consent a modern fleet, like the ancient fleet, must have a squadron of battle-ships as its back-bone. With these must be associated cruisers of various kinds—the "eyes of the fleet"—and vessels of the torpedo flotilla. Opinions differ as to the most suitable proportion of cruisers to battle-ships. Some advocate three cruisers to each pair of battle-ships; others would have two cruisers of different types to each battle-ship; and others consider that, to complete a group, there should be a battle-ship, two cruisers and a torpedo vessel.

The Naval Defence Programme provided for seventy vessels: ten battle-ships, forty-two cruisers and eighteen torpedo gunboats. Most of the designs were novel in character. Eight of the battle-ships are

380 feet long and 14,150 tons in displacement. They are the largest completed ships in the Royal Navy, and the most powerfully armed. Each vessel carries four 67-ton guns, ten 6-inch quick-firing guns and twenty-eight small quick-firers for use against torpedo boats, as well as in action with other ships. The maximum smooth-water speeds are 17½ to 18 knots. In protection, armament, speed and coal-supply they surpass all their predecessors. The ships are of high freeboard, carry their guns at a great height above water, and are specially adapted for service in the Atlantic.

Two of the battle-ships are of less dimensions: 360 feet in length and of 10,500 tons displacement. In speed and coal-supply they compare well with the larger vessels. They are inferior in armament and protection. The heaviest guns are 29 tons each in weight, and the largest quick-firers are 4·7-inch. These vessels were designed especially for service on distant stations, and can pass through the Suez Canal.

There are four distinct types of cruisers. Nine are of the first class, 360 feet long and from 7350 to 7700 tons in displacement. They have maximum speeds in smooth water of 20 to 21 knots, and large coal supplies; powerful armaments and good protection to guns, gun crews and vitals. The heaviest guns weigh 22 tons each, and the main armament consists of ten 6-inch quick-firers, with seventeen smaller guns.

Twenty-nine vessels are second class cruisers, eight being of one type and twenty-one of another type. They are 300 to 320 feet in length and 3400 to 4400 tons in displacement. Their maximum smooth water speeds are about 20 knots, and they have good coal supplies. The armaments include 6-inch and 4·7-inch quick-firers, besides smaller guns, and they have fair protection.

Four cruisers of the third class are 265 feet long and of 2600 tons displacement. They are about a knot slower than the smaller second-class cruisers, and not quite so well armed, but they are equal in protection.

Torpedo gunboats are of comparatively recent introduction, and are the smallest sea-going vessels built to accompany fleets. In length they vary from 230 to 250 feet, in displacement from 750 to 1100 tons. They have a light gun armament and a powerful torpedo armament, the maximum smooth water speeds range from 19 to 20 knots. Experience has proved them to be excellent sea-boats in the heaviest weather.

It will be noted that all these vessels are of high speed, and capable of acting together as a fleet. Further, that the Naval Defence Programme provided not merely for the largest proportionate number of cruisers to battle-ships above mentioned, but gave a considerable margin over and above those requirements available for service in the protection of commerce or in other ways. If a fully constituted fleet were formed from the Naval Defence ships, including all the battle-ships and the equivalent number of cruisers, it would surpass in speed and fighting power any equal number of completed ships of similar classes that could possibly be brought against it from existing navies. Having been created rapidly and simultaneously, it is more homogeneous in character and better equipped for manœuvring at high speed. Its armament, also, is of the most modern description, being distinguished by the preponderance of quick-firing guns. These guns can be fired about thrice as fast as guns of equal calibre but earlier patterns, and the supplies of ammunition have been proportionately increased.

In the fleet 1342 guns are mounted. Of these 776 are 6-pounders or under, and over 500 are 6-inch and 4·7-inch quick-firers, while 56 are from 9·2 inch in the cruisers up to 13·5 inch in the large battle-ships.

Torpedo armaments, including submerged and above-water discharges, are carried in all the ships, but are subordinated to the gun armaments, except in the torpedo gunboats. There are 322 torpedo-ejecting tubes in the seventy ships.

All the larger ships have their bows strengthened for ramming. That method of attack, however, involves special risks, particularly since torpedo armaments have been so considerably developed.

Electric search-lights and internal lighting, net defences and all other means for protecting the ships against torpedo-boat attacks have been adopted in the larger cruisers and battle-ships. The smaller cruisers and torpedo vessels have no net defences.

Mechanical appliances of all kinds have been freely employed to reduce or assist manual labour. In habitability and sanitary arrangements the ships surpass previous constructions.

The aggregate total weight of the ships, fully equipped, exceeds 335,000 tons; the total power of the propelling engines, working under conditions of maximum development, is about 600,000 horse-power. This proportion of power to weight—averaging nearly two horse-power to each ton—is a clear proof of the relatively high speed of the Naval Defence fleet. Until ten or twelve years ago the maximum speeds of battle-ships in smooth water ranged from 14 to 15 knots, and of swift cruisers from 15 to 17 knots. Comparing these figures with those given above for the Naval Defence ships, it will be seen that a great stride has been made. Improvements in marine engines have greatly aided progress, but there has necessarily been a considerable increase in engine-power. As speeds increase, so does the rate of growth in expenditure of power increase most rapidly. A first-class battle-ship, for example, can be driven 10 knots an hour by 2000 horse-power. At 14 knots 5500 horse-power is necessary; at 18 knots, 13,500 horse-power. To gain 4 knots from 10 knots means an increase of 3500 horse-power; an equal gain in speed from 14 knots involves an increase of 8000 horse-power.

Modern ships depend solely upon steam propulsion, and are practically destitute of sail-power. Their range of action and power of keeping the sea depends, therefore, entirely upon their coal supplies and rate of coal consumption. By the use of higher steam-pressures and greater expansion the rate of coal consumption has been greatly reduced in the last thirty years. A first-class battle-ship of 1860 required to burn about 5 to 5½ lbs. of coal per indicated horse-power per hour, whereas a ship of similar class in the Naval Defence fleet burns 2 to 2¼ lbs. only.

On the other hand, in recent ships great demands are made on coal for various auxiliary purposes formerly non-existent. Large quantities of sea water have to be distilled for use in the boilers. Internal electric lighting makes considerable inroads on the coal. The multiplication of auxiliary machinery for all purposes does the same, whereas in earlier ships most of the operations now done by such machinery were performed by manual power.

Taking a broad view of the situation, it may be said that modern ships have much larger coal endurance, and can steam over longer distances. When cruising at sea or making passages under ordinary conditions war-ships proceed at moderate speeds. Comparisons of coal-endurances are, therefore, commonly made at the speed of 10 knots. A battle-ship of the first class built in 1861 carried 750 tons of coal, and could keep the sea steaming continuously at 10 knots for six days. She had auxiliary sail-power also, and could economise coal under favourable circumstances of wind and weather. A first class battleship of the Naval Defence fleet leaves port with nearly twice as much coal on board, and can steam continuously at 10 knots for twenty to twenty-one days before her coal is exhausted. She has no sail power; her machinery and propellers are duplicated for the sake of greater safety against disablement and better utilisation of the engine-power at high speeds.

The armaments of modern ships have been made proportionately heavier, not so much in the way of increasing the weight of the most powerful guns, as by developing the secondary armaments of quick-firing guns and increasing the supplies of ammunition. It will be remarked that the heaviest guns mounted in the Naval Defence fleet are 67 tons in weight, whereas preceding ships of less size carry 110 ton guns. Indeed, had there been a satisfactory 12-inch gun of about 50 tons available in 1889 it would probably have been adopted by preference. Since that date such a gun has been produced, and has been made the principal armament of the *Majestic* class.

A distinctive feature in recent battle-ships is the great power and efficient protection of the secondary armament of quick-firing guns. It is within the truth to say that with this portion of the armament alone, a ship of the *Royal Sovereign* class could make a good fight, having regard to the rapidity of fire and the energy of the projectiles. A 6-inch quick-firing gun delivering five or six aimed projectiles per minute, with energies sufficient to perforate a foot of iron armour at close range, is clearly a formidable weapon.

Armour protection in the Naval Defence ships has been most carefully considered. No other feature in war-ship construction has given rise to greater controversies than the proper method of disposing the armour. On the whole the system adopted in 1889 has given general satisfaction. It involves large proportionate weights and costs. On a ship like the *Royal Sovereign* the thick vertical armour weighs about 3000 tons, and costs over a quarter of a million sterling.

Great improvements in armour have been made in recent years, increasing its defensive power for a given thickness and weight. But in view of remarkable developments in explosives and ordnance there is no disposition to diminish weights of armour on battle-ships. In fact increased protection to secondary armaments involves greater weights in proportion to displacements.

Since modern war-ships have higher speeds, greater coal supplies, more powerful armaments, and better protection, it is inevitable that they should be of greater size and cost than their predecessors. In the mercantile marine, also, the demands for higher speeds or greater carrying power have involved considerable enlargement of dimensions and additional first cost. The largest passenger and cargo steamers, as a matter of fact, exceed in dimensions and displacements the largest battle-ships and cruisers. Their costs are less than those of the war-ships, because they are much less elaborately fitted and carry no armaments. On the Trans-Atlantic service there are employed passenger steamers from 525 to 600 feet in length, and from 15,000 to 20,000 tons displacement. The largest battle-ships yet laid down for the Royal Navy—the *Majestic* class—are 390 feet long and of 14,900 tons displacement. The largest cruisers—the *Powerful* class—are 500 feet long and of 14,200 tons displacement. Analysing the designs of war-ships, and comparing them with merchant ships—as far as comparisons are reasonable between vessels built for entirely different services—one is forced to the conclusion that the sizes and cost of recent war-ships are relatively moderate.

If size and cost are to be reduced, as some persons strongly urge, then it will be absolutely necessary to reduce some or all of the qualities associated in the designs of the large ships; to accept lighter guns, less weight of protection, lower speeds, or lessened coal supplies. In other words, to produce fighting machines of smaller individual power, comparing badly with the ships of most recent design built or building abroad. There would be no difficulty, of course, in producing a larger number of less powerful ships for a given expenditure. But it would be a new departure in British naval policy to deliberately accept individual inferiority in our ships to foreign ships for the purpose of securing greater numbers. If the necessary expenditure is faced, superiority in numbers as well as in the powers of individual ships can be secured; and the weight of public and professional opinion undoubtedly inclines to that side.

If the constitution of the Naval Defence fleet is considered, it will be noted that only the ten battle-ships are really of large dimensions, out of the total number, seventy. This is an illustration of general practice, although the advocates of moderate dimensions frequently proceed on the hypothesis that only large ships are built. As a matter of interest the ships built or building from my designs, since I took office in 1885, have been classified. Out of a total of 131 ships, only 15 are above 10,000 tons in displacement, 12 from 7000 to 9000 tons, 46 from 2500 to 5600 tons, 11 from 1000 to 2500 tons, and 47 are 1000 tons or under.

War-ship dimensions and cost are not to be regulated by arbitrarily chosen limits. The proper procedure is obviously to decide what qualities shall be possessed by each type, and to produce ships possessing those qualities. No better guide under existing circumstances, and apart from actual experience in naval warfare, can be found than in making provision for meeting the possible attacks of foreign fleets, and securing superiority in numbers and in fighting efficiency in each class. Since British ships are built for operating on an enemy's coast, it is the practice to give them larger coal supplies, more stores, equipment and ammunition. Hence they are, class for class, of larger displacement than foreign ships. They are not, however, of greater cost than foreign ships of less displacement. A *Royal Sovereign* can be produced in a Government dockyard for a net cost, excluding armament, of about 760,000*l*. The corresponding cost for a French, Russian or American battle-ship of the first class is from 900,000*l*. to 1,000,000*l*. Consequently in the matter of money value risked on each ship we have a distinct advantage, thanks to our more economical construction.

Taking armament and stores into account, one of the larger battle-ships in the Naval Defence fleet represents in round figures a million sterling when equipped for sea. It is a great responsibility to command such a costly and complicated fighting machine. Naval officers have, however, risen to the occasion, as it is their habit to do. As regards manageability and manœuvring power, the big ships have proved most satisfactory, being as thoroughly under command as much smaller ships. It is a very striking thing to see one or two men steering a ship of 14,000 tons moving at high speed, with the aid of a steam or hydraulic engine. The huge mass answers every motion of the helm, and can be made to reverse its course at full speed in 3 to 3¼ minutes, and in a path whose diameter is about five times the ship's length.

A modern fleet requires large expenditure for its construction and equipment. The seventy ships of the Naval Defence Programme will cost about 22¾ millions, including armaments. Excluding armaments, ammunition and reserves, the cost has been about 18 millions, or an average cost per ship of more than a quarter of a million sterling. This average cost exceeds the cost of the largest unarmoured screw three-deckers, carrying 121 guns, which were the most powerful ships in the Royal Navy thirty-five years ago. It is more than double the cost of the largest sailing three-deckers built about eighty years ago. What has been said above furnishes the explanation of this remarkable increase in outlay on modern ships. The range of net cost in the dockyard-built ships is from about 780,000*l.* for a first class battle-ship down to 50,000*l.* for a torpedo gunboat.

Allowing for alterations in designs, changes in the rates of wages to dockyard workmen, and variations in systems of accounts that have been made since the scheme was first framed, there has been a remarkably close agreement between the original estimate and the actual outlay. That estimate was 21½ millions, the probable expenditure 22¾ millions, and specific causes of increased cost represent about one million. There are few engineering works of great magnitude where the agreement between estimate and expenditure has been so close.

The work of construction has been divided between the Royal Dockyards and private firms. Ten millions represent the value of the contract ships and their armaments; 12¾ millions the corresponding outlay on and for the Dockyard ships. As a matter of fact, the real expenditure in the Dockyards has been on labour, representing about 3¾ millions. Materials, machinery, guns, gun-mountings and other items of equipment have been made outside the Dockyards. These figures indicate how large an employment of the manufacturing and industrial resources of the country has been involved in carrying out a programme which adds greatly to our naval strength.

It is the more remarkable that the programme should have been practically carried through as proposed, when it is remembered that the five years over which it has extended have been years of unprecedented activity in merchant ship construction. No better proof could be given of the surpassing resources of this country for shipbuilding and engineering. The great requirements in guns and gun-mountings have also been met with ease. One incidental result of the Naval Defence Act which deserves mention is the enlargement of our resources for the manufacture of ordnance, many eminent firms having undertaken and satisfactorily executed important contracts, and the guns having been ready in time for the ships. A necessary condition of rapid construction is, of course, thorough prevision and pre-arrangement in all departments, so that there shall be no hindrance of work while waiting for portions of armament or equipment. Rapid construction also means ample financial provision, adjusted to the greatest rate of progress obtainable. Unless such provision is made the work must linger on, and progress will be regulated by the means available.

In this brief summary of what is involved in making a modern fleet, it has been impossible to dwell upon the many difficulties that have to be met in connection with war-ship designs. War-ships are primarily fighting machines. Fighting efficiency dominates their designs, and more particularly the arrangements laid down as necessary for armaments and protection. Every cubic foot of internal space has to be appropriated to and fitted for some special purpose. Accepting these fixed conditions, the endeavour of naval architects is to fulfil them in ships which shall be strong, stable and sea-worthy, possessing the speeds and coal-supplies specified for various types. If complete success is not attained in all cases it should be remembered that the problems which have to be solved, are of increasing difficulty and complexity. And, on the whole, it may be claimed that the designers of modern war-ships, with the aid of their collaborateurs—marine and mechanical engineers, electricians, artillerists and metallurgists—have achieved remarkable results. Speeds have been greatly increased, offensive and defensive powers developed, and sea-keeping qualities maintained.

Those who have to design and build war-ships, as well as those who have to fight them, may be pardoned if they sometimes wish that earlier and simpler conditions had continued. But the progress of invention and the constant struggle for maritime supremacy demand continuous effort, in order that Her Majesty's ships shall in no sense be inferior to those produced in other countries.

[W. H. W.]

WEEKLY EVENING MEETING,

Friday, March 16, 1894.

The Right Hon. Earl Percy, F.S.A. 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 Scientific Work of Tyndall.

It is fitting that the present season should not pass without a reference on these evenings to the work of him whose tragic death a few months since was felt as a personal grief and loss by every member of the Royal Institution. With much diffidence I have undertaken the task to-night, wishing that it had fallen to one better qualified by long and intimate acquaintance to do justice to the theme. For Tyndall was a personality of exceeding interest. He exercised an often magical charm upon those with whom he was closely associated, but when his opposition was aroused he showed himself a keen controversialist. My subject of to-night is but half the story.

Even the strictest devotion of the time at my disposal to a survey of the scientific work of Tyndall will not allow of more than a very imperfect and fragmentary treatment. During his thirty years of labour within these walls he ranged over a vast field, and accumulated results of a very varied character, important not only to the cultivators of the physical sciences, but also to the biologist. All that I can hope to do is to bring back to your recollection the more salient points of his work, and to illustrate them where possible by experiments of his own devising.

In looking through the catalogue of scientific papers issued by the Royal Society, one of the first entries under the name of Tyndall relates to a matter comparatively simple, but still of some interest. It has been noticed that when a jet of liquid is allowed to play into a receiving vessel, a good deal of air is sometimes carried down with it, while at other times this does not happen. The matter was examined experimentally by Tyndall, and he found that it was closely connected with the peculiar transformation undergone by a jet of liquid which had been previously investigated by Savart. A jet as it issues from the nozzle is at first cylindrical, but after a time it becomes what the physiologists call *varicose;* it swells in some places and contracts in others. This effect becomes more exaggerated as the jet descends, until the swellings separate into distinct drops, which follow one another in single file. Savart showed that under the influence of vibration the resolution into drops takes place more rapidly, so that the place of resolution travels up closer to the nozzle.

Tyndall's observation was that the carrying down of air required a jet already resolved into drops when it strikes the liquid. I hope to be able to show you the experiment by projection upon the screen. At the present moment the jet is striking the water in the tank previous to resolution into drops, and is therefore carrying down no air. If I operate on the nozzle with a vibrating tuning-fork, the resolution occurs earlier, and the drops now carry down with them a considerable quantity of air.

Among the earlier of Tyndall's papers are some relating to ice, a subject which attracted him much, probably from his mountaineering experiences. About the time of which I am speaking Faraday made interesting observations upon a peculiar behaviour of ice, afterwards called by the name of regelation. He found that if two pieces of ice were brought into contact they stuck or froze together. The pressure required to produce this effect need not be more than exceedingly small. Tyndall found that if fragments of ice are squeezed they pack themselves into a continuous mass. We have here some small ice in a mould, where it can be subjected to a powerful squeeze. The ice under this operation will be regelated, and a mass obtained which may appear almost transparent, and as if it had never been fractured at all. The flow of glaciers has been attributed to this action, the fractures which the stresses produce being mended again by regelation. I should say, perhaps, that the question of glacier motion presents difficulties not yet wholly explained. There can be no doubt, however, that regelation plays an important part.

Another question treated by Tyndall is the manner in which ice first begins to melt under the action of a beam of light passing into it from an electric lamp. Ice usually melts by conducted heat, which reaches first the outside layers. But if we employ a beam from an electric lamp, the heat will reach the ice not only outside but internally, and the melting will begin at certain points in the interior. Here we have a slab of ice which we project upon the screen. We see that the melting begins at certain points, which develop a crystallised appearance resembling flowers. They are points in the interior of the ice, not upon the surface. Tyndall found that when the ice gives way at these internal points there is a formation of apparently empty space. He carefully melted under water such a piece of ice, and found that when the cavity was melted out there was no escape of air, proving that the cavity was really vacuous.

Various speculations have been made as to the cause of this internal melting at definite points, but here again I am not sure if the difficulty has been altogether removed. One point of importance brought out by Tyndall relates to the plane of the flowers. It is parallel to the direction in which the ice originally froze, that is, parallel to the original surface of the water from which it was formed.

I must not dwell further upon isolated questions, however interesting; but will pass on at once to our main subject, which may be divided into three distinct parts, relating namely to heat, especially dark radiation, sound, and the behaviour of small particles, such as compose dust, whether of living or dead matter.

The earlier publications of Tyndall on the subject of heat are for the most part embodied in his work entitled 'Heat as a Mode of Motion.' This book has fascinated many readers. I could name more than one now distinguished physicists who drew their first scientific nutriment from it. At the time of its appearance the law of the equivalence of heat and work was quite recently established by the labours of Mayer and Joule, and had taken firm hold of the minds of scientific men; and a great part of Tyndall's book may be considered to be inspired by and founded upon this first law of thermodynamics. At the time of publication of Joule's labours, however, there seems to have been a considerable body of hostile opinion, favourable to the now obsolete notion that heat is a distinct entity called caloric. Looking back, it is a little difficult to find out who were responsible for this reception of the theory of caloric. Perhaps it was rather the popular writers of the time than the first scientific authorities. A scientific worker, especially if he devotes himself to original work, has not time to examine for himself all questions, even those relating to his own department, but must take something on trust from others whom he regards as authorities. One might say that a knowledge of science, like a knowledge of law, consists in knowing where to look for it. But even this kind of knowledge is not always easy to obtain. It is only by experience that one can find out who are most entitled to confidence. It is difficult now to understand the hesitation that was shown in fully accepting the doctrine that heat is a mode of motion, for all the great authorities, especially in England, seem to have favoured it. Not to mention Newton and Cavendish, we have Rumford making almost conclusive experiments in its support, Davy accepting it, and Young, who was hardly ever wrong, speaking of the antagonistic theory almost with contempt. On the Continent perhaps, and especially among the French school of chemists and physicists, caloric had more influential support.

As has been said, a great part, though not the whole of Tyndall's work was devoted to the new doctrine. Much relates to other matters, such as radiant heat. Objection has been taken to this phrase, not altogether without reason; for it may be said that when heat it is not radiant, and while radiant it is not heat. The term dark radiation, or dark radiance as Newcombe calls it, is preferable, and was often used by Tyndall. If we analyse, as Newton did, the components of light, we find that only certain parts are visible. The invisible parts produce, however, as great, or greater, effects in other ways than do the visible parts. The heating effect, for example, is vastly greater in the invisible region than in the visible. One of the experiments that Tyndall devised in order to illustrate this fact I hope now to repeat. He found that it was possible by means of a solution of iodine in bisulphide of carbon to isolate the invisible rays. This solution is opaque to light; even the sun could not be seen through it; but it is very fairly transparent to the invisible ultra-red radiation. By means of a concave reflector I concentrate the rays from an arc lamp. In their path is inserted the opaque solution, but in the focus of invisible radiation the heat developed is sufficient to cause the inflammation of a piece of gun-cotton.

Tyndall varied this beautiful experiment in many ways. By raising to incandescence a piece of platinum foil, he illustrated the transformation of invisible into visible radiation.

The most important work, however, that we owe to Tyndall in connection with heat is the investigation of the absorption by gaseous bodies of invisible radiation. Melloni had examined the behaviour of solid and liquid bodies, but not of gaseous. He found that transparent bodies like glass might be very opaque to invisible radiation. Thus, as we all know, a glass screen will keep off the heat of a fire, while if we wish to protect ourselves from the sun, the glass screen would be useless. On the other hand rock salt freely transmitted invisible radiation. But nothing had been done on the subject of gaseous absorption, when Tyndall attacked this very difficult problem. Some of his results are shown in the accompanying table. The absorption of the ordinary non-condensable, or rather, not easily condensable gases—for we must not talk of non-condensable gases now, least of all in this place—the absorption of these gases is very small; but when we pass to the more compound gases, such as nitric oxide, we find the absorption much greater—and in the case of olefiant gas we see that the absorbing power is as much as 6000 times that of the ordinary gases.

	Relative Absorption at 1 Inch Pressure.
Air	1
Oxygen	1
Nitrogen	1
Hydrogen	1
Carbonic acid	972
Nitric oxide	1590
Ammonia	5460
Olefiant gas	6030

There is one substance as to which there has been a great diversity of opinion—aqueous vapour. Tyndall found that aqueous vapour exercises a strong power of absorption—strong relatively to that of the air in which it is contained. This is of course a question of great importance, especially in relation to meteorology. Tyndall's conclusions were vehemently contested by many of the authorities of the time, among whom was Magnus, the celebrated physicist of Berlin. With a view to this lecture I have gone somewhat carefully into this question, and I have been greatly impressed by the care and skill showed by Tyndall, even in his earlier experiments upon this subject. He was at once sanguine and sceptical—a combination necessary for success in any branch of science. The experimentalist who is not sceptical will be led away on a false tack and accept conclusions which he would find it necessary to reject were he to pursue the matter further; if not sanguine, he will be discouraged altogether by the difficulties encountered in his earlier efforts, and so arrive at no conclusion at all. One criticism, however, may be made. Tyndall did not at first describe with sufficient detail the method and the precautions which he used. There was a want of that precise information necessary to allow another to follow in his steps. Perhaps this may have been due to his literary instinct, which made him averse from overloading his pages with technical experimental details.

The controversy above referred to I think we may now consider to be closed. Nobody now doubts the absorbing power of aqueous vapour. Indeed the question seems to have entered upon a new phase; for in a recent number of Wiedemann's 'Annalen,' Paschen investigates the precise position in the spectrum of the rays which are absorbed by aqueous vapour.

I cannot attempt to show you here any of the early experiments on the absorption of vapours. But some years later Tyndall contrived an experiment, which will allow of reproduction. It is founded on some observations of Graham Bell, who discovered that various bodies became sonorous when exposed to intermittent radiation.

The radiation is supplied from incandescent lime, and is focussed by a concave reflector. In the path of the rays is a revolving wheel provided with projecting teeth. When a tooth intervenes, the radiation is stopped; but in the interval between the teeth the radiation passes through, and falls upon any object held at the focus. The object in this case is a small glass bulb containing a few drops of ether, and communicating with the ear by a rubber tube. Under the operation of the intermittent radiation the ether vapour expands and contracts; in other words a vibration is established, and a sound is heard by the observer. But if the vapour were absolutely diathermanous, no sound would be heard.

I have repeated the experiment of Tyndall which allowed him to distinguish between the behaviour of ordinary air and dry air. If, dispensing with ether, we fill the bulb with air in the ordinary moist state, a sound is heard with perfect distinctness, but if we drop in a little sulphuric acid, so as to dry the air, the sound disappears.

According to the law of exchanges, absorption is connected with radiation; so that while hydrogen or oxygen do not radiate, from ammonia we might expect to get considerable radiation. In the following experiment I aim at showing that the radiation of hot coal gas exceeds the radiation of equally hot air.

The face of the thermopile, protected by screens from the ball itself, is exposed to the radiation from the heated air which rises from a hot copper ball. The effect is manifested by the light reflected from a galvanometer mirror. When we replace the air by a stream of coal gas, the galvanometer indicates an augmentation of heat, so that we have before us a demonstration that coal gas when heated does radiate more than equally hot air, from which we conclude that it would exercise more absorption than air.

I come now to the second division of my subject, that relating to Sound. Tyndall, as you know, wrote a book on Sound, founded on lectures delivered in this place. Many interesting and original discoveries are there embodied. One that I have been especially interested in myself, is on the subject of sensitive flames. Professor Leconte in America made the first observations at an amateur concert, but it was Tyndall who introduced the remarkable high-pressure flame now before you. It issues from a pin-hole burner, and the sensitiveness is entirely a question of the pressure at which the gas is supplied. Tyndall describes the phenomenon by saying that the flame under the influence of a high pressure is like something on the edge of a precipice. If left alone, it will maintain itself; but under the slightest touch it will be pushed over. The gas at high pressure will, if undisturbed, burn steadily and erect, but if a hiss is made in its neighbourhood it becomes at once unsteady, and ducks down. A very high sound is necessary. Even a whistle, as you see, does not act. Smooth pure sounds are practically without effect unless of very high pitch.

I will illustrate the importance of the flame as a means of investigation by an experiment in the diffraction of sound. I have here a source of sound, but of pitch so high as to be inaudible. The waves impinge perpendicularly upon a circular disc of plate glass. Behind the disc there is a sound shadow, and you might expect that the shadow would be most complete at the centre. But this is not so. When the burner occupies this position the flame flares; but when by a slight motion of the disc the position of the flame is made eccentric, the existence of the shadow is manifested by the recovery of the flame. At the centre the intensity of sound is the same as if no obstacle were interposed.

The optical analogue of the above experiment was made at the suggestion of Poisson, who had deduced the result theoretically, but considered it so unlikely that he regarded it as an objection to the undulatory theory of light. Now, I need hardly say, it is regarded as a beautiful confirmation.

It is of importance to prove that the flame is not of the essence of the matter, that there is no need to have a flame, or to ignite it at the burner. Thus, it is quite possible to have a jet of gas so arranged that ignition does not occur until the jet has lost its sensitiveness. The sensitive part is that quite close to the nozzle, and the flame is only an indicator. But it is not necessary to have any kind of flame at all. Tyndall made observations on smoke-jets, showing that a jet of air can be made sensitive to sound. The difficulty is to see it, and to operate successfully upon it; because, as Tyndall soon found, a smoke-jet is much more difficult to deal with than flames, and is sensitive to much graver sounds. I doubt whether I am wise in trying to exhibit smoke-jets to an audience, but I have a special means of projection by which I ought at least to succeed in making them visible. It consists in a device by which the main part of the light from the lamp is stopped at the image of the arc, so that the only light which can reach the screen is light which by diffusion has been diverted out of its course. Thus we shall get an exhibition of a jet of smoke upon the screen, showing bright on a dark ground. The jet issues near the mouth of a resonator of pitch 256. When undisturbed it pursues a straight course, and remains cylindrical. But if a fork of suitable pitch be sounded in the neighbourhood, the jet spreads out into a sort of fan, or even bifurcates, as you see upon the screen. The real motion of the jet cannot of course be ascertained by mere inspection. It consists in a continuously increasing *sinuosity*, leading after a while to complete disruption. If two forks slightly out of unison are sounded together, the jet expands and re-collects itself, synchronously with the audible beats. I should say that my jet is a very coarse imitation of Tyndall's. The nozzle that I am using is much too large. With a proper nozzle, and in a perfectly undisturbed atmosphere—undisturbed not only by sounds, but free from all draughts—the sensitiveness is wonderful. The slightest noise is seen to act instantly and to bring the jet down to a fraction of its former height.

Another important part of Tyndall's work on Sound was carried out as adviser of the Trinity House. When in thick weather the ordinary lights fail, an attempt is made to replace them with sound signals. These are found to vary much in their action, sometimes being heard to a very great distance, and at other times failing to make themselves audible even at a moderate distance. Two explanations have been suggested, depending upon acoustic refraction and acoustic reflection.

Under the influence of variations of temperature refraction occurs in the atmosphere. For example, sound travels more quickly in warm than in cold air. If, as often happens, it is colder above, the upper part of the sound wave tends to lag behind, and the wave is liable to be tilted upwards and so to be carried over the head of the would-be observer on the surface of the ground. This explanation of acoustic refraction by variation of temperature was given by Prof. Osborne Reynolds. As Sir G. Stokes showed, refraction is also caused by wind. The difference between refraction by wind and by temperature variations is that in one case everything turns upon the direction in which the sound is going, while in the second case this consideration is immaterial. The sound is heard by an observer down wind, and not so well by an observer up wind. The explanation by refraction of the frequent failure of sound signals was that adopted by Prof. Henry in America, a distinguished worker upon this subject. Tyndall's investigations, however, led him to favour another explanation. His view was that sound was actually reflected by atmospheric irregularities. He observed, what appears to be amply sufficient to establish his case, that prolonged signals from fog sirens give rise to echoes audible after the signal has stopped. This echo was heard from the air over the sea, and lasted in many cases a long time, up to 15 seconds. There seems here no alternative but to suppose that reflection must have occurred internally in the atmosphere. In some cases the explanation of the occasional diminished penetration of sound seems to be rather by refraction, and in others by reflection.

Tyndall proved that a single layer of hot air is sufficient to cause reflection, and I propose to repeat his experiment. The source of sound, a toy reed, is placed at one end of one metallic tube, and a sensitive flame at one end of a second. The opposite ends of these tubes are placed near each other, but in a position which does not permit the sound waves issuing from the one to enter the other directly. Accordingly the flame shows no response. If, however, a pane of glass be held suitably, the waves are reflected back and the flame is excited. Tyndall's experiment consists in the demonstration that a flat gas flame is competent to act the part of a reflector. When I hold the gas flame in the proper position, the percipient flame flares; when the flat flame is removed or held at an unsuitable angle, there is almost complete recovery.

It is true that in the atmosphere no such violent transitions of density can occur as are met with in a flame; but, on the other hand, the interruptions may be very numerous, as is indeed rendered probable by the phenomena of stellar scintillation.

The third portion of my subject must be treated very briefly. The guiding idea of much of Tyndall's work on atmospheric particles was the application of an intense illumination to render them evident. Fine particles of mastic, precipitated on admixture of varnish with a large quantity of water, had already been examined by Brücke. Chemically precipitated sulphur is convenient, and allows the influence of size to be watched as the particles grow. But the most interesting observations of Tyndall relate to precipitates in gases caused by the chemical action of the light itself. This may be illustrated by causing the concentrated rays of the electric lamp to pass through a flask containing vapour of peroxide of chlorine. Within a few seconds dense clouds are produced.

When the particles are very small in comparison with the wave length, the laws governing the dispersion of the light are simple. Tyndall pursued the investigation to the case where the particles have grown beyond the limit above indicated, and found that the polarisation of the dispersed light was effected in a peculiar and interesting manner.

Atmospheric dust, especially in London, is largely organic. If, following Tyndall, we hold a spirit lamp under the track of the light from the electric lamp, the dark spaces, resulting from the combustion of the dust, have all the appearance of smoke.

In confined and undisturbed spaces the dust settles out. I have here a large flask which has been closed for some days. If I hold it to the lamp, the track of the light, plainly visible before entering and after leaving the flask, is there interrupted. This, it will be evident, is a matter of considerable importance in connection with organic germs.

The question of the spontaneous generation of life occupied Tyndall for several years. He brought to bear upon it untiring perseverance and refined experimental skill, and his results are those now generally accepted. Guarding himself from too absolute statements as to other times and other conditions, he concluded that under the circumstances of our experiments life is always founded upon life. The putrefaction of vegetable and animal infusions, even when initially sterilised, is to be attributed to the intrusion of organic germs from the atmosphere.

The universal presence of such germs is often regarded as a hypothesis difficult of acceptance. It may be illustrated by an experiment from the inorganic world. I have here, and can project upon the screen, glass pots, each containing a shallow layer of a supersaturated solution of sulphate of soda. Protected by glass covers, they have stood without crystallising for forty-eight hours. But if I remove the cover, a few seconds or minutes will see the crystallisation commence. It has begun, and long needles are invading the field of view. Here it must be understood that, with a few exceptions, the crystalline germ required to start the action must be of the same nature as the dissolved salt; and the conclusion is that small crystals of sulphate of soda are universally present in the atmosphere.

I have now completed my task. With more or less success I have laid before you the substance of some of Tyndall's contributions to knowledge. What I could not hope to recall was the brilliant and often poetic exposition by which his vivid imagination illumined the dry facts of science. Some reminiscence of this may still be recovered by the reader of his treatises and memoirs; but much survives only as an influence exerted upon the minds of his contemporaries, and manifested in subsequent advances due to his inspiration.

[RAYLEIGH.]

GENERAL MONTHLY MEETING,

Monday, April 2, 1894.

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

Sydney Walter Barnaby, Esq.
Montagu Guest, Esq.
Hugh Cecil Robinson, Esq.
Miss Ethelwyn Simpson,

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to Mrs. Barton for her present of a Portrait of the late Dr. John Peter Gassiot, *M.R.I.*

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

Professor Dewar	£50
Hugh Leonard, 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.:—

FROM

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Catalogue of Vases, Vol. II. 8vo. 1893.
Catalogue of Fans and Fan Leaves. 8vo. 1893.
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      No. 3, Grands et petits voyages of De Bry. 4to. 1884.
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  Author for March, 1894.
  Brewers' Journal for March, 1894. 4to.
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  Electrical Engineer for March, 1894. fol.
  Electrical Engineering for March, 1894.
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  Scots Magazine for Dec. 1893 to March, 1894. 8vo.
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WEEKLY EVENING MEETING,

Friday, April 6, 1894.

Sir Richard Webster, G.C.M.G. M.P. Q.C. LL.D. Vice-President, in the Chair.

Professor Victor Horsley, M.B. B.S. F.R.C.S. F.R.S. M.R.I.

The Destructive Effects of Projectiles.

The effects of small projectiles when driven at high velocity through the tissues of the brain have always excited the deepest interest, for very obvious reasons.

This interest must always be two-sided, namely: (1) Physical; (2) Pathological; and it is upon these two points of view that I propose to speak to you this evening.

Conceive a cylindrical bullet with a conical head flying through the air some ten or fifteen times faster than an express train.

We have now to study what it is doing in its aerial flight, and what will happen when that terminates by the projectile striking both hard and soft substances.

This embodies matter for the purely physical side of the work.

But imagine, further, that the hard and soft substances just mentioned are the skull and brain respectively, what will happen then?

This is the pathological part of the question, and it is one of the greatest moment; for whereas it is true that a few persons do survive being shot in the head, the large majority die; and it is my object to show you how a combination of physical and pathological experiments has revealed the reason why the majority do die, and revealed it, fortunately, so distinctly as to suggest means for warding off the fatal result.

1. *Physical Considerations.*—First take the case of a bullet flying through the atmosphere. Here in this extremely beautiful photograph, kindly lent me by Professor Boys, you observe that the bullet drives before it a wave of compressed air. Now this compressed air-wave is what is popularly called the wind of the shot, and to it used to be ascribed by military surgeons a certain proportion of deaths. The origin of this theory is difficult to discover, as the only case I am aware of in which the post-mortem examination did not reveal hæmorrhage, fracture, &c., indicating that the shot had actually struck the body (though without injuring the highly elastic skin), is the instance given by the great Russian military surgeon, Pirogoff, in

his interesting surgical experiences of the Crimean war. Even this instance finds *à priori* a more reasonable explanation in syncope, and we shall see directly that the wind of the shot not only cannot under any circumstances kill a man, but also that its energy is far too slight for it to have any destructive effect whatever. It is rather curious to find that but few attempts have been made directly to estimate the wind of the shot, and those by Pelikan and others are only for large shot and by too coarse methods to be applicable in the case of a bullet, as the following experiment shows.

An extremely light vane of paper carrying a delicate mirror is suspended to a cocoon fibre, and carefully protected from currents of air in the room. A very gentle puff causes the vane to fly out most vigorously, yet we shall find that the ·380 bullet moving 1000 feet a second may pass within 8 inches of it without causing the least deviation of a ray of light reflected from the mirror. It is only when the bullet passes within an inch or two of the edge of the vane that there is some slight rotation. The ·303 magazine service rifle, with a velocity of twice that of the larger bullet, produces little more than the same result. It is therefore obvious in this case that the far higher velocity is more than compensated for by the lesser sectional area of the projectile displacing the air. Although there was no proof of much displacement of the air, it was pretty generally held that when the bullet entered any substance the compressed air driven before it exercised an explosive effect. This opinion was more particularly supported by the Belgian physicist Melsens, who actually described it by the term "projectile air." The matter was taken up from the point of view of pure physics, and Magnus demonstrated that if a body like a bullet entered water, e.g. in falling the funnel which the displaced water makes in the axis of the body as soon as that is fully immersed, entangles air, and that it is this air which is carried by the body into the fluid, rather than that any air is forced in in front of the bullet. In answer to Magnus, Laroque invented the following ingenious experiment. He allowed a long body, incapable of wholly sinking, to drop into the water, and then found that there was air driven in in front of it; while, by the nature of the experiment, he had, of course, excluded the possibility of any air following the base of the projectile. I have repeated all these experiments (employing in Laroque's a slender rod of wood) and found that while his contention that air is driven in front of the bullet is completely substantiated, yet Magnus' observation is so far correct that air is also drawn in after it, the fact being that the two conditions are not opposed but simultaneous. Magnus' view was further supported by the adverse criticism of the theory of projectile air of the celebrated French artillerist Morin, which criticism amounted to this, that when a projectile was directed against a *solid* body it must necessarily follow that so elastic a substance as air should be completely reflected from the surface. I should like to draw your attention to this word *solid*, because I believe that in that we find the key to the difficulty, and the apparent paradoxes presented to us are to be explained by the fact that the results are wholly dependent upon the simple question of the relative viscosities of the substances entered. To solve this I employed the same falling bodies, and examined their entanglement of air in water and glycerine respectively, and found that whereas, in the case of water, Laroque's non-floating rod drove air in front of it as well as probably at the side, yet when the same rod was caused to fall into glycerine of high concentration there was no air in front, but air-bubbles could be seen clinging to the sides of the rod. Further, in glycerine the entanglement of air in the funnel formed by the base of the bullet, as described by Magnus, was very striking. It appeared to me that whatever air was driven in front of it was wholly reflected by the sufficiently viscous fluid, and hence it must be, *à fortiori*, still more completely reflected from the surfaces of hard and soft solids like the skull and brain respectively.

To sum up, the so-called projectile air can have no real bursting effect, since, as I have demonstrated, in the first place it exerts very feeble pressure, as tested on a delicate vane, and in the second place it is certainly easily reflected from surfaces of but moderate density.

The Influence of Rotation produced by Rifling.—It is commonly thought that the spin of the bullet communicated to it by the rifling of the barrel, and which is very great, causes a considerable amount of the disturbance created in the interior of moist substances, which is usually spoken of as the bursting or explosive effect. Kocher thought that this would not be appreciable, and that the rotatory movement would only cause the displaced particles to take a course tangential to the surface of the bullet rather than perpendicular. Although the smooth surface of the bullet of course adds force to the idea that its rotation is not very effective, it is obviously a matter of both interest and importance that the matter should be more closely studied. Colonel Henrad made plaster casts of the tracks of shots, and obtained distinct spiral markings indicative of the rotation in question. Acting on this suggestion, it was easy to institute a series of experiments of the following kind. Pure modelling clay of firm consistence (for the influence of the water present *vide infra*) was rendered homogeneous by kneading, shaped into square blocks of varying length, and supported in a hard flat surface or in a box, the ends being open. The cavity made by the bullet in entering and traversing the mass was then filled with liquid plaster-of-paris, and a cast obtained. Examples of such casts are before you, and they completely display the rotation in question.

The first point which has to be borne in mind is the relation of the rotation to the projection or forward movement of the bullet. In passing through a body of little resistance like the air, it is clear that for every given unit of distance travelled, the displacement evoked by the rotation must be something very small, because although the bullet turns one and a half times in traversing the barrel, that is nearly a yard in length, consequently, so far as the rotation is concerned, that for a unit, say one inch, of the flight of the bullet would be extremely small, namely about one-twentieth of the circumference of the bullet, which roughly speaking, would be (for the ·380 bullet) about one-twentieth of an inch, the insignificance of which is obvious. The matter, however, assumes a somewhat different aspect when a bullet is engaged in a solid substance through which it is forcing its way with rapidly diminishing velocity. In such a case, where the projection journey of the projectile is quickly coming to an end, it becomes of special importance to see what is becoming of the factor of rotation. The plaster casts obtained in the manner indicated show clearly enough the interesting fact that the rotation persists to the end, when the bullet has simply taken its course through the atmosphere and then entered the soft clay. Further, the casts also show what is a necessary deduction from our earlier considerations on this matter, namely, that as the rotation is preserved till the end of the trajectory, the twist is proportionately more pronounced as the forward movement is lost.

It is for our present purpose important to see whether the rotation is well marked when the projectile is completely deformed. To examine this point a new series of experiments was undertaken, in which the bullet was first caused to penetrate a flat bone before entering the clay. It is very clear that the rotation is still present. In discussing this question I have left unnoticed the fact that owing to the resistance of a body like clay, the cohesiveness of which of necessity varies slightly from point to point, there will be a great tendency for the bullet to change its direction, more especially as the base is heavier than the apex, and to this change of direction must be attributed in part the change of surface simulating the rotation effects due to the rifling. The two conditions, however, can be distinguished readily on careful examination.

So far as destructive effects in the brain are concerned, it is therefore clear that relatively little is to be ascribed to rotation.

Projection Destructive Effects.—The destruction by the bullet moving forward through a solid body is the most important matter for us to consider. There are two sets of factors determining the degree of destruction in any given substance.

1. Factors due to the bullet.
2. Factors due to the *physical constitution* of the solid.

1. Factors due to the bullet. So far as the projectile is concerned, the chief considerations are (*a*) its momentum; (*b*) its sectional area; (*c*) its becoming heated.

(*a*) *Momentum.*—Although it will of course be generally understood the greater the velocity the greater the damage for the same weight of shot, still, in connection with the small-bore service rifles of the present day, some seem to think that the small bullet, by virtue of its travelling at a great pace, would pierce the tissues without causing much general damage. The fallacy involved in this belief we shall see directly; but a single glance at the casts arranged in order of the velocities of the bullets, shows immediately the unreality of the notion. In every case the particles of the substance are hurried forward (particularly evident in the casts before mentioned) in front of the bullet, and thus by increasing the size of the moving mass such particles practically constitute a larger projectile. Much destruction is due to this, as Delorme has more particularly demonstrated in the well-known case of firing a bullet into a book, wherein one may see the laceration of the pages successively increased, although the momentum of the bullet is steadily diminishing, and in proportion to the increasing laceration so discs of increasing diameter are found in the cavity, having been cut from the preceding pages. The hurrying forward of the particles is very beautifully shown by Professor Boys in his photographs of the débris of glass plates after a bullet has passed through them. In one case a large fragment of glass is shown to be moving parallel to the bullet, *i.e.* with the same velocity. This question of accessory damage is of much importance to the pathological problem how much damage is effected in the brain. I have found discs of bone forced through the brain, such discs (as will appear directly) being larger than the projectile itself. Small fragments are also hurried forward with the same velocity as the bullet, as these casts show, the plaster method thus confirming Professor Boys' photographic record.

(*b*) *Sectional Area.*—From what has just been said, it is plain that the crushing effect of the bullet will be greatly increased if its diameter is enlarged; and it is understood that this was the reason why the Duke of Wellington opposed the introduction of the smaller bore weapon for the old musket called "Brown Bess." But few words, therefore, are requisite in dealing with this point. I wish, however, to draw attention to an extremely common result of the employment of leaden bullets, and a result which is wholly dependent on the principle just enunciated. In the photograph of the penetration of an iron plate by the magazine rifle bullet, it will be noticed that the diameter of the holes is almost twice that of the bullet as it leaves the muzzle of the rifle. When the bullet is picked up, however, after it has passed through the plate, the reason of this seeming absurdity is at once recognised, for the bullet is compressed into a hard mass of lead and nickel by its first impact on the front of the plate, of the size of the hole shown. It is important, therefore, for the military surgeon to consider what proportion of the damage is due to deformation of the projectile on its striking the body, but the sectional area demands very little attention when compared to the velocity as a source of destruction.

(*c*) *Heating.*—The notion that a bullet produced some of its destructive effects in consequence of its being raised in temperature, as a natural result of some of its momentum being converted into heat, has always been before scientists ever since the invention of firearms, and endless have been the suggestions put forward to support this idea. I am not going to waste your time on the matter, because, in spite of the plausible papers of Hagenbach and Socin, there are certain facts plain and simple enough which, to my mind, completely dispose of the notion put forward by those authors, namely, that the bullet undergoing deformation on striking a hard substance like bone becomes heated so intensely that it partly fuses. The simplest observation of all is that, I think, made by Von Beck, and which I have often confirmed, namely that a bullet, though completely deformed by impact, may enclose a hair or piece of wood without these being in the least degree altered by heat; while as for its being heated in the barrel, &c., that cannot amount to 40 C., for Messner has shown that a bullet traversing dirty clothing carries with it living microbes, and deposits them in the object it strikes, still in a living state, so that they grow therein if the soil is a suitable one; and these observations have been fully confirmed by Delorme and Laveran. It is to be hoped that we have heard the last of this unquestionably exaggerated idea of the heating of a bullet.

2. Factors due to the physical constitution of the solid.

We now enter upon the discussion of the most interesting of all the physical considerations determining the well-known bursting effect which a bullet produces on certain substances, e.g. clay, brain, &c., while simply perforating others, e.g. wood, iron, &c. The reason why a bullet behaves apparently quite differently when it is forcing its way through solids of different kinds, has been, as a matter of fact, answered ever since 1848, when Huguier made some remarkable, but little known, researches on the effects of bullets on soft tissues, after he had observed the results of the wounds inflicted in the fighting in Paris in 1848. It will be remembered that in that struggle, as in others, the appearance of bursting within the tissues was very noteworthy, and gave rise to the notion of explosive bullets having been employed by the combatants contrary to the received opinions of international comity. The whole question is a perfectly simple matter, and resolves itself merely into the proposition that destructive effects vary in direct proportion to the cohesiveness, i.e. the fluidity of the particles composing the body. Ever since the observations of Tresca, Roberts-Austen and others, we have been made familiar with the phenomenon of the flow of metals when these are subjected to powerful pressure, and the mode of the displacement of the particles has always been compared to the displacement observed in viscous fluids. The extreme case in which fluidity is least present is that of the substances which we term brittle. In these, while much pulverisation occurs, the displacement of particles laterally is very slightly marked. Contrast the penetration of an example of this class, namely a flat, thin bone, with the effect produced on a more or less plastic solid like brain, and a striking difference presents itself, for whereas the bone is simply penetrated in the long axis of the bullet, the brain is thrown aside in every direction. Huguier made observations on certain dead organs, e.g. lung, liver, &c., and suggested that the reason why there was so much lateral disturbance was that the tissues contained water in large quantity and that the energy of the moving projectile being imparted to the particles of water, caused the dispersion of these in a hydrodynamic fashion. Kocher, in 1874 to 1876, was the first who thoroughly dealt with this question in the manner shadowed forth by Huguier, and he proved, in a series of interesting experiments, which Dr. Kramer and myself have fully confirmed, that the effect is really a hydrodynamic one. One of the simplest of his observations you see before you, and is made as follows:—Two tin canisters are taken of precisely the same size and strength, and are filled with equal quantities of lint; but in the one case the lint is dry, in the other saturated with water. When a bullet of moderate velocity is fired through these canisters, it simply perforates the dry one, but causes the wet one to burst explosively. It is, however, not a simple question in dealing with these artificial schemata merely to provide a porous substance the cavities of which are filled with water, for I have found that if the intervening septa are strong, as, for instance, in the case of sponge, that the bursting effect is not so great. In fact, the water must be thoroughly incorporated with the substance, or, to speak more correctly, the substance must be more perfectly fluid. This can be easily demonstrated by taking dough containing different percentages of water, and since dough is a substance in which the incorporation of the water is very complete, it affords a particularly good example to employ. By firing bullets of precisely the same velocity through these samples, you see that the destruction is effected strictly proportionally to the fluidity of each specimen. This is the reason why it is really of no absolute value to make experiments on dead tissues, for the brain in a state of *rigor mortis* is practically a solid, since both its living protoplasm and blood in the blood-vessels have coagulated, whereas in the living condition the first is semi-fluid and the second quite fluid. It was to investigate this point, as well as the previous questions, that I have paid more especial attention to the proportionate relation existing between the velocity and the explosive effect. The results are very obvious in the casts before you. This work has been immensely facilitated by the kindness of Sir Andrew Noble, who caused to be constructed at my request a modification of a 22-calibre rifle, whereby I can fire a 40-grain bullet with any velocity I wish from a few hundred feet per second to over 3500 feet per second.

The casts show that the effect in the clay is proportional to (1) the velocity of the bullet, (2) the wetness of the clay.

The method of proof is so convincing I need not detain you further in this discussion.

Since the question is, we now see, all-important, it becomes a matter of no small moment to study the effect exerted by bullets entering fluid (for example, water). In the first place, as may be seen by these experiments, the effect of the perforation of a skull, filled with water, by a bullet, as was first done by Kocher, is to cause the bursting of the sutures. I would draw your attention to the fact that the separation of the bones is most marked on the side of the entry of the bullet. It was the observation of this latter point which led me to think that it might be possible to automatically record the disturbance of the fluid, and this was effected in the following way.

A long trough having been prepared, with one end closed with rubber one-eighth of an inch thick, and a tall, white, flat surface lowered vertically into the trough, the latter is filled with a solution of methylene blue. A small bullet of low velocity (600 feet per second) is fired in the long axis of the trough, 1 cm. below the surface of the water. As a result, a wave is thrown up against the white screen, which is consequently marked with a blue splash, the same describing a curve indicating, firstly, that the disturbance is greatest where the velocity and resistance, increased by compression, are both at their highest, i.e. soon after the bullet enters the fluid; and secondly, that the displacement diminishes gradually as the momentum lessens.

Complete confirmation of the parallelism between soft solids and fluids in their behaviour to the rapidly-moving bullet, is seen in comparing the cast of the track made by a bullet moving through clay with the curves obtained by the water record.

In both the maximal displacement occurs shortly after the bullet has entered the substance, and in both the diminution of disturbance is much more gradual than its development, and is evidently proportional in the main to the loss of momentum. A final proof is afforded by suspending columns of methylene blue in clear water or salt solution, and then firing through the whole. With the ·380 bullet and three grains of smokeless powder the last column in the 4-foot trough was not disturbed.

This, doubtless, is a result which would be generally foreseen, but it was worth while to test it experimentally, and it certainly very strikingly demonstrates how localised the bursting disturbance is, which completely explains the limitation of the explosive effect on the skull on the side of entry. Sundry interesting subordinate points arose in the course of these experiments, and have served to afford the necessary control of the method, e.g. the peculiar splash of the bullet striking the rubber end of the trough alone, i.e. not penetrating; and, again, the tracing made by a bullet which, being fired a little too superficially, records the elevation of successive waves as it ricochets along the surface; and, finally, the record of a bullet deflected by the resistance of the water (as well as by want of horizontality, showing a long, oblique splash where it has carried up the fluid into the air.

From all these experiments on the pure physics of this subject we are justified in believing that when a bullet is fired against the head (whether that of a man or any other warm-blooded animal), so as to penetrate the cerebral hemispheres in a transverse direction, the following series of phenomena occurs. The impact of the bullet on the bone causes depression of that bone over an area larger than the diameter of the uninjured bullet, this causing a slight rise of tension in the skull, since that cavity is completely filled with fluid, e.g. the cerebro-spinal fluid and blood in the blood-vessels, together with the living brain, which, as has already been stated, is a semi-viscous substance. In the next instant the bullet enters the cavity, and the slight rise of tension is instantly converted by the universal displacement (explosive effect) of the contents, into a very severe rise of pressure, most marked on the side of entry. The lines of force which this pressure takes are shown in this diagram, and it will be obvious to you that these forces will, on meeting the rigid skull, tend, as I have already shown, to burst it, and if they fail in that, then they will certainly be reflected on to the brain, a matter, as we shall presently see, of special pathological significance. As you see from the diagram, the brain substance must be driven against the internal surface of the globular cranium. This driving of the brain against the hard bone is exemplified in every post-mortem examination. A good instance is seen in the accompanying specimen, in which, although the bullet traversed the extreme tips of the frontal lobe and the olfactory bulbs, numerous bruises are seen on the hinder portions, where they have been crushed against the bone. Similarly evidences of the direct transmission of the pressures are to be found at the base of the brain in the longitudinal fissure, &c., wherever, in short, the brain can be pressed against an unyielding substance. The final proof of the correctness of this interpretation is to be referred to directly, in which the vault of the cranium is removed before the shot, so that the energy of the pressure is expended in ejecting portions of the brain into the air, and not so much on the basal regions as just described. So, too, the energy of the bullet is communicated in the same way to the fluid in the ventricular cavities (Duret's "choc cephalo-rachidien"), which tunnel the brain down to the medulla oblongata. The medulla oblongata is thus subjected to pressure from two sources: (1) the hydrodynamic displacement of the brain *en masse*; (2) the direct crushing effect due to the movement of the cerebro-spinal fluid in the ventricles.

We are now brought to the aim and object of these preliminary considerations, namely, the reason why these disturbances within the skull cause death, and how the fatal issue is produced: in short, we must pass from the questions of pure physics to the more complex problems of pathology.

2. *Pathological Considerations.*—The experiments, the results of which I now wish to lay before you, constitute a long series which was carried out last year by Dr. Kramer and myself. We arranged the experiments as follows: A dog was placed under ether, and one femoral artery connected with a mercurial manometer to give record of the heart beats and pressure of the blood in the trunk arteries of the circulatory system. Another similar manometer was connected with the peripheral end of an artery so as to record the changes of

pressure in the small capillary vessels, changes which I may remark incidentally are usually due to those disturbances in the central nervous system which we call vaso-motor. Thirdly, the movements of respiration are traced on the recording surface by means of rubber tambours, known as Bert's and Marey's respectively. If the pressure within the skull was also to be recorded, then a steel tube filled with the salt solution was fixed into a trephine opening, and connected by a rubber air-tube also with a Marey's tambour. Occasionally we put on the same paper a record of the contraction of the rectus femoris muscle, this latter being directly connected with a Fick's spring myograph. At the bottom of the tracing is given, firstly, the record of the movements of an electro-magnet signal (Smith's) interrupted by a metronome beating seconds. The last line traced is that from a Smith's signal in the circuit of a single cell, and one of the wires from which is made of very slender brass and fixed across the muzzle of the pistol or rifle, so that when the shot leaves the muzzle it cuts it and breaks the contact (Woolwich method).

When a bullet of low velocity (600 f.p.s.) strikes the skull in a glancing fashion, there is only a trifling disturbance of respiration, but when the bullet enters the cranial cavity and sets up the powerful hydrodynamic pressure before referred to, a very severe effect is produced, namely complete arrest of the respiration and a slight fall of the central blood pressure, this causing a similar feeble fall in the peripheral blood pressure. A little later (5–10 secs.) than the arrest of respiration a remarkable rise in the blood pressure occurs, this rise continuing until the normal tension is exceeded. These observations prove beyond doubt that the first cause of death is not what it is usually supposed to be, and as taught in the text-books, namely arrest of the heart and syncope, since, as you see, the heart goes on beating although the respiration has completely stopped. Furthermore, if we quickly perform artificial respiration we obtain recovery from the otherwise fatal arrest.

This suggests very strongly that the police and persons who are trained in giving the first aid to the wounded should be taught that with a gun-shot wound of the cerebral hemispheres, the proper thing to do is to employ artificial respiration rather than the giving of stimulants, &c. But, as you may well expect, the matter does not stop here, nor is it so very simple, because we find that there are certain conditions under which the secondary rise of blood pressure does not occur.

It is now quite evident that the fatal phenomenon of the gun-shot wound of the cerebral hemisphere is in the first instance cessation of the breathing, and I have now to indicate in detail how this is produced by the hydrodynamic disturbance evoked within the skull cavity by the energy of the bullet. It is perhaps necessary to first remind you that the upper part of the spinal cord or medulla oblongata contains the chief centre for the movements of respiration.

I would also draw your attention to the fact that therein is also the centre of origin of the vagus nerve, which nerve has the power of slowing the heart. Thus there are two important centres in the medulla which are liable to be affected by changes of tension around them induced, as above stated, when the bullet traverses the cerebral hemispheres in a transverse direction. It may be that the centres are principally affected by the mechanical pressure of the explosive effect, but this latter of necessity produces a certain amount of anæmia of the nerve centres; some of the effect may also be produced by that condition too. Supposing that the artificial respiration has been properly carried out, and the respiratory centre is revived into activity, there is yet another condition to be overcome, without which the animal or person dies, and for a long recognised reason, namely, that the bullet having in its passage cut through various blood-vessels, blood is poured out within the skull, and consequently raises very severely the intra-cranial tension. This constitutes, as a matter of fact, a second cause of death, for under these circumstances the accumulated blood causes such severe compression that it not only again paralyses the respiratory centre but also irritates the vagus centre, causing a marked slowing of the heart. The proof of the truth of this statement is given at once the moment we cut the vagi nerves, for if these are divided the heart immediately resumes its former rhythm. The next curves are to exhibit the increase in the intra-cranial tension, which occurs the moment the bullet enters the skull. The line drawn by the Marey's tambour shows a violent increase of pressure at the moment of shot (first or explosive effect) and a certain recoil therefrom, this recoil being directly changed for a steady increase in tension brought about by the secondary cause of death, namely, the hæmorrhage, of which I have already spoken. To treat such hæmorrhage only ordinary surgical measures are requisite, but these will be impossible if the activity of the respiratory centre has not previously been restored in the manner already indicated.

To sum up, the basis of scientific discussion of the nature and causation of the phenomena evoked by bullet wounds of the cerebral hemispheres must rest on two principal factors—the velocity of the projectile and the development of hydrodynamic movement in the wet living tissues.

I am glad to have had the opportunity of laying before you the facts on a subject which combines the pleasure of pure physical research with the interest inseparable from the resolution of pathological problems.

[V. H.]

WEEKLY EVENING MEETING,

Friday, April 13, 1894.

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

Professor J. J. Thomson, M.A. Sc.D. F.R.S.

Electric Discharge through Gases.

One of the most important and interesting branches of physical science is that which deals with the connection between electrical and chemical effects.

The investigations on electrolysis made within these walls by Davy and Faraday proved that the important class of electrical phenomena associated with the passage of electricity through liquids, are connected in the closest way with chemical action. They proved that no electricity will pass through most liquids unless chemical action occurs, and that for each unit of electricity which passes through the liquid there is a definite amount of chemical decomposition.

This case, though it is one where the laws are most accurately known, is but one among many electrical phenomena which are inseparable from chemical action.

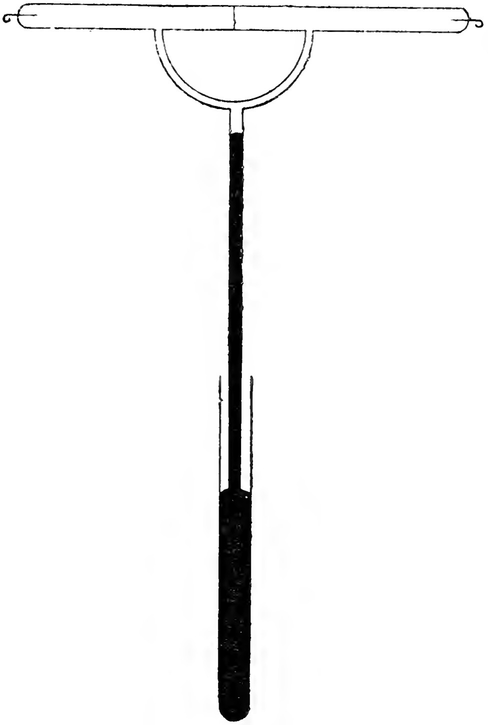
So many instances of this kind have been discovered that we may perhaps venture to hope that we are not far from the time when it will be universally recognised that many of the most fundamental questions in chemistry and electricity are but different aspects of one and the same phenomenon.

Anything which throws light on the connection between electricity and matter, interesting as it is on its own account, acquires additional interest when regarded as elucidating the connection between chemical and electrical effects, and no phenomena seem more suitable for this purpose than those which are the subject of the discourse this evening—the discharge of electricity through gases. For in gases we have matter in the state in which its properties have been most carefully studied, while the investigation of the electrical effects is facilitated by the visibility of the discharge, affording us ocular, and not merely circumstantial, evidence of what is taking place.

The points to which I wish to refer particularly this evening are, firstly, some phenomena connected with the passage of electricity from the gas to the electrode, or from the electrode to the gas; and secondly, some of the properties of the discharge when its course lies entirely in the gas.

By taking a long discharge tube, say, one 50 feet long, and observing the luminous discharge through a rotating mirror, we can trace the course of the luminosity due to a single discharge, say, one due to once breaking the primary circuit of an induction coil; if we do so we find that the luminosity follows the direction of the positive current through the tube. That is, the luminosity begins at the positive electrode, it then rushes down the tube with enormous velocity, but when it gets to the negative electrode, it receives a check; it does not disappear at once in that electrode like a rabbit going down a hole, but lingers around the electrode some time before entering it. In consequence of this delay in the positive discharge in getting out of the gas, there is an accumulation of positive electricity in the neighbourhood of the negative electrode until the potential fall at this electrode increases to about 200 or 300 volts.

Fig. 1.



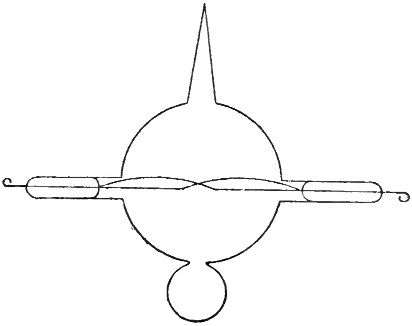
The positive electricity which accompanies the discharge thus finds considerable difficulty in getting from the gas to the metal, though, as I hope to show you later on, as long as it keeps in the gas, it meets with what we may, in consideration of the views sometimes enunciated on this subject, call a ridiculously small amount of resistance, its real difficulty is to get out of the gas.

Though this effect has long been known, it is so important that I will venture to show one or two experiments which illustrate it. The arrangement of the first experiment is shown on the screen, the apparatus consists of a main discharge tube, across which is fastened a diaphragm made of excessively thin platinum leaf; there is a side passage from the tube, leading from one side of the diaphragm to the other, this is connected to a barometer tube, and by raising the cistern containing the mercury I can stop up the passage by a pellet of mercury. We will first observe the discharge when the side passage is open; you see that the discharge, instead of passing across the thin piece of platinum leaf, takes the very much longer route round the side tube, so as to avoid crossing the metal. We will now raise the mercury cistern and close the side tube by a pellet of mercury; the discharge now has no alternative but to cross the metal at some part of its course, and you see that the main portion of the discharge goes back into the main tube.

In the second experiment the metal diaphragm is replaced by a very thin plate of mica; when the side passage is opened the discharge goes round, but when this is closed by a pellet of mercury the discharge prefers to go across the mica than through the mercury.

A second experiment which shows the same thing is the following. Two long electrodes are fused into a bulb, so that the tip of an electrode is a considerable distance from the place where it passes through the glass. We will now send an alternating discharge through the tube, and you will see, I think, that the discharge, instead of going straight across the short distance between the ends of the electrodes, goes from the tip of one electrode to the place where the other passes through the glass, thus staying as long as possible in the gas before passing into the metal. The appearance of the discharge shows that the positive electrode is at the end of the wire, the negative at the junction of the wire with the glass.

Fig. 2.



Another interesting example of the difficulty the discharge experiences in passing from gas to metal is the discovery made by Professors Liveing and Dewar, that when the discharge passes through a gas containing a large quantity of metallic dust, the light from the discharge, when examined in the spectroscope, does not show any of the lines of the metal.

The difficulty which the positive electricity finds in passing from the gas to the electrode depends a great deal upon the nature of the gas, as well as upon that of the electrode; it is influenced by the position of the gas and the electrode relatively to one another in the electro-chemical series.

I have lately made a series of experiments on this point in the following way. An alternating discharge from a high tension transformer was made to pass between two electrodes fused into a bulb, which could be filled with the gases under examination. Another electrode connected to an electrometer, passed into the bulb, and was arranged so that it could be moved about from one part of it to the other. When the electrodes were metal and the bulb was filled with the electro-negative gas oxygen, the electrode received a positive charge in whatever part of the bulb it was situated; if now the bulb was filled with hydrogen at atmospheric pressure, then in the regions remote from the arc the electrode received a positive charge, but in the immediate neighbourhood of the arc itself it received a negative charge. When the pressure was reduced the region in which the charge was negative contracted, and finally at pressures about one-third of an atmosphere, seemed to disappear, and the electrode got a slight positive charge in whatever position it was placed. If now, instead of using metallic electrodes we use well-oxidised copper ones, and repeat the experiment in hydrogen, working at a pressure when there was only positive electricity when the electrodes were bright and polished, we find that with the oxidised electrodes every particle of positive electricity is taken out of the tube, and a negative charge is left. This negative charge remains until the copper oxide is completely reduced; when this occurs the negative charge disappears, and is replaced by positive. Thus, under the same conditions as to the nature of the gas and the pressure, the bright copper electrodes leave a positive charge in the gas, while the oxidised ones leave a negative charge.

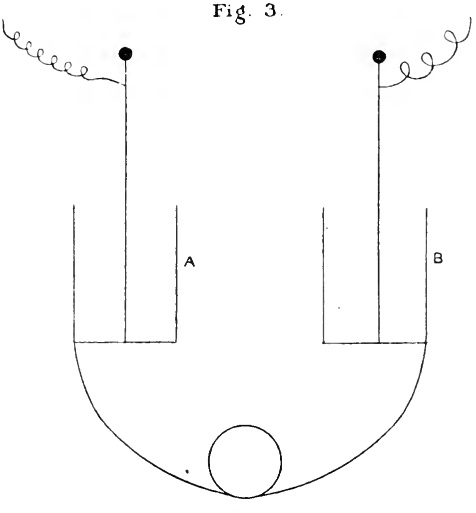
The most probable explanation of these results seems to me to be the view that the communication of electricity from gas to the electrode, or from the electrode to gas, is facilitated by the temporary formation of something of the nature of a chemical compound between the gas and the metal. In all such compounds the metal is the electro-positive element, and has the positive charge, the gas being the electro-negative and carrying the negative charge. Now consider the case when the negative charge is on the gas, and the positive charge on the metal; then the gas and metal have got the charges proper to them in any compound they may form, and are thus in a fit state to combine, or, according to this view, allow the negative electricity to pass from the gas to the copper. But, now, suppose the gas was positively electrified, the gas and the metal have now opposite charges to those proper to them in a compound, and before the union of gas and metal in this state could result in anything but a most unstable compound, an additional process must be gone through—i. e. the charges on the gas and metal must be interchanged. Thus the conditions for the combination of the gas and metal are more complex when the gas is positively electrified than when it is negatively electrified, and thus, on the view that the communication of electricity between the gas and the metal involves a sort of chemical combination, we see that the negative electricity will escape more easily from the gas to the metal than the positive. Now consider the case when the gas was hydrogen, the electrodes oxidised copper; the hydrogen combines now not with the metal, but with the oxygen, forming water, in which hydrogen is the electropositive element; thus, in this case, it is the positively charged hydrogen which is in the state best fitted for pairing. The consequence is, the positive charge would be most readily removed from the gas and the negative left—exactly the opposite to that which occurred when the electrodes were bright. This reversal, as I stated before, is verified by experiment.

I have hitherto only spoken of the phenomena which accompany the passage of electricity from the electrode to the gas, or from the gas to the electrode.

I shall now pass on to consider the properties of the discharge when it is entirely confined to the gas.

We may produce a discharge which, during the whole of its course, shall be confined to the gas in the way represented in the diagram.

Fig. 3.



The two poles of a Wimshurst machine are connected to the insides of two jars A and B, while the outsides of these jars are connected together by a metal wire wound so as to form a coil. The electricity from the Wimshurst machine charges up the jars, the difference of potential between the poles increases until a spark passes. The passage of the spark puts the insides of the two jars in connection, and the jars are discharged. The discharge of the jar, as was proved from the theory of electro-magnet action by Lord Kelvin more than forty years ago, and shortly afterwards confirmed by the experiments of Feddersen, is an oscillatory one, producing currents surging backwards and forwards through the wires with extraordinary rapidity. The subject of these oscillatory currents is one which in this year is tinged with melancholy. The year had hardly commenced when we lost Hertz, whose splendid work on these electrical oscillations is known to you all. The Managers of this Institution have marked their sense of the importance of this work by devoting a special lecture this session to this work alone, and they have entrusted that lecture to a most distinguished worker in the same field as Hertz. It would therefore be presumptuous on my part to refer in any detail to Hertz's work, but no physicist, and least of all one who is a member of Maxwell's University, could pass over in silence the death of Hertz.

When Hertz began his magnificent experiments on electric oscillations, there were many theories of electrical action. When he had finished them there was only one, Clerk Maxwell's.

Hertz's work was done with very much quicker vibrations than those produced by the apparatus now on the screen; this, however, gives rise to currents through the coil changing their direction some million times a second. If we place in the coil an exhausted bulb the bulb in reality will be the secondary of an induction coil, and will be exposed to electromotive forces tending to produce circular currents parallel to the plane of the coil.

I will now place a bulb inside this coil, and you see that a circular ring discharge passes through it, and this discharge passes entirely in the gas.

The gas in the bulb now in the coil is the vapour of silicon tetrachloride; it happens to be the bulb which gives a brighter ring than any others I possess.

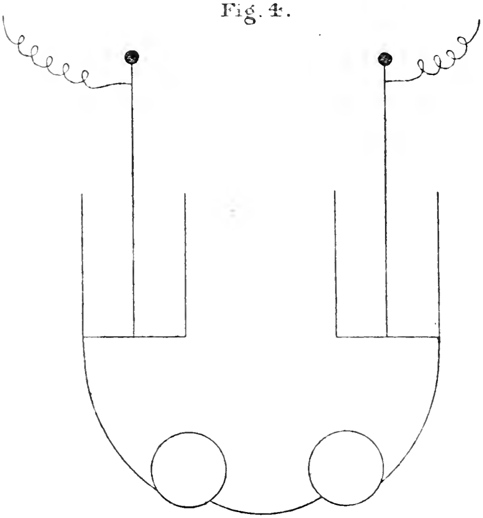
If this ring discharge passes through air at different pressures, the colour of the discharge changes very considerably. The first bulb I put in was at fairly high pressure, about $\frac{1}{10}$ of a millimetre or so. I will now put in another at a lower pressure, and then one at a still lower pressure. Mr. Newall, who has been working at the spectra of these discharges, finds that at the pressure in the first bulb the spectrum is due to nitrogen; at the second stage it is due to mercury vapour; the bulb was pumped by a mercury pump, so that there is in the bulb a certain quantity of mercury vapour.

The apple-green colour in the more highly exhausted bulb is due to some compound of sulphur, which has got into the bulb from the sulphuric acid used to dry the gas. Mr. Newall finds that if the ordinary discharge from a coil between electrodes is taken in such a bulb, there is no trace of this sulphur spectrum. He has also found that when the bulb is at a pressure intermediate between what I may call the mercury and the sulphur stage, when the mercury and sulphur lines are both visible, these sets of lines come from different layers, the sulphur lines coming from a layer nearer the surface than the other.

If we take the discharge through a bulb containing oxygen you see that the ring discharge is succeeded by a bright glow; at first the colour is somewhat opaque, but gradually gets more transparent and changes colour. This gives a continuous spectrum crossed by a few bright lines. If we take the discharge through cyanogen you see that the glow is even more persistent than the oxygen, though it is not so bright; all the gases which show this glow belong to the class of substances which polymerise—that is, whose molecules can combine with each other. I imagine that what takes place in bulbs filled with these substances is that the discharge produces a polymeric modification, and that this gradually returns to its original state, and while doing so gives out a phosphorescent light. It is in accordance with this that at a high temperature where ozone cannot exist a discharge through an oxygen bulb does not show any glow.

I said at the beginning of this discourse, that gases were exceedingly good conductors of electricity. I will now endeavour to show an experiment which proves that statement. The apparatus which I shall use for this purpose is a slight modification of the one I have used for producing the ring discharge; the only difference is that in the wire connecting the two coatings of the jars there are two loops instead of one. In one of these loops an exhausted bulb is placed to serve as a kind of galvanometer; the brightness of the discharge is an indication of the strength of the current flowing round the coil. If I place a second conductor in the other loop, currents will be started in it and part of the energy of the discharge will be absorbed; this will leave less energy available for the bulb in the first, so that the discharge in this bulb will be dimmer. The effect produced on the discharge will depend upon the conductivity of the substance placed in the second loop.

Fig. 4.

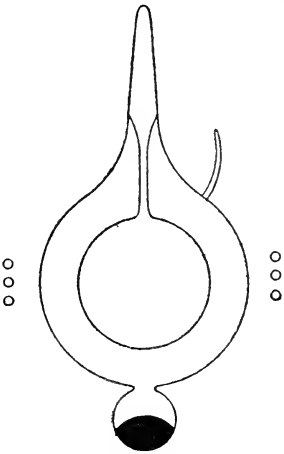


The effect is not directly proportional to the conductivity, in fact, a perfect conductor would not produce any diminution, nor would an absolute non-conductor; for a given period and with apparatus of given dimensions, there is a certain conductivity which gives a maximum effect; this follows easily from the theory of induction of currents, but at this late period in the evening I will take a shorter course and prove it by an experiment.

I put a piece of brass in this loop, and you see it produces but a small effect upon the brightness of the discharge. Instead of brass, I now insert a plumbago crucible, which, though a conductor, is not nearly so good a one as the brass, and you see the discharge in the indicating bulb is completely stopped.

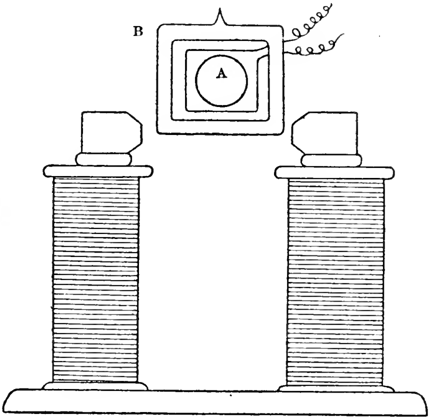
I will now place in the second loop an exhausted bulb; you see it produces a decided diminution in the intensity of the discharge in the galvanometer bulb. I now replace the bulb by another of the same size containing dilute sulphuric acid; you see it does not produce nearly so large an effect as the exhausted bulb: this might be due, as we have seen, to the sulphuric acid being either too good or too bad a conductor. I can show that it is the latter by putting a bulb in filled with a stronger solution, which has a higher conductivity than the weak solution; if the smallness of the effect produced by the weak acid were due to its being a better conductor than the gas, then increasing the conductivity would still further diminish the effect of the acid, you see, on the contrary, that the strong acid produces a distinctly greater effect than the weak, hence the rarefied gas in the bulb is a better conductor even than the strong electrolyte. Let us consider for a moment the molecular conductivities of the two substances, the rarefied gas and the electrolyte. The pressure of the gas is about $\frac{1}{100}$ of a millimetre, while in the electrolyte there are sufficient molecules of the acid to produce, if they were in the gaseous state, a pressure of more than 100 atmospheres; thus the conductivity of the gas estimated per molecule is about 10 million times that of the acid, this is greater than the molecular conductivity of even the best conducting metals.

Fig. 5.



If the pressure of the gas is diminished below a certain point, the conductivity begins to diminish. I have here an experiment which I hope will show this. The apparatus (Fig. 5) consists of two bulbs, one outside the other; the inner bulb contains air at a low pressure, while the space between the two bulbs is a very high vacuum containing practically nothing but a little mercury and its vapour. The amount of mercury vapour in this space is, at the temperature of the room, exceedingly small, but as the apparatus is heated the vapour pressure increases, and we are thus able to produce a fairly wide range of pressure in the space between the bulbs. The outer sphere is surrounded by the coil connecting the outer coatings of the two Leyden jars. When the space between the bulbs is a conductor, the alternating currents circulating in the coil will induce in this conductor currents whose inductive effect is opposite to that of the currents in the coil; and in this case this layer will screen off from the inner bulb the electromotive force due to the alternating currents in the coil. If, on the other hand, the space between the bulbs is a non-conductor, the inner bulb will be exposed to the full effect of these forces. We now try the experiment: you observe that when the mercury is cold, and consequently the pressure in the space between the bulbs very low, a bright discharge passes through the inner bulb, while the space between the bulbs remains quite dark; when we heat the mercury so as to increase the pressure of its vapour, a bright discharge passes through the outer layer, while the inner bulb is quite dark; the outer layer is now a conductor, and by its action screens off from the inner bulb the induction of the coil.

Fig. 6.



The last experiment I have to show is one on the effect produced by a magnetic field on the discharge. When the discharge has to flow across the lines of magnetic force, the pressure of the magnetic field retards the discharge; when however, the discharge flows along the lines of magnetic force, the discharge is helped by the magnetic field. This is shown in the following experiment. A is a bulb; B a square tube, one side of which is placed between the poles of an electromagnet; the coil C, which connects the outside coatings of the jars, can be adjusted so that when the magnet is "off," the discharge passes through the bulb but not round the square tube; when, however, the magnet is "on," the discharge passes in the square tube but not in the bulb. In the square tube the discharge passes along the lines of magnetic force and is helped; in the bulb it passes across them and is retarded.

[J. J. T.]

WEEKLY EVENING MEETING,

Friday, April 20, 1894.

Sir Douglas Galton, K.C.B. D.C.L. LL.D. F.R.S. Vice-President, in the Chair.

J. G. Garson, M.D. V.P. Anthrop. Inst.

Early British Races.

Before proceeding to trace the early history of man in Britain it is necessary to refer briefly to the physical changes which geologists tell us have occurred since the close of the Tertiary period in the configuration and temperature of the north-western portion of Europe.

At the beginning of the Pleistocene period the temperature of Northern Europe became colder, and an ice cap, like that which now covers Greenland, gradually extended itself probably as far south as Middlesex and covered the greater part of Wales and the northern half of Ireland. This is known as the Great Ice Age. At that time, the land being more elevated than now, Great Britain and Ireland formed part of the continent of Europe, and the western coast-line extended some three or four hundred miles further into the Atlantic Ocean than it does at present. This period of cold was gradually succeeded by a more genial one, during which, but before the ice had disappeared, a great depression and submergence of the land took place, varying from about 600 feet to over 3000 feet below the present level at different parts of the country, but least in the south of England. The climate again became colder, and on the higher parts of Wales, the north of England and in Scotland glaciers were once more formed, but not to the same extent as formerly. Then followed, in late Pleistocene times, a re-elevation of the land to at least 600 feet above its present level, and Great Britain and Ireland once more became joined to the Continent, and the climate became temperate. In all probability the geographical conditions of Britain, or rather the British corner of Europe, in early and late Pleistocene times were almost identical. Finally the land connection with the Continent became severed by submergence till almost the present coast-line was reached, and the sea once more rolled in over the beds of the German Ocean and the English Channel. These changes in the geographical conformation of the north-western part of Europe took place slowly, and were consequently spread over an immense interval of time.

According to some eminent geologists, man first took up his abode in the British portion of Europe, either during the early Glacial or Pre-glacial Period. The evidence of his existence here at that early period rests upon the discovery of many flint implements of peculiar and special type on certain high chalk plateaux in Kent, in drift resting on Pleiocene beds, in drift deposits of Norfolk and Suffolk, and in certain caves in which glacial drift is believed to be deposited over the flints. All these implements are of the rudest make, more or less stained, like the drift flints with which they are associated, of a deep brown colour. They show a considerable amount of wear, as though they had been rubbed and knocked about a good deal, so that the worked edges are commonly rounded off and blunt. In few instances have they been wrought out of larger flints, and the amount of trimming they have received is very slight, and has been generally made on the edges of rude natural flints picked up from old flint drift; indeed, sometimes the work is so slight, as to be scarcely apparent, in other specimens it is considered by some sufficient to show design and object. These implements indicate the very infancy of art, and are probably the earliest efforts of man to fabricate tools and weapons from other substances than wood or bone. They give us some slight insight into the occupations and surroundings of the race who used them, as they appear to have been used for breaking bones to extract the marrow, scraping skins, and rounding sticks and bones for use as tools or poles. From the absence of large massive implements, it would seem as though offensive and defensive weapons had not been much needed, either from the absence of large mammalia, or from the habits and character of these early people. Many archæologists are not satisfied with the evidence yet adduced as to these flints being of the early date claimed for them, consequently of man's existence in Britain at that time, and regard the implements just described as belonging to the early part of the next period.

Whatever may be the ultimate decision arrived at as to the age of these flints, all geologists and others are agreed that after the Glacial period had passed away and Britain had once more become a part of the continent of Europe after its submergence, a race of men known to us as *Palæolithic man* migrated into the country from the Continent, across the valley of the English Channel. Man of this period is known to us from various remains of him found in drifts of Post-glacial age, and in the lower deposits of certain caves. As some evidence has been brought forward to show that the River-drift people, as they are called, are earlier than the Cave-dwellers, we will consider the remains of the former people first.

Remains of man from British River-drifts have only been found in the south of England from Chard, Axminster, and the Bristol Channel in the west, to the Straits of Dover, the lower Thames, Suffolk and Norfolk on the east, and as far north as Cambridge. They are conspicuous by their absence north-west of a line passing from Bristol to the Wash. The remains consist of a small portion of a skull reputed to be of this period, implements of flint, quartzite and chert, antlers of deer, and of certain fossil shells, probably used as ornaments.

The portion of skull was found by the late Mr. Henry Prigg, in 1882, at Westley, in Suffolk, seven and a half feet from the surface, in a pocket of brick earth eroded in the chalk, and in an adjoining pocket two molar teeth of mammoth and four Palæolithic flint implements were found.* The fragment of skull was part of the vertex, and included the upper portions of the frontal and parietal bones with part of the coronal and sagittal sutures. It was examined by Mr. Worthington Smith, and in transit back to the finder of it was unfortunately smashed. As it was not a characteristic part of the skull it shed little light on the cranial characters of its owner. With this exception, no human bones have been found in fluviatile deposits in Britain.

The implements from the River-drift consist principally of oval-pointed flints which have been fashioned by chipping, and were used without handles, oval or rounded flints with a cutting edge all round, scrapers for preparing skins, pointed flints used for boring, flakes struck off from blocks or cores by means of large hammer stones, often of quartzite, and choppers of pebble chipped to an edge on one side. The tools with which these implements were manufactured consisted of anvil stones of large blocks of flint, pointed flints or punches, and carefully made fabricators. All the implements, though simple and rude, show signs of manufacture, the more finely finished specimens having been prepared by delicate chipping. Their manufacture seems to have been carried on at certain localities on the banks of rivers, and other places where there was plenty of material from which to make them. It will be observed that at this time there were no flint arrow-heads, and that man was but poorly equipped for the chase, although it was undoubtedly by that means he gained his livelihood. Besides these flints man doubtless used wood and bone implements; pieces of pointed stakes made of wood have been found on the Palæolithic floors where he worked by Mr. Worthington Smith. Bead-like fossil shells of *Coscinopora globulosa* have also been found by Mr. Smith, with artificial enlargement of their natural orifices, among his implements, which would indicate that they had been used for necklaces or ornaments, so that he seems not to have been unmindful of his personal adornment even at that early time.

It is of importance to consider for a moment the animals which lived with man at this period. There are found in the same strata with him remains of the hippopotamus, two species of elephants and of rhinoceros, the cave bear and lion, the wild cat, hyena, urus, bison, the wild horse and boar, stag, roe, reindeer, and other animals, many of which are now extinct. Man at that time had no domestic animals. The only clothing he had, if he wore any, was made from the skins of the animals he killed in the chase and used for food. Being far from the sea, if he used fish as food, they would be such as he was able to catch in the rivers.

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* Jour. Anthrop. Inst., vol. xiv. p. 51.

Let us now trace man of this period on the Continent. In the fluviatile deposits of the Somme and the Garonne stone implements have been found and recognised by such competent authorities as Sir John Evans, Mr. Franks, Professor Boyd Dawkins and others, as identical with the drift Palæolithic implements found in England; similar ones have been found in Spain, near Madrid, in Italy, Greece, Germany and other places in Europe, also in Northern Africa, Palestine and India. From these finds we learn that man has lived in a similar state of civilisation to what he did in Britain, over a very wide area; they also show that he must have remained in this stage of culture for a very long time; but they give no evidence that the places where they are found were once inhabited by one and the same race of people, as might be inferred from some authors.

As regards his skeletal remains on the Continent, but few have been found. At Canstadt, near Stuttgart, it is stated that a portion of a skull was discovered in 1700, in deposits presumed to be of Palæolithic age, with bones of the cave bear and hyena, and mammoth.* At Eguisheim, near Colmar, Schaffhausen, a portion of another cranium was found with mammoth and other animal remains of this period. At Clichy, in the valley of the Seine, a skull and some bones were found at depths varying from 4 to 5·4 metres from the surface, in undisturbed strata, with mammoth, woolly rhinoceros, horse and stag. The skull in these instances is long and narrow in shape, with prominent supra-orbital and glabellar ridges; the thigh and leg bones of the Clichy skeleton are laterally compressed, the former having a greatly developed *linea aspera*, the latter being markedly platycnemic. Futher reference will be made to these specimens when we deal with the Cave skeletons.

Caverns and rock shelters are well known to have been used not only by man but also by animals from remote times down to the present day. The strata which have been deposited in them at different times by their successive occupants and the vicissitudes of climate are often well marked and give much valuable and reliable information, but great care is required in discriminating the different periods, which their contents represent. The remains of Palæolithic man deposited in caves are much more widely distributed over England than those from the River-drift, having been found as far north as Yorkshire and Derbyshire, in North and South Wales, Gloucestershire, Monmouthshire, Somersetshire and Devonshire, also in Ireland, although these latter have not been much worked. The Palæolithic Cave stratum shows three sub-strata, in the two lower ones the flint implements are precisely similar to those of the

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* Since this was written Prof. Boyd Dawkins has informed me that in the original record of the finds made at Canstadt in 1700 there is no mention of this skull having been found, and that the first mention of it having been found with them is in 1835. M. Cartailhac gives this later date as that when the skull is first mentioned.

River-drifts, but flat pebbles of quartzite are also found with part of the natural smooth surface retained, while the rest is chipped and fashioned into an implement. In the upper sub-stratum more highly finished articles, which would point to a higher and probably a different social condition later in time, are obtained. We have in this higher substratum flints of a lanceolate form, trimmed flakes, borers, and rounded hammer stones. These are of smaller size than the earlier implements, and some of them had evidently been let into handles of wood. Bone needles, with an eye bored at one end, bone awls, scoops, and harpoons barbed on one or both sides made of deer's antler are also met with. Of great importance are the representations of animals which have been found incised on bone, as for example, the portion of rib with the incised figure of a horse upon it, found in this layer in Robin Hood Cave in Derbyshire.

No portions of the human skeleton have been found in the Palæolithic stratum of British caves, except a single tooth.

On the Continent many caves have been discovered in France, Belgium, Germany and Switzerland, with similar deposits and implements to those found in England, and showing also the same two stages of culture. More numerous examples of figure carving of the same type as that found in the Derbyshire cave, have been obtained in French caves, and the teeth of carnivorous animals and shells, both artificially bored for ornaments.

By associating British and Continental evidence, we can form a good idea of the mode of life of the Cave-dwellers of Palæolithic times. The caves gave him shelter in cold weather, from which he also protected himself by fires, and clothing made from the skins of animals secured in the chase, sewn together by means of bone needles threaded with shreds of the tendons of reindeer. Armed with flint-tipped spears, and daggers of bone ornamented with carved handles representing the chase, he lived by hunting the reindeer, the wild horse and the bison; he also lived on birds and fish, which he speared with barbed harpoons. The game brought home was cut up with flint knives and cooked, and the long bones were broken with heavy flints for the marrow they contained, which was evidently considered a delicacy. The manufacture of the flint implements he used when engaged in the chase must have formed an important part of his work. The ornamental carvings on bone which he frequently made show that he was an artist of no mean order in depicting animals, but give us little information regarding his own morphology, as they seldom bear representations of himself—when they do, only his miniature outlines are figured naked; the carvings also show that he was in the habit of wearing long gloves to cover his hands and arms. Probably, he painted his body of a red colour, and ornamented himself with perforated shells, pieces of bone, ivory and teeth. Like the River-drift people he possessed no domestic animals, not even a dog to assist him in hunting.

In Continental caves human skeletons of this period have been found; of these, perhaps, the best known is the famous Neanderthal one, from a cave near Düsseldorf. Upon this skeleton alone it would not have been prudent to have based the characters of Palæolithic Cave men, because the circumstances under which it was found have given rise to some doubt as to its being of this age, and it is considered by some to belong to the next period which we have to deal with. When it is taken in conjunction with others presenting similar characters, and regarding which there can be no doubt as to the age to which they belong, the evidence it affords is considerably strengthened. The find of two skeletons at Spey in Belgium in 1886 has been most important, both in advancing our knowledge and confirming the characters ascribed to this race from various less complete specimens. The cranium of the Neanderthal skeleton, though very imperfect, is long and proportionately narrow in form, having a cephalic index of 72, the glabella, brow ridges, and external orbital processes are enormously developed, the forehead is remarkably flattened, the occiput is prominent, and the elevation of the whole vault is extremely low. The skulls of both the Spey skeletons are also long and narrow, one having a cephalic index of 70, and the other of 74·6, the superciliary ridges, and also the glabella, are very prominent, the frontal sinuses are large, the external orbital processes are thick and projecting, the ridges on the frontal, parietal and temporal bones for muscular attachments are strongly developed, the occiput is prominent with a well marked "torus" at the junction of the curved muscular ridges, which are also large, the cranial vault is low and flattened from above downwards, and presents an antero-posterior curve very similar to the outline of the side of an ellipse; the malar bones have thick and broad orbital processes, the orbital cavities are deep, and the orbital breadth is but slightly inferior to the width, the zygomatic arches are large. The size of the lower molar teeth increases from before backwards, the first molar being the smallest, and the last molar the largest. The lower jaw shows no prominence of the chin, indeed, it recedes somewhat from the alveolar border downwards, and has a symphesial angle of 111°. It is thus a counterpart of the Naulette mandible, which presents similar characters, both as regards the molars and the symphesial angle. The stature of the Neanderthal skeleton, estimated from the length of the femur, is 1·640 metres (5 ft. 3 in.), and from the humerus 2 centimetres less; that of the Spey skeleton (there being only one of these in which the long bones could be measured), estimated from the femur and tibia, is 1·504 metres (4 ft. 11¼ in.), and from the femur alone, 1·540 metres (5 ft. 0¾ in.). The stature of the Naulette skeleton, that of a woman, estimated from the ulna, is 1·433 metres (4 ft. 4½ in.), and shows that she also was very short.

The long bones of the upper and lower limbs of the Neanderthal skeleton are characterised by their unusual thickness and the great development of the elevations and depressions for the attachment of muscles, the articular ends of the femur are of larger size than usual. The femur of the Spey skeleton is more arched forward than usual, somewhat flattened from side to side in section, and the articular ends are of large size, especially the lower, in which there is enormous antero-posterior development of the articular surface of the condyles. The tibia is actually and proportionately very short, flattened laterally, and therefore platycnemic. The bones generally are remarkable for their stoutness, and indicate that the muscles attached to them were large and powerful, especially those of the lower limb. In respect to the platycnemism of the tibia, the Spey skeleton corresponds to the Langerie Basse and Madelaine bones from the Perigord Caves, and confirms in a very positive manner the evidence of their surroundings and relics that Palæolithic people were sons of the chase, as it is connected with the development of the *tibialis posticus* muscle, and not a race character.

Portions of skulls and skeletons found in various parts of the Continent, associated with Palæolithic implements and animal remains of late Pleistocene times, support the peculiar race characters of the specimens just described. The osteological remains of Palæolithic age now in hand, from different parts of the Continent, seem to me to afford sufficient evidence of the existence both in drift and in cave deposits of a race of men possessing physical characters quite distinct from those of the Neolithic period, which we will next consider. The assertions which have been made at various times with respect to individual specimens being more or less pathological, will, to my mind, not hold good when we find specimen after specimen from the same deposits showing similar characters. It may not be possible in some cases to establish the fact that the specimen cannot have been deposited at a later period in the stratum in which it is found, but a careful examination of each specimen, such, for example, as Professor Topinard has made of the mandible from Naulette, shows anatomical conditions which, not in one respect, but in several, indicate as distinctly as his implements the progress of man's evolution, and preclude the idea of this type being a variety of the Neolithic people. The specimens of Palæolithic man seem to me to show identity of race, whether they have been found in the River-drift or in the Palæolithic stratum of caves. The idea of Professor Boyd Dawkins, that the implements found in the River-drifts and later Palæolithic deposits of caves give evidence of there being two Palæolithic races, is not supported by the osteological remains yet to hand. From extensive examination of ancient British skeletons, I do not consider that there is any evidence of the existence of the direct descendants of Palæolithic man among the osteological remains of Neolithic or subsequent date in Britain. Here he seems to be as extinct as many of his contemporary animals of the late Pleistocene period; this, may or may not be the case with respect to his existence in other parts of Europe. Whether he has still representatives in America, as surmised by Professor Boyd Dawkins and some American anthropologists, is an interesting question, but does not come within the scope of this lecture.

The next period at which we find remains of man in Britain is separated from the previous one by a space of time measurable only by the changes occurring in the interval. Great Britain and Ireland had again become islands almost of the same dimensions as at the present day, but with a moister and more continental climate —hotter in summer and colder in winter—abundant forests extending as far as the extreme north of Scotland, and numerous morasses and peat bogs. Not less significant was the advance in civilisation man had made since Palæolithic times, as we now find him dwelling in fixed habitations, with a knowledge of the arts and agriculture, with domestic animals, and with stone implements not only of the earlier type but of a much more developed character, as he had now learned to smooth them by grinding and polishing.

These Neolithic people, as they are called, lived on the tops or sides of hills or in suitable valleys. Their camping grounds were intersected with numerous drains or ditches, which would show that the climate was moist. Inside the camp they hollowed out pits, in or round which they dwelt. From these camps have been obtained spindle whorls and bone combs toothed at one end, showing that they were acquainted with the arts of spinning and weaving, bone needles, fragments of coarse pottery made by hand and not turned on the wheel, either plain or ornamented with simple lines or dots, bones of the roe, red deer, dog, goat, short-horned ox, horse, pig, &c., and fish, but no trace of metal is found. Of all their implements the stone axe is, perhaps, the most important. Flints used for implement-making were now often quarried from below the soil, with antlers of deer as picks. The implements were distributed over districts far removed from where they were made, probably by barter; thus, Jadite or Nephrite implements have been found in Britain, which Mr. Rudler has shown were probably obtained from Switzerland, Silesia or Styria. They possessed canoes formed out of the trunks of trees, in which they probably reached this country from the Continent.

They buried their dead in caves which had been used as dwellings, in their camps, and in chambered and unchambered barrows. The most characteristic British barrows of this period are of long oval shape, and often of large size, but Neolithic interments are also found in circular barrows. The dead were buried in a contracted or crouched position, and, with them, stone and bone implements of various kinds, and pottery, which would seem to show that these articles were intended for the use of the dead or their spirits. Relics of art in the form of carvings are seldom found, and are very inferior to those of late Palæolithic times.

Osteological remains of the Neolithic people are distributed all over Britain, from the south of England to the extreme north of Scotland. They are most numerous in the south-west of England especially in Wilts and Gloucestershire, the part of the country occupied by the Drobuni or Silures at the beginning of the historic period. They have been found in considerable numbers in Yorkshire, Derbyshire and Stafford. Huxley and Wilson have described the same race from horned cairns in Caithness, and from other places in Scotland. I have described them from Wiltshire, Middlesex, Yorkshire and from Orkney.

There is some doubt of their existence at an early period in Ireland, as Professor Macalister informs me that he has not recognised them, and no long barrows are found there. Sir William Wilde, on the other hand, recognised Neolithic skulls from Somersetshire, as identical with certain ancient Irish skulls. Any skulls from Ireland I have seen which have shown characters similar to the Neolithic skulls from England, are of later date, but Huxley describes them from chambered tombs, peat mosses and river deposits of Ireland, of the long, narrow type. I think we may conclude as regards Ireland, that although it is doubtful whether the Neolithic people were there at as early a date as in Britain, certainly they were there later.

The characters of the skeletons are well marked. The skull is large and well formed, the calvaria is long and proportionally narrow, having a cephalic index of about 70, and of oval shape. The superciliary ridges and glabella are moderately or even feebly developed, the forehead is well formed, narrow, and curves gracefully to the occiput, which is full and rounded. The upper margins of the orbits are thin, and the malar bones are never prominent, the profile of the face is vertical, and there is no tendency to prognathism, the chin is prominent, the symphesial angle is from 70° to 80°, the length of the face from the root of the nose is comparatively short, but, as a whole, it is oval in form ; the jaws are small and fine, the teeth are of medium size, and generally in a good state of preservation, not much worn down ; the last molar is the smallest tooth of that series. The facial characters are mild, and without exaggerated development in any one direction ; the same may be said of the calvaria generally. The age of the persons to whom they belong averages, according to Dr. Thurnam, forty-five years, which would seem to indicate that the duration of life at that time was rather short.

The stature of the Neolithic people is short. From Dr. Thurnam's measurements of the femora of twenty-five skeletons, it averages 1·674 metres (5 ft. 6½ in.) by Rollet's formula, but from my own observations on other specimens which have passed through my hands, I am inclined to consider this is too high an average. In their general characters the bones are slender, often with a well marked *linea aspera* on the femur and platycnemic tibia, which would show that the Neolithic people still led a very active life as hunters. Dr. Thurnam has noted that sometimes two or more of the cervical or dorsal vertebræ have a tendency to anchylosis, but I cannot say that I have ever seen this.

On the continent of Europe remains of the Neolithic people are found chiefly in caves, and show much the same state of culture and physical features as just described, as for instance the well-known Cro-Magnon and Engis skulls, but the sequence of their existence there is not so well defined as in Britain, where they held, apparently, undisputed possession of the country for a considerable period. Indeed, it is only lately that Continental anthropologists have admitted their priority to that of people presenting the characters of the next race we shall have to deal with.

From the evidence to hand, it seems probable that the Neolithic people at one time occupied the whole of the west of Europe; and I agree with several other observers in considering that they are to be identified with the old Iberian race, of which the Basque may be considered a remnant. There is, certainly, a strong similarity between Basque skulls and those of the Neolithic people of Britain.

Unlike Palæolithic man, the Neolithic people have never become extinct in Britain, and their descendants exist to the present time. It is true that subsequent invaders drove them in many instances to particular parts of the country, where they remained isolated for a long period, as early history and the excavations of General Pitt Rivers and others show, but skeletons from ancient tombs indicate that they also mixed with their conquerors. The observations of several anthropologists show that they are associated with the short, dark-complexioned, and dolichocephalic people found in considerable proportions in some parts of the country, especially in certain parts of the west of England.

The next people to appear upon the scene previous to the dawn of history, are those who were in possession of the greater part of Britain at the time of the Roman invasion. They came into Britain from northern France and Belgium at a considerably earlier period, and gained possession of the country from the Neolithic race. These are the so-called Celts. Their advent is marked by the introduction of the use of metals into Britain, and they are associated with the Bronze Age. From the custom they had of interring their dead (whom they chiefly cremated) in barrows of a circular shape, they are often known as the Round Barrow people. They show a marked advance in civilisation beyond that of Neolithic times, as they were agriculturists, and lived by tilling the soil; they manufactured weapons and ornaments of bronze and richly decorated pottery; their flint implements also were of better make, as evidenced by their beautiful barbed arrow-heads. To this period belong many of the curious Lake dwellings found all over Great Britain and Ireland, Picts' houses of Scotland, and Bee-hive houses of Ireland.

Their osteological remains show that the skull was large, with strongly developed superciliary ridges and glabella, the brow well formed and broad, the upper occipital region not projecting, the tuberosity being the most prominent. In general form the brain case is broader and rounder than in the Neolithic race, the cephalic index centering round 81; they were, therefore, a distinctly brachycephalic people. The upper border of the orbit is thick, the malar bones are prominent and large. The jawbones are large, *macrognathous*, and likewise the teeth, which are often much ground down; the profile of the upper jaw is somewhat prominent, which gives a prognathous look to the skull; the chin is well formed. The face, as a whole, is of an angular lozenge form. The ridges for muscular attachments both on the cranium and face are well developed, and the expression is very rugged and savage like. Thurnam estimated from the skulls the average age of the persons interred in the Round Barrows to be fifty-five years, while that of the Long Barrows was ten years less.

The stature of the Round Barrow race averages 1·747 metres (5 ft. 9 in.), which is more than the mean stature of the population of the British Isles at the present day. The limb bones are large, with strongly developed ridges and depressions for muscular attachments.

This race is everywhere to be found over Great Britain and Ireland, and, although conquered by the Romans and subsequent invaders, forms a very important element in the population to the present day.

[J. G. G.]

WEEKLY EVENING MEETING,

Friday, April 27, 1894.

Hugo W. Müller, Esq. Ph.D. F.R.S. Vice-President, in the Chair.

Professor H. Marshall Ward, D.Sc. F.R.S. F.L.S.

Action of Light on Bacteria and Fungi.

The Italians have a proverb which runs, "*Dove non va il sole va il medico*"—Where the sun does not enter the doctor does—and which may be taken as an expressive summary of the experience of a people who have had opportunities of learning by many and varied trials how important for health is a due exposure to sunlight. Moreover we find the same proverb, in almost the same form, in the Provençal dialect —"*Di lo mésou enté n'entro pa lou sonlé riebo lou médeci*"—and that this empirical recognition of the sanitary powers of sunshine is common to many nations, and every one who has resided in India and the East knows how naturally the servants expose clothing and other articles to the direct rays of the sun.

In answer to the question, "Does the above proverb, and its equivalents in the languages of other peoples who have much to do with intense sunshine, really imply any knowledge or suspicion of the direct action of solar rays on organic materials or objectionable living beings? we are no doubt justified in replying, No. If they have thought about the matter at all, the people have assumed that the heat of the sun's rays and the ordinary action of the air are the factors concerned; and that it is no doubt a sort of mixed effect of dryness and ventilation that is to be aimed at when they expose the interiors of dwelling rooms, soiled clothing and so forth to the process of being "aired," as they have it.

On the other hand, we find that experience of similarly varied and often bitter kind has also taught the children of sunny climates that the very brilliant sunshine which they so correctly extol as efficient in purifying their houses and apparel is a dangerous enemy to human beings who are unduly exposed to the direct solar rays. All the painful experience connected with snow-blindness, sunburn and sunstroke may be invoked in support of this statement, and it requires but a careful study of this subject to be convinced that in these cases at any rate the effects cannot be put down to the mere heating power of the sun's rays.

Closer examination of the whole question drives us to the conclusion that the effects referred to, and many other effects of direct insolation, are not due to the heat rays at all, but to actions of quite other kinds induced by rays long unsuspected of having anything like the hygienic importance they really have. But it has required much careful experimental investigation to decide this matter, and I propose to show you some of the principal steps in the chain of proof.

The fact that bacteria, which would multiply at an enormous rate in certain organic liquids, as, for instance, dilute meat broth, cease more or less evidently to do so if the fluid containing them is exposed to intense sunshine, was first pointed out by Messrs. Downes and Blunt in 1877 and 1878, and further experiments have abundantly confirmed their result. The establishment of this truth led to a more or less desultory controversy, turning on the question whether the effect was merely one of relatively high temperature, or whether it was really due to the light rays—in other words, whether those rays of the solar spectrum known to us especially by their thermal actions, induced the death of the insolated bacteria, or whether other rays of the solar spectrum were concerned.

This controversy was brought virtually to an end by the various experiments of Downes and Blunt, Arloing, Janowsky, and others, who showed that in many cases at any rate the bacteria in the tubes of broth, &c., were either killed outright, or rendered almost incapable of further development in ordinary daylight, or in tubes cooled by ice, or—in later experiments—in tubes behind screens which cut off the heat-rays to such an extent that no question of temperature could possibly be raised.

Meanwhile, another controversy arose, and was also carried on in a more or less disjointed manner through a series of years. This turned on the question—granted that the bactericidal effect is not due to high temperatures, may it not be merely the result of some poisoning effect of certain substances produced in the food-medium (broth, &c.) under the oxidising effect of the air in the illuminated tubes.

Duclaux was, I believe, the first to assert this definitely, and it was regarded by many as confirmed by experiments performed by Roux in 1887. It was pointed out that certain rays—more especially those towards the violet end of the spectrum, and known popularly as the "chemical" rays—are very apt to promote oxidative decompositions in solutions of organic substances, such as the broth used for cultivating these bacteria. It was also shown that the death of the bacteria in question did not result if all traces of oxygen are removed from the tubes previous to insolation. It had also been rendered at least highly probable—though there was as yet no agreement on this point—that certain rays of light only, some thought the red, others the blue-violet rays, are especially concerned in the process. The conclusion drawn, therefore, was that the effect was primarily due to a poisoning action of the broth or other food-material due to the energetic oxidations promoted in it by the solar rays when exposed to the light of the sun.

Meanwhile, other questions were being asked by those who were not satisfied with this explanation, or in the ordinary course of experimental inquiry into the subject.

It had already been decided that Downes' and Blunt's experiments could not be accepted as conclusive, because they were not conducted with pure cultures, but with mixed infections of various bacteria, the growth or non-growth of which was decided merely by inspection of the turbidity or otherwise of the broth. Duclaux was one of the first to use pure cultures in 1885, and Arloing about the same period showed that some injurious effects on the spores could be induced by artificial light, such as strong gas-light, though it did not kill the bacteria.

Then Geissler in 1892 came to the conclusion that the light from a powerful electric arc-lamp has some retarding action on the typhoid bacillus, though it was much feebler than direct sunlight, a result confirmed later by Chmelewsky, thus making it probable that the action of light on these organisms is merely dependent on the kind or intensity of the rays and not on the source of the light.

But the most interesting controversy was that which had meantime arisen concerning which of the various rays of the spectrum are the really effective—or the most effective ones.

That the various rays of the solar spectrum act differently on plants, has long been an established fact in Botanical Science, and the experiments of Famintzin, Batalin, Sachs, Pfeffer, Paul Schmidt, Naegeli, Pringsheim, Elfving and others have put beyond doubt that it is of material importance to an ordinary plant not only whether it is excluded from or exposed to light, but also whether the light to which it is exposed contains the normal proportions of the various rays known to us in the visible and invisible spectrum, or is deficient in some of these.

To mention one or two illustrative cases only. The highly refrangible rays in the blue-violet region of the spectrum have a powerful effect on the processes of growth proper in a plant, and in heliotropic curvatures of the growing parts, whereas the process of carbon-dioxide assimilation is chiefly dependent on the less refrangible red-orange rays at the other end of the spectrum. Again, the infra-red rays, if only in virtue of their thermic effects, are known to be of importance in growth, though in quite a different way from the rays above mentioned; while Sachs has given reasons for believing that the invisible ultra-violet rays at the other extreme of the spectrum have quite different effects again in the development of coloured flowers, and so on.

It was natural, therefore, to raise the question whether all, or only some, and if so which of the rays composing the solar light are effective in retarding and killing bacteria—especially when we remember that bacteria are plants—or at any rate that many of the organisms so denominated belong to the vegetable kingdom.

Two methods have been employed for the purpose of answering this question. The first, employed by Downes and Blunt, Arloing, Janowsky, Geissler and Kotljar, was to use screens of coloured glass, or coloured solutions, &c., behind which tubes of broth or other liquids containing the bacteria were placed, and to judge of the effect by comparing how rapidly the broth became turbid; or behind which gelatine tubes, or agar or potato cultures were exposed, and a rough estimate made as to how well or ill the bacteria grew on the surfaces.

The second was to place such tubes in the various regions of the spectrum, and compare the results, as before, by judging of the turbidity of the broth, or the rate of growth on potato, &c. &c. This method was employed by Arloing, Janowsky, Chmelewsky and Geissler.

Quite apart from other drawbacks to any such methods as these, and while fully recognising their historical necessity as pioneer work, it is obvious that no very accurate results could be expected from them, and as matter of fact we find up to 1892 that opinions were so divided on the question which are the effective rays, that no definite conclusion could be drawn. For instance, Arloing could not distinguish the action of any one particular set of rays from that of others, while Gaillard concluded that all the rays have a feeble effect. Santori concluded that neither the red nor the violet rays are the active ones, whereas Janowsky thought both the red and blue-violet were the effective rays.

Roux and Duclaux thought the action was due to a poisoning of the bacteria by products of oxidation of the food-medium, while Arloing and others insisted it was a direct action of the light. Some denied the facts altogether, or believed that all the light did was to retard the germination of the spores; while others insisted that the bacilli are more easily destroyed than the spores, and that it was all an obscure matter of temperature. In fact, previous to 1892, the utmost confusion existed, and all kinds of statements were current on the subject among the very small band of observers who concerned themselves at all with it; the best indication of the want of definite knowledge being, perhaps, the silence of the text-books on the matter.

The fact is that the methods in use up to that period were inadequate for the solution of the problems that had arisen, and many cautious bacteriologists were consequently sceptical as to the bactericidal effects of the sunlight, or indeed any light at all.

During the course of my investigations into the vitality of the anthrax bacillus when its spores are left in water, I was struck with the rapidity with which the bacillus disappears from tubes exposed to the sunlight, a fact often observed before, and recorded by Straus, Buchner, Momont, Frankland and others, and usually supposed to be due to the spores germinating out into tender bacteria, which are then killed off. This is by no means the case, however, for the spores are themselves so acted on by the light-rays that they become incapable of germinating at all—they are *directly killed* by some injurious action set up in their interior by certain rays, and this at temperatures much lower than that we are now employing, *whereas these spores are not killed by mere boiling* for a minute or two, and will withstand *for hours*, and even days, temperatures *far higher than any they are subjected to in the experiments* I am going to describe.

In experiments where the spores of *Bacillus anthracis* were left in distilled water exposed to light for a few hours, it was found that many of the spores died, while of those which were left the germinating power was very much weakened. If, for instance, a number of spores were distributed in pure distilled water, and thoroughly shaken up, and the infected water was then divided into two portions, one exposed to light and the other not, every sample drop taken from the former tube was found to contain far fewer living spores than any sample drop from the latter. This confirmed the results observed by others, and already referred to—the sunlight in some way kills the spores, even in pure water.

It was further noticed that if I mixed a drop of the freshly infected water with sterile gelatine or agar, and then poured the liquid transparent mass on a shallow glass dish, and allowed it to set or stiffen there into a transparent film, considerable differences were observable according as such a plate-culture was left exposed to the light or covered up from it.

In the latter case each of the numerous invisible spores contained in the drop of water, and now evenly distributed through the transparent mass of solidified gelatine, &c., germinated out in a few hours, and formed a colony of bacteria so dense and opaque that the previously clear gelatine, &c., became studded closely with them, and looked as if peppered over with a dense cloud of dust; whereas in the former case the light was found to have killed so many of the spores that only a very few colonies were formed on the plate at wide intervals, and separated from one another by the portions of transparent gelatine in which no growth had occurred.

[Plate-cultures shown illustrating this.]

It must be borne in mind that the gelatine, which remains transparent in this case, is just as richly charged with spores as in the other case, only the spores remain invisible owing to their minuteness; they are dead, and therefore cannot germinate and grow and develope into the colonies which render the gelatine, &c., opaque.

It was pretty obvious that if the light was the agent which thus killed the exposed spores, the effect ought to be better brought out by exposing part only of the gelatine plate, and covering up the rest. This I did as follows, by a modification of a method long known to botanists and practised by myself for years. A large quantity of spores was thoroughly shaken up in distilled sterile water, so that every cubic centimetre of the water contained from 1,000,000 to 5,000,000 of the invisible spores. Then about 5 drops of this water was mixed with gelatine, warmed until quite liquid, and the whole rapidly poured in an even thin film over the flat floor of one of these thin clear glass dishes. In five or ten minutes the gelatine had set to a thin transparent film, containing say 5,000,000 invisible spores. The bottom of the dish, outside, was now covered with a zinc stencil-plate, through which a letter of the alphabet (say E) was cut, and every other part of the dish wrapped over with tin-foil and black paper, so that no light could reach the film except that which passed through the E-shaped opening in the metal stencil-plate in contact with the thin glass on which the gelatine film rests.

[Demonstration of method of making and exposing such plates.]

The plate, letter downwards, was then suspended over a plane silvered glass mirror, placed at an angle such that the rays of the sun could be reflected up vertically through the letter, for two, three or more hours.

After this process of exposure, the coverings and stencil-plate were removed from the plate, and the latter put in a warm dark incubator. The film showed no effects, but was still an even transparent sheet of gelatine, because the spores, dead or alive, are so minute that they in no way perceptibly interfere with the clearness of the gelatine, and none have had time as yet to germinate out.

On allowing such an exposed plate to remain, in the dark, at a temperature suitable for promoting the germination of the living spores, however, a marked and striking result is obtained in from twenty-four to forty-eight hours or so; for it is found that those spores which lie imbedded in the parts of the gelatine sheltered from the light, by the foil and other coverings, have germinated out normally and developed into visible colonies, which render the gelatine opaque, and which are so numerous that they run into one another, and so produce a general grey, white or other coloured layer, according to the kind of bacterium, &c., used, whereas those spores in the E-shaped area exposed to light do not alter, because they are *dead*—killed by the light—and therefore do not affect the clearness and transparency of the gelatine, being invisible on account of their minuteness.

Consequently we see a transparent letter E picked out from an opaque matrix of growing bacteria.

[Photographs and plates shown.]

Buchner, by a method essentially similar to this, showed that a few hours' exposure to the intense light of the sun in summer suffices for this, but I showed that the much feebler rays of the winter sun are capable, even after reflection from a mirror, of totally killing the spores of the anthrax bacillus, at a temperature so low that even the gelatine ordinarily employed in cultures, and which runs at 29° C., is not melted. Since the spores employed will withstand very much higher temperatures—55° C. to 60° C.—for many hours, it is clear that these experiments are conclusive against the supposition that the bactericidal action is due to the temperature. Having established this point for this and a number of other forms, including some mould fungi and yeasts, the next step was to settle the question whether the light-action is really direct on the spores, or due to some poisoning action owing to products of oxidation of the food-materials.

This problem I attacked in the following manner. Spores were shaken up in pure water, and the mixture poured into sterile shallow flat glass dishes—the Petri dishes used for plate-cultures—and dried there in a hot oven at 60° C. to 70° C., or even higher; this does not injure the spores, but renders them air-dry, and they stick as a thin powdery film to the bottom of the plate in this condition.

I then prepared several plates of agar films, sterile and without spores, and proceeded further according to the following argument:—If the solar action depends on the formation of some poisonous oxidation product in the agar, then, if I expose one of these sheets of agar, covered with a stencilled letter, to the sun, and then superpose it on an *un-exposed* film of the spores only, the latter ought to refuse to germinate on the letter-shaped area of the agar which had been exposed, whereas they should grow on the parts of the agar which had been protected from the sun.

But on trying the experiment, such proved not to be the case. On the contrary, the spores germinated equally well *all over the agar film*, on the parts exposed as well as on those not exposed. In other words, the agar is not rendered in any way a worse pabulum for the spores by exposure to the sun's rays—a fact quite in accordance with numerous other experiments I tried.

This result by itself, however, is far less conclusive than when taken with the reciprocal experiment, as follows:—

Side by side with the sterile agar film above referred to, I also exposed a film of the dried spores—without agar—for the same period, and under the same conditions as to covering, stencil-letter, &c. If, now, the solar action is direct on something in the spore itself, then those spores in that letter-shaped area of the film to which the stencil-plate allows the sun's rays to have access, ought to be killed: consequently, on superposing a non-exposed film of agar on to one of these exposed films of spores, I ought to obtain a transparent letter after the spores around have grown out into opaque colonies.

And such proved to be the case, as the accompanying photograph shows. [Experiments and photographs demonstrated.]

These reciprocal experiments taken together, conclusively show that the light-action is really direct on the spores themselves, and that the solar rays do not perceptibly affect the food-value of the agar films.

The same result is also obtained by taking some of the germinating spores from the non-exposed parts of a gelatine or agar plate in the previous experiment, and placing them on the exposed area where all the spores have been killed: they grow and flourish on this exposed area as well as they do on the non-exposed one, whence may be inferred that the bactericidal action, whatever it is, is not a mere consequence of poisoning from outside. It is evidently due to the light-rays directly inducing some changes in the substance of the spore itself.

The practical hygienic importance of this discovery is considerable, and would not be diminished even if it had turned out to be true that the light-action was really on something at the immediate surface of the spore; for it is clear that when spores escape into the air, and are exposed to the direct rays of the sun, they run exactly the risks I here subjected them to, and, as matter of fact, we find it is of the utmost importance in bacteriological investigations not to allow the spores of bacilli, fungi, &c. (even when fully ripe and otherwise fit for keeping in dry tubes, &c., for years), to be exposed to the light, for under such circumstances they gradually deteriorate.

Now we can see why it is so essential to health that our streets, houses, clothes, &c., should be thoroughly exposed to the sunshine. Moreover, as will be clearer in the sequel, these experiments render intelligible why it is that epidemics of parasitic fungi are so often connected in people's minds with dull, cloudy, sunless weather, and the Italian proverb with which I opened this lecture has for us a meaning far deeper and more significant than it had before.

It is evident that the foregoing method is capable of application in several different directions, and I now proceed to attack the problem as to which rays of light are most effective in the process of destroying living germs, on entirely new lines.

Starting from the thought that all the older attempts to compare the growth in two or more tubes exposed to different coloured lights are open to the criticism that it is impossible to be sure that each tube-culture is alike at the commencement, and extremely difficult to contrast them as growth proceeds, it was obviously a step forward to compare the effects of different coloured lights on one and the same plate culture. This I did in various ways, but the following is one of the most instructive.

A block of vulcanite is cut in the shape of a Roman capital E lying on its back ш, and a thin glass plate is cemented to each side: this gives a double celled screen, of which each cell is equal as regards depth of liquid poured in, kind of glass, and so on, points of more importance than may appear at first sight. Having prepared such a screen with, say, clear water in one cell and the orange-coloured solution of bichromate of potassium in the other, an agar plate of spores is made and *two* stencil-letters placed on it, so arranged that one letter is covered by each cell of the screen.

[Screen apparatus of various kinds shown and explained.]

On exposing such a screened plate, one of course gets the comparative effects of the *same* light, acting at the *same* temperature, for the *same* period on the *same* culture: the one condition altered being that certain rays—in this case the blue-violet—are cut off by the bichromate screen.

[Plates thus exposed and photographs of such shown.]

By thus using various coloured solutions capable of cutting off certain rays of light—their behaviour in this respect being carefully determined by spectroscopic examination—in the one cell of the double screen, and water in the other, I was enabled to show very clearly the comparative action of the coloured light and the undecomposed light passing through water, and to establish the following conclusions.

There is no perceptible bactericidal action behind any screen which cuts off the *blue-violet rays*, whereas *the action is the more pronounced the more these rays are transmitted.* It matters not how much of the red-orange and yellow end of the spectrum is transmitted, so long as the blue-violet end is cut off by the screen the spores are not killed at the ordinary temperatures. Results entirely confirmatory of these were also obtained with glass screens, although I now lay less stress on these, because they are in some respects less satisfactory for exact quantitative results, owing to the variations which glass screens show as regards their capacities for absorption and radiation.*

[Photographs of exposures behind other screens.]

Numerous experiments were also made, with screens and without, to test a number of other points of discussion which arose during the progress of the investigation—e. g. how long an exposure was necessary to kill all the spores; how far temperature aids or combats the destructive effects of the blue rays; and so on. In these cases screens of quartz, alum, quinine, æsculin, rock-salt and various other substances were used, and a number of interesting facts obtained as to details.

During the course of the inquiry as to the time necessary, it was found to be useful to photograph the plate after exposure in order to record the gradual process of "development of the image"—as the photographer would term it—as the spores on the non-illuminated parts of the agar film germinated out and grew into colonies, the opacity of which showed up the clear figure of the exposed area by contrast: and some suggestive facts were elucidated by these experiments.

On unwrapping the exposed plate nothing is visible at first, as already explained, but after twelve to twenty-four hours or so at a suitable temperature in the dark, the spores on the non-illuminated parts of the plate gradually produce a faint grey opacity around the exposed area, which is so far a clear ill-defined patch. In the course of another twelve to twenty-four hours or so the outlines of the letter or other shaped area begin to show more definitely, and the figure can be recognised, and later on it becomes sharper and sharper as the contrast between the more and more opaque and dense bacterial

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* I am glad to take this opportunity of thanking Mr. G. W. Walker, of the Royal College of Science, for the trouble he has taken recently in physically testing a series of these glass screens for me.

growth around, and the clear area containing the invisible dead—killed—spores becomes greater.

In certain cases, especially on very hot sunny days, when the light is particularly bright, it is found that the image is blurred for a long time, owing to the *retardation* (not death) of the spores in the immediate neighbourhood of the margins of the figure. The explanation of this phenomenon seems to be that the excessively intense light passing through the stencil letter and the transparent floor on which the spore-laden agar-film rests, is partly reflected and slightly diffused from the surface of the glass lid of the Petri dish, and this reflected light is still strong enough to have some effect, and consequently the spores nearest the margins of the area through which the light comes are retarded, but not killed.

[Demonstration of the gradual development of the figure during incubation.]

Another phenomenon of interest here is that a few spores here and there sometimes escape being killed, even on the most exposed parts of the illuminated area, and after a few days slowly grow out into feeble colonies. For some time this puzzled me considerably. It is clearly not evidence in support of any poison theory; but it seems very simple if we suppose these spores to have been so disposed that a number of other spores, vertically situated between them and the incident rays of light, sheltered them more or less completely—a disposition quite possible when we remember the millions of spores present in the film—and so saved them from complete destruction. And all the evidence points to this being the right explanation.

It only remains to be added here that all the effects already referred to can be got with the electric arc-light, instead of with solar light, but there are some difficulties in employing this source of radiation which may easily lead to the erroneous conclusion that the electric light is less efficacious as a bactericidal agent than sunlight. The principal of these is that, as we shall see, *glass in any form* is powerfully *obstructive to the rays required.*

It occurred to me at an early stage in this investigation—in fact, my earlier efforts were especially directed to this end—that the most direct answer to the question, What rays are the really effective ones? ought to be best obtainable by *shining the spectrum itself directly on the film of spores*, and making the latter *record the effects themselves*, by their subsequent behaviour, according as red, yellow, blue, &c., rays fell on them. For, obviously, if the spores are killed by blue-violet rays, and not injured by red and yellow ones, those spores which lay in the blue, &c., of the spectrum ought to die, and the agar remain clear, while those in the red, &c., ought to grow and render the agar opaque, and so on, and I ought to obtain a *photograph, in living and dead bacteria, of the spectrum itself!*

It is too long a story to go over all the difficulties and disappointments incident to the preliminary trials; but the ultimate success was so astoundingly complete and instructive that I must give some account of it.

The chief difficulty to be overcome here is the great weakening of intensity of the dispersed rays of the beam of light decomposed to form the spectrum; for not only are we here distributing the incidence of the rays themselves over a larger area, but they are passing through lenses, prisms, &c., which absorb or reflect many of them. This, of course, has to be compensated by condensing the beam and exposing for longer periods; but even then, when anything like a pure spectrum is employed, the slit has to be so narrow that relatively little light can be utilised. Consequently, the exposure has to be much more protracted than with undecomposed light.

Then came in another difficulty. It was found, when working with the electric light, that very feeble effects could be got as compared with those produced by exposure to the solar rays on a clear blue day, and the reason of this is that the blue and violet rays are stopped to a large extent by the glass surfaces through which the rays must pass. The effect of the glass is practically the same as the effect of mist or haze, &c., in the atmosphere, which so filters out the blue-violet rays, that the light of a dull day is of little effect in my experiments.

During the progress of the experiments I had the good fortune to obtain the aid of Professor Oliver Lodge, who was so kind as to expose a large number of my plates to a powerful electric arc for me, with most satisfactory results. The difficulties referred to were got over by employing *quartz* instead of glass—the lenses and prisms were of quartz and a quartz plate was used for covering the agar film with its embedded spores.

In this manner it was possible to obtain a very pure spectrum sufficiently rich in blue and violet rays to kill the spores in a few hours.

With the solar rays, I found it quite easy to obtain satisfactory results in the summer, however, even with glass mirrors, lenses and prisms, and exposures of five to six hours, though in winter I find the exposures necessary are so long as to be almost impracticable.

I will first describe the experiments with the solar spectrum which I made at Cooper's Hill this last summer.

The plate of agar, with spores or bacilli in it, is made exactly as before, but instead of the ordinary glass lid I employ a sheet of thin flat ground glass, in which a slot is cut about three inches long by one inch wide. This slot is covered by either a thin sheet of glass or a plate of quartz. The whole of the covered plate—except the slot described—is then protected from the light by black paper and tin foil, and is then laid, bottom downwards, on a box kept cold with ice, in such a position that the spectrum falls vertically across the slot, the coloured bands crossing the long axis of the slot at right angles.

[Demonstration of quartz-covered plates and apparatus for spectrum and method of exposure, &c.]

The red rays are made to fall across, say the left end of the slot, starting about half an inch from its extremity, then come the orange-yellow and green, then the blue and violet, the end of the visible violet being about an inch from the right end of the slot. Beyond the visible red, to the left, the invisible infra-red rays are falling on the plate, and beyond the visible violet, to the right, fall the invisible ultra-violet rays.

Owing to the obstruction of the glass, however, very little of what ultra-violet escapes absorption in the atmosphere reaches the agar-plate itself; though, as we shall see, a good deal of the large proportion of these rays which emanate from the electric arc are incident on the plate provided no glass is employed at all.

The plate which I now have thrown on the screen [Photographs of solar spectra in bacteria] was thus exposed for five hours in August to the spectrum obtained by decomposing a beam of solar light reflected from the mirror of a heliostat, and you see that the result is a most satisfactory proof (1) that the infra-red, red, orange and yellow rays are totally without effect—as shown by the bacteria exposed to these rays having germinated and developed as rapidly and as strongly as those in the dark; (2) that the bactericidal effect occurs in the blue-violet region, diminishing in intensity at both ends as we pass into the green (to the left), or into the ultra-violet (to the right); (3) that the most destructive rays are the blue rays to the right and the violet rays.

These results are—if we allow for differences of absorption in the screens, &c.—entirely in accordance with my screen-experiments, and prove most conclusively that *the rays which kill the bacteria are the blue and violet* ones. This explains why I find these organisms destroyed so much more rapidly by the light of the summer sun than in winter; why a clear blue sky is so much more effective than a hazy one; why direct sunlight acts so much more quickly than reflected or diffuse daylight; and many other experimental facts. It will be noticed that no question of temperature comes in; the plates are exposed on ice, and the hotter regions of the spectrum are totally without effect—the bactericidal action is here emphatically due to rays which affect our eyes as blue and violet. Now let us examine the results obtained with the electric spectrum.

The methods are exactly as before, excepting that the light, concentrated by quartz lenses, is passed through quartz prisms, &c., and is not allowed to pass through glass at all, unless, as in the case I will select, a piece of thin glass is interposed for experimental purposes, to determine the difference in effect between it and quartz. In this case two slots were cut in the glass lid, one of which was covered with thin glass, the other with quartz. Everything else was as before. The plate was exposed for twelve hours to a very pure spectrum, and it brings out very clearly the following facts. [Photographs of electric spectra shown.]

(1) That when there are plenty of ultra-violet rays, and no

glass is interposed, the effect extends far beyond the visible spectrum into the ultra-violet, to the right; but, when even a thin sheet of glass is interposed it obstructs these rays, and the effect is like that of the solar spectrum, as the photograph shows.

(2) It shows even more clearly than before that the infra-red, red, orange-yellow and green are without effect, and that the effect weakens as we pass beyond the visible violet, and

(3) That the *maximum* bactericidal effect is due to the blue violet rays, for the bellying out of the cleared region about there is due to the action of these rays reflected from the interior of the plate, showing that here alone are the reflected rays powerful enough to act just beyond the edges of the area.

It is also worth notice how slight an obstacle suffices to cut off the ultra-violet rays, for the two small protuberances visible in the lower photograph are merely due to two little drops of the Canada balsam used for cementing the quartz to the glass having oozed out and overflowed beyond the edge of the slot—nevertheless, this sufficed to block out the rays there.

These experiments will suffice to convince you that the blue-violet end of the spectrum is the effective one. They compel to the conclusion that the more I can expose the spores, &c., to the unobstructed rays, the more rapidly are the germs destroyed, and they show very clearly how fallacious may be the results where glass has to be employed throughout—a fact which will be impressed yet more upon us as I proceed.

There is another thought which arises here, and that is, what a poor chance such germs as these spores of fungi, yeasts, bacteria, &c., must have if they were not shrouded from the solar rays by the atmosphere! This reflection is not without its significance towards certain wild hypotheses as to the origin of life on our planet.

A more practical outcome of these results with the electric arc suggests itself in the probable efficiency of the naked arc light as a disinfecting agent in hospital wards, railway and other carriages, &c., as I have pointed out elsewhere; and we cannot overlook their significance in connection with such experiments as those of the late Sir William Siemens on the application of the electric light in horticulture, &c.

If now we look at these extremely suggestive experimental results from another point of view, it is obvious that these incubated plates showing the contrast effects between dead and living bacteria, or bacteria partly killed, as the case may be, and according as they have been exposed to light or not, or exposed to certain rays or to others, or exposed for longer or shorter times, and so on, are really *photographs;* but they are photographs where the sensitive agent is a living organism, such as a bacterium, in place of a merely chemical substance, such as a silver salt suspended in a medium which acts as a carrier—here gelatine or agar, in place of the prepared gelatine to the photographer.

The plates I have shown you are really *photographs in living bacteria* of the solar and electric spectra, only instead of being recorded by the various stages of decomposition of the invisible particles of silver salts, which are then treated by chemical solutions, &c., in the processes of development, fixing, &c., they are recorded by the various capacities for germination of the invisible spores, the image—i. e. contrast effect—being then *developed by my favouring their germination and further growth into opaque colonies* by the application of slight heat.

If this is so, it ought to be possible to use such a film of bacteria spores as a sensitive plate for printing from a negative, according to the principles laid down, and just as a piece of paper or glass, covered with a film impregnated with silver salts, &c., is used in contact printing in photography; and this, too, I have succeeded in doing. The chief difficulty to be overcome here is to bring the film impregnated with bacteria sufficiently close to the gelatine surface of the negative to be printed from, for of course it is impossible to bring them in actual contact, since the bacterial film must be kept covered by sterilised glass, &c., and the bacterial culture kept safe from contamination by spores in the air. By employing sheets of extremely thin glass, such as that used for cover-slips, however, on which to support the bacterial films, I have overcome this difficulty, and now throw on the screen a *photograph in living bacteria of a piece of English landscape*, printed from an ordinary negative by throwing the solar rays, reflected from a polished mirror of speculum metal, through the negative, the gelatine surface of which was only separated from the bacterial film by such an extremely thin plate of glass as I have described.

The bacterium employed was a beautiful purple one obtained from the Thames, and which is very sensitive to light—so much so, indeed, that I am convinced that with thin quartz plates I could get a print in half an hour on a clear day. In the case shown the exposure was only two hours.

It only remains to add that similar results with stencil-letters, the solar and electric spectra, and photographs such as that described have been obtained with anthrax, typhoid, several ordinary river bacteria, and fungi and yeasts, and that—apart from details which need not enter into discussion here—the general results are the same.

In order to try and obtain a further insight into the physiological and pathological phenomena concerned in this bactericidal action of light, however, and in view of various disadvantages inseparable from the employment of cultures in the mass, I have attempted with success to trace the effects of the light on the *individual bacterium cell* itself, isolated under the microscope. To do this, some form of culture chamber is necessary in which the organism can be grown in a minute drop of food-material—such as broth, gelatine, or even water—and can be kept under a high power of the microscope for some hours, or even days if necessary. This is managed as follows:—

[Demonstration of culture cells and methods of using them.]

A glass tube about 3 inches long, has a bulge in the middle: this bulge is ground down on two opposite sides so that it is pierced by two large round openings. One of these openings is placed downwards on a piece of glass or quartz, and cemented to the latter by any convenient means—I usually employ paraffin: the other opening, above, is covered by a thin piece of glass, to the under side of which, in the centre, is hung a minute drop of the food-material containing the spore to be examined. The two arms of this culture chamber are stuffed with wet cotton wool, and a layer of water is placed on the floor of this "damp chamber," and the whole is now ready for use. All the materials employed have undergone the necessary cleaning and sterilising, and with proper precautions there is no reason why such a hanging drop, with the growing organism in it, should not be kept under observation until the latter has either exhausted all the food supplies, or completed its life-cycle, in focus under the microscope.

When in position on the stage of the microscope, the light reaches the organism through the glass or quartz, and of course it is easy to arrange shutters, screens, &c., so that only the light reflected from the mirror of the microscope below shall reach the drop and the organism in it. Again, it is easy to interpose coloured or other screens of glass, coloured solutions, &c., and so allow only certain rays to reach the hanging drop.

Now suppose a single spore in the drop to be placed in focus under the microscope, and that in the eye-piece of the latter a graduated scale is fixed, the value of whose divisions is known. In a few hours the spore, the length of which is measured, begins to germinate, and developes into a rod—a bacillus—the length of which is then measured; then measurements of the increasing length of the growing rod are taken at successive intervals, as convenient, and so ideas of the rate of growth of the rod obtained. If I sketch the elongating rod at appropriate intervals on sectional paper, to scale, always starting each sketch from the same horizontal base-line, and at a distance along this line proportional to the time which has elapsed since the preceding sketch was made, the line joining the free ends of the rods gives a curve, which will be steeper or the reverse according to the rapidity of growth of the rod. Considerable use can be made of this *curve of growth of the bacillus.*

[Demonstration of the measurement of growth of the bacillus.]

Obviously it is easy to record these measurements by plotting them out in the form of a curve—the intervals of time being measured along a base line, and vertical lines (ordinates) in each case proportional to the length of the growing rod at the time, being erected at these points, and the joined upper ends of these ordinates give the curve.

[Photograph of a curve of growth of a bacillus.]

Now I find that the curve of growth thus obtained is remarkably constant in shape under constant conditions of temperature, illumination, &c., but varies if these conditions vary. By comparing a number of such curves, moreover, much curious information is coming to hand, but as this does not concern our present theme, I shall not dwell on it here.

The steepness of the curve—i. e. the rate of growth of the rods—increases with the age and length of the latter, when the conditions of growth are constant, because, as the rod increases in age and length, it exposes a larger and larger surface of absorption to the food-materials, and possesses more and more power of utilising them for further growth.

If two cultures such as those described are started at the same time, one under one microscope and the other under another by its side; and if *one* condition at a time is varied for one of these cultures, then the differences in the curves ought to give us a very clear idea of the effect of the difference of condition on the growth of the bacillus—and we must remember that the rate of growth is our best criterion for the well or ill doing of the organism.

It is, however, by no means an easy matter to vary *one* condition at a time in these cultures, and the principal difficulty of the whole of this more recent part of the research has been the attainment of this object as near as possible.

The most prominent and annoying form in which this difficulty has presented itself has been the following:—If I allow the sun's rays to be reflected up from the mirror into the culture-cell, in order to brightly illuminate the growing bacillus, the heat-rays at the red-end of the spectrum become effective in raising the temperature of the culture; whereas the culture on the other microscope, protected from the light, is also sheltered from these heat rays, and consequently the two cultures are not simply differing by *one* condition—it is not true that the one is growing merely in the dark and the other merely in the light, but the one is growing in the light *at a higher temperature* than the other.

This and similar difficulties had to be overcome, and it was clear that some means must be devised for recording the temperature *inside* the culture cell, and this I accomplished by placing a very small delicate thermometer, with its bulb blackened, *inside the culture cell itself*, so that each time when the register of the length of the bacillus was taken, the temperature of the culture itself was also recorded.

[Photograph and description of the control culture cells.]

By these means I have now succeeded in comparing the effect of light of even low intensity by direct observations of the bacillus itself, under high powers of the microscope, and in comparing the curves of growth under various conditions of light and of temperature, and this, moreover, on a Thames bacillus, which is among the least sensitive of those which have yet come under my notice.

Since the details of this investigation have not yet been published, however, I must content myself with a few references only.

Just to illustrate how strikingly different the curves of growth in darkness and in the light may be, however, I now throw on the screen the curves obtained by measuring at intervals the growth of two cultures of this bacillus, one under a microscope fully exposed to the light, and the other under similar conditions, but in the dark.

[Photograph of curves shown.]

The temperatures recorded by the thermometers are given also; but I do not intend to go into this matter here, as I have a large number of records bearing on the matter—which is, moreover, an extremely complex one. I wish you to understand, however, that the enormous difference between these two curves is principally due to the differences in illumination, and that, as you see, the bacillus in the light took as long to grow 200 units of length as that in the dark did to grow 560 of the same units—in other words, the light *retards* the growth even of the actively vegetating bacillus, a fact quite in accordance with what we know of the growth of other plants. Moreover, I find the curves of growth differ when the bacilli are examined growing behind coloured screens, and in just the same way as we should expect from the foregoing.

The curves now shown were obtained by measuring the growth of two cultures in the light, one behind a blue glass, the other behind a red one, and alum screens were employed to diminish the heating effect. Here, where bright sunlight was used, it took the bacillus in blue light the same time to grow 50 units of length that the one in red light required to grow over 1200 units.

[Photograph of curves of growth in red and blue light.]

And a large number of similar experiments all agree in pointing to the same end—the light acts as a retarding agent on the growth of the vegetative bacillus, as well as injuriously on the germinating power of the spores. In both cases it is the blue-violet rays which are effective, and if these rays are sufficiently intense, or act for a sufficiently long period, they kill the organism in the manner we have already seen.

Undoubtedly the most interesting results have been some of those obtained with light of relatively low intensity, acting on the spores themselves. First let me show you the curves of growth of three cultures of a bacillus from the Thames which I have been studying for some time, in which the spores had been dried by placing them at 80° C. in a hot oven, and allowing the temperature to fall slowly—it took two hours to cool to 30° C., as the best proof I can give you that these roasting hot temperatures do not injure the life of the spore, and that the results to be described are in no way due to temperature.

[Curves of growth of spores dried at high temperatures.]

As the curves show, these spores germinate as rapidly and well as if merely ripened at the ordinary temperature and at once sown.

I may also add that the spores of the bacillus I am showing you will endure the broth or water, in which they are suspended, being raised to boiling point for a minute, and may be kept at 55° to 60° C., and even higher temperatures, for many hours without apparent damage, though they do not survive prolonged boiling. Here is sufficient evidence, however, to prove that we are not dealing with spores which are at all tender to high temperatures, but with forms which are wholly unscathed by any such temperatures as they have to endure on exposure to the sunlight.

Yet an exposure of half-an-hour or so to the direct rays of the sun kills these spores outright.

But direct sunlight is not necessary, as the following experiment shows. Four culture cells were prepared as described, each hanging drop having a small number (about 15 to 25) of spores in it, and placed each with a thermometer control cell as follows. One was under a darkened bell jar; a second under a large flat-bottomed glass dish filled with clear water; a third under a similar dish filled with deep-blue ammoniacal solution of cupric oxide; and the fourth under a similar dish of deep orange-coloured solution of potassium bichromate. [Demonstration of methods of exposure.] The first, therefore, was excluded altogether from the light, while the second received all the light passing through water and clear glass. The third was exposed to the feeble light passing through the very deep-coloured copper solution, and from which all red and yellow rays were cut off; while the fourth was exposed to the red and yellow rays only, all the blue-violet being excluded by the screen.

Now mark the conditions of exposure, as recorded on the chart. They were all outside, at the north, and in the shade of a building, so that no direct sunlight reached them at all—only the lights from the blue sky and clouds; moreover the temperature was low, and so nearly the same in each culture cell as that of the surrounding air, that even the most sceptical critic would not ascribe the results to the minute differences, even if we did not know from the other evidence that he would be wrong in doing so.

These cultures were exposed for five hours as described, and then all brought into the laboratory and placed in the dark at the same temperature—21° C.—a temperature chosen as being the most favourable for their further development. After a sufficient interval of time, the lengths of all the germinated bacilli in the drops were measured, and the means and averages taken, and plotted on sectional paper, with the results now shown.

[Curves of average growth of these thrown on screen.]

The results were that the spores which had been excluded from the light and those which had been sheltered behind the bichromate from all blue-violet rays, germinated out normally, and the bacilli rapidly developed and grew; those exposed to the blue rays (though of feeble intensity) were so far retarded, that the curve of growth is markedly depressed; while those exposed to the more intense light coming through the clear water, and therefore to more intense light containing more of the blue and violet, were killed altogether, for they refused to germinate in all cases and even after several days' nursing.

Now the special interest of this new line of investigation I have been pursuing turns largely on the fact that this particular bacillus, which is not uncommon in the river Thames, is by no means a very sensitive one to light, as compared with several others known to me.

For if such a form can be so easily destroyed in a few hours by the light from an April sky passing through several inches of water and two sheets of glass, we may fairly conclude that more sensitive forms are easily destroyed in the river, exposed to the rays of a summer sun during the long days of June, July and August.

As matter of fact—and the fact has been confirmed by many other observations—I found that in spite of the high temperature of the past summer, which we should expect to favour the development of bacteria; and in spite of the river being low, and presumably more concentrated as a food-solution for bacteria; and, further, in spite of the fair expectation that, per unit of volume, the water ought to contain more bacteria than during the cold months of October, November and December, when the river is more diluted, and so on; *the number of bacteria per cubic centimeter is decidedly and markedly less in the summer than in the winter months!*

This is very clearly shown by the statistical curves I now throw on the screen.

[Curves of average number of bacteria per 1 cubic centimeter of Thames water in August, October and December, 1893.]

Now I am perfectly aware that it has been the custom to regard this well-known fact as due to a series of causes of quite different kinds, the action of which may or may not co-operate in bringing about the variations in the quantities of bacteria per unit of volume in such a river as the Thames, but I submit to you whether, in view of the new facts I have elicited, it is not at least highly probable that the increased intensity of the light, acting during a longer period through the summer days, is a very powerful agent in keeping down the numbers of these bacterial and fungoid organisms in the river.

It must be remembered that in my experiments, although I get such pronounced positive results wherever I employ water enclosed in glass vessels as the screen, I certainly do not get anything like the effects that the same exposures give *if I dispense with the glass* of the containing vessels, mirrors, &c.; for even a thin plate of glass is a decided obstruction to the rays at the violet end of the spectrum, and so I have to record positive results considerably below what I should get if the light from the sun and blue sky reached the organisms through the medium of water only. In the experiments I am now continuing, no glass whatever is employed, and the light is not allowed to traverse or be reflected from any but quartz or metal surfaces.

Unfortunately, we are as yet but scantily informed as to how far the blue-violet rays penetrate into deep water, but experiments by Regnard, Fol, Forel and others have shown that they do penetrate to a considerable depth. Fol, for instance, showed that some light gets down as far as 400 meters, and Forel got effects with photographic paper at 45 to 100 meters in the Lake of Geneva, and so on, depths quite sufficient for my purposes, though I hope that more accurate and extended information will yet be obtained on the subject.

Some consequences of this view of the destruction of bacteria in water by light are of the utmost importance. For instance, we can understand how the typhoid bacillus, if it falls into turbid dirty water in the summer, may multiply at the high temperature and at the expense of the food-materials dissolved from the organic matters in suspension, and this at its pleasure, so to speak, because the suspended particles of matter, to which the turbidity is due, favour it in (1) supplying it with food, (2) absorbing heat and helping to raise the temperature of the water, and (3) *impeding the penetration of the destructive light-rays.*

The results of the investigation also suggest explanations for many other facts which have hitherto been unexplained. Thus Pasteur and Miquel pointed out some time ago that the germs floating about in the air *are for the most part dead!* This we can explain by the bactericidal action of the sun's rays, just as we can also probably explain the freedom from germs of the Alps.

Again, Martinaud has shown that certain yeasts which normally vegetate on the exterior of ripening grapes, are destroyed if the sunshine is very intense; and Giunti has stated that the ingress of sunlight hinders acetic fermentation, a process which depends on the life-processes of a well-known bacterium.

I have myself observed many cases where the free access to light is inimical to the germination and development of fungi of various kinds, and Elfving has recently studied very thoroughly the action of light on a common mould-fungus with astonishing results, quite intelligible in the light of the foregoing investigation. These matters are of more significance in connection with the spread of the potato and other diseases in dull warm weather than I have time to explain here; and probably have a wide bearing on the deterioration of forest and agricultural soils exposed to the sun, as well as on many questions of greenhouse practice too numerous to even mention.

They also bear on the question of sun-baths, and the whole hygiene of sunshine treatment; as well as sun-burn, "tanning," snow-blindness, &c. But still more startling probabilities must be looked at, though I have brought my lecture now to a point where I must remember that your patience in listening to me should not be abused.

We have seen that in my experiments certain colour-screens are efficacious in so cutting off the access of the destructive rays, that the protected germs, bacilli, fungus-spores, &c., can germinate and develop as easily as if no light at all was playing on them. It struck me during the progress of these experiments that *just such colour-screens are very common in Nature,* in cases where just such spores and tender growing cells are compelled to begin or carry on their vegetative processes in the light. This led me a short time ago to publish the following opinions based on the facts even then to hand:—It seems likely that pollen-grains, many fungus-spores and a large number of such organisms as are exposed to light, are provided with the colour screens they possess, *and which screens are usually red or orange, or some shade such as cuts out the blue-violet rays*, as an adaptive protection against the injurious rays of light. It is but an extension of this to see in the green chlorophyll-screen of all our ordinary plants in part a protection against the blue-violet rays of ordinary sunlight, the ingress of which to the cell-contents would bring about destructive changes of the order discussed in this lecture; but we must not forget that the chlorophyll-apparatus is more complex than this, and is essential to the process of carbon assimilation, absorbing and making use of the energy derived from the red-orange rays.

In conclusion, it is impossible to put forward in a single lecture all the results obtained, and still less the bearings of these results and the experiments now being continued. I can merely say that there is evidence to show that the slow and continued action of even comparatively low intensities of light so act on these bacteria I am working with that even where they are not killed, but only partially injured by the inimical rays, their behaviour is so altered that the resulting growths are perceptibly different from those of the same organisms not exposed to light, that their powers of fermenting organic substances are interfered with, and that various profound morphological and physiological changes are induced in them. The bearings of these matters—which are very complex, and the investigation of which is full of peculiar difficulties moreover, are so important in regard to various questions concerning fermentation, the germ-theory of disease and other departments of bacteriology, that I have no hesitation in saying that no subject more vital to the interests of mankind exists in the whole domain of biological science.

[H. M. W.]

ANNUAL MEETING,

Tuesday, May 1, 1894.

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 1893, testifying to the continued prosperity and efficient management of the Institution, was read and adopted. The Real and Funded Property now amounts to above 102,000*l.* entirely derived from the Contributions and Donations of the Members and of others appreciating the value of the work of the Institution.

Sixty-two new Members were elected in 1893.

Sixty-three Lectures and Nineteen Evening Discourses were delivered in 1893.

The Books and Pamphlets presented in 1893 amounted to about 250 volumes, making, with 584 volumes (including Periodicals bound) purchased by the Managers, a total of 834 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. LL.D. F.R.S. M. Inst. C.E.

Managers.

Sir Frederick Abel, Bart. K.C.B. D.C.L. LL.D. F.R.S.
Captain W. de W. Abney, C.B. D.C.L. F.R.S.
The Right Hon. Lord Belhaven and Stenton.
John Birkett, Esq. F.R.C.S.
Edward Frankland, Esq. D.C.L. LL.D. F.R.S.
Sir Douglas Galton, K.C.B. D.C.L. LL.D. F.R.S.
Robert Hannah, Esq.
William Huggins, Esq. D.C.L. LL.D. F.R.S.
Alfred Bray Kempe, Esq. M.A. F.R.S.
George Matthey, Esq. F.R.S.
Ludwig Mond, Esq. F.R.S.
Hugo Müller, Esq. Ph.D. F.R.S.
Sir Andrew Noble, K.C.B. F.R.S. M. Inst. C.E.
William S. Playfair, M.D. LL.D. F.R.C.P.
Basil Woodd Smith, Esq. F.R.A.S. F.S.A.

Visitors.

Charles Edward Beevor, M.D. F.R.C.P.
Francis Woodhouse Braine, Esq. F.R.C.S.
Arthur Carpmael, Esq.
Joseph Gordon Gordon, Esq. F.C.S.
Carl Haag, Esq.
Donald William Charles Hood, M.D. F.R.C.P.
John Imray, Esq. M.A. M. Inst. C.E.
Raphael Meldola, Esq. F.R.S.
Hugh Leonard, Esq.
Lachlan Mackintosh Rate, Esq. M.A.
Boverton Redwood, Esq. F.C.S.
Sir Owen Roberts, M.A. D.C.L. F.S.A.
John Bell Sedgwick, Esq. J.P. F.R.G.S.
Judge Frederick Meadows White, Q.C.
Wm. Henry White, Esq. C.B. LL.D. F.R.S.

WEEKLY EVENING MEETING,

Friday, May 4, 1894.

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

Professor Charles Stewart, M.R.C.S. Pres. L.S.

Sound Production of the Lower Animals.

[Abstract deferred.]

GENERAL MONTHLY MEETING,

Monday, May 7, 1894.

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, Bart. K.C.B. D.C.L. LL.D. F.R.S.
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.
Ludwig Mond, Esq. F.R.S.
Hugo Müller, Esq. Ph.D. F.R.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.*

The Rt. Hon. The Earl of Carnarvon.
Alfred Hurry Dawbarn, Esq.
The Rt. Hon. George Denman.
Edward J. Duveen, Esq.
Richard E. Farrant, Esq.
Henry Grinling, Esq.
Bertram Keightley, Esq. M.A.
Mrs. Adelaide J. Moseley.
F. W. Passmore, Esq. Ph.D.
Henry Rofe, Esq. M. Inst. C.E.
Miss Edith Woodd Smith.
E. Viles, Esq.
The Hon. Sir Robert Samuel Wright, M.A.

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 at Low Temperatures:—

Sir William O. Priestley, M.D.	£50
Sir David L. Salomons, Bart.	£50

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—Report on Public Instruction in Bengal for 1892-93. fol. 1893.
Great Trigonometrical Survey of India: Vol. XV. 4to. 1893.
Annual Progress Report of the Archæological Survey Circle. 4to. 1893.
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Classe di Scienze Morali, etc.: Rendiconti, Serie Quinta, Vol. III. Fasc. 1, 2, 8vo. 1894.
Agricultural Society, Royal—Journal, Vol. V. Part 1. 8vo. 1894.
Aristotelian Society—Proceedings, Vol. II. No. 3, Part 1. 8vo. 1894.
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Batavia Observatory—Magnetical and Meteorological Observations, Vol. XV. fol. 1893.
Rainfall in East Indian Archipelago, 1892. 8vo. 1893.
Bickerton, Professor A. W. (the Author)—A New Story of the Stars, Part 1. 8vo. 1894.
British Architects, Royal Institute of—Journal, 3rd Series, Vol. I. Nos. 11, 12. 4to.
British Astronomical Association—Journal, Vol. IV. No. 5. 8vo. 1894.
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Camera Club—Journal for April, 1894. 8vo.
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Analyst for April, 1894. 8vo.
Athenæum for April, 1894. 4to.
Brewers' Journal for April, 1894. 8vo.
Chemical News for April, 1894. 4to.
Chemist and Druggist for April, 1894. 8vo.
Electrical Engineer for April, 1894. fol.
Electrical Review for April, 1894. 8vo.
Electric Plant for April, 1894. 4to.
Engineer for April, 1894. fol.
Engineering for April, 1894. fol.
Horological Journal for April, 1894. 8vo.
Industries and Iron for April, 1894. fol.
Ironmongery for April, 1894. 4to.
Machinery Market for April, 1894. 8vo.
Monist for April, 1894. 8vo.
Nature for April, 1894. 4to.
Nuovo Cimento for March, 1894. 8vo.
Open Court for April, 1894. 4to.
Photographic News for April, 1894. 8vo.
Photographic Work for April, 1894. 8vo.
Scots Magazine for April, 1894. 8vo.

Editors—continued.
Transport for April, 1894.
Tropical Agriculturist for April, 1894.
Work for April, 1894. 8vo.
Zoophilist for April, 1894. 4to.
Electrical Engineers, Institution of—Journal, Vol. XXII. No. 110. 8vo. 1894.
Ellis and Elvey, Messrs. (the Publishers)—General Catalogue of Rare Books and MSS. 8vo. 1894.
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Johns Hopkins University—American Journal of Philology, Vol. XV. No. 1. 8vo. 1894.
American Chemical Journal, Vol. XVI. No. 4. 8vo. 1894.
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The Dentists' Register for 1894. 8vo.
Ministry of Public Works, Rome—Giornale del Genio Civile, 1894, Fasc. 1. 8vo. And Designi. fol. 1894.
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Transactions, Vol. XIV. 8vo. 1894.
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Sociedad Cientifica "Antonio-Alzate," Mexico—Memorias, Tomo VII. Num. 7, 8. 8vo. 1894.
Société Archéologique du Midi de la France—Bulletin, No. 12. 8vo. 1893.
Society of Architects—Journal, New Series, Vol. I. No. 6. 4to. 1894.

Society of Arts—Journal for April, 1894. 8vo.
Statistical Society, Royal—Journal, Vol. LVII. Part 1. 8vo. 1894.
St. Petersburg Imperial Academy of Sciences—Mémoires, Tome XLI. Nos. 4, 5. 4to. 1893.
Tacchini, Prof. P. Hon. Mem. R.I. (the Author)—Memorie della Società degli Spettroscopisti Italiani, Vol. XXIII. Disp. 3ª. 4to. 1894.
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  Experiment Station Record, Vol. V. Nos. 5–7. 8vo. 1894.
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  Proceedings. 1893, Part 4. 8vo. 1894.
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WEEKLY EVENING MEETING,

Friday, May 11, 1894.

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

The Rev. S. Baring-Gould, M.A.

English Folk Song.

It has been a received commonplace that we English have no folk music of our own. Nothing can be more erroneous. We have had our folk song and music as truly as the Scots, the Irish, the Welsh and the Germans. Unhappily, though there have been many collectors of English ballads and of old English music, such collectors have not gone to the people themselves, but to the shelves of the British Museum, or have entrusted second-hand booksellers to gather for them at sales such sheets of broadside or engraved and printed music as came into the market. It is one thing to go into the reading-room of the British Museum, and in a few days to pick out a sufficient number of ballads to form a book of English ballads, or of airs engraved in old music books, to form a volume of English song; it is quite another thing altogether to hunt up old singers, few and far between, only to be heard of after diligent search, and, when found, to win their confidence, overcome their reserve, and get them to disclose the treasures buried in the inmost recesses of their memories. Not only is this difficult, and a matter of much time and tact, but it is also expensive work, requiring long journeys and lodging at inns, entertainment of the singers, and also much patience under disappointment. Yet this is positively the only way in which the folk song of the English peasantry can be got at. The songs published in collections purporting to represent the folk airs of England are precisely those they do *not* sing. The songs of our peasantry have been, till quite recently, as little printed as have been those of the lark, the blackbird and the thrush.

Folk airs go through strange mutations. An old English melody has crossed into Ireland and comes back as "The Wearing of the Green"; "Paul's Steeple" has passed the border, and also St. George's Channel, and has become in one place "John Anderson, my Jo," and in the other "The Cruiskeen Lawn"; "The Pride of Kildare" has become extremely solemn as "The Story of the Cross," sung in Holy Week; and there is a hymn in Ancient and Modern I never like to hear, as the melody is that of the schoolboy song, "When I was a small boy, just fifteen, then very little Greek I knew." The song writers of the end of last century and the beginning of this took many of our folk airs, altered them slightly, and adapted to them humorous songs of their own composition. Such were Clifton, Beuler, Hudson, Sam Cowell, &c. Good old English airs were vulgarised to such words as "Villikins and his Dinah," and "Billy Barlow," and "Ben was a Hackney-coachman rare."

It is in vain to look through the printed music that issued from the press from the time of Tom Durfey to find the melodies most dear to the hearts of the old singers in country places. Some of their airs are older than any that have been published. One man I know sings nothing that is not in an old church mode—a hypo-Dorian or mixolydian melody suits him down to the ground. He cannot abide a tune of the modern sort. But it is not so with all. They have had to accommodate themselves to the altered taste of the times, and they sing songs of all dates.

In my Introduction to "Songs of the West" I expressed a doubt whether the melodies were not heirlooms from a cultured past. I doubted their being genuine productions of the folk muse. Mr. Sheppard differed from me, and further consideration and research induce me to modify my opinion.

It is the impossibility of tracing these airs, either in the printed or the engraved songs of the past, that makes me think that a good many of them may have originated among the people themselves. Their melodies are of all ages and styles. There are those that savour of the Elizabethan madrigal, there are others distinctly earlier, old minstrel-ballad melodies. There are tunes that bear the impress of the age of Purcell and Arne and Green, and others that are Dibdinian. Yet none of these can be found among compositions of these masters.

The words are those of the peasant poet. We can find them in the halfpenny broadside, often grossly mutilated, for the broadside ballad is the echo, not the original. It is therefore likely that the airs as well sprang spontaneously from the joyous or sad hearts of the people. Their sorrows and their mirth found natural expression in song. They were like the birds of the air, they sang because it was a necessity to sing.

But then they sang according to the style of singers in vogue, as they talked and dressed in the prevailing fashion, perhaps always a little after a fashion had passed. Now and then they accepted songs that had been composed for them, not often, but occasionally, and sometimes songs that never had been intended for them, but which hit their fancy. When such came to them and were accepted, they generally modified them after their own taste.

There is one feature in our English peasant song which makes it essentially worth preservation, This is its entire genuineness. The French *chanson populaire*, according to M. Loquin, is entirely derived from the opera music of the last century. The German Volks-Lieder-Schatz is made up to a vast extent of compositions by cultured musicians of the end of last century and the beginning of the nineteenth, as Kreuzer, Nägeli, Reichardt, Silcher, Weber, &c., and only a small percentage are really spontaneous productions of the people themselves. I do not assert that the peasant song in England is utterly uncultured music; I believe that there has always been, and in almost every village, an element of musical culture, fostered by the parish orchestra in the west gallery of the church; and that most—nearly all—of our folk airs have been composed by men of no musical note beyond their own parishes, but not without musical knowledge—men with hearts full of melody and heads with a knowledge of musical rule, and themselves belonging to the peasant class.

There is something even in music to be learned from our healthy English peasantry. None of the mawkishness of the French muse, but a robustness, a freshness, a joyousness, sometimes a pathos, in character with the people from whom these airs sprang.

If a musician desires to enjoy an unusual holiday, and reap a good and delightful harvest, let him take my advice. Let him put on an old coat and hat and go on a tramp through England, lodging at little taverns, and associate with the labourers in fields and over the tavern table, and about the tavern fire.

There is no time to be lost. Every winter with its storms sweeps away some of our old singers. The young know nothing; the middle-aged nothing. I sat over a peat fire with a Scottish shepherd one night and asked him to sing to me, and he gave me nothing but music-hall balderdash. But then he was a young man. Many of the oldest singers can no longer be heard in the public-houses, they must be sought out in their humble homes.

A good many of the folk songs being in Gregorian modes, or having startling intervals, and being irregular in construction, puzzle the ear. It is some time before we become accustomed to the new style—or rather to the old style, so new to us accustomed to the dilute treacle of the modern song. But it is worth while becoming acquainted with it.

Finally, even suppose that this rustic muse of ours have not all the charms I claim for her, suppose that she be a little lacking in the languor of the drawing-room and the affectation of the stage, yet surely the Englishman should regard her with respect, with love, and say of her, as said Touchstone of Audry, "A poor thing, sir—but mine own."

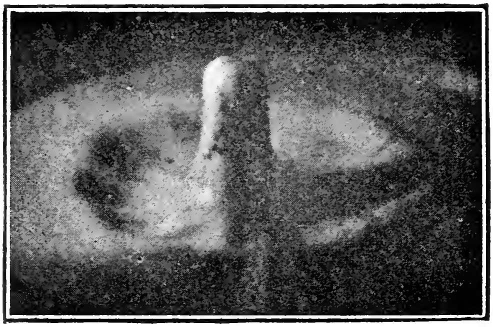
[S. B.-G.]

INSTANTANEOUS PHOTOGRAPHS

OF THE SPLASH OF A WATER-DROP FALLING INTO MILK.

Time after contact = ·0262 sec.

Time after contact = ·0391 sec.



WEEKLY EVENING MEETING,

Friday, May 18, 1894.

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

Professor A. M. Worthington, M.A. F.R.S.

The Splash of a Drop and Allied Phenomena.

The splash of a drop is a transaction which is accomplished in the twinkling of an eye, and it may seem to some that a man who proposes to discourse on the matter for an hour must have lost all sense of proportion. If that opinion exists, I hope this evening to be able to remove it and to convince you that we have to deal with an exquisitely regulated phenomenon, and one which very happily illustrates some of the fundamental properties of fluids. It may be mentioned also that the recent researches of Lenard in Germany and J. J. Thomson at Cambridge, on the curious development of electrical charges that accompanies certain kinds of splashes, have invested with a new interest any examination of the mechanics of the phenomenon. It is to the mechanical and not to the electrical side of the question that I shall call your attention this evening.

The first well directed and deliberate observations on the subject that I am aquainted with were made by a schoolboy at Rugby some twenty years ago, and were reported by him to the Rugby Natural History Society. He had observed that the marks of accidental splashes of ink-drops that had fallen on some smoked glasses with which he was experimenting, presented an appearance not easy to account for. Drops of the same size falling from the same height had made always the same kind of mark, which when carefully examined with a lens showed that the smoke had been swept away in a system of minute concentric rings and fine striæ. Specimens of such patterns, obtained by letting drops of mercury, alcohol and water fall on to smoked glass, are thrown on the screen, and the main characteristics are easily recognised. Such a pattern corresponds to the footprints of the dance that has been performed on the surface, and though the drop may be lying unbroken on the plate, it has evidently been taking violent exercise, and were our vision acute enough we might observe that it was still palpitating after its exertions.

A careful examination of a large number of such footprints showed that any opinion that could be formed therefrom of the nature of the motion of the drop must be largely conjectural, and it occurred to me about eighteen years ago to endeavour by means of the illumination of a suitably timed electric spark to watch a drop through its various changes on impact.

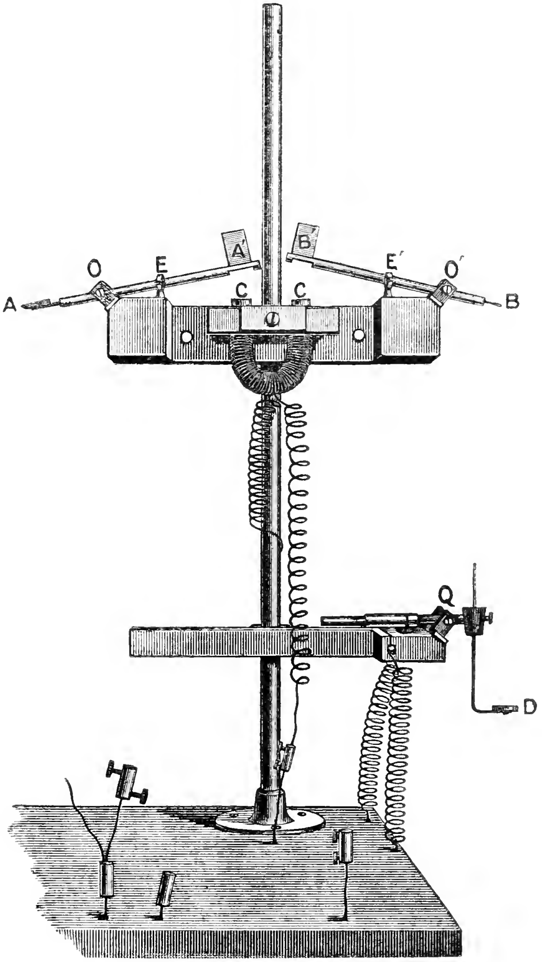
The reason that with ordinary continuous light nothing can be satisfactorily seen of the splash, is not that the phenomenon is of such short duration, but because the changes are so rapid that before the image of one stage has faded from the eye the image of a later and quite different stage is superposed upon it. Thus the resulting impression is a confused assemblage of all the stages, as in the photograph of a person who has not sat still while the camera was looking at him. The problem to be solved experimentally was therefore this: to let a drop of definite size fall from a definite height in comparative darkness on to a surface, and to illuminate it by a flash of exceedingly short duration at any desired stage, so as to exclude all the stages previous and subsequent to the one thus picked out. The flash must be bright enough for the image of what is seen to remain long enough on the eye for the observer to be able to attend to it, even to shift his attention from one part to another, and thus to make a drawing of what is seen. If necessary the experiment must be capable of repetition, with an exactly similar drop falling from exactly the same height, and illuminated at exactly the same stage. Then, when this stage has been sufficiently studied, we must be able to arrange with another similar drop to illuminate it at a rather later stage, say $\frac{1}{1000}$ second later, and in this way to follow step by step the course of the whole phenomenon.

The apparatus by which this has been accomplished is on the table before you. Time will not suffice to explain how it grew out of earlier arrangements very different in appearance, but its action is very simple and easy to follow by reference to the diagram (Fig. 1).

A A′ is a light wooden rod rather longer and thicker than an ordinary lead pencil, and pivoted on a horizontal axle O. The rod bears at the end A a small deep watch-glass, or segment of a watch-glass, whose surface has been smoked, so that a drop even of water will lie on it without adhesion. The end A′ carries a small strip of tinned iron, which can be pressed against and held down by an electromagnet C C′. When the current of the electromagnet is cut off the iron is released, and the end A′ of the rod is tossed up by the action of a piece of india-rubber stretched catapult-wise across two pegs at E, and by this means the drop resting on the watch-glass is left in mid-air free to fall from rest.

B B′ is a precisely similar rod worked in just the same way, but carrying at B a small horizontal metal ring, on which an ivory timing sphere of the size of a child's marble can be supported. On cutting off the current of the electromagnet the ends A′ and B′ of the two levers are simultaneously tossed up by the catapults, and thus drop and sphere begin to fall at the same moment. Before, however, the drop reaches the surface on which it is to impinge, the timing sphere strikes a plate D attached to one end of a third lever pivoted at Q, and thus breaks the contact between a platinum wire bound to the under side of this lever and another wire crossing the first at right angles. This action breaks an electric current which has traversed a second electromagnet F (Fig. 2), and releases the iron armature N

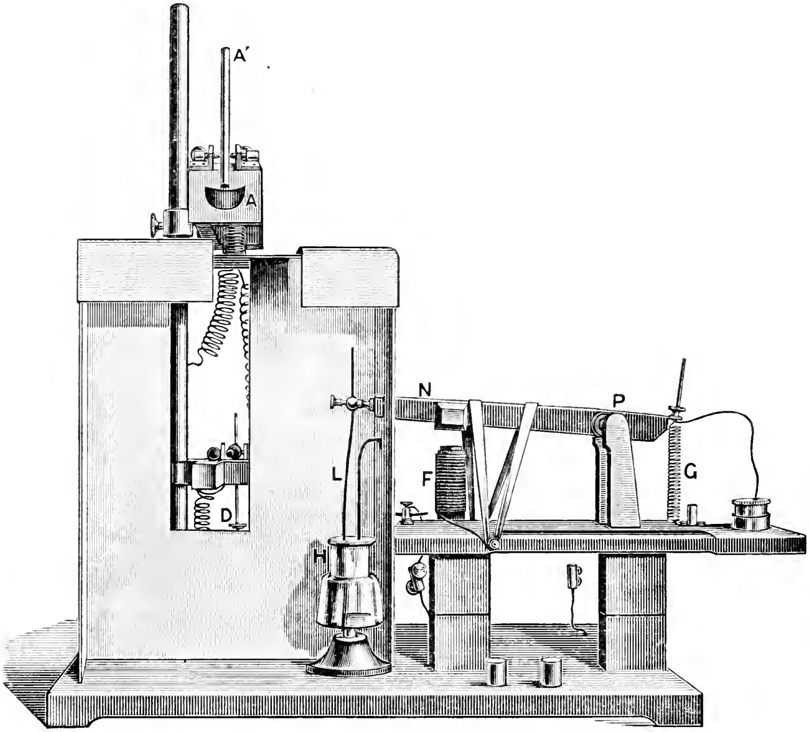
Fig. 1.



of the lever N P, pivoted at P, thus enabling a strong spiral spring G to lift a stout brass wire L out of mercury, and to break at the surface of the mercury a strong current that has circulated round the primary circuit of a Ruhmkorff's induction coil; this produces at the surface of the mercury a bright self-induction spark in the neighbourhood of the splash, and it is by this flash that the splash is viewed. The illumination is greatly helped by surrounding the place where the splash and flash are produced by a white cardboard enclosure, seen in Fig. 2, from whose walls the light is diffused.

It will be observed that the time at which the spark is made will depend on the distance that the sphere has to fall before striking the plate D, for the subsequent action of demagnetising F and pulling the wire L out of the mercury in the cup H is the same on each occasion. The modus operandi is consequently as follows:—The observer, sitting in comparative but by no means complete darkness, faces the apparatus as it appears in Fig. 2, presses down the ends A′ B′ of the levers first described, so that they are held by the electromagnet C (Fig. 1). Then he presses the lever N P down on the electromagnet F, sets the timing sphere and drop in place, and then by means of a bridge between two mercury cups, short-circuits and

Fig. 2.



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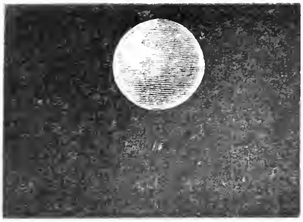

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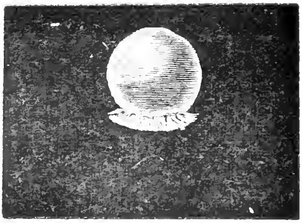

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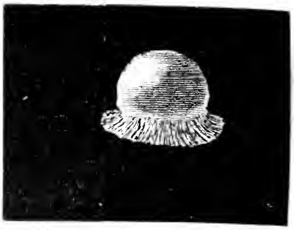

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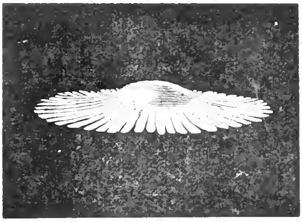

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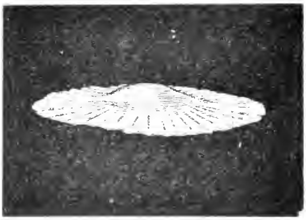

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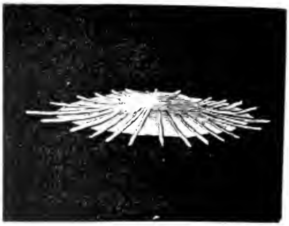

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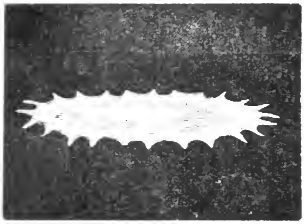

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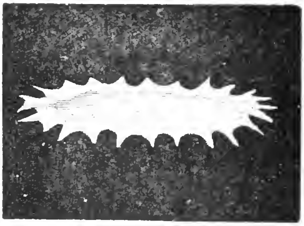

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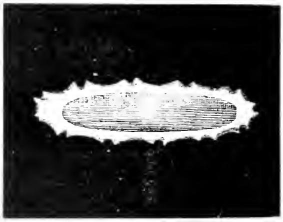

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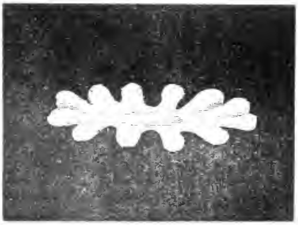

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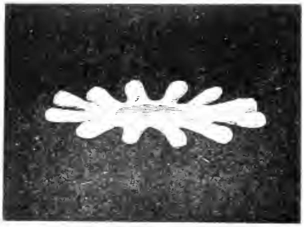

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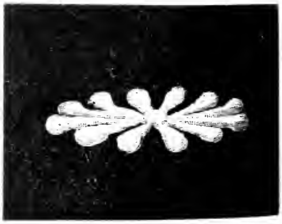

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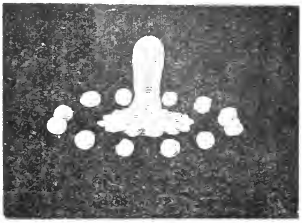

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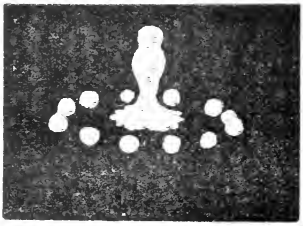

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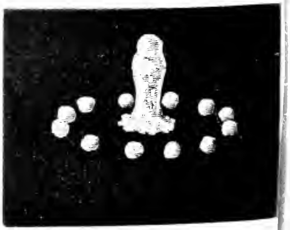

SERIES.

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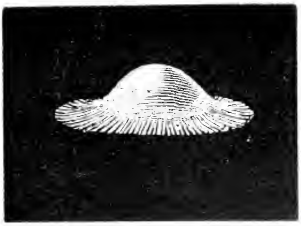

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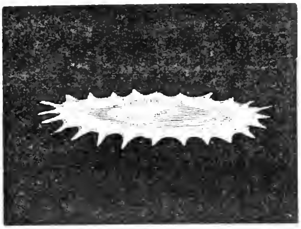

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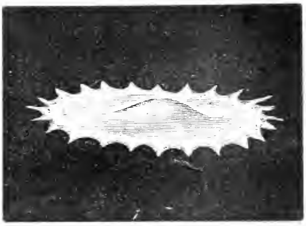

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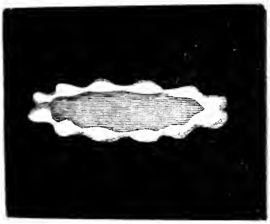

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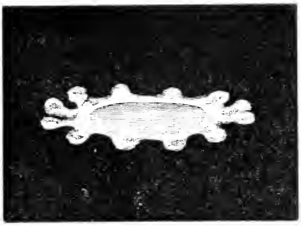

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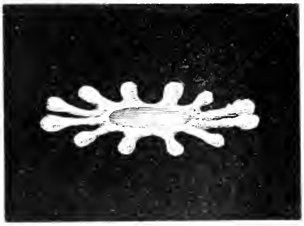

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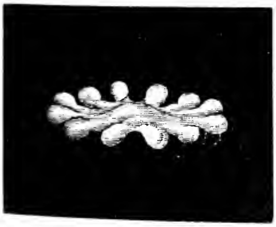

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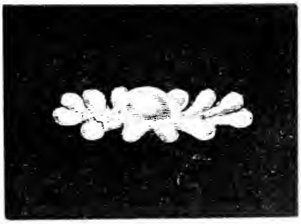

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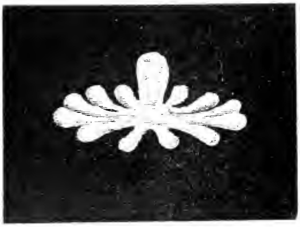

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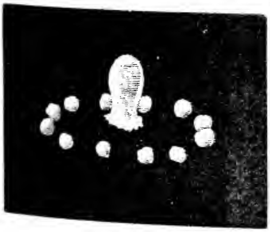

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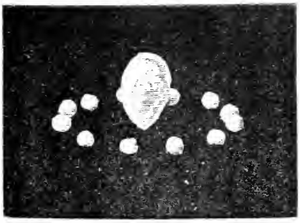

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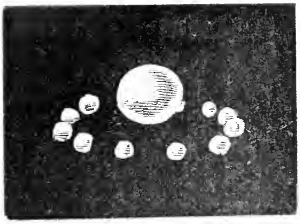

thus cuts off the current of the electromagnet C. This lets off drop and sphere, and produces the flash. The stage of the phenomenon that is thus revealed having been sufficiently studied by repetition of the experiment as often as may be necessary, he lowers the plate D a fraction of an inch and thus obtains a later stage. Not only is any desired stage of the phenomenon thus easily brought under examination, but the apparatus also affords the means of measuring the time interval between any two stages. All that is necessary is to know the distance that the timing sphere falls in the two cases. Elementary dynamics then give us the interval required. Thus, if the sphere falls one foot and we then lower D $\frac{1}{4}$ inch, the interval between the corresponding stages will be about ·0026 second.

Having thus described the apparatus, which I hope shortly to show you in action, I pass to the information that has been obtained by it.

This is contained in a long series of drawings, of which a selection will be presented on the screen. The First Series that I have to show represents the splash of a drop of mercury 0·15 inch in diameter that has fallen 3 inches on to a smooth glass plate. It will be noticed that very soon after the first moment of impact, minute rays are shot out in all directions on the surface. These are afterwards overflowed or united, until, as in Fig. 8, the outline is only slightly rippled. Then (Fig. 9) main rays shoot out, from the ends of which in some cases minute droplets of liquid would split off, to be left lying in a circle on the plate, and visible in all subsequent stages. By counting these droplets when they were thus left, the number of rays was ascertained to have been generally about 24. This exquisite shell-like configuration shown in Fig. 9, marks about the maximum spread of the liquid, which, subsiding in the middle, afterwards flows into an annulus or rim with a very thin central film, so thin, in fact, as often to tear more or less irregularly. This annular rim then divides or segments (Figs. 14, 15, 16) in such a manner as to join up the rays in pairs, and thus passes into the 12-lobed annulus of Fig. 16. Then the whole contracts, but contracts most rapidly between the lobes, the liquid then being driven into and feeding the arms, which follow more slowly. In Fig. 21 the end of this stage is reached, and now the arms continuing to come in, the liquid rises in the centre; this is, in fact, the beginning of the rebound of the drop from the plate. In the case before us the drops at the ends of the arms now break off (Fig. 25), while the central mass rises in a column which just fails itself to break up into drops, and falls back into the middle of the circle of satellites which, it will be understood, may in some cases again be surrounded by a second circle of the still smaller and more numerous droplets that split off the ends of the rays in Fig. 9. The whole of the 30 stages described are accomplished in about $\frac{1}{20}$ second, so that the average interval between them is about $\frac{1}{600}$ second.

It should be mentioned that it is only in rare cases that the subordinate drops, seen in the last six figures, are found lying in a very complete circle after all is over, for there is generally some slight disturbing lateral velocity which causes many to mingle again with the central drop, or with each other. But even if only half or a quarter of the circle is left, it is easy to estimate how many drops, and therefore how many arms there have been. It may be mentioned that sometimes the surface of the central lake of liquid, Figs. 14, 15, 16, 17, was seen to be covered with beautiful concentric ripples, not shown in the figures.

The question now naturally presents itself, why should the drop behave in this manner? In seeking the answer it will be useful to ask ourselves another question. What should we have expected the drop to do? Well, to this I suppose most people would be inclined, arguing from analogy with a solid, to reply that it would be reasonable to expect the drop to flatten itself, and even very considerably flatten itself, and then, collecting itself together again, to rebound, perhaps as a column such as we have seen, but not to form this regular system of rays and arms and subordinate drops.

Now this argument from analogy with a solid is rather misleading, for the forces that operate in the case of a solid sphere that flattens itself and rebounds, are due to the bodily elasticity which enables it not only to resist, but also to recover from any distortion of shape or shearing of its internal parts past each other. But a liquid has no power of recovering from such internal shear, and the only force that checks the spread, and ultimately causes the recovery of shape is the *surface tension*, which arises from the fact that the surface layers are always in a state of extension and always endeavouring to contract. Thus we are at liberty when dealing with the motions of the drop to think of the interior liquid as not coherent, provided we furnish it with a suitable elastic skin. Where the surface skin is sharply curved outwards, as it is at the sharp edge of the flattened disc, there the interior liquid will be strongly pressed back. In fact the process of flattening and recoil is one in which energy of motion is first expended in creating fresh liquid surface, and subsequently recovered as the surface contracts. The transformation is, however, at all moments accompanied by a great loss of energy as heat. Moreover, it must be remembered that the energy expended in creating the surface of the satellite drops is not restored if these remain permanently separate. Thus the surface tension explains the recoil, and it is also closely connected with the formation of the subordinate rays and arms. To explain this it is only necessary to remind you that a liquid cylinder is an unstable configuration. As you know, any fine jet becomes beaded and breaks into drops, but it is not necessary that there should be any flow of liquid along the jet; if, for example, we could realise a rod of liquid of the shape and size of this ruler and liberate it in the air, it would not retain its cylindrical shape, but would segment or divide itself up into a row of drops regularly disposed according to a definite and very simple numerical law, viz. that the distances between the centres of contiguous drops would be equal to the circumference of the cylinder. This can be shown by calculation to be a consequence of the surface tension, and the calculation has been closely verified by experiment. If the liquid cylinder were liberated on a plate, it would still topple into a regular row of drops, but they would be further apart; this was shown by Plateau. Now imagine the cylinder bent into an annulus. It will still follow the same law,* i. e. it will topple into drops just as if it were straight. This I can show you by a direct experiment. I have here a small thick disc of iron, with an accurately planed face and a handle at the back. In the face is cut a circular groove, whose cross section is a semicircle. I now lay this disc face downwards on the horizontal face of the lantern condenser, and through one of two small holes bored through to the back of the disc I fill the groove with quicksilver. Now, suddenly lifting the disc from the plate I release an annulus of liquid, which splits into the circle of very equal drops which you see projected on the screen. You will notice that the main drops have between them still smaller ones, which have come from the splitting up of the thin cylindrical necks of liquid which connected the larger drops at the last moment.

Now this tendency to segment or topple into drops, whether of a straight cylinder or of an annulus, is the key to the formation of the arms and satellites, and indeed to much that happens in all the splashes that we shall examine. Thus in Fig. 12 we have an annular rim, which in Figs. 13 and 14 is seen to topple into lobes by which the rays are united in pairs, and even the special rays that are seen in Fig. 9 owe their origin to the segmentation of the rim of the thin disc into which the liquid has spread. The proceeding is probably exactly analogous to what takes place in a sea wave that curls over in calm weather on a slightly sloping shore. Any one may notice how, as it curls over, the wave presents a long smooth edge, from which at a given instant a multitude of jets suddenly shoot out, and at once the back of the wave, hitherto smooth, is seen to be furrowed or "combed." There can be no doubt that the cylindrical edge topples into alternate convexities and concavities; at the former the flow is helped, at the latter hindered, and thus the jets begin, and special lines of flow are determined. In precisely the same way the previously smooth circular edge of Fig. 8 topples, and determines the rays and lines of flow of Fig. 9.

Before going on to other splashes I will now endeavour to reproduce a mercury splash of the kind I have described, in a manner that shall be visible to all. For this purpose I have reduplicated the apparatus which you have seen, and have it here so arranged that I can let the drop fall on to the horizontal condenser plate of the lantern, through which the light passes upwards, to be afterwards thrown upon this

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* See Worthington on the "Spontaneous Segmentation of a Liquid Annulus," Proc. Roy. Soc. No. 200, p. 49 (1879).

screen. The illuminating flash will be made inside the lantern, where the arc light would ordinarily be placed. I have now set a drop of mercury in readiness and put the timing sphere in place, and now if you will look intently at the middle of the screen I will darken the room and let off the splash. (The experiment was repeated four or five times, and the figures seen were like those of Series X.) Of course all that can be shown in this way is the outline, or rather a horizontal section of the splash; but you are able to recognise some of the configurations already described, and will be the more willing to believe that a momentary view is after all sufficient to give much information if one is on the alert and has acquired skill by practice.

The general features of the splash that we have examined are not merely characteristic of the liquid mercury, but belong to all splashes of a liquid falling on to a surface which it does not wet, provided the height of fall or size of the drop are not so great as to cause complete disruption,* in which case there is no recovery and rebound. Thus a drop of milk falling on to smoked glass will, if the height of fall and size of drop are properly adjusted, give forms very similar to those presented by a drop of mercury. The whole course of the phenomenon depends, in fact, mainly on four quantities only: (1) the size of the drop; (2) the height of fall; (3) the value of the surface tension; (4) the viscosity of the liquid.

The next series of drawings illustrates the splash of a drop of water falling into water.

In order the better to distinguish the liquid of the original drop from that into which it falls, the latter was coloured with ink or with an aniline dye, and the drop itself was of water rendered turbid with finely-divided matter in suspension. Finally drops of milk were found to be very suitable for the purpose, the substitution of milk for water not producing any observable change in the phenomenon.

In Series II. the drop fell 3 inches, and was $\frac{1}{5}$ inch in diameter.

[In most of the figures of this and of succeeding series the central white patch represents the original drop, and the white parts round it represent those raised portions of the liquid which catch the light. The numbers at the side of each figure give the time interval in seconds from the occurrence of the first figure, or of the figure marked T = 0.]

It will be observed that the drop flattens itself out somewhat, and descends at the bottom of a hollow with a raised beaded edge (Fig. 2). This edge would be smooth and circular but for the instability which causes it to topple into drops. As the drop descends the hollow becomes wider and deeper, and finally closes over the

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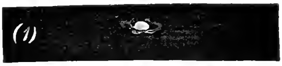
* Readers who wish a more detailed account of a greater variety of splashes are referred to papers by the author. Proc. Roy. Soc. vol. xxv. pp. 261 and 498 (1877); and vol. xxxiv. p. 217 (1882).

SERIES II.

The Splash of a Drop, followed in detail by Instantaneous Illumination.

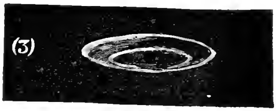
Diameter of Drop,
$\frac{1}{5}$ inch.
Height of Fall,
$3\frac{1}{5}$ inches.

Time in Seconds.



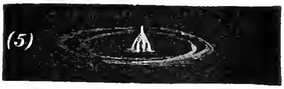
$\tau = 0$

$\tau = 0$



$\tau = \cdot 0097$

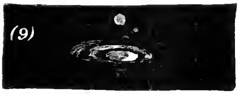
$\tau = \cdot 0392$



$\tau = \cdot 0392$

$\tau = \cdot 0979$

$\tau = \cdot 1095$



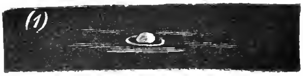
$\tau = \cdot 167$

SERIES III.

The Splash of a Drop, followed in detail by Instantaneous Illumination—cont.

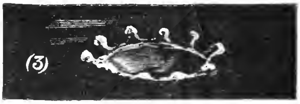
Diameter of Drop, $\frac{1}{5}$ inch. Height of Fall, 1 ft. 5 in.

Time in Seconds.



$\tau = 0$

$\tau = \cdot 00314$

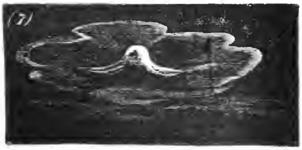


$\tau = \cdot 0317$

$\tau = \cdot 0389$

$\tau = \cdot 0498$

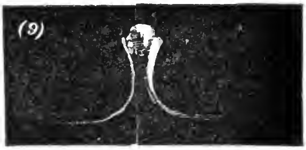
$\tau = \cdot 0551$

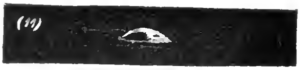


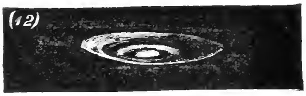
$\tau = \cdot 0759$

Time in Seconds.

$\tau = \cdot 0901$







$\tau = \quad 295$

drop (Fig. 3), which, however, soon again emerges as the hollow flattens out, appearing first near, but still below the surface (Fig. 4), in a flattened, lobed form, afterwards rising as a column somewhat mixed with adherent water, in which traces of the lobes are at first very visible.

The rising column, which is nearly cylindrical, breaks up into drops before or during its subsequent descent into the liquid. As it disappears below the surface the outward and downward flow causes a hollow to be again formed, up the sides of which an annulus of milk is carried, while the remainder descends to be torn again a second time into a vortex ring, which, however, is liable to disturbance from the falling in of the drops which once formed the upper part of the rebounding column.

It is not difficult to recognise some features of this splash without any apparatus beyond a cup of tea and a spoonful of milk. Any drinker of afternoon tea, after the tea is poured out and before the milk is put in, may let the milk fall into it drop by drop from one or two inches above it. The rebounding column will be seen to consist almost entirely of milk, and to break up into drops in the manner described, while the vortex ring, whose core is of milk, may be seen to shoot down into the liquid. But this is better observed by dropping ink into a tumbler of clear water.

Let us now increase the height of fall to 17 inches. Series III. exhibits the result. All the characteristics of the last splash are more strongly marked. In Fig. 1 we have caught sight of the little raised rim of the hollow before it has beaded, but in Fig. 2 special channels of easiest flow have been already determined. The number of ribs and rays in this basket-shaped hollow seemed to vary a good deal with different drops, as also did the number of arms and lobes seen in later figures, in a somewhat puzzling manner, and I have made no attempt to select drawings which are in agreement in this respect. It will be understood that these rays contain little or none of the liquid of the drop, which remains collected together in the middle. Drops from these rays or from the larger arms and lobes of subsequent figures are often thrown off high into the air. In Figs. 3 and 4 the drop is clean gone below the surface of the hollow, which is now deeper and larger than before. The beautiful beaded annular edge then subsides, and in Fig. 5 we see the drop again, and in Fig. 6 it begins to emerge. But although the drop has fallen from a greater height than in the previous splash, the energy of the impact, instead of being expended in raising the same amount of liquid to a greater height, is now spent in lifting a much thicker adherent column to about the same height as in the last splash. There was sometimes noticed, as is seen in Fig. 9, a tendency in the water to flow up past the milk, which, still comparatively unmixed with water, rides triumphant on the top of the emergent column. The greater relative thickness of this column prevents it splitting into drops, and Figs. 10 and 11 show it descending below the surface to form the hollow of

Fig. 12, up the sides of which an annular film of milk is carried (Figs. 12 and 13), having been detached from the central mass, which descends to be torn again, this time centrally into a well-marked vortex ring.

If we keep to the same size of drop and increase the fall to something over a yard, no great change occurs in the nature of the splash, but the emergent column is rather higher and thinner and shows a tendency to split into drops.

When, however, we double the volume of the drop and raise the height of fall to 52 inches, the splash of Series IV. is obtained, which is beginning to assume quite a different character. The raised rim of the previous series is now developed into a hollow shell of considerable height, which tends to close over the drop. This shell or dome is a characteristic feature of all splashes made by large drops falling from a considerable height, and is extremely beautiful. In the splash at present under consideration it does not always succeed in closing permanently, but opens out as it subsides, and is followed by the emergence of the drop (Fig. 8). In Fig. 9 the return wave overwhelms the drop for an instant, but it is again seen at the summit of the column in Fig. 10.

But on other occasions the shell or dome of Figs. 4 and 5 closes permanently over the imprisoned air, the liquid then flowing down the sides, which become thinner and thinner, till at length we are left with a large bubble floating on the water (see Series V.). It will be observed that the flow of liquid down the sides is chiefly along definite channels, which are probably determined by the arms thrown up at an earlier stage. The bubble is generally creased by the weight of the liquid along these channels. It must be remembered that the base of the bubble is in a state of oscillation, and that the whole is liable to burst at any moment, when such figures as 6 and 7 of the previous series will be seen.

Such is the history of the building of the bubbles which big raindrops leave on the smooth water of a lake, or pond, or puddle. It is only the bigger drops that can do it, and reference to the number at the side of Fig. 5 of Series IV. shows that the dome is raised in about two-hundredths of a second. Should the domes fail to close, or should they open again, we have the emergent columns which any attentive observer will readily recognise, and which have never been better described than by Mr. R. L. Stevenson, who, in his delightful 'Inland Voyage,' speaks of the surface of the Belgian canals along which he was canoeing, as thrown up by the rain into "an infinity of little crystal fountains."

Very beautiful forms of the same type indeed, but different in detail, are those produced by a drop of water falling into the lighter and more mobile liquid, petroleum.

It will now be interesting to turn to the splash that is produced when a solid sphere, such as a child's marble, falls into water.

I found to my great surprise that the character of the splash at

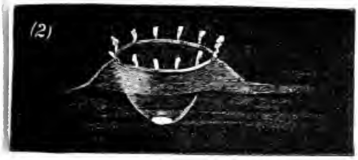
SERIES IV.

The Splash of a Drop, followed in detail by Instantaneous Illumination—cont.

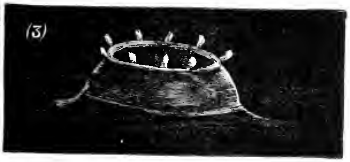
Diameter of Drop, $\frac{1}{4}$ inch. Height of Fall, 4 ft. 4 in.

Time in Seconds.

$\tau = 0$

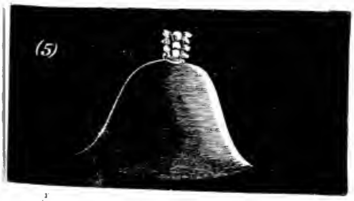


$\tau = \cdot 0021$



$\tau = \cdot 0042$

$\tau = \cdot 0165$

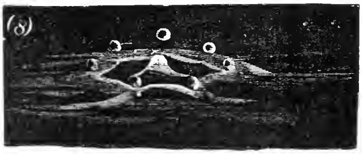


$\tau = \cdot 0206$

Time in Seconds.

$\tau = \cdot 0443$

$\tau = \cdot 0482$



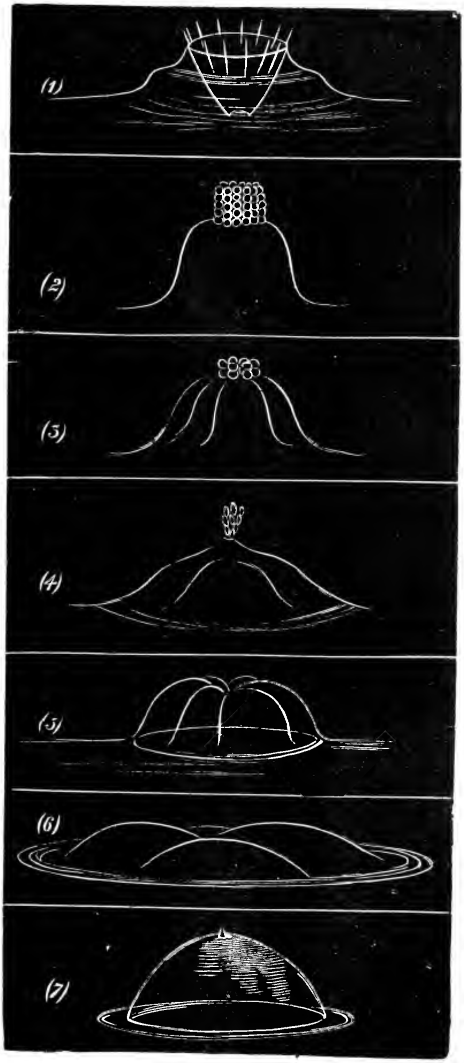
$\tau = \cdot 0595$

$\tau = \cdot 0707$

SERIES V.

The Splash of a Drop, followed in detail by Instantaneous Illumination—continued.

The Size of Drop and Height of Fall are the same as before, but the hollow shell (see figs. 4 and 5 of the previous Series) does not succeed in opening, but is left as a bubble on the surface. This explains the formation of bubbles when *big* raindrops fall into a pool of water.

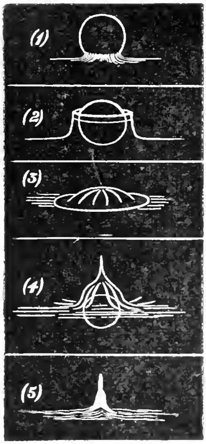


SERIES VI., VII., VIII.

Splash of a Solid Sphere (a marble $\frac{1}{2}$ inch in diameter falling 2 feet into water).

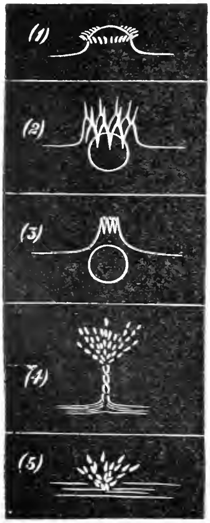
SERIES VI.

When the sphere is *dry* and *polished*.



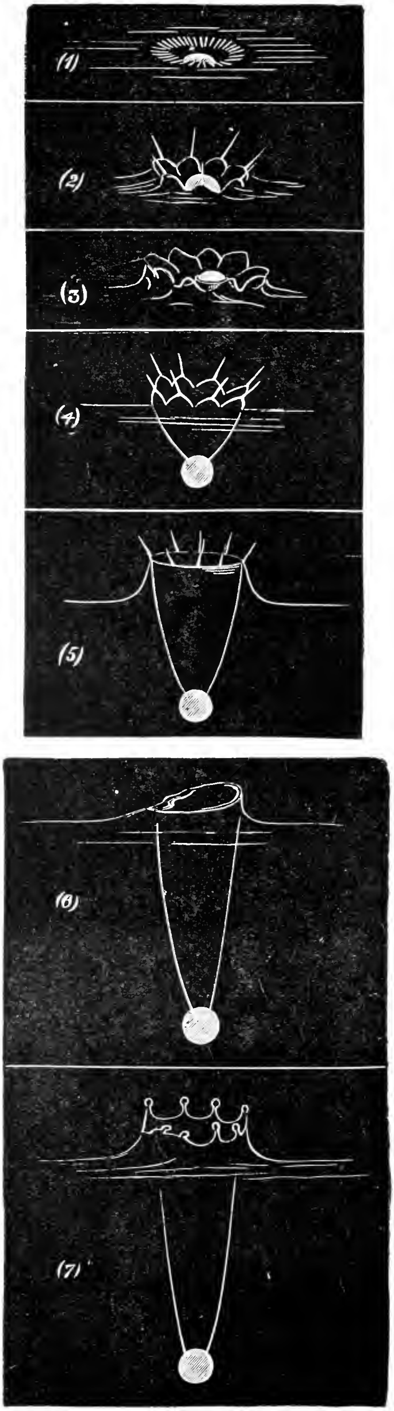
SERIES VII.

When the sphere is *not* well *dried* and *polished*.

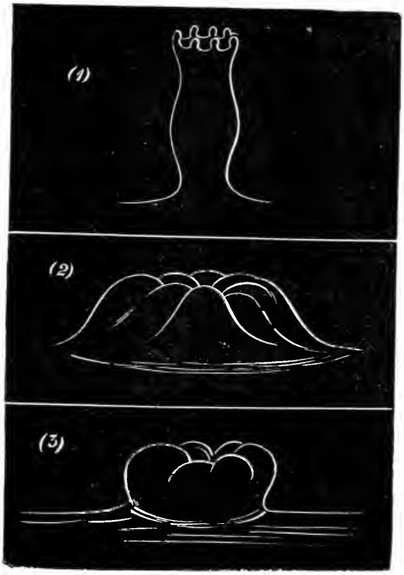


SERIES VIII.

When the sphere is *rough* or *wet*.



SERIES IX.



When the sphere is rough or wet, and falls above 5 feet.

any rate up to a height of fall of 4 or 5 feet, depends entirely on the state of the surface of the sphere. A polished sphere of marble about 0·6 of an inch in diameter, rubbed very dry with a cloth just beforehand and dropped from a height of 2 feet into water, gave the figures of Series VI., in which it is seen that the water spreads over the sphere so rapidly, that it is sheathed with the liquid even before it has passed below the general level of the surface. The splash is insignificantly small and of very short duration. If the drying and polishing be not so perfect, the configurations of Series VII. are produced; while if the sphere be roughened with sand-paper, or *left wet*, Series VIII. is obtained, in which it will be perceived that, as was the case with a liquid drop, the water is driven away laterally, forming the ribbed basket-shaped hollow, which, however, is now prolonged to a great depth, the drop being followed by a cone of air, while the water seems to find great difficulty in wetting the surface completely. Part of this column of air was carried down at least 16 inches, and then only detached when the sphere struck the bottom of the vessel.

Figs. 6 and 7 show the crater falling in, but this did not always happen, for the walls often closed over the hollow exactly as in Figs. 4 and 5 of Series IV. Meanwhile the long and nearly cylindrical portion below breaks up into bubbles which rise quickly to the surface.

By increasing the fall to 5 feet we obtain the figures of Series IX. The tube of Fig. 1 corresponds to the dome of Series IV. and V., and is not only elevated to a surprising height, but is also in the act of cleaving (the outline being approximately that of the unduloid of M. Plateau). Figs. 2 and 3 show the bubble formed by the closing up of this tube, weighed down in the centre as in Figs. 5 and 6 of Series V. Similar results were obtained with other liquids, such as petroleum and alcohol.

It is easy to show in a very striking manner the paramount influence of the condition of the solid surface. I have here a number of similar marbles; this set has been well polished by rubbing with wash leather. I drop them one by one through a space of about 1 foot into this deep, wide, cylindrical glass vessel, lighted up by a lamp placed behind it. You see each marble enters noiselessly and with hardly a visible trace of splash. Now I pick them out and drop them in again (or to save trouble, I drop in these other wet ones), everything is changed. You see how the air is carried to the very bottom of the vessel, and you hear the "φλοῖσβος" of the bubbles as they rise to the surface and burst. These dry but rough marbles behave in much the same way.

Such are the main features of the Natural History of Splashes, as I made it out between thirteen and eighteen years ago. Before passing on to the photographs that I have since obtained, I desire to add a few words of comment. I have not till now alluded to any imperfections in the timing apparatus. But no apparatus of the kind can be absolutely perfect, and as a matter of fact, when everything is adjusted so as to display a particular stage, it will happen that in a succession of observations there is a certain variation in what is seen. Thus the configuration viewed may be said to oscillate slightly about the mean for which the apparatus is adjusted. Now this is due both to small imperfections in the timing apparatus and to the fact that the splashes themselves do actually vary within certain limits. The reasons are not very far to seek. In the first place the rate of demagnetisation of the electromagnets varies slightly, being partly dependent on the varying resistance of the contacts of crossed wires, partly on the temperature of the magnet, which is affected by the length of time for which the current has been running. But a much more important reason is the variation of the slight adhesion of the drop to the smoked watch-glass that has supported it, and consequently of the oscillations to which, as we shall see, the drop is subjected as it descends. Thus the drop will sometimes strike the surface in a flattened form, at others in an elongated form, and there will be a difference, not only in the time of impact, but in the nature of the ensuing splash; consequently some judgment is required in selecting a consecutive series of drawings. The only way is to make a considerable number of drawings of each stage, and then to pick out a consecutive series. Now, whenever judgment has to be used, there is room for error of judgment, and moreover, it is impossible to put together the drawings so as to tell a consecutive story, without being guided by some theory, such as I have already sketched, as to the nature of the motion and the conditions that govern it. You will therefore be good enough to remember that this chronicle of the events of a tenth of a second is presented by a fallible human historian, whose account, like that of any other contemporary observer, will be none the worse for independent confirmation. That confirmation I am fortunately able in some measure to supply. When I endeavoured eighteen years ago to photograph the splash of a drop of mercury, I was unable to obtain plates sufficiently sensitive to respond to the very short exposures that were required, and consequently abandoned the attempt. But in recent years plates of exquisite sensitiveness have been produced, and such photographs as those taken by Mr. Boys of a flying rifle bullet, have shown that difficulties on the score of sensitiveness have been practically overcome. Within the last few weeks, with the valuable assistance of my colleague at Devonport, Mr. R. S. Cole, I have succeeded in obtaining photographs of various splashes. Following Prof. Boys' suggestion, we employed Thomas's cyclist plates, or occasionally the less sensitive "extra-rapid" plates of the same makers, and as a developer, Eikonogen solution of triple strength, in which the plates were kept for about 40 minutes, the development being conducted in complete darkness.

A few preliminary trials with the self-induction spark produced at the surface of mercury by the apparatus that you have seen at work, showed that the illumination, though ample for direct vision, was not

SERIES X.

(1) *Instantaneous Shadow Photographs (life size) of the Splash of a Drop of Mercury falling 8 cm. on to the Photographic Plate.*

1

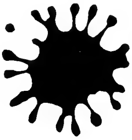
Actual size of the Drop, 4·83 mm.

2

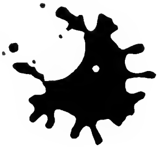
$\tau = 0$

3

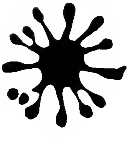
4



5



6



7

$\tau = \cdot 048$

SERIES XI.

(2) *Instantaneous Shadow Photographs (life size) of the Splash of a Drop of Mercury falling* 15 *cm. on to Glass.*

1

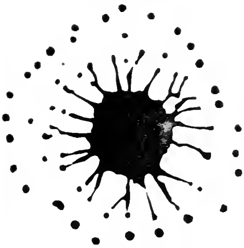
Actual size, 4·83 mm.
in diameter.

2

$\tau = 0$

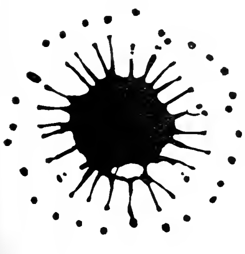
3

4

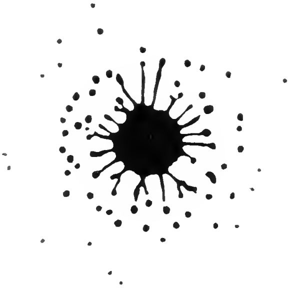


$\tau = \cdot 0032$

4A

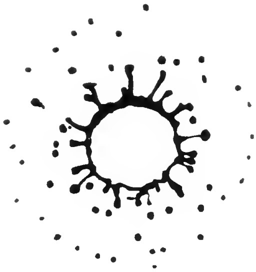


5



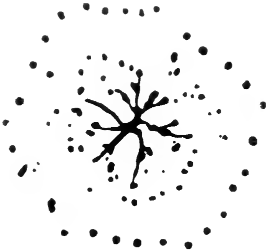
$\tau = \cdot 0063$

5A



$\tau = \cdot 0094$

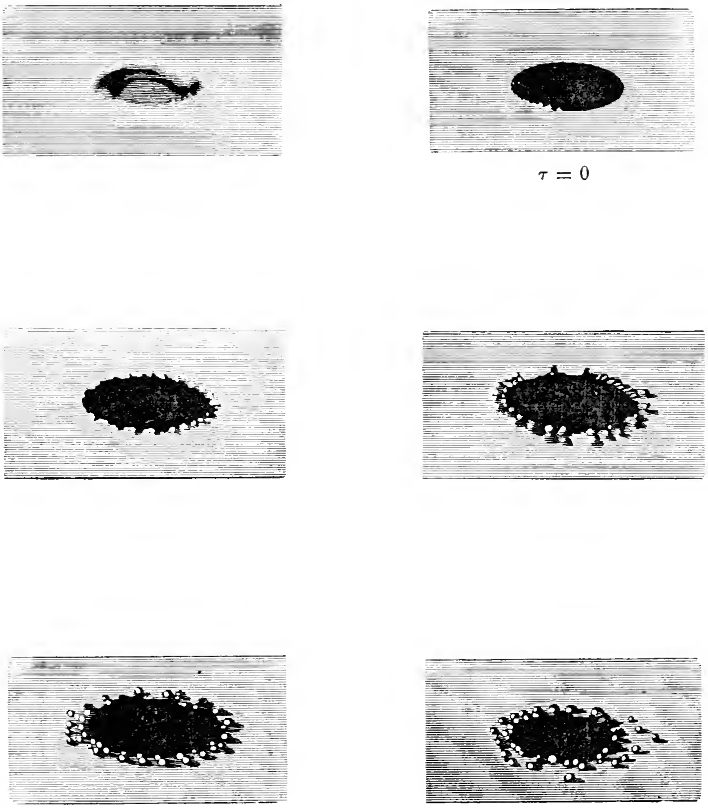
6



$\tau = \cdot 0134$

SERIES XII.

Engravings from Instantaneous Photographs ($\frac{16}{17}$ of the real size) of the Splash of a Drop of Mercury, 4·83 mm. in diameter, falling 8·9 cm. on to a hard polished surface.



$\tau = 0$

$\tau =$ ·0195 sec.

sufficient for photography. When the current strength was increased, so as to make the illumination bright enough for the camera, then the spark became of too great duration, for it lasted for between 4 and 5 thousandths of a second, within which time there was very perceptible motion of the drop and consequent blurring. It was therefore necessary to modify the apparatus so as to employ a Leyden-jar spark whose duration was probably less than 10-millionths of a second. A very slight change in the apparatus rendered it suitable for the new conditions, but time does not permit me to describe the arrangements in detail. It is, however, less necessary to do so as the method is in all essentials the same as that described in this room two years ago by Lord Rayleigh in connection with the photography of a breaking soap-film.* I therefore pass at once to the photographs themselves.

The first two series (X. and XI.) may be described as shadow photographs; they were obtained by allowing a drop of mercury to fall on to the naked photographic plate itself, the illuminating spark being produced vertically above it, and they give only a horizontal section of the drop in various stages. The first series corresponds to a mercury splash very similar to that first described, and the second to the splash of a larger drop such as was not described. In each series, the tearing of the thin central film to which allusion was made is well illustrated. I think the first comment that any one would make is that the photographs, while they bear out the drawings in many details, show greater irregularity than the drawings would have led one to expect. On this point I shall presently have something to say.

Comparing the first set of drawings with the photographs of Series X. it will be seen that

Photograph	2	corresponds to	drawing	4 or 5,
„	3	„	„	9
„	4	„	„	18
„	6	„	„	20
„	7	„	„	24

but the irregularity of the last photograph almost masks the resemblance.

Series XII. gives an objective view of a mercury splash as taken by the camera. Only the first of this series shows any detail in the interior. The polished surface of the mercury is, in fact, very troublesome to illuminate, and this splash proved the most difficult of all to photograph.

Series XIII. shows the splash of a drop of milk falling on to a smoked glass plate, on which it runs about without adhesion just as

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* A detailed account of the optical, mechanical, and electrical arrangements employed, written by Mr. Cole, will be found in 'Nature,' vol. l. p. 222 (July 5, 1894).

mercury would. Here there is much more of detail. In Fig. 4 the central film is so thin in the middle that the black plate beneath it is seen through the liquid. In Fig. 8 this film has been torn.

Series XIII. exhibits the splash of a water drop falling into milk. The first four photographs show the oscillations of the drop about a mean spherical figure as it approaches the surface.

In the subsequent figures it will be noticed that the arms which are thrown up at first, afterwards segment into drops which fly off and subside (see Fig. 8), to be followed by a second series which again subside (Fig. 11), to be again succeeded by a third set. In fact, so long as there is any downward momentum the drop and the air behind it are penetrating the liquid, and so long must there be an upward flow of displaced liquid. Much of this flow is seen to be directed into the arms along the channels determined by the segmentation of the annular rim. This reproduction of the lobes and arms time after time on a varying scale goes far to explain the puzzling variations in their number which I mentioned in connection with the drawings. I had not, indeed, suspected this, which is one of the few new points that the photographs have so far revealed.

With respect to these photographs,* the credit of which I hope you will attribute firstly to the inventors of the sensitive plates, and secondly to the skill and experience of Mr. Cole, I desire to add that they are, as far as we know, the first really detailed objective views that have been obtained with anything approaching so short an exposure.

Even Mr. Boys' wonderful photographs of flying bullets were after all but shadow-photographs, and did not so strikingly illustrate the extreme sensitiveness of the plates, and I want you to distinguish between such and what (to borrow Mr. F. J. Smith's phrase) I call an "objective view."

It remains only to speak of the greater irregularity in the arms and rays as shown by the photographs. The point is a curious and interesting one. In the first place I have to confess that in looking over my original drawings I find records of many irregular or unsymmetrical figures, yet in compiling the history it has been inevitable that these should be rejected, if only because identical irregularities never recur. Thus the mind of the observer is filled with an ideal splash—an "Auto-Splash"—whose perfection may never be actually realised.

But in the second place, when the splash is nearly regular it is very difficult to detect irregularity. This is easily proved by projecting on the screen with instantaneous illumination such a photo-

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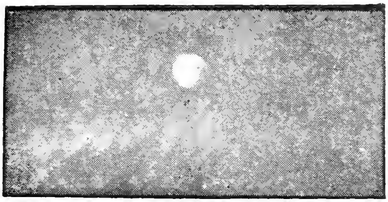
* Three of these photographs, viz. Nos. 11, 12 and 17, are reproduced full size, as a frontispiece, by a *photographic* process, to enable the reader to form a more correct idea than can be gathered from the engravings, of the amount of detail actually obtained.

The black streaks seen in Figs. 11, 15, 16 and 17 are due to particles of lampblack carried down by the drop from the smoked surface on which it rested.

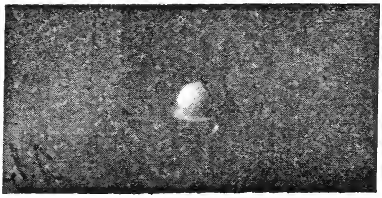
SERIES XIII.

Engravings of Instantaneous Photographs ($\frac{16}{17}$ of the real size) of the Splash of a Drop of Milk falling 20 cm. on to smoked glass.

1

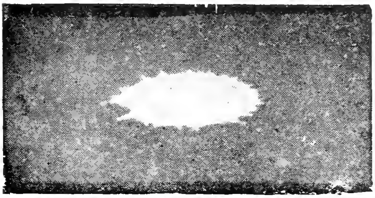


2



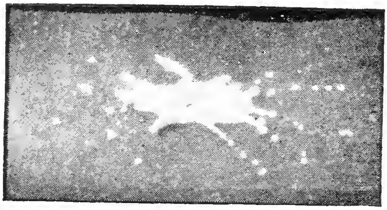
$\tau = 0$

3



$\tau =$ ·0025 sec.

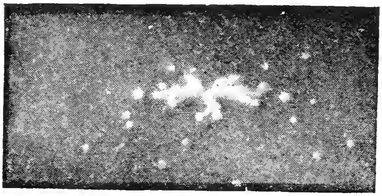
7



$\tau =$ ·0128 sec.

8

9



$\tau =$ ·0149 sec.

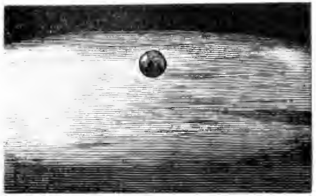
(It was not found possible to reproduce satisfactorily the missing figures of this series.)

SERIES XIV.

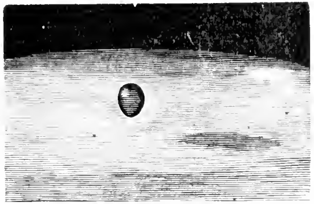
Engravings of Instantaneous Photographs of the Splash of a Drop of Water falling 40 cm. into Milk.

Scale about $\frac{6}{10}$ of actual size.

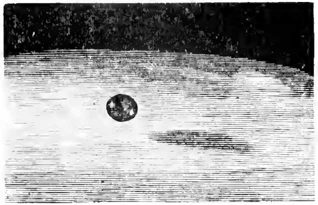
1



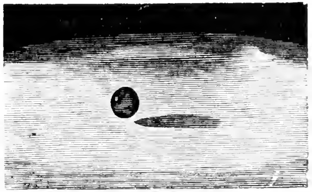
2



3

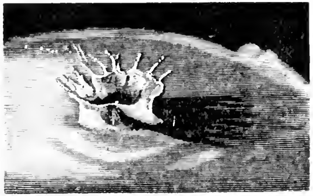


4

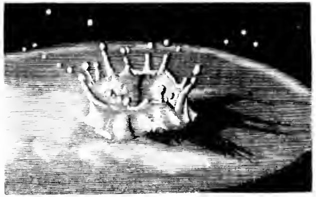


$\tau = 0$

5



6



$\tau = \cdot 0056$ sec.

7

$\tau = \cdot 0163$ sec.

8

9

$\tau = \cdot 0182$ sec.

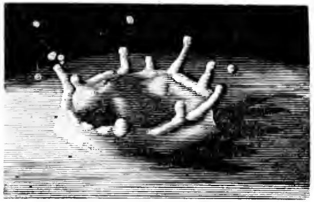
10

$\tau = \cdot 0197$ sec.

11

$\tau = \cdot 0262$ sec.

12



$\tau = \cdot 0391$ sec.

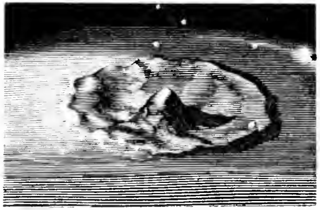
13

$\tau = \cdot 0514$ sec.

14

$\tau = \cdot 0601$ sec.

15



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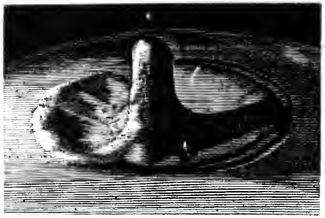

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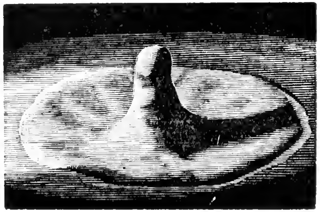


graph as that of Series X. Fig. 6. My experience is that most persons pronounce what they have seen to be a regular and symmetrical star-shaped figure, and they are surprised when they come to examine it by detail in continuous light to find how far this is from the truth. Especially is this the case if no irregularity is suspected beforehand. I believe that the observer, usually finding himself unable to attend to more than a portion of the rays in the system, is liable instinctively to pick out for attention a part of the circumference where they are regularly spaced, and to fill up the rest in imagination, and that where a ray may be really absent he prefers to consider that it has been imperfectly viewed.

This opinion is confirmed by the fact that in several cases I have been able to observe with the naked eye a splash that was also simultaneously photographed, and have made the memorandum "quite regular," though the photograph subsequently showed irregularity. It must, however, be observed that the absolute darkness and other conditions necessary for photography are not very favourable for direct vision.

And now my tale is told, or rather as much of it as the limits of the time allowed me will permit. I think you will agree that the phenomena are very beautiful, and that the details of this transaction, familiar though it has been to all mankind since the world began, have yet proved worthy of an hour's attention.

[A. M. W.]

WEEKLY EVENING MEETING,

Friday, May 25, 1894.

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

Sir Howard Grubb, F.R.S. F.R.A.S.

The Development of the Astronomical Telescope.

Since I last had the honour of lecturing in this theatre, astronomical research has had opened up to it a totally new field of work, one which appears almost without limit in its scope.

When Dr. Gill, of the Cape of Good Hope Observatory, made arrangements to photograph the comet of 1882 with a long exposure, using only an ordinary photographic lens, even his fertile and sanguine spirit did not, I think, anticipate the possibilities opened to astronomical research since photography has been called in to its aid.

I do not propose in this lecture to discuss any of the interesting astronomical and physical problems that have been opened up by the adoption of the "New Astronomy," as it is aptly called. I leave these subjects to be dealt with by those who have made such their special study, and are better qualified to speak of them.

I propose to-night to discuss the differing conditions which astronomical instruments are required to fulfil under the new system as compared with the old, and to point out the possibilities that appear to exist for improvement and future development, touching only on the astronomical part of the work so much as may be necessary to explain the instrumental equipment required.

It would be well, perhaps, that I should first call to your recollection some of the chief lines of work which have been opened by the introduction of the new photographic method.

The International Photographic Survey of the Heavens has been undertaken by sixteen of the principal observatories of the world, which have agreed to co-operate in producing two series of photographic pictures of the entire heavens; the first series including all stars down to the 11th magnitude, a catalogue of which is to be formed from the photographic plates; while the other series will include all stars to the 14th magnitude, and will be photographically reproduced as a chart.

The enormous advantages of photography for this work, as compared with the old system, have been brought so often and so prominently before you by Dr. Gill and other lecturers that it is unnecessary for me to dwell upon them here. As a supplement to this international work we have the independent surveys of Dr. Gill and Professor Pickering, taken photographically with short focus lenses on a small scale.

We have also the recording by the aid of photography of specially interesting objects, star clusters and groups, comets, &c., and more particularly very faint objects such as nebulæ, which require long exposure. The results in this field of work demonstrate perhaps more than any other the powerful agent that photography becomes in the hands of the astronomer. It is not necessary to go into any detail in considering this work, the results of which are tolerably familiar to you. It is only necessary to mention the names of Draper, Common, Gill, the Brothers Henry, Roberts, Gothard, Barnard Russell and Dr. Max Wolf, to indicate how much we owe to long exposure photography on these objects.

Of new minor planets, 33 were discovered by photography in 1893, and several lost planets were rediscovered.

We have also the study of the parallax of fixed stars and of nebulæ by this means, as carried out by Rutherford and Jacoby in America, Professor Pritchard at Oxford, Sir Robert Ball and Professor Rambaut at Dunsink and Dr. Wilsing at Potsdam.

Turning to spectroscopy, we find again the enormous importance of the photographic method. The development of Fraunhofer's original idea of a slitless spectroscope has given us the objective prism of to-day, and with this instrument we are able to simultaneously photograph the spectra of several hundreds of stars on a single plate, these spectra being available for study at leisure, so that they can be classified and selected for future work and more crucial examination and investigation. The results from a single plate are more accurate than could possibly be given by months of very close observation by the older method. The Draper Catalogue, which we owe to Professor E. C. Pickering, of Cambridge, Mass., gives the spectra of over 10,000 northern stars obtained in this manner. In addition to this he has in preparation a similar catalogue of the southern stars. The beautiful photographs obtained by Professor Lockyer at South Kensington, in which the detail is so fine that the spectra can be enlarged to a length of five feet, show the great value of this method for complete study of the spectra after the first rough cataloguing. With the slit spectroscope equally important work has been done in the "New Astronomy." Not only can we get the spectra of celestial objects with comparison spectra of terrestrial substances on the same plate, and thus investigate the chemical and physical constitution of these bodies, but by the adaptation of the beautiful discovery of Dr. Huggins we can detect and measure the motion in the line of sight, the photographic method giving far more accurate results than can be obtained by visual means. In the hands of Dr. Vogel, of Potsdam, this spectrographic method has been used for nearly fifty stars, and he is only waiting for a larger instrument to further extend this work. We have also in this connection the discovery of spectroscopic double stars, stars so close that we cannot hope to see them double by any possible optical means, and yet of which we know sufficient by the aid of photography to calculate their masses, distances, periods and rates of motion with considerable accuracy. In this work Professor Pickering, at Cambridge, Mass., and Dr. Vogel, at Potsdam, led the way, but Father Sidgreaves, at Stonyhurst, has extended the work by his beautiful analysis of the variations in the spectrum of β Lyræ.

Lastly may be mentioned the work of Professor Hale at Chicago, on the photography of solar prominences and faculæ with the spectro-heliograph. In this apparatus, instead of using the full light of the sun, only light of one wave-length is allowed to act on the plate. There are several methods of accomplishing this, but the latest form is that of an ordinary spectroscope with a metal plate to receive the spectrum, a narrow slit being arranged in this plate to select the particular wave-length in which it is desired to work. The photographic plate is behind a second slit and in the image formed of the spectrum. The whole spectroscope is given a motion such that the front slit passes over the image of the sun formed by a photographic object glass, while the selecting slit moves at the same rate in front of the photographic plate. A complete picture of the prominences, chromosphere and faculæ of the sun is thus obtained, and by an ingenious adaptation of clock-work Professor Hale has been able to make his apparatus automatic, and to set it to take 36 plates of the sun at any desired interval of time between each, without any superintendence whatever from the observer in charge. Photography has also been extended to the study of solar spots, lunar and planetary detail, and many other departments of astronomical research too numerous to mention, but not having any special interest for us at present in their bearing upon the instrumental arrangements.

Every one of these branches of work has already been not only suggested but put into actual practice with more or less success, but, as usual in the inception of such work, there are many lessons to be learned from the first few years' experience. The most evident fact, and one easily learned from any one of the various branches mentioned, is that the utmost perfection is necessary in the apparatus which enables the telescope to follow the object to be photographed. Before enumerating the various points necessary to be attended to to ensure this accuracy, perhaps it would be well to explain why this increased amount of accuracy is necessary when using the photographic method of observation. In the older methods it sufficed if the star remained on the wires of the micrometer during the actual observation, which rarely lasted many seconds, and even if the star did move off the wire the observer could see that it did so, and would move up his wire again to the star, repeat the observation, and would not record it unless he was satisfied that all was right at the moment of bi-section.

In the photographic method, however, the record of the observation is not that of any one moment, it is the aggregate of all the impressions made every second and every part of a second during the exposure. The photographic plate, unlike the eye, takes note of, and records every position of the star image, and not the one selected position as the eye does; hence you can easily see the great necessity of having the utmost possible perfection in the clock driving arrangements.

This condition of perfection is popularly supposed to be satisfied by having a perfect clock, but there is hardly a portion of the instrument that can be mentioned, the perfection of which does not contribute in some way or other to the accuracy of the motion.

1. The instrument must have a stability far beyond what is necessary for ordinary work, otherwise the very handling of the slow motions will sensibly affect the positions of the images and injure the results.
2. It is evident that the axis on which the instrument revolves must be of extreme accuracy, otherwise the instrument will not move truly.
3. The anti-friction arrangements must be of the most efficient nature in order to give the clock a fair chance of doing its work.
4. The slow motions must be extremely perfect, as otherwise it will be impossible to bring the guiding star on the cross wires of the "guider" with that accuracy necessary for the best results.
5. The arrangements, even for the adjustment of the instrument, so far at least as the placing of the polar axis parallel to the pole of the earth, must be such as to enable the observer to make this adjustment with an accuracy not at all necessary for visual observations.

Professor Rambaut, Royal Astronomer of Ireland, has recently shown that whereas for ordinary visual work it suffices if the polar axis of a telescope be adjusted in altitude and azimuth with an accuracy of 1 minute of arc, errors of a few seconds of arc only are allowable with a photographic telescope, and that this great degree of accuracy is best obtained from measurements of the photographic plate itself.

An instrument, therefore, that is required to give the best results used photographically, should be made with a view to such work in all its details from the very beginning, for an instrument that may be excellent for ordinary observations will most probably break down under the stringent conditions necessary for the more modern work.

In the usual form of mounting it is necessary to reverse the instrument when observations have been made on a star as it passes from the east to the west of the meridian. This is a great disadvantage in photographic work, and in designing the new 26-inch photographic telescope, which Sir Henry Thompson has undertaken to present to Greenwich, I have arranged to allow complete circumpolar motion so that this reversing on the meridian will not be necessary, and the telescope will follow any star through the whole of its path in the heavens so long as it is above our horizon.

But no matter how perfect the instrument may be in all the details above spoken of, it is not possible to attain the necessary perfection of motion without a good clock, and I thought it would be interesting for you to see the working of such a clock, and have here one which is identical with those used in the standard instruments of the International Photographic Survey. This clock is the combination of a good frictional governor, supplemented by a system of control from an independent pendulum. Perhaps you will allow me to explain why this control is necessary. A clock such as this will go well and smoothly and keep good time from second to second, but no uniform motion clock that I have ever met with can be depended on for long periods. This one, I find, can be depended on to about 1 second in 600, but as it is necessary, or at least desirable, to be able to depend on the clock for longer periods than this, while no error of more than $\frac{1}{20}$th part of a second can be permitted, it is evidently necessary to supplement this by control from an independent pendulum which can be relied upon to the required amount of accuracy.

There is another very important reason why an independent control is necessary. When an error occurs in the clock driving, owing perhaps to some morsel of adventitious matter in the bearings of the polar axis, or some little extra stiffness due to want of perfect balance, &c., the tendency of all these governors is to bring the *rate* of the clock which has been disturbed back again to the normal. This answers perfectly well for visual work because, if such an accidental error does take place occasionally, it merely means that the star slightly shifts in the field and the wires can be again brought up to the star and a satisfactory measurement taken, provided that the image does not again shift during the few seconds required for taking the observation.

But in the case of the photographic telescope such an error would be fatal, because the star has already impressed the photo plate at one certain point. When the error occurs the image shifts, and even if the rate of the clock continues perfectly right for the whole of the remainder of the exposure, the result will be of course a double or distorted image.

In photographic work we require some arrangement by which any error which is accidentally introduced will be effectually and as quickly as possible wiped out, the star image brought back again to its original position on the plate and then the clock to resume its normal rate, and this is a condition which no uncontrolled clock can fulfil. The only solution which has yet been suggested to fulfil these conditions is to have some means by which the clock of the equatorial (which, as I said, goes well and smoothly for short intervals) is checked and controlled every second from an independent pendulum.

The lecturer here exhibited in action an equatorial clock controlled from an independent pendulum as above described, with an arrangement of bells added by which the audience were enabled to judge of the perfect synchronism of the controlled clock and the controlling pendulum. The lecturer purposely introduced errors into the clock train to illustrate the power of the controlling apparatus to erase these errors.

Suppose now we have our clock as perfect as is possible, it is further necessary to see that that perfectly uniform motion is transmitted to the instrument; in other words, that any gearing between the controlled clock and the polar axis be as far as possible without error. This gearing consists mainly of the endless screw, called the right ascension screw, and the toothed sector. The precautions taken for the ensuring of this perfection have been elsewhere described, and are of too technical a character to deal with here, but one observation only I would desire to make.

In a recent paper by Professor Pickering, commenting on instruments he saw during a recent visit over here, he is kind enough to make complimentary allusions to some of these arrangements, but he takes exception to the use by us in this country of long radius sectors for driving the polar axis instead of entire circles. Perhaps it may be well, therefore, if I take this opportunity of saying why we prefer the sector.

Bear in mind that the greatest possible perfection of clock driving is what we are aiming at, and you will easily see our reason for adopting the sector. When a sector or portion of a circle only is used it is possible to get a radius much greater than in the case of an entire circle.

No mechanism ever made is absolutely free from error. Call the residual error of this screw anything you like, one 10-thousandth or one 20-thousandth of an inch; whatsoever that error be its effect on the accuracy of the driving of the telescope will be exactly in the inverse ratio of the radius at which it acts; therefore any given error will only have one-third the effect on the driving of the telescope if working (as it may in a sector) at three times the radius. One 10-thousandth of an inch at say 10-inch radius will produce an angular error of about 2 seconds of arc; at 30 inches radius it will only produce two-thirds of a second error. This may seem a small advantage, but the nearer we approach to perfection the more difficult it is to obtain any given increment.

It does not take much coal to increase the speed of a locomotive from 10 to 11 miles an hour, but it is very different if we want to increase it the same 10 per cent. from say 60 to 66.

Another fact that has been brought to light by the experiments of the last few years is that atmospheric disturbance, the *bête noire* of the astronomer, has not so much effect on most of these photographic results as in the case of visual observations.

In the number of the 'Observatory' published in December 1889, Dr. Gill makes the following remarks in a note accompanying a specimen photograph which he sent over. "The picture is sent in corroboration of a fact I have suspected for some time, viz. that for stellar photographs, after a certain period of exposure, it is quite immaterial whether the atmospheric definition is good or bad, the photographic images of stars will be equally sharp in either case. That good measurable pictures can be taken on nights when refined eye observations of any value are impossible is a very remarkable fact, and one that *a priori* would probably be deemed unlikely. The explanation appears to be that the discs on the developed film which represents stars are very much larger than the minute circle formed by the converging cone of rays from the object glass at its intersection with the plane of the film. These discs are produced by so-called photographic irradiation; in other words, by chemical action set up in the film, having origin in the central point of light, and extending gradually and symmetrically over a wider radius from that centre. This being so, whenever the radius of the disc becomes greater than the radius of extreme oscillation of the optical image from a mean point, the resultant photographic action produced by the rapidly moving point of light becomes identical with the effect produced by a similar steady point of light occupying the same mean position."

Again, in the case of some of the spectroscopic methods of observation, more particularly when a slit is used, this peculiarity of the photographic method is perhaps still more apparent.

On this subject Dr. Gill, in a lecture delivered in this theatre just three years ago, says, "On account of irregularities in atmospheric refraction, the image of a star in the 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 on the slit the plate goes on recording what it sees, and when the star is not on 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 star is steady and the star's image therefore always on the slit, the exposure takes less time than when the star is unsteady. That is one reason why the Potsdam results, in the determination of stellar motion in the line of sight, are so accurate. And there are many other reasons besides, into which I cannot now enter. What, however, it is 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."

Those who are familiar with the use of large telescopes, know only too well that the larger the aperture the fewer are the opportunities on which it can be used with advantage, and the question has often been discussed as to whether the useful limit of aperture has not already been reached, except in cases when it may be possible to transport the instrument to Arequipa or some such favoured locality. No doubt large instruments so placed ought to be, and are, capable of doing much more and better work than if placed in a less favoured spot, say in the neighbourhood of a town, but experience has shown that other influences often arise which militate against the possibility of taking full advantage out of the improved locality. The conditions of life in some such isolated stations are not the pleasantest, and though human nature may put up with inconveniences and unpleasantness as a temporary arrangement, for the sake of science, yet, as a permanency, this state of things is not found compatible with the production of the best work, and in some cases it has been found necessary to send relays of workers to these isolated stations, a plan, no doubt, which meets to some extent this difficulty, but is evidently open to other objections.

If however, as it appears, the new photographic system is to a great extent independent of atmospheric disturbance, it ought to be possible not only to use, and use with efficiency, large instruments in situations within measurable distance of the haunts of civilisation (a great gain in itself), but it will also be possible to use with advantage, even in such accessible positions, instruments of far greater power than have ever yet been built, and of whose practical value there have been well founded doubts so long as the old system of eye observations was the only one available.

When this fact forces itself upon the attention of the scientific world, as it must do before long, and the necessity of adding to the power of our telescopes becomes apparent, there is little doubt but that the means will be found to satisfy the necessity; but as the magnitude of these instruments becomes greater, the importance of studying beforehand the necessary conditions to fulfil and the mistakes to be avoided becomes all the greater, and therefore I have thought it may not be amiss to bring under your notice a few suggestions as to the possibility of obtaining increased optical power in our telescopes.

Before we discuss the conditions to be fulfilled in the case of the mounting of more powerful telescopes, perhaps it would be well to get a clear idea of what is meant by the power of a telescope as distinct from *magnifying* power. You are aware that most of the work done with our very large refractors is done with magnifying powers which are equally useable with instruments of half the aperture or less, but it must not be assumed that the power of the instrument (used in its broad sense, i.e. its capability of distinctly viewing minute objects, or the details of such) is then only the same as that of the smaller instrument used with the same magnifying power. On the contrary, Jupiter or Saturn viewed with a power of 600, with 28 inches aperture, is a very different object to what it is when viewed with the same magnifying power and an aperture of say 8 or 10 inches. This is not due to extra brilliancy from the larger amount of light collected by the bigger object-glass, for even in the case of observing the moon (say), when it is necessary to use tinted screens to moderate the brilliancy, this superiority of the larger aperture is just as evident, but it may be explained in this way:—

In a lecture by Dr. Common, delivered in this theatre in May 1890, he gave a very neat explanation of the fact that a certain amount of magnification of image is required in order to see a certain amount of detail in that image. He showed that the sensation by which the brain is excited is carried from the retina by an enormous number of fine nerves, which are excited by small bodies called technically "rods and cones," and that each of these produces one distinct sensation as relating to the particular part of the image which falls on that particular part of the retina; the image, therefore, as presented to the brain is a kind of mosaic, and it is evident that the larger the image that falls on the retina the finer will the mosaic be in proportion to the details of the image, and therefore the better will the details be appreciated.

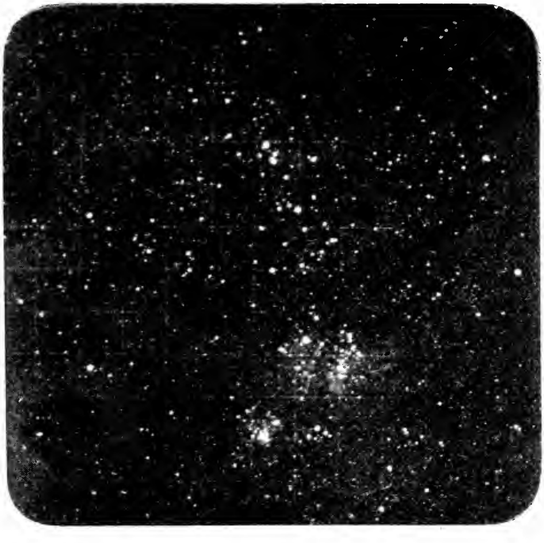
A similar explanation may be given of the different character of the image given by large aperture telescopes and small. The image of a star, as given by a telescope's objective, is not exactly a point; it is, owing to certain physical reasons which it would be impossible to enter into in this lecture, in the form of a small disc of light which, if the object is of a sufficient brilliancy, is surrounded by diffraction rings. The diameter of this spurious disc depends amongst other conditions on the diameter of the object-glass; the larger the diameter of the object-glass the smaller the diameter of the disc; in other words, the discs, as seen in large object-glasses, are smaller than those seen in smaller object-glasses; or putting it in another way, if a certain size of object-glass be found to give a spurious disc of a certain size, *reducing* the aperture of the object-glass will *increase* the size of the spurious disc.

Every object may be considered to be made up of an infinity of points, of every one of which the object-glass gives an image in the form of a little disc. It is evident that the image that is made up of the larger spurious discs will not be as fine or as delicate, or show as much detail as that made up with the smaller discs. The image of such an object as Jupiter or Saturn as seen in the small telescope, compared with that as seen in the large telescope, will be as a drawing made in the first case with a coarse crayon or stump, to that made in the second case with a finely pointed lead pencil; or we may compare the first to a very coarse mezzotint engraving, while the second may be compared to the very finest work that can possibly be turned out.

This is the reason that the larger aperture telescope, even when used with powers corresponding only to those which can be effectively used with a smaller instrument, show objects with a clearness and distinctness that it is impossible to obtain in the smaller instrument, no matter how perfect the workmanship may be.

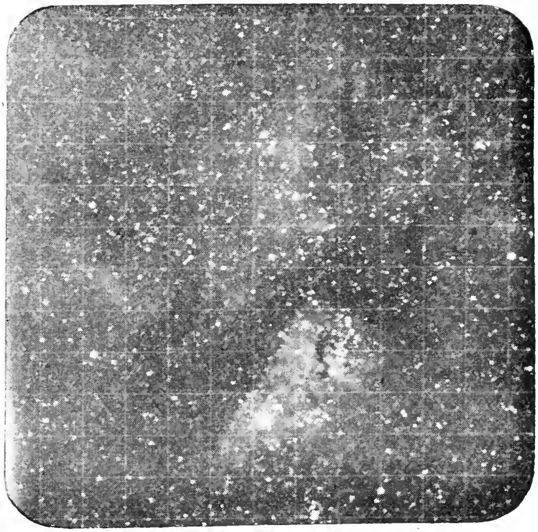
And now we come to the question of the probabilities of our

ω Centauri.

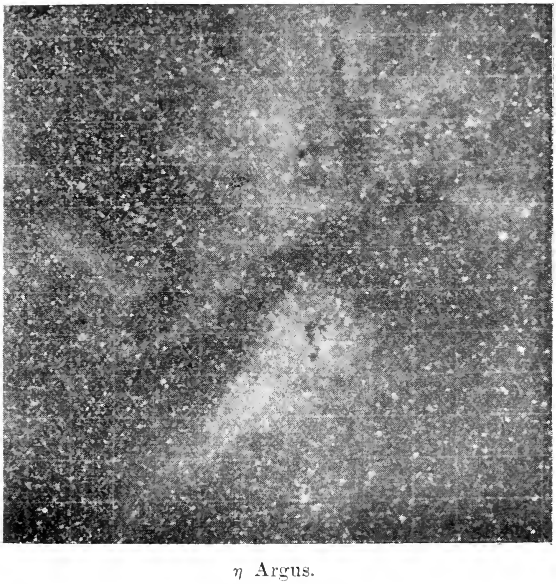


η **Argus.**
45 minutes' exposure.

Reproductions of photographs taken by Dr. D. Gill with the 13·2 inch Capetown Astrographic Telescope.



η Argus.
3 hrs. 12 min. exposure.



η Argus.
12 hrs. 12 min. exposure.

Reproductions of photographs taken by Dr. D. Gill with the 13·2 inch Astrographic Telescope.

being able to increase to any great extent the powers of our telescopes, and with this also naturally arises a question which, judging by the number of queries that reach me about it, seems highly interesting to the general astronomical world. "Will the great telescopes of the future be Refractors or Reflectors?"

Mr. Alvan Clark, whose large refractors in the United States testify to his great skill, declares emphatically for refractors, perhaps naturally so, but his reasons do not appear altogether convincing, and there are others well qualified to judge who give an opposite opinion. It is a question which only the future can decide. Of course, if we all make up our minds that the coming telescope is to be a refractor it will be so, for all our energies will be devoted to its development; but the same might be said of the reflector, which, I believe, is capable of being greatly improved if attention were directed to it.

There is one reason that I believe has been overlooked, which explains to some extent why the reflector has not been developed of late years as has the refractor. This matter is not of a scientific, but purely of an economic character, and I should, perhaps, ask pardon for introducing it into a scientific lecture; still, it is necessary for explanatory purposes.

Reflectors are, unfortunately for themselves, much less costly than refractors, and I believe that this has much to do with their comparatively neglected condition at present; this may seem curious, but it is easily explained. An object-glass of 18 inches is worth, say 1000*l.*; a mirror of 18 inches is worth, say 100*l.* No one who wanted to have good mounting would object to pay 1000*l.* to mount the 1000*l.* object-glass, but there are many who would object to pay the same 1000*l.* to mount the 100*l.* mirror; and yet why should it not be equally well mounted? and if not so, how can it be expected to give as good results as the refractor? As a matter of fact there are greater difficulties in mounting a reflector than a refractor, and these greater difficulties mean increased cost for an equally good mounting. I believe this simple economic question has much to answer for in bringing the reflector into disrepute with many. It has often been remarked that the reflectors that have been best worked have been constructed and worked by amateurs, the reason, to a great extent, being that this economic point does not then enter so largely into the question.

There are great difficulties in the mounting of reflectors, more especially when required for use as a photographic telescope, and these difficulties have never yet been satisfactorily solved, but I believe there is nothing unsolvable in them, and that it only wants attention to be drawn to them to ensure a solution.

Only within the last month Dr. Johnstone Stoney has devised a most ingenious arrangement for supporting the great mirrors of reflecting telescopes on an air support, graduating the pressure according to the angle of inclination of the telescope by an automatic contrivance. Possibly this apparatus in its present form may be capable of improvement, but it is at least a step, and a very important step, in the direction of solving one of the most troublesome and difficult problems to be met with in the attempt to obtain a really satisfactory mounting for reflecting telescopes.

That the reflecting telescope is capable of doing excellent work in the hands of those who take sufficient care and trouble with the adjustment and in the working, is sufficiently evidenced in the results obtained by Draper, De la Rue, Common and Roberts, but the fact is that to obtain good results with the reflector in its present imperfect state of development, more labour and patience is required than most observers care to bestow on the work, and there is much to be said in excuse for this, for if an astronomer's time be taken up with the necessary attention to the details of his instrument he will not be able to pay that undivided attention otherwise possible, and at all times desirable, for his more legitimate work in the obtaining of results with that instrument. The fact that the reflector brings all rays of light to a common focus, irrespective of their wave-lengths, while the refractor is at best but a compromise, tells strongly in favour of the ultimate success of the reflector over its rival.

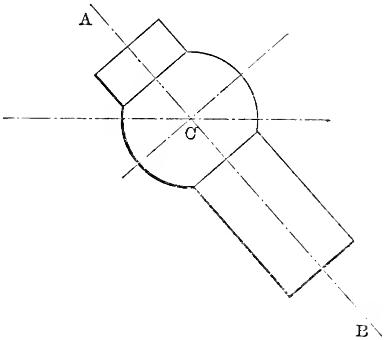
True, it may be said that the experiments in glass-making that have been carried on for some years at the Jena glass manufactory may yet eventuate in producing qualities of glass which will remove this reproach from the refractor, and enable us to perfectly balance the chromatic error, and at the same time be of a sufficiently permanent character to justify its use in the case of large objectives. No one would be foolish enough to attempt to make a large objective of any material which was not known from previous experience to be capable of preserving its perfection of surface for at least 20 or 25 years.

Unfortunately there is no test of permanency except the lapse of years; even if therefore some such glass were in existence at the present time, no maker who had any desire that his name should live after him in his work, would care to use this untried material until actual experience proved its character for permanence. This, and the fact that it has not yet been found possible to produce perfect pieces of this Jena glass of one-tenth of the weight of those already produced of the more ordinary varieties of optical glass, cuts off any hope we might otherwise have of being able for the present to produce large objectives with perfect correction for the chromatic aberration, and so long as this is the case, the reflector, which treats rays of all refrangibilities alike, has in this respect the advantage.

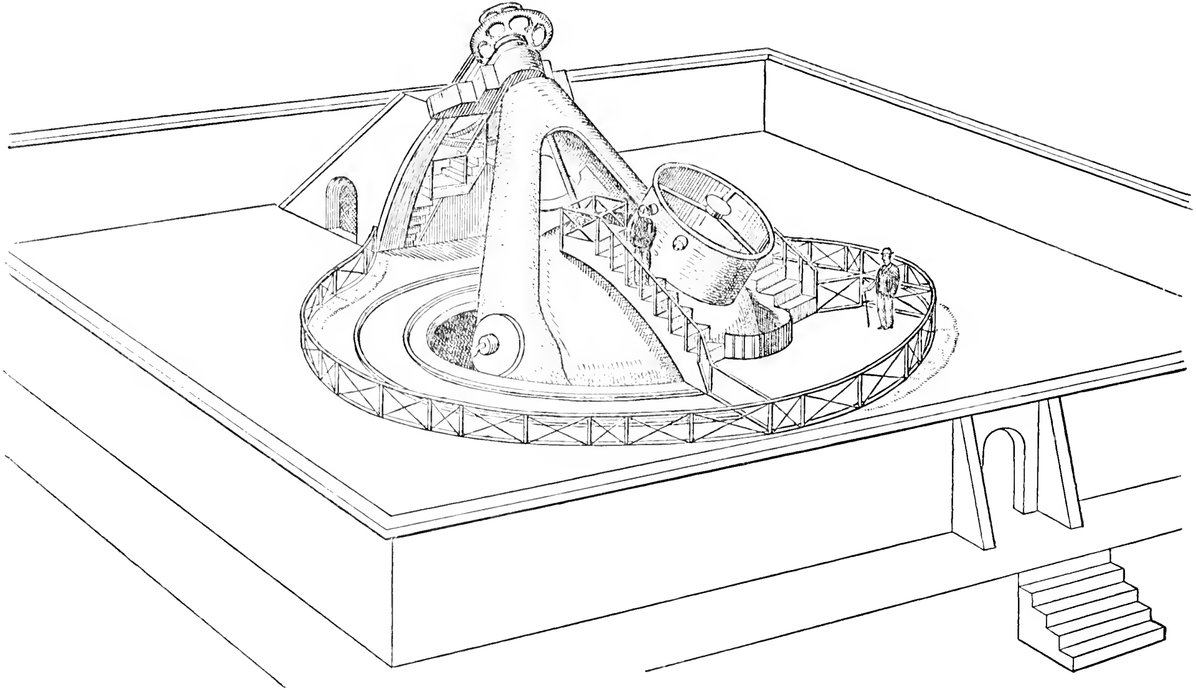
When we consider that the largest optical discs ever yet produced, and which were rightly considered a perfect triumph of art, are only 40 inches in diameter, and that on the other hand Lord Rosse's reflector of 72 inches diameter is now half a century old, it is tolerably evident that for the present, at least, we must look to reflectors if we want to increase to any large extent the power of our telescopes.

With this view, and understanding that it is likely an attempt will be made to build an 8 or 10-foot reflector for the great exhibition to be held in Paris in 1900, it may be interesting to consider the conditions desirable to be fulfilled for such an instrument, and the most promising construction to satisfy those conditions. If a monster telescope, such as this, is to be mounted only in such a manner as will satisfy the ordinary conditions of star gazing, I fear the results will be disappointing, but let it be mounted in such a manner as to render it useable for the more delicate and refined work of the modern astronomy, and a grand and productive field of work is open to it.

But the problem of mounting an enormous instrument such as this, whose weight would probably amount to from 50 to 100 tons, so perfectly poised and so accurately driven by clockwork as never to vary from its true position by a quantity greater than the apparent motion of a star in one-twentieth of a second of time, is sufficiently difficult to justify almost a doubt of its possibility, and this difficulty has been appreciated by others; for Dr. Common, who, as the maker of the largest equatorially mounted reflector ever completed, must be considered as the first authority, proposed some short time since to resort to the alt-azimuth form of mounting, with which it would, of course, be impossible to satisfy the above condition.



Dr. Common himself has made a splendid advance in adopting the system of flotation of the polar axis; this principle of flotation appears to me to be capable of further development. It is perfectly possible to make a tube for a Newtonian reflecting telescope (which is necessarily closed at the lower end) of such a weight, and with its weight so distributed that it will not only float in water submerged to a certain point (preferably near the upper end), but will be in a state of equilibrium when placed at any or in every position down to a certain angle, which angle depends on the exact outside form of the tube. For instance, if *A B* (Fig. 1) be a tube closed at *B* and perfectly symmetrical round the axis *A B*, and the total weight of the tube be equal to the weight of water which is displaced when the tube is sunk to *C*, the weight of the different



sections along the axis *A B* can be so distributed that the tube will equally well remain in any other position, except it be so far turned over that the cylindrical part of the tube is lifted out of the water at one end and dipped at the other.

By making the spherical part of about the proportions of the figure, the tube can be depressed to within 25° of the horizon, and still remain in perfect equilibrium.

Now, suppose the tube to have a pair of trunnions attached at the water line, and these carried on a polar axis of, say, the English type (see Fig. 2), we have an equatorially-mounted telescope of any size, without any weight whatever on the bearings of the Dec axis, or, the tube may be lightened by an amount nearly equal to the weight of the polar axis, and there will then be practically no weight whatever on the bearings of that axis. So here we have a case of, say, an 80-ton telescope mounted and carried by an equatorial, but without throwing any weight whatever on that equatorial; and the force necessary to drive the instrument is independent of the weight of the telescope, and dependent only on the friction necessary to be overcome in carrying the tube at an exceedingly slow rate through the water.

Let us inquire into any possible disadvantages that may be urged against this form of mounting:—

1. That the temperature of the water will often be different from that of the air; and consequently that there will be a detrimental mixture, at the mouth of the tube, of air from inside the tube, which will partake of the temperature of the water, with the outside air. This I would propose to avoid by making the tube double, with a space of some 3 inches between inside and outside tubes, hermetically closed except at the lower end, where there would be apertures in the inside envelope. The space between the two tubes would be connected through the trunnions with an air pump, worked by a gas or other motor, which would continually exhaust the air from between the two tubes, and thus cause a current of the outside air to pass continually down the tube and back to the pump by the space between the two tubes. This would keep the temperature of the inside tube and the air in the tube constant with that of the outside air.

2. The limited range of the equatorial. I have stated that the instrument would be in perfect balance down to 25° from the horizon. If desired, though no longer perfectly balanced, it can be used lower by employing a chain or wire rope connected between the lower end of the tube and the upper end of the polar axis, and the amount which the instrument would be out of balance, between 25° and 20°, would be very trifling.

Again, it will not be convenient to use the instrument within some 15° of the pole. It could be planned to go somewhat closer, but when it is considered that nine-tenths of the work required to be done can be commanded by this instrument, it is clearly better to design it to do that nine-tenths well than to strain it into doing another 5° that would only be useful on very rare occasions.

3. It may be urged that the friction of the water will prevent the rapid setting of the instrument. In a telescope of this size all the motions would be effected by motors of some description, guided by the observer from a commutator-board at the eye end, and there would be no difficulty in setting the telescope quite as quickly as could be expected considering its great size.

4. It may be objected that currents will be set up in the water by the moving of the telescope, which currents will affect the steadiness. No doubt this will be the case to some extent, but these will soon subside, and the motion necessary for following the stars will be so slow that no perceptible effect of this kind will be felt from it.

As to convenience in getting at the eye end, there need be no difficulty whatever in this form. As the eye-piece is only about 15 feet from the centre of motion, the movement of the observer is never more than 3 feet per hour. By means of a platform such as that shown in Fig. 2, running on rails, and quite independent of the instrument, the eye end is readily accessible at all times. To overcome the rotation of the tube as the instrument moves in right ascension, I would pierce the tube for eye-pieces every 30° round its circumference, and mount the flat mirror and cell in a collar so as to enable it to be readily rotated through intervals of 30°. By these means the image of the celestial object to be observed could be sent through either or any of the perforations of the tube, and the observer always observe in the direction most convenient to himself.

There are various difficulties about this construction which may naturally suggest themselves, but there are none, I believe, which cannot be overcome.

This is hardly the place to discuss details, but if there be any here who are sufficiently interested in this new form of equatorial to desire further information, I would refer them to an article in the present month's issue of 'Knowledge' which deals with most of the difficulties.

Putting aside now the question of reflectors *versus* refractors, there are some directions, applicable equally to reflectors and refractors, in which it is evidently possible to improve our designs for large equatorial instruments. It is not the first time that I have urged similar developments, but the advantages of what I recommended were not so apparent then as they are now in the present advanced state of astronomical research.

It may be remembered that when I lectured here in the year 1886 I strongly urged the desirability of employing some form of motive power for carrying out the various motions required in manipulating the large equatorials, domes, &c., and exhibited a model design I had made for the Lick Observatory, which illustrated the manner in which these various motors could be controlled by the observer, as well as the then newly devised lifting floor arrangement. The latter, that is the lifting floor, was, as you are aware, adopted by the trustees of the Lick Observatory, and I suppose we may assume that it was considered a success, as it has been copied in the case of the two large observatories built since in the United States, viz. the Washington and the Chicago Observatories. The Lick Trustees, however, rejected the proposals for the employment of motors for the equatorial movements and dome. That experience gained since the construction of this telescope has confirmed the correctness of my views as respects the desirability of adopting this system is evident, as, in their latest and most perfect instrument, that of the Yerkes equatorial at Chicago, practically all the suggested improvements in that model exhibited before you in 1886 have been adopted, and I hope will contribute in no small degree to the quantity as well as the quality of the output of work we may expect from that splendid instrument; but to show that this by no means represents all that can be done in this direction, I have here a rough and unfinished model of an observatory in which the principle is carried still further.

It will be observed that in the Lick design the astronomer is relieved of all physical exertion, but still his attention is required during all the process of setting of the instrument. In our latest design we are able to relieve the astronomer of even the mental strain in this way. In some of our modern equatorials the setting of the circles can be placed (by a new arrangement) at the eye end of the telescope. Suppose this to be so arranged in the large telescope and that an arrangement be added something similar in principle to the steam steering gear of our large steamers, with which every one is now familiar. In this machine the construction is such that when a small light wheel is turned any quantity to port or starboard the motors are automatically set to work and force the helm over in the right direction, and do not stop until the position of the helm itself exactly corresponds to that of the light guiding wheel. A very simple arrangement of electrical contacts suffices to effect this in the case of the telescope, and the working of the instrument is effected thus:—

The astronomer decides what AR and what DEC he desires the telescope to be set at, he walks over to the eye end of the telescope, which, as will be seen further, *can only be* at a convenient height from the floor, and sets a pair of pointers at the eye end to the particular readings he wishes, and then presses a button to start the motors and "awaits developments."

Without any more attention from the astronomer the instrument now sets itself in exactly the position he requires, the motors continuing to revolve the telescope on its axes till that position is attained, and *then they stop.* Meanwhile as certain contacts are arranged at the upper end of the tube connected with the motor which drives the dome, and at the lower end with motors which elevate and depress the floor, the dome revolves if necessary and keeps the opening opposite the upper end of the telescope, while the floor rises and falls as may be necessary, always keeping at a convenient distance below the eye end; and so, as I said, without any attention or physical or mental strain on the part of the observer, he finds after a few minutes his telescope set correctly in AR and Decn—the dome opening is opposite the object-glass and the floor at a height most convenient for observation.

The idea may seem almost Utopian, but there are no particular difficulties in carrying it out, nor would it add sensibly to the cost of a large instrument.

If I have succeeded, even very imperfectly, in rendering a necessarily technical, and therefore somewhat dry, subject sufficiently interesting to have enabled you to follow me, you will have no difficulty in seeing that the principal ideas I desired to convey may be summed up shortly in this way:—

That, while on the one hand, the adoption of new methods for prosecuting astronomical research have created a set of conditions under which it is possible to use, and use with advantage, instruments of greater optical power than hitherto, yet, on the other hand, the mechanical arrangements for mounting these instruments must be of a much higher standard than has been necessary for the older methods; in fact, they must be mounted as instruments of precision, in the highest sense of the term, and while the mounting of instruments even larger than we have at present and suitable to the older conditions would not present any serious engineering difficulties, the problem of mounting them as instruments of precision is one of considerable magnitude—a fact which is well recognised by those who have studied the subject.

The time has gone by when with very few inches increase of aperture some sensational discovery is expected.

The astronomer of the future will not be satisfied with mere stargazing instruments. The experiments of the last few years show that there is no royal road to great astronomical discoveries, but that patient, honest and self-recording work is necessary to enable us to add (as our ambition is) course after course to the great edifice of astronomical truth.

But the ever-advancing work of the astronomer demands an ever-increasing perfection of his instruments, and the records of the past show that few of these improvements are to be credited either entirely to the astronomer, be he ever so practical, or to the instrument-maker, however ingenious, but to both working harmoniously together, the astronomer finding out the weak points of existing instruments, and the instrument-maker continually devising new contrivances to meet the difficulties of the astronomer.

I myself would like to take this opportunity of saying that whatever measure of success I may have had in my work is due in no small degree to the helpful kindness I have invariably received from those astronomers with whom I have been in communication.

As it has been in the past, so, let us hope, it will be in the future, for in this harmonious working together of those who design and those who work the instruments lies our strongest hope of the future development of the astronomical telescope.

[H. G.]

WEEKLY EVENING MEETING,

Friday, June 1, 1894.

Ludwig Mond, Esq. F.R.S. Vice-President, in the chair.

Professor Oliver Lodge, D.Sc. LL.D. F.R.S.

*The Work of Hertz.**

The untimely end of a young and brilliant career cannot fail to strike a note of sadness and awaken a chord of sympathy in the hearts of his friends and fellow-workers. Of men thus cut down in the early prime of their powers there will occur to us here the names of Fresnel, of Carnot, of Clifford, and now of Hertz. His was a strenuous and favoured youth; he was surrounded from his birth with all the influences that go to make an accomplished man of science—accomplished both on the experimental and on the mathematical side. The front rank of scientific workers is weaker by his death, which occurred on January 1, 1894, the thirty-seventh year of his life. Yet did he not go till he had effected an achievement which will hand his name down to posterity as the founder of an epoch in experimental physics.

In mathematical and speculative physics others had sown the seed. It was sown by Faraday, it was sown by Thomson and by Stokes, by Weber also doubtless, and by Helmholtz; but in this particular department it was sown by none more fruitfully and plentifully than by Clerk Maxwell. Of the seed thus sown Hertz reaped the fruits. Through his experimental discovery, Germany awoke to the truth of Clerk Maxwell's theory of light, of light and electricity combined, and the able army of workers in that country (not forgetting some in Switzerland, France and Ireland) have done most of the gleaning after Hertz.

This is the work of Hertz which is best known, the work which brought him immediate fame. It is not always that public notice is so well justified. The popular instinct is generous and trustful, and it is apt to be misled. The scientific eminence accorded to a few energetic persons by popular estimate is more or less amusing to those working on the same lines. In the case of Hertz no such mistake has been made. His name is not over well known, and his work is

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* The illustrations in this abstract appeared, after the delivery of the discourse, in a little book called "The Work of Hertz and some of his Successors," by Professor Lodge, and are inserted here by the kind permission of the proprietors of the *Electrician*.

immensely greater in every way than that of several who have made more noise.

In closing these introductory and personal remarks, I should like to say that the enthusiastic admiration for Hertz's spirit and character, felt and expressed by students and workers who came into contact with him, is not easily to be exaggerated. Never was a man more painfully anxious to avoid wounding the susceptibilities of others; and he was accustomed to deprecate the prominence given to him by speakers and writers in this country, lest it might seem to exalt him unduly above other and older workers among his own sensitive countrymen.

Speaking of the other great workers in physics in Germany, it is not out of place to record the sorrow with which we have heard of the recent death of Dr. August Kundt, Professor in the University of Berlin, successor to Von Helmholtz in that capacity.

When I consented to discourse on the work of Hertz, my intention was to repeat some of his actual experiments, and especially to demonstrate his less known discoveries and observations. But the fascination exerted upon me by electric oscillation experiments, when I, too, was independently working at them in the spring of 1888,* resumed its hold, and my lecture will accordingly consist of experimental demonstrations of the outcome of Hertz's work rather than any precise repetition of portions of that work itself.

In case a minority of my audience are in the predicament of not knowing anything about the subject, a five minutes' explanatory prelude may be permitted, though time at present is very far from being "infinitely long."

The simplest way will be for me hastily to summarise our knowledge of the subject before the era of Hertz.

Just as a pebble thrown into a pond excites surface ripples, which can heave up and down floating straws under which they pass, so a struck bell or tuning-fork emits energy into the air in the form of what are called sound waves, and this radiant energy is able to set up vibrations in other suitable elastic bodies.

If the body receiving them has its natural or free vibrations violently damped, so that when left to itself it speedily returns to rest, Fig. 1, then it can respond fully to notes of almost any pitch. This is the case with your ears and the tones of my voice. Tones must be exceedingly shrill before they cease to excite the ear at all.

If, on the other hand, the receiving body has a persistent period of vibration, continuing in motion long after it is left to itself, Fig. 2, like another tuning fork or bell, for instance, then far more facility of response exists, but great accuracy of tuning is necessary if it is to be fully called out; for if the receiver is not thus accurately syntonised with the source, it fails more or less completely to resound.

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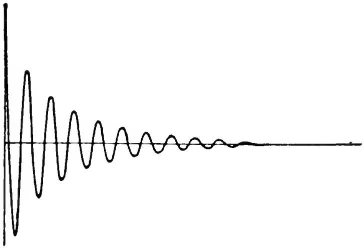
* Phil. Mag., xxvi. pp. 229, 230, August 1888; or "Lightning Conductors and Lightning Guards" (Whittaker), pp. 104, 105; also Proc. Roy. Soc., vol. 50, p. 27.

Conversely, if the *source* is a persistent vibrator, correct tuning is essential, or it will destroy at one moment, Fig. 3, motion which it originated the previous moment. Whereas, if it is a dead-beat or strongly-damped exciter, almost anything will respond equally well or equally ill to it.

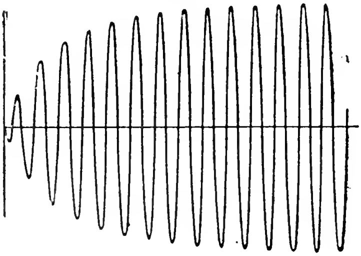
What I have said of sounding bodies is true of all vibrators in a medium competent to transmit waves. Now a sending telephone or a microphone, when spoken to, emits waves into the ether, and this radiant energy is likewise able to set up vibration in suitable bodies. But we have no delicate means of directly detecting these electrical or ethereal waves; and if they are to produce a perceptible effect at a distance, they must be confined, as by a speaking-tube, prevented from spreading, and concentrated on the distant receiver.

Fig. 1.



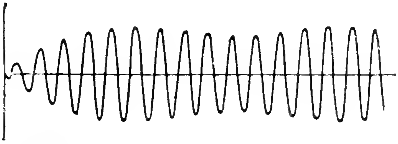
Oscillations of Dumb-bell Hertz Vibrator (after Bjerknes).

Fig. 2.



Oscillation of Ring-shaped Hertz Resonator excited by the syntonic Vibrator which gave the curve Fig. 1 (after Bjerknes).

Fig. 3.



Oscillation of Ring Resonator similarly excited but not quite syntonic with Radiator. (For method of obtaining these curves see Fig. 14.)

This is the function of the telegraph wire; it is to the ether what a speaking-tube is to air. A metal wire in air (*in function*, not in details of analogy) is like a long hollow cavity surrounded by nearly rigid but slightly elastic walls.

Furthermore, any conductor electrically charged or discharged with sufficient suddenness must emit electrical waves into the ether, because the charge given to it will not settle down instantly, but will surge to and fro several times first; and these surgings or electric oscillations must, according to Maxwell, start waves in the ether, because at the end of each half-swing they cause electrostatic, and at the middle of each half-swing they cause electromagnetic effects, and the rapid alternation from one of these modes of energy to the other constitutes ethereal waves.* If a wire is handy they will run along it, and may be felt a long way off. If no wire exists they will spread out like sound from a bell, or light from a spark, and their intensity will decrease according to the inverse square of the distance.

Maxwell and his followers well knew that there would be such waves; they knew the rate at which they would go, they knew that they would go slower in glass and water than in air, they knew that they would curl round sharp edges, that they would be partly absorbed but mainly reflected by conductors, that if turned back upon themselves they would produce the phenomena of stationary waves, or interference, or nodes and loops; it was known how to calculate the length of such waves, and even how to produce them of any required or predetermined wave-length from 1000 miles to a foot. Other things were known about them which would take too long to enumerate; any homogeneous insulator would transmit them, would refract or concentrate them if it were of suitable shape, would reflect none of a particular mode of vibration at a certain angle, and so on, and so on.

All this was *known*, I say, known with varying degrees of confidence; but by some known with as great confidence as, perhaps even more confidence than, is legitimate before the actuality of experimental verification.

Hertz supplied the verification. He inserted suitable conductors in the path of such waves, conductors adapted for the occurrence in them of induced electric oscillations, and to the surprise of every one, himself, doubtless, included, he found that the secondary electric surgings thus excited were strong enough to display themselves by minute electric sparks.

I shall show this in a form which requires great precision of tuning, or syntony, both emitter and receiver being persistently vibrating things giving some 30 or 40 swings before damping has a serious effect. I take two Leyden jars with circuits about a yard in diameter, and situated about two yards apart, Fig. 4. I charge and discharge one jar, and observe that the surgings set up in the other can cause it to overflow if it is syntonised with the first.†

A closed circuit such as this is a feeble radiator and a feeble

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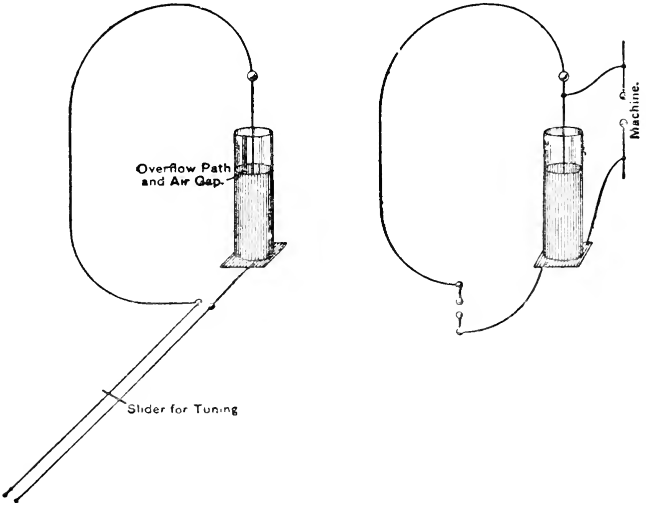
* Strictly speaking, in the waves themselves there is no lag or difference of phase between the electric and the magnetic vibrations; the difference exists in emitter or absorber, but not in the transmitting medium. True radiation of energy does not begin till about a quarter wave-length from the source, and within that distance the initial quarter period difference of phase is obliterated.

† *See* 'Nature,' vol. xli. p. 368; or J. J. Thomson, 'Recent Researches,' p. 395.

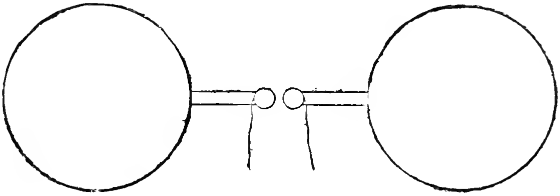
absorber, so it is not adapted for action at a distance. In fact, I doubt whether it will visibly act at a range beyond the $\frac{1}{4}\lambda$ at which true radiation of broken-off energy occurs. If the coatings of the jar are separated to a greater distance, so that the dielectric is more exposed, it radiates better; because in true radiation the electrostatic and the magnetic energies are equal, whereas in a ring circuit the magnetic energy greatly predominates. By separating the coats of the jar as far as possible we get a typical Hertz vibrator, Fig. 5, whose dielectric extends out into the room, and this radiates very powerfully.

FIG. 4.



Experiment with syntonic Leyden Jars.

FIG. 5.



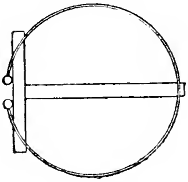
Standard Hertz Radiator.

In consequence of its radiation of energy, its vibrations are rapidly damped, and it only gives some three or four good strong swings, Fig. 1. Hence it follows that it has a wide range of excitation; i. e. it can excite sparks in conductors barely at all in tune with it.

The two conditions, conspicuous energy of radiation and persistent vibration electrically produced, are at present incompatible. Whenever these two conditions coexist, considerable power or activity will, of course, be necessary in the source of energy. At present they only coexist in the sun and other stars, in the electric arc and in furnaces.

The receiver Hertz used was chiefly a circular resonator, Fig. 6, not a good absorber but a persistent vibrator, well adapted for picking up disturbances of precise and measurable wave-length. Its mode of vibration when excited by an emitter in tune with it is depicted in Fig. 2. I find that the circular resonators can act as senders too; here is one (Fig. 6A) exciting quite long sparks in a second one.

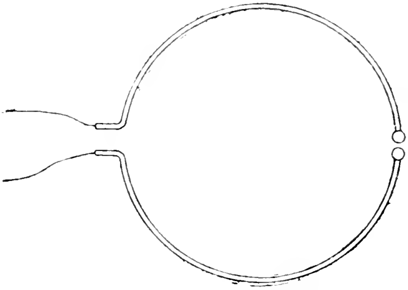
FIG. 6.



Circular Resonator. (The knobs ought to nearly touch each other.)

Electric Syntony:—that was his discovery, but he did not stop there. He at once proceeded to apply his discovery to the verification of what had already been predicted about the waves, and by laborious and difficult interference experiments he ascertained that the previously calculated length of the waves was thoroughly borne out by fact. These interference experiments in free space are his greatest achievement.

FIG. 6A.

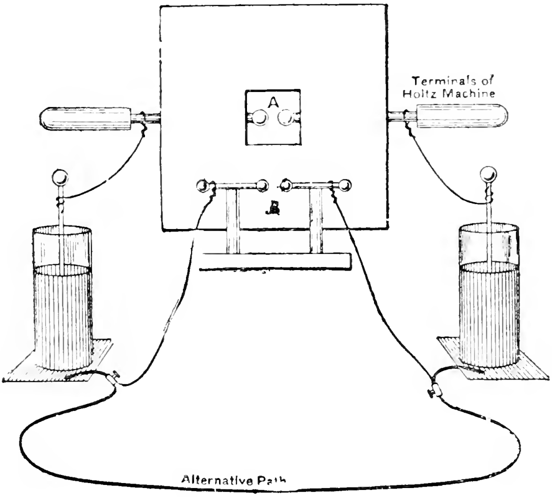


Any circular resonator can be used as a sender by bringing its knobs near the sparking knobs of a coil; but a simple arrangement is to take two semicircles, as in above figure, and make them the coil terminals. The capacity of the cut ends can be varied, and the period thereby lengthened, by expanding them into plates.

He worked out every detail of the theory splendidly, separately analysing the electric and the magnetic oscillation, using language not always such as we should use now, but himself growing in theoretic insight through the medium of what would have been to most physicists a confusing maze of troublesome facts, and disentangling all their main relations most harmoniously.

While Hertz was observing sparks such as these, the primary or exciting spark and the secondary or excited one, he observed as a bye-issue that the secondary spark occurred more easily if the light from the primary fell upon its knobs. He examined this new influence of light in many ways, and showed that although spark light and electric brush light were peculiarly effective, any source of light that gave very ultra-violet rays produced the same result.

Fig. 7.



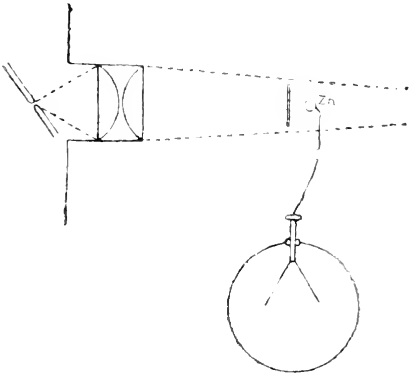
Experiment arranged to show effect on one spark of light from another. The B spark occurs more easily when it can see the A spark through the window, unless the window is glazed with glass. A quartz pane transmits the effect.

The above figure represents my way of showing the experiment. It will be observed that with this arrangement the B knobs are at the same potential up to the instant of the flash, and in that case the ultra-violet portion of the light of the A spark assists the occurrence of the B spark. But it is interesting to note, what Elster and Geitel have found, that if the B knobs were subjected to steady strain instead of to impulsive rush—e. g. if they were connected to the inner coats of the jars instead of to the outer coatings—that then the effect of ultra-violet light on either spark-gap would exert a deterrent influence, so that the spark would probably occur at the other, or non-illuminated gap. With these altered connections it is, of course, not feasible to illuminate one spark by the light of the other; the sparks are then alternative, not successive.

Wiedemann and Ebert, and a number of experimenters, have repeated and extended this discovery, proving that it is the cathode knob on which illumination takes effect; and Hallwachs and Righi independently made the important observation, which Elster and Geitel, Stoletow, Branly and others have extended, that a freshly-polished zinc or other oxidisable surface, if charged negatively, is gradually discharged by ultra-violet light.

It is easy to fail in reproducing this experimental result if the right conditions are not satisfied; but if they are it is absurdly easy, and the thing might have been observed nearly a century ago.

Fig. 8.

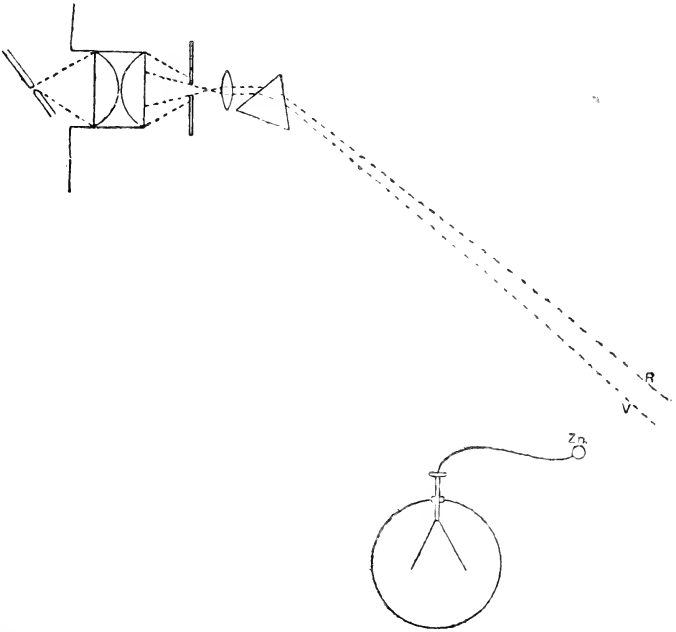


Zinc Knob in Arc Light, protected by Glass Screen. The lenses are of quartz, but there is no need for any lenses in this experiment; leakage begins directly the glass plate is withdrawn.

Take a piece of zinc, clean it with emery paper, connect it to a gold leaf electroscope, and expose it to an arc lamp, Fig. 8. If charged positively nothing appears to happen, the action is very slow; but a negative charge leaks away in a few seconds if the light is bright. Any source of light rich in ultra-violet rays will do; the light from a spark is perhaps most powerful of all. A pane of glass cuts off all the action; so does atmospheric air in sufficient thickness (at any rate, town air), hence sunlight is not powerful. A pane of quartz transmits the action almost undiminished, but fluor-spar may be more transparent still. Condensing the arc rays with a quartz lens and analysing them with a quartz prism or reflection grating, we find that the most effective part of the light is high up in the ultra-violet, surprisingly far beyond the limits of the visible spectrum* (Fig. 9).

This is rather a digression, but I have taken some pains to show it properly because of the interest betrayed by Lord Kelvin on this matter, and the caution which he felt about accepting the results of the Continental experimenters too hastily.

Fig. 9.



Zinc Knob discharging Negative Electricity in the very Ultra-violet Light of a Spectrum formed by a Quartz Train. Right position of Knob shown.

It is probably a chemical phenomenon, and I am disposed to

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* While preparing for the lecture it occurred to me to try, if possible during the lecture itself, some new experiments on the effect of light on negatively charged bits of rock and ice, because if the effect is not limited to metals it must be important in connection with atmospheric electricity. When Mr. Branly coated an aluminium plate with an insulating varnish, he found that its charge was able to soak in and out of the varnish during illumination ('Comptes Rendus,' vol. cx. p. 898, 1890). Now, the mountain tops of a negatively charged earth are exposed to very ultra-violet rays, and the air is a dielectric in which quiet up-carrying and sudden downpour of electricity could go on in a manner not very unlike the well-known behaviour of water vapour; and this, perhaps, may be the reason, or one of the reasons, why it is not unusual to experience a thunderstorm

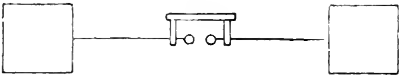
express it as a modification of the Volta contact effect* with illumination.

Return now to the Hertz vibrator, or Leyden jar with its coatings well separated, so that we can get into its electric as well as its magnetic field. Here is a great one giving waves 30 metres long, radiating while it lasts with an activity of 100 H.P., and making ten million complete electric vibrations per second (Fig. 10).

Its great radiating power damps it down very rapidly, so that it does not make above two or three swings; but nevertheless, each time it is excited, sparks can be drawn from most of the reasonably elongated conductors in this theatre.

A suitably situated gas-leak can be ignited by these induced sparks. An Abel's fuse connecting the water pipes with the gas pipes will blow off; vacuum tubes connected to nothing will glow (this fact has been familiar to all who have worked with Hertz waves since 1889); electric leads, if anywhere near each other, as they are in some incandescent lamp holders, may spark across to each other, thus striking an arc and blowing their fuses. This blowing of fuses by electric radiation frequently happened at Liverpool till the suspensions of the theatre lamps were altered.

FIG. 10.



Large Hertz Oscillator on reduced scale, $\frac{1}{10}$ inch to a foot.

The striking of an arc by the little reverberating sparks between two lamp-carbons connected with the 100-volt mains I incidentally

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after a few fine days. I have now tried these experiments on such geological fragments as were handy, and find that many of them discharge negative electricity under the action of a naked arc, especially from the side of the specimens which was somewhat dusty, but that when wet they discharge much less rapidly, and when positively charged hardly at all. Ice and garden soil discharge negative electrification, too, under ultra-violet illumination, but not so quickly as limestone, mica schist, ferruginous quartz, clay and some other specimens. Granite barely acts: it seems to insulate too well. The ice and soil were tried in their usual moist condition, but, when thoroughly dry, soil discharges quite rapidly. No rock tested was found to discharge as quickly as does a surface of perfectly bright metal, such as iron, but many discharged much more quickly than ordinary dull iron, and rather more quickly than when the bright iron surface was thinly oiled or wetted with water. To-day (June 5) I find that the leaves of Geranium discharge positive electrification five times as quickly as negative, under the action of an arc-light, and that glass cuts the effect off while quartz transmits it.

[*Added later.* Arrhenius has had the same notion about atmospheric electricity; and Elster and Geitel have made elaborate and careful experiments on the subject. Wied. Ann. vols. xxxix., xl., xli., &c.]

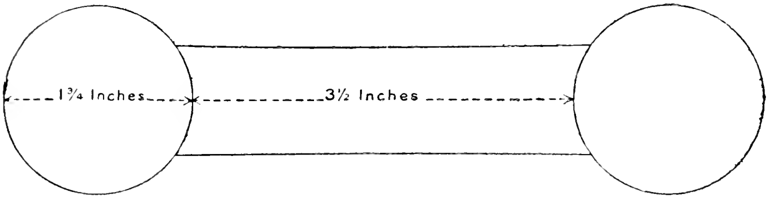
* See Brit. Assoc. Report, 1884, pp. 502–519, or Phil. Mag., vol. xix. pp. 267–352.

now demonstrate. An arc is started directly the large Hertz vibrator is excited at a distance.

There are some who think that lightning flashes can do none of these secondary things. They are mistaken.

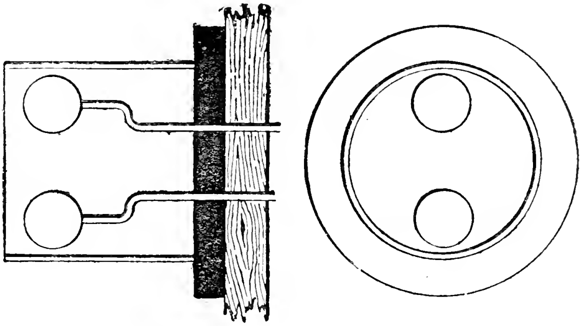
On the table are specimens of various emitters and receivers such as have been used by different people; the orthodox Hertz radiator of dumb-bell type, Fig. 5, and the orthodox Hertz receivers:—a circular ring, Fig. 6, for interference experiments, because it is but little damped, and a straight wire for receiving at a distance, because it is a much better absorber. Beside these are the spheres and ellipsoids (or elliptical plates), which I have mainly used, Fig. 19, because they are powerful radiators and absorbers, and because their theory has been worked out by Horace Lamb and J. J. Thomson. Also dumb-bells, Fig. 11, without air-gap, and many other shapes, the most recent of mine being the inside of a hollow cylinder with sparks at ends of a diameter, Fig. 12; this being a feeble radiator, but a very

FIG. 11.



A Dumb-bell Form of Radiator.

FIG. 12.

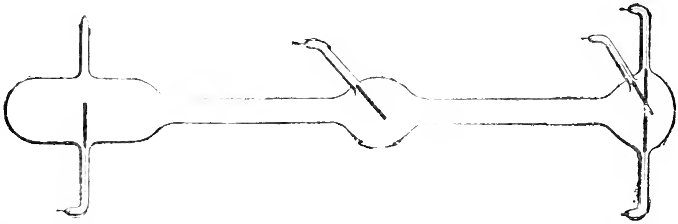


Dr. Lodge's Hollow Cylindrical Radiator, arranged horizontally against the outside of a metal-lined box. Half natural size. Emitting 3-inch waves.

persistent vibrator,* and, therefore, well adapted for interference and diffraction experiments. But, indeed, spheres can be made to vibrate longer than usual by putting them into copper hats or enclosures, in which an aperture of varying size can be made to let the waves out, Figs. 20 and 21.

Many of these senders will do for receivers too, giving off sparks to other insulated bodies or to earth; but, besides the Hertz type of receiver, many other detectors of radiation have been employed. Vacuum tubes can be used, either directly or on the trigger principle, as by Zehnder, Fig. 13,† the resonator spark precipitating a discharge from some auxiliary battery or source of energy, and so making a feeble disturbance very visible. Explosives may be used for the same purpose, either in the form of mixed water-gases or in the form of an Abel's fuse. Fitzgerald found that a tremendously sensitive galvanometer could indicate that a feeble spark had passed, by reason of the consequent disturbance of electrical equilibrium which settled down again through the galvanometer.‡ This was the method he used in this theatre four years ago. Blyth used a one-sided electrometer, and V. Bjerknes has greatly developed this method, Fig. 14, abolishing the need for a spark, and making the electrometer metrical, integrating and satisfactory.§ With this detector many measurements have been made at Bonn by Bjerknes, Yule, Barton, and others, on waves concentrated and kept from space dissipation by guiding wires.

Fig. 13.



Zehnder's Trigger Tube. Half natural size. The two right-hand terminals, close together, are attached to the Hertz receiver; another pair of terminals are connected to some source just not able to make the tube glow until the scintilla occurs and makes the gas more conducting—as observed by Schuster and others.

Mr. Boys has experimented on the mechanical force exerted by electrical surgings, and Hertz also made observations of the same kind.

Going back to older methods of detecting electrical radiation, we have, most important of all, a discovery made long before man

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[*] J. J. Thomson, 'Recent Researches,' 344.

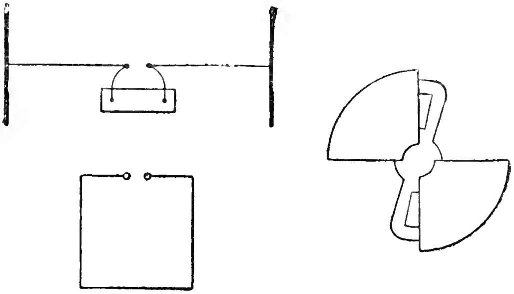
[†] Wied. Ann., xlvii. p. 77.

[‡] Fitzgerald, 'Nature,' vol. xli. p. 295, and vol. xlii. p. 172.

[§] Wied. Ann., xliv. p. 74.

existed, by a creature that developed a sensitive cavity on its skin; a creature which never so much as had a name to be remembered by (though perhaps we now call it trilobite). Then, in recent times we recall the photographic plate and the thermopile, with its modification, the radiomicrometer; also the so-called bolometer, or otherwise-known Siemens' pyrometer, applied to astronomy by Langley, and applied to the detection of electric waves in wires by Rubens and Ritter and Paalzow and Arons. The thermal junction was applied to the same purpose by Klemencik, D. E. Jones and others.

Fig. 14.



Bjerknes' Apparatus, showing (1) a Hertz vibrator connected to an induction coil; (2) a nearly-closed circuit receiver properly tuned with the vibrator; and (3) a one-sided electrometer for inserting in the air-gap of 2. The receiver is not provided with knobs, as shown, but its open circuit is terminated by the quadrants of the electrometer, which is shown on an enlarged scale alongside. The needle is at zero potential and is attracted by both quadrants. By calculation from the indications of this electrometer Bjerknes plotted the curves 1, 2 and 3 on page 329. Fig. 1 represents the oscillations of the primary vibrator, rapidly damped by radiation of energy. Fig. 2 represents the vibrations thereby set up in the resonating circuit when the two are accurately in tune; and which persist for many swings. Fig. 3 shows the vibrations excited in the same circuit when slighly out of tune with the exciter. A receiver of this kind makes many swings before it is seriously damped.

And, before all these, the late Mr. Gregory, of Cooper's Hill, made his singularly sensitive expansion meter, whereby waves in free space could be detected by the minute rise of temperature they caused in a platinum wire, a kind of early and sensitive form of Cardew voltmeter. Boys, Briscoe and Watson developed this method.

Going back to the physiological method of detecting surgings, Hertz tried the frog's-leg nerve and muscle preparation, which to the steadier types of electrical stimulus is so surpassingly sensitive, and to which we owe the discovery of current electricity. But he failed to get any result. Ritter has succeeded; but in my experience, failure is the normal and proper result. Working with my colleague, Prof. Gotch, at Liverpool, I too have tried the nerve and muscle preparation of the frog, Fig. 15, and we find that an excessively violent stimulus of a rapidly alternating character, if pure and unaccompanied by secondary actions, produces no effect—no stimulating effect, that is, even though the voltage is so high that sparks are ready to jump between the needles in direct contact with the nerve.

All that such oscillations do, if continued, is to produce a temporary paralysis or fatigue of the nerve, so that it is unable to transmit the nerve impulses evoked by other stimuli, from which paralysis it recovers readily enough in course of time.

This has been expected from experiments on human beings, such experiments as Tesla's and those of d'Arsonval. But an entire animal is not at all a satisfactory instrument wherewith to attack the question; its nerves are so embedded in conducting tissues that it may easily be doubted whether the alternating type of stimulus ever reaches them at all. By dissecting out a nerve and muscle from a deceased frog after the historic manner of physiologists, and applying the stimulus direct to the nerve, at the same time as some other well known $\frac{1}{100}$ of a volt stimulus is applied to another part of the same nerve further from the muscle, it can be shown that rapid electric alternations, if entirely unaccompanied by static charge or by resultant algebraic electric transmission, evoke no excitatory response until they are so violent as to give rise to secondary effects such as heat or mechanical shock. Yet, notwithstanding this inaction they gradually and slowly exert a paralysing or obstructive action on the portion of the nerve to which they are applied, so that the nerve impulse excited by the feeble, just perceptible $\frac{1}{100}$-volt stimulus above is gradually throttled on its way down to the muscle, and remains so throttled for a time varying from a few minutes to an hour after the cessation of the violence.

FIG. 15.

Experiment of Gotch and Lodge on the physiological effect of rapid pure electric alternations. Nerve-muscle preparation, with four needles, or else non-polarisable electrodes applied to the nerve. C and D are the terminals of momentary rapidly alternating electric current from a conductor at zero potential, while A and B are the terminals of an ordinary very weak galvanic or induction coil stimulus only just sufficient to make the muscle twitch.

Among trigger methods of detecting electric radiation, I have spoken of the Zehnder vacuum tubes; another method is one used by Boltzmann.* A pile of several hundred volts is on the verge of charging an electroscope through an air gap just too wide to break

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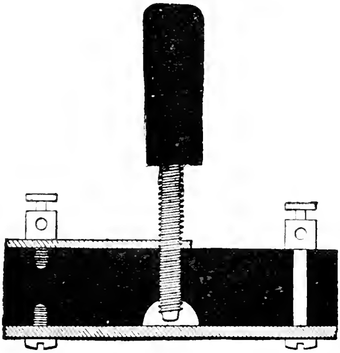
* Wied. Ann., xl. p. 399.

down. Very slight electric surgings precipitate the discharge across the gap, and the leaves diverge. I show this in a modified and simple form. On the cap of an electroscope is placed a highly-polished knob or rounded end connected to the sole, and just not touching the cap, or rather just not touching a plate connected with the cap, Fig. 16, the distance between knob and plate being almost infinitesimal, such a distance as is appreciated in spherometry. Such an electroscope overflows suddenly and completely with any gentle rise of potential. Bring excited glass near it, the leaves diverge gradually and then suddenly collapse, because the air space snaps; remove the glass and they rediverge with negative electricity; the knob above the cap being then charged positively and to the verge of sparking. In this condition any electrical waves, collected if weak by a foot or so of wire projecting from the cap, will discharge the electroscope by exciting surgings in the wire, and so breaking down the air-gap. The chief interest about this experiment seems to me the extremely definite dielectric strength of so infinitesimal an air space. Moreover, it is a detector for Hertz waves that might have been used last century; it might have been used by Benjamin Franklin.

Fig. 16.



Air-gap for Electroscope. Natural size. The bottom plate is connected to, and represents, the cap of an electroscope; the "knob" above it, mentioned in text, is the polished end of the screw, whose terminal is connected with the case of the instrument or "earth."

For to excite them no coil or anything complicated is necessary; it is sufficient to flick a metal sphere or cylinder with a silk handkerchief and then discharge it with a well-polished knob. If it is not well polished the discharge is comparatively gradual, and the vibrations are weak; the more polished are the sides of an air-gap, the more sudden is the collapse and the more vigorous the consequent radiation, especially the radiation of high frequency, the higher harmonics of the disturbance.

For delicate experiments it is sometimes well to repolish the knobs every hour or so. For metrical experiments it is often better to let the knobs get into a less efficient but more permanent state. This is true of all senders or radiators. For the generation of the, so to speak, "infra-red" Hertz waves any knobs will do, but to generate the "ultra-violet" high polish is essential.

Receivers or detectors, which for the present I temporarily call microphonic, are liable to respond best to the more rapid vibrations.

Their sensitiveness is to me surprising, though of course it does not approach the sensitiveness of the eye; at the same time, I am by no means sure that the eye differs from them in kind. It is these detectors that I wish specially to bring to your notice.

Prof. Minchin, whose long and patient work in connection with photo-electricity is now becoming known, and who has devised an instrument more sensitive to radiation than even Boys' radiomicrometer, in that it responds to the radiation of a star while the radiomicrometer does not, found some years ago that some of his light-excitable cells lost their sensitiveness capriciously on tapping, and later he found that they frequently regained it again while Mr. Gregory's Hertz-wave experiments were going on in the same room.

These "impulsion-cells," as he terms them, are troublesome things for ordinary persons to make and work with—at least I have never presumed to try—but in Mr. Minchin's hands they are surprisingly sensitive to electric waves.*

The sensitiveness of selenium to light is known to every one, and Mr. Shelford Bidwell has made experiments on the variations of conductivity exhibited by a mixture of sulphur and carbon.

Nearly four years ago M. Edouard Branly found that a burnished coat of porphyrised copper spread on glass diminished its resistance enormously, from some millions to some hundreds of ohms when it was exposed to the neighbourhood, even the distant neighbourhood, of Leyden jar or coil sparks. He likewise found that a tube of metallic filings behaved similarly, but that this recovered its original resistance on shaking. Dr. Dawson Turner exhibited this fact recently at the Edinburgh meeting of the British Association, and Mr. Croft has shown it to the Physical Society. M. Branly also made pastes and solid rods of filings, in Canada balsam and in sulphur, and found them likewise sensitive.†

With me the matter arose somewhat differently, as an outcome of the air-gap detector employed with an electroscope by Boltzmann. For I had observed in 1889 that two knobs sufficiently close together, far too close to stand any voltage such as an electroscope can show, could, when a spark passed between them, actually cohere; conducting an ordinary bell-ringing current if a single voltaic cell was in circuit; and, if there were no such cell, exhibiting an electromotive force of their own sufficient to disturb a low resistance galvanometer vigorously, and sometimes requiring a faintly perceptible amount of force to detach them. The experiment was described to the Institution of Electrical Engineers,‡ and Prof. Hughes said he had observed the same thing.

Well, this arrangement, which I call a coherer, is the most

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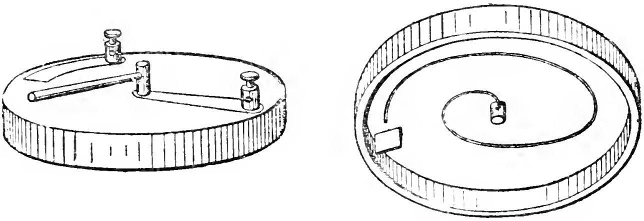
* Phil. Mag., vol. xxxi. p. 223.

† E. Branly, 'Comptes Rendus,' vol. cxi. p. 785; and vol. cxii. p. 90.

‡ 'Journal' Institution of Electrical Engineers, 1890, vol. xix. pp. 352-4; or 'Lightning Conductors and Lightning Guards' (Whittaker), pp. 382-4.

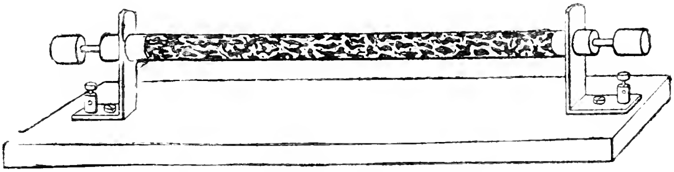
astonishingly sensitive detector of Hertz waves. It differs from an actual air-gap in that the insulating film is not really insulating; the film breaks down not only much more easily, but also in a less discontinuous and more permanent manner, than an air-gap. A tube of filings, being a series of bad contacts, clearly works on the same plan; and though a tube of filings is by no means so sensitive, yet it is in many respects easier to work with, and, except for very feeble stimuli, is more metrical. If the filings used are coarse, say turnings or borings, the tube approximates to a single coherer; if they are fine, it has a larger range of sensibility. In every case what these receivers feel are sudden jerks of current; smooth sinuous vibrations are ineffective. They seem to me to respond best to waves a few inches long, but doubtless that is determined chiefly by the dimensions of some conductor with which they happen to be associated. Figs. 17 and 18.

Fig. 17.



Coherer, consisting of a spiral of thin iron wire mounted on an adjustable spindle and an aluminium plate. When the lever is moved clockwise, the tip of the iron wire presses gently against the aluminium plate.

Fig. 18.



Iron Borings Tube. One-third natural size.

I picture to myself the action as follows: Suppose two fairly clean pieces of metal in light contact—say two pieces of iron—connected to a single voltaic cell; a film of what may be called oxide intervenes between the surfaces, so that only an insignificant current is allowed to pass, because a volt or two is insufficient to break down the insulating film, except perhaps at one or two atoms.* If the film is not permitted to conduct at all, it is not very sensitive; the most sensitive condition is obtained when an infinitesimal current passes, strong enough just to show on a moderate galvanometer.

Now let the slightest surging occur, say by reason of a sphere being charged and discharged at a distance of forty yards; the film at once breaks down—perhaps not completely, that is a question of intensity—but permanently. As I imagine, more molecules get within each other's range, incipient cohesion sets in, and the momentary electric quiver acts somewhat like a flux. It is a singular variety of electric welding. A stronger stimulus enables more molecules to hold on, the process is surprisingly metrical; and, as far as I roughly know at present, the change of resistance is proportional to the energy of the electric radiation, from a source of given frequency.

It is to be specially noted that a battery current is not needed to *effect* the cohesion, only to demonstrate it. The battery can be applied after the spark has occurred, and the resistance will be found changed as much as if the battery had been on all the time.

The incipient cohesion electrically caused can be mechanically destroyed. Sound vibrations or any other feeble mechanical disturbances, such as scratches or taps, are well adapted to restore the contact to its original high-resistance sensitive condition. The more feeble the electrical disturbance the slighter is the corresponding mechanical stimulus needed for restoration. When working with the radiating sphere, Fig. 19, at a distance of forty yards out of window, I could not for this reason shout to my assistant, to cause him to press the key of the coil and make a spark, but I showed him a duster instead, this being a silent signal which had no disturbing effect on the coherer or tube of filings. I mention 40 yards, because that was one of the first outdoor experiments; but I should think that something more like half a mile was nearer the limit of sensitiveness. However, this is a rash statement not at present verified. At 40 or 60 yards the exciting spark could be distinctly heard, and it was interesting to watch the spot of light begin its long excursion and actually travel a distance of 2 inches or 3 inches before the sound arrived. This experiment proved definitely enough that the efficient cause travelled quicker than sound, and disposed completely of any sceptical doubts as to sound-waves being, perhaps, the real cause of the phenomenon.

Invariably, when the receiver is in good condition, sound or other mechanical disturbance acts one way, viz. in the direction of increasing resistance, while electrical radiation or jerks act the other way, decreasing it. While getting the receiver into condition, or when it is getting out of order, vibrations and sometimes electric discharges act irregularly; and an occasional good shaking does the

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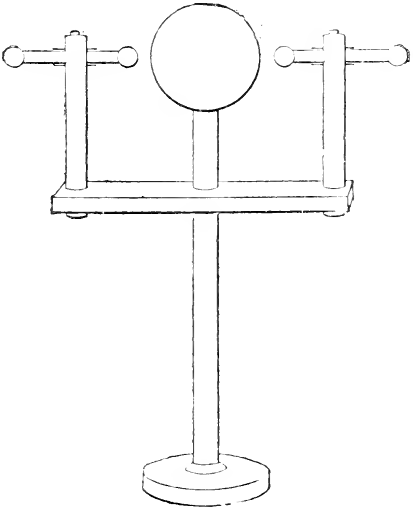
* *See* Phil. Mag. Jan. 1894, p. 94.

filings good. I have taken rough measurements of the resistance, by the simple process of restoring the original galvanometer deflection by adding or removing resistance coils. A $\frac{1}{2}$-inch tube 8 inches long of selected iron turnings, Fig. 18, had a resistance of 2500 ohms in the sensitive state. A feeble stimulus, caused by a distant electrophorus spark, brought it down 400 ohms. A rather stronger one reduced it by 500 and 600, while a trace of spark given to a point of the circuit itself, ran it down 1400 ohms.

This is only to give an idea of the quantities. I have not yet done any seriously metrical experiments.

From the wall diagram which summarises the various detectors, and which was prepared a month or so ago, I see I have omitted selenium, a substance which in certain states is well known to behave to visible light as these other microphonic detectors behave to Hertz waves. It is now inserted, but with a query to indicate that its position in the table is not *certainly* known.

Fig. 19.



Radiator used in the library of the Royal Institution, exciting the Coherer (Fig. 17) on the lecture table in the theatre. (Sphere 5 inches diameter.)

And I want to suggest that quite possibly the sensitiveness of the eye is of the same kind. As I am not a physiologist I cannot be seriously blamed for making wild and hazardous speculations in that region. I therefore wish to guess that some part of the retina is an electrical organ, say like that of some fishes, maintaining an electromotive force which is prevented from stimulating the nerves solely by an intervening layer of badly conducting material, or of conducting material with gaps in it; but that when light falls upon the retina these gaps become more or less conducting, and the nerves are stimulated. I do not feel clear which part is taken by the rods and cones, and which part by the pigment cells; I must not try to make the hypothesis too definite at present.

If I had to make a demonstration model of the eye on these lines, I should arrange a little battery to excite a frog's nerve-muscle preparation through a circuit completed all except a layer of filings or a single bad contact. Such an arrangement would respond to Hertz waves. Or, if I wanted actual light to act, instead of grosser waves, I would use a layer of selenium.

But the bad contact and the Hertz waves are the most instructive, because we do not at present really know what the selenium is doing, any more than what the retina is doing.

And observe that (to my surprise, I confess) the rough outline of a theory of vision thus suggested is in accordance with some of the principal views of the physiologist Hering. The sensation of light is due to the electrical stimulus; the sensation of black is due to the mechanical or tapping back stimulus. Darkness is physiologically not the mere cessation of light. Both are positive sensations, and both stimuli are necessary; for until the filings are tapped back vision is persistent. In the eye model the period of mechanical tremor should be, say, $\frac{1}{10}$ second, so as to give the right amount of persistence of impression.

No doubt in the eye the tapping back is done automatically by the tissues, so that it is always ready for a new impression, until fatigued. And by mounting an electric bell or other vibrator on the same board as a tube of filings, it is possible to arrange so that a feeble electric stimulus shall produce a feeble steady effect, a stronger stimulus a stronger effect, and so on; the tremor asserting its predominance, and bringing the spot back whenever the electric stimulus ceases.

An electric bell thus close to the tube is, perhaps, not the best vibrator; clockwork might do better, because the bell contains in itself a jerky current, which produces one effect, and a mechanical vibration, which produces an opposite effect; hence the spot of light can hardly keep still. By lessening the vibration—say, by detaching the bell from actual contact with the board, the electric jerks of the intermittent current drive the spot violently up the scale; mechanical tremor brings it down again.

You observe that the eye on this hypothesis is, in electrometer language, heterostatic. The energy of vision is supplied by the organism; the light only pulls a trigger. Whereas the organ of hearing is idiostatic. I might draw further analogies between this arrangement and the eye, e. g. about the effect of blows or disorder causing irregular conduction, and stimulation, of the galvanometer in the one instrument, of the brain cells in the other.

DETECTORS OF RADIATION.

Physiological.	Chemical.	Thermal.	Electrical.	Mechanical.	Microphonic.
					Selenium. (?)
Eye.	Photographic Plate.	Thermopile.	Spark. (Hertz.)	Electrometer. (Blyth and Bjerknes.)	Impulsion Cell. (Minchin.)
× Frog's Leg. (Hertz and Ritter.)	Explosive Gases.	Bolometer. (Rubens and Ritter.)	{Telephone; Air-gap and Arc.} (Lodge.)	Suspended Wires. (Hertz and Boys.)	Filings. (Branly.)
	Photoelectric Cell.	Expanding Wire. (Gregory.)	Vacuum Tube. (Dragoumis.)		Coherer. (Hughes and Lodge.)
		Thermal Junction. (Klemencik.)	Galvanometer. (Fitzgerald.)		
			Air-gap and Electroscope. (Boltzmann.)		
			Trigger Tube. (Warburg and Zehnder.)		

× The cross against the frog's leg indicates that it does not appear really to respond to radiation unless stimulated in some secondary manner. The names against the other things are unimportant, but suggest the persons who applied the detector to electric radiation. The interrogation mark against Selenium indicates that its position in the microphonic column may be doubtful.

A handy portable exciter of electric waves is one of the ordinary hand electric gas-lighters, containing a small revolving doubler, i. e. an inductive or replenishing machine. A coherer can feel a gas-lighter across a lecture theatre. Minchin often used them for stimulating his impulsion cells. I find that when held near they act a little even when no ordinary spark occurs, plainly because of the little incipient sparks at the brushes or tinfoil contacts inside. A Voss machine acts similarly, giving a small deflection while working up before it sparks.

And notice here that our model eye has a well-defined range of vision. It cannot see waves too long for it. The powerful disturbance caused by the violent flashes of a Wimshurst or Voss machine it is blind to. If the knobs of the machine are well polished it will respond to some high harmonics, due to vibrations in the terminal rods; and these are the vibrations to which it responds when excited simply by an induction coil. The coil should have knobs instead of points. Sparks from points or dirty knobs hardly excite the coherer at all. But hold a well-polished sphere or third knob between even the dirty knobs of a Voss machine, and the coherer responds at once to the surgings got up in it.

Feeble short sparks, again, are often more powerful exciters than are strong long ones. I suppose because they are more sudden.

This is instructively shown with an electrophorus lid. Spark it to a knuckle, and it does very little. Spark it to a knob and it works well. But now try this experiment:—first spark it to an insulated sphere, there is some effect; discharge the sphere, and take a second spark, without recharging the lid; do this several times; and at last, when the spark is inaudible, invisible, and otherwise imperceptible, the coherer some yards away responds more violently than ever, and the spot of light rushes from the scale.

If a coherer be attached by a side wire to the gas pipes, and an electrophorus spark be given to either the gas pipes or the water pipes, or even to the hot-water system in any other room of the building, the coherer responds.

In fact, when thus connected to gas-pipes one day when I tried it, the spot of light could hardly keep still for five seconds. Whether there was a distant thunderstorm, or whether it was only picking up telegraphic jerks, I do not know. The jerk of turning on or off an extra Swan lamp can affect it when sensitive. I hope to try for long-wave radiation from the sun, filtering out the ordinary well known waves by a blackboard or other sufficiently opaque substance.

We can easily see the detector respond to a distant source of radiation now, viz. to a 5-inch sphere placed in the library between secondary coil knobs; separated from the receiver, therefore, by several walls and some heavily gilded paper, as well as by 20 or 30 yards of space (Fig. 19).

Also I exhibit a small complete detector made by my assistant, Mr. Davies, which is quite portable and easily set up. The essentials (battery, galvanometer and coherer) are all in a copper cylinder three inches by two. A bit of wire a few inches long, pegged into it, helps it to collect waves. It is just conceivable that at some distant date, say by dint of inserting gold wires or powder in the retina, we may be enabled to see waves which at present we are blind to.

Observe how simple the production and detection of Hertz waves are now. An electrophorus or a frictional machine serves to excite them; a voltaic cell, a rough galvanometer, and a bad contact serve to detect them. Indeed, they might have been observed at the beginning of the century, before galvanometers were known. A frog's leg or an iodide of starch paper would do almost as well.

A bad contact was at one time regarded as a simple nuisance, because of the singularly uncertain and capricious character of the current transmitted by it. Hughes observed its sensitiveness to sound waves, and it became the microphone. Now it turns out to be sensitive to electric waves, if it be made of any oxidisable metal (not of carbon),* and we have an instrument which might be called a micro-something, but which, as it appears to act by cohesion, I call at present a coherer. Perhaps some of the capriciousness of an anathematised bad contact was sometimes due to the fact that it was responding to stray electric radiation.

The breaking down of cohesion by mechanical tremor is an ancient process, observed on a large scale by engineers in railway axles and girders; indeed, the cutting of small girders by persistent blows of hammer and chisel reminded me the other day of the tapping back of our cohering surfaces after they have been exposed to the welding effect of an electric jerk.

If a coherer is shut up in a complete metallic enclosure, waves cannot get at it, but if wires are led from it to an outside ordinary galvanometer, it remains nearly as sensitive as it was before (nearly, not quite), for the circuit picks up the waves and they run along the insulated wires into the closed box. To screen it effectively, it is necessary to enclose battery and galvanometer and every bit of wire connection; the only thing that may be left outside is the needle of the galvanometer. Accordingly, here we have a compact arrangement of battery and coil and coherer, all shut up in a copper box (see left-hand side of Fig. 21). The coil is fixed against the side of the box at such height that it can act conveniently on an outside

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* Fitzgerald tells me that he has succeeded with carbon also. My experience is that the less oxidisable the metal, the more sensitive and also the more troublesome is the detector. Mr. Robinson has now made me a hydrogen vacuum tube of brass filings, which beats the coherer for sensitiveness. July, 1894.

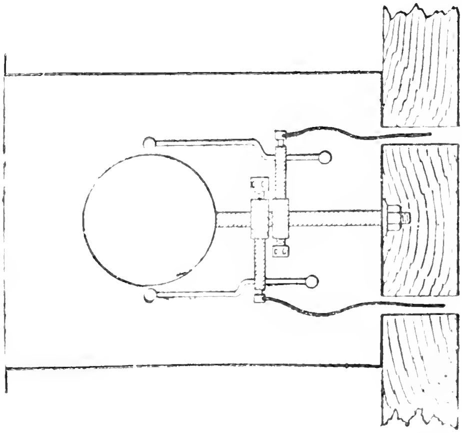
I wish to express my obligation to Mr. Edward E. Robinson for his extremely competent aid with all these experiments.

suspended compass needle. The slow action of the coil has no difficulty in getting through copper, as every one knows; only a perfect conductor could screen off that, but the Hertz waves are effectively kept out by sheet copper.

It must be said, however, that the box must be exceedingly well closed for the screening to be perfect. The very narrowest chink permits their entrance, and at one time I thought I should have to solder a lid on before they could be kept out entirely. Clamping a copper lid on to a flange in six places was not enough. But by the use of pads of tinfoil, chinks can be avoided, and the inside of the box becomes then electrically dark.

If even an inch of the circuit protrudes, it at once becomes slightly sensitive again; and if a mere single wire protrudes through the box, provided it is insulated where it passes through, the waves will utilise it as a speaking tube, and run blithely in. And this whether the wire be connected to anything inside or not, though it acts more strongly when connected.

FIG. 20.



Spherical Radiator for emitting a Horizontal Beam, arranged inside a Copper Hat, fixed against the outside of a metal-lined box. One-eighth natural size. The wires pass to the coil and battery inside the box through glass tubes not shown.

In careful experiments, where the galvanometer is protected in one copper box and the coherer in another, the wires connecting the two must be encased in a metal tube, Fig. 21, and this tube must be well connected with the metal of both enclosures, if nothing is to get in but what is wanted.

Similarly, when definite radiation is desired, it is well to put the radiator in a copper hat, open in only one direction (Fig. 20). And in order to guard against reflected and collateral surgings running along the wires which pass outside to the exciting coil and battery, as they are liable to do, I am accustomed to put all these things in a packing case lined with tinfoil, to the outside of which the sending hat is fixed, and to pull the key of the primary exciting circuit by a string from outside.

Even then, with the lid of the hat well clamped on, something gets out, but it is not enough to cause serious disturbance of qualitative results. The sender must evidently be thought of as emitting a momentary blaze of light which escapes through every chink. Or, indeed, since the waves are some inches long, the difficulty of keeping them out of an enclosure may be likened to the difficulty of excluding sound; though the difficulty is not quite so great as that, since a reasonable thickness of metal is really opaque. I fancied once or twice I detected a trace of transparency in such metal sheets as ordinary tinplate, but unnoticed chinks elsewhere may have deceived me. [Further investigation fails to detect real transparency even in good tinfoil.]

One thing in this connection is noticeable, and that is how little radiation gets either in or out of a small round hole. A narrow long chink in the receiver box lets in a lot; a round hole the size of a shilling lets in hardly any, unless indeed a bit of insulated wire protrudes through it like a collecting ear trumpet.

It may be asked how the waves get out of the metal tube of an electric gas-lighter. But they do not; they get out through the handle, which being of ebonite is transparent. Wrap up the handle tightly in tinfoil, and a gas-lighter is powerless.

Optical Experiments.

And now in conclusion I will show some of the ordinary optical experiments with Hertz waves, using as source either one of two devices; either a 5-inch sphere with sparks to ends of a diameter, Fig. 19, an arrangement which emits 7-inch waves but of so dead-beat a character that it is wise to enclose it in a copper hat to prolong them and send them out in the desired direction; or else a 2-inch hollow cylinder with spark knobs at ends of an internal diameter, Fig. 12. This last emits 3-inch waves of a very fairly persistent character, but with nothing like the intensity of one of the outside radiators.

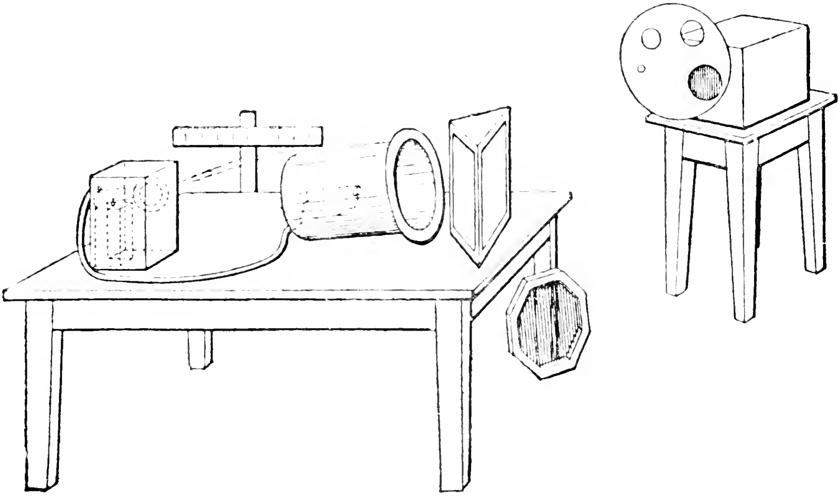
As receiver there is no need to use anything sensitive, so I employ a glass tube full of coarse iron filings, put at the back of a copper hat with its mouth turned well askew to the source, which is put outside the door at a distance of some yards, so that only a little direct radiation can reach the tube. Sometimes the tube is put lengthways in the hat instead of crossways, which makes it less sensitive, and has also the advantage of doing away with the polarising, or rather analysing, power of a crossway tube.

The radiation from the sphere is still too strong, but it can be stopped down by a diaphragm plate with holes in it of varying size, clamped on the sending box, Fig. 21.

Having thus reduced the excursion of the spot of light to a foot or so, a metal plate is held as reflector, and at once the spot travels a couple of yards. A wet cloth reflects something, but a thin glass plate, if dry, reflects next to nothing, being, as is well known, too thin to give anything but "the black spot." I have fancied that it reflects something of the 3-in. waves.

With reference to the reflecting power of different substances it may be interesting to give the following numbers, showing the motion of the spot of light when 8-in. waves were reflected into the copper hat, the angle of incidence being about 45°, by the following mirrors :—

FIG. 21.



General arrangement for optical experiments; showing Metal Box on a Stool, inside which the Radiators were fixed (this box was really like Fig. 20); the Copper Hat containing the Coherer, with the metal Box containing Battery and Galvanometer Coil connected to it by a compo pipe conveying the wires; a Paraffin Prism; and a Polarising Grid.

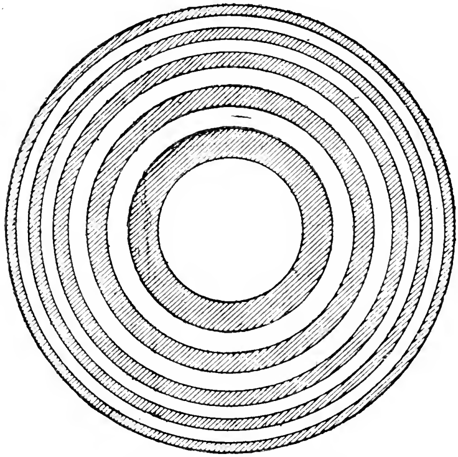
Sheet of window glass	0 or at most 1 division.
Human body	7 divisions.
Drawing board	12 ,,
Towel soaked with tap-water	12 ,,
Tea-paper (lead?)	40 ,,
Dutch metal paper	70 ,,
Tinfoil	80 ,,
Sheet copper	100 and up against stops.

A block of paraffin about a cubic foot in volume is cast into the shape of a prism with angles 75°, 60°, and 45°. Using the large angle, the rays are refracted into the receiving hat, Fig. 21, and produce an effect much larger than when the prism is removed.

An ordinary 9-in. glass lens is next placed near the source, and by means of the light of a taper it is focussed between source and receiver. The lens is seen to increase the effect by concentrating the electric radiation.

The lens helps us to set correctly an 18-in. circular copper disc in position for showing the bright diffraction spot. Removing the disc the effect is much the same as when it was present; in accordance with the theory of Poisson. Add the lens and the effect is greater.

FIG. 22.



Zone-plate of Tinfoil on Glass.
Every circular strip is of area equal to central space.

With a diffraction grating of copper strips 2 in. broad and 2 in. apart, I have not yet succeeded in getting good results. It is difficult to get sharp nodes and interference effects with these sensitive detectors in a room. I expect to do better when I can try out of doors away from so many reflecting surfaces; indoors it is like trying delicate optical experiments in a small whitewashed chamber well supplied with looking-glasses; nor have I ever succeeded in getting clear concentration with this zone-plate having Newton's rings fixed to it in tinfoil. But really there is nothing of much interest now in diffraction effects except the demonstration of the waves and the measure of their length. There was immense interest in Hertz's time, because then the wave character of the radiation had to be proved;

but every possible kind of wave must give interference and diffraction effects, and their theory is, so to say, worked out. More interest attaches to polarisation, double refraction and dispersion experiments.

Polarisation experiments are easy enough. Radiation from a sphere is already strongly polarised, and the tube acts as a partial analyser, responding much more vigorously when its length is parallel to the line of sparks than when they are crossed; but a convenient extra polariser is a grid of wires something like what was used by Hertz, only on a much smaller scale; say an 18-in. octagonal frame of copper strip with a harp of parallel copper wires (*see* Fig. 21, on floor). The spark-line of the radiator being set at 45°, a vertical grid placed over receiver reduces the deflection to about one-half, and a crossed grid over the source reduces it to nearly nothing.

Rotating either grid a little, rapidly increases the effect, which becomes a maximum when they are parallel. The interposition of a third grid, with its wires at 45° between two crossed grids, restores some of the obliterated effect.

Radiation reflected from a grid is strongly polarised, of course, in a plane normal to that of the radiation which gets through it. They are thus analogous in their effect to Nicols, or to a pile of plates.

The electric vibrations which get through these grids are at right angles to the wires. Vibrations parallel to the wires are reflected or absorbed.

To demonstrate that the so-called plane of polarisation of the radiation transmitted by a grid is at right angles to the electric vibration,* i. e. that when light is reflected from the boundary of a transparent substance at the polarising angle the electric vibrations of the reflected beam are perpendicular to the plane of reflection, I use the same paraffin prism as before; but this time I use its largest face as a reflector, and set it at something near the polarising angle. When the line of wires of the grid over the mouth of the emitter is parallel to the plane of incidence, in which case the electric vibrations are perpendicular to the plane of incidence, plenty of radiation is reflected by the paraffin face. Turning the grid so that the electric vibrations are in the plane of incidence, we find that the paraffin surface set at the proper angle is able to reflect hardly anything. In other words, the vibrations contemplated by Fresnel are the electric vibrations; those dealt with by McCullagh are the magnetic ones.

Thus are some of the surmises of genius verified and made obvious to the wayfaring man.

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* Cf. Trouton, in 'Nature,' vol. xxxix. p. 393; and many optical experiments by Mr. Trouton, vol. xl. p. 398. Also by Klemencik, (Wied. Ann. vol. xlv.). Righi (Acc. dei Lincei, vol. xi.), and Elster and Geitel (Phil. Mag. July 1894, p. 158).

LIST OF HERTZ'S PAPERS.

1878–79. Wied. Ann. 1880, x. p. 414. Experiments to establish an Upper Limit for the Kinetic Energy of Electric Flow.

1880. Inaugural Dissertation (Doctor Thesis) on Induction in Rotating Spheres.

1881. Wied. Ann. xiii. p. 266. On the Distribution of Electricity on the Surface of Moving Conductors.

1881. Crelle, xcii. p. 156. On the Contact of Solid Elastic Bodies.

1881. Wied. Ann. xiv. p. 581. Upper Limits for the Kinetic Energy of Moving Electricity.

1882. Verhandlungen des Vereins des Gewerbfleisses (Sonderabdruck). On the Contact of Solid Elastic Bodies and on Hardness.

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1882. Verhandln. d. phys. Gesellschaft in Berlin, p. 18. On a New Hygrometer.

1883. March. Schlömilch Zeitschrift, p. 125. On the Distribution of Pressures in an Elastic Circular Cylinder.

1883. Wied. Ann. xix. p. 78. On an appearance accompanying Electric Discharge.

1883. Ib. xix. p. 782. Experiments on Glow Discharge.

1883. Wied. Ann. xx. p. 279. On the Property of Benzine as an Insulator and as showing Elastic Reaction (Rückstandsbildner).

1883. Zeitschrift für Instrumentenkunde. Dynamometric Contrivance of Small Resistance and Infinitesimal Self-Induction.

1884. Met. Zeitschrift, November–December. Graphic Methods for the Determination of the Adiabatic Changes of Condition of Moist Air.

1884. Wied. Ann. xxii. p. 449. On the Equilibrium of Floating Elastic Plates.

1884. Ib. xxiii. On the Connection between Maxwell's Electro-dynamic Fundamental Equations and those of Opposition Electro-dynamics.

1885. Ib. xxiv. p. 114. On the Dimension of a Magnetic Pole in different Systems of Units.

1887–1889. Papers incorporated in his book, 'Ausbreitung der Elektrischen Kraft,' translated under the title of 'Electric Waves.'

1892. Wied. Ann. xlv. p. 28. On the Passage of Cathode Rays through Thin Metal Sheets.

1894. A Posthumous work on the Principles of Mechanics exhibited on a new plan. With a Preface by Von Helmholtz.

[O. J. L.]

GENERAL MONTHLY MEETING,

Monday, June 4, 1894.

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

Sir George Barclay Bruce, M. Inst. C.E.
Sir Thomas D. Gibson-Carmichael, Bart. M.A.
Frederick Dye, Esq.
H.H. Prince Victor Duleep Singh,
Major-General Edward R. Festing, R.E. F.R.S.
Lennox H. Lindley, Esq. B.A.
The Duchess of Marlborough,
Captain Frederic Lewis Nathan, R.A.
Cuthbert Edgar Peek, Esq. M.A. F.S.A.
Hugh Walsham, Esq. M.A. M.B. M.R.C.P.

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

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WEEKLY EVENING MEETING,

Friday, June 8, 1894.

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

C. Vernon Boys, Esq. F.R.S. A.R.S.M. *M.R.I.*

The Newtonian Constant of Gravitation.

It is probably within the knowledge of most of those present that Sir Isaac Newton, by his great discovery of gravitation and its laws, was able to show that a single principle, ideally simple, viz. that every particle in the universe attracts any other particle towards itself with a force which is proportional to the product of their masses divided by the square of the distance between them, would completely and absolutely account for the three laws of planetary motion which Kepler had given to the world.

Newton also showed that a spherical body, whether uniformly dense or varying in density according to any law from the centre to the surface, would attract bodies outside with the same force that it would do if it could all be concentrated at its centre, i.e. that all the attractions varying in amount and direction produced by particles in all parts of a sphere need not be considered separately, but may be treated in this simple way.*

Nevertheless, though Newton's great discovery is sufficient to bring the whole of the movements of the planets and of their satellites, whether their simple Keplerian motions or the disturbances produced by their mutual gravitation, the motions of comets, of binary stars, of the tides, or the falling apple, under the domain of a single and simple principle, though it enables one to compare the masses of the sun, the planets and their satellites, and of those binary stars whose parallax has been determined, one thing can never be made known by astronomical research alone, though we may know that twenty-eight suns would be required to make one Sirius; that the sun is equal to 1048 Jupiters, that Jupiter is more than double all the rest of the solar system put together, or that the moon is 1/80 of the earth; no observations of these bodies can ever tell us how many tons of matter go to make up any one of them.

Though we know from first principles of dynamics, by the mere

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* Only last night I learned that it was the difficulty of proving this, and not the erroneous value of the moon's distance, that delayed the publication of Newton's discovery for so long.

consideration of centrifugal force, that the whole sun attracts each ton of the earth with a force equal to a weight here of a little more than one pound, and that if it were not for this, every ton of the earth would continue its journey into space in a straight line for ever, and though we know in the same way that the whole earth attracts each ton in the moon with a force equal to the weight of ten ounces and no more, we cannot tell by any astronomical observation whatever how many tons there are in all.

Newton showed that to complete his law and to put in the numerical constant (the Newtonian Constant of Gravitation) that would convert his proportion into an equality, two methods are available: we may either make observations on the disturbance of the earth's gravitation by the action of isolated parts of it, we may either find the relative attraction of an isolated mountain or the strata above the bottom of a deep mine, or we may make an artificial planet of our own and find the attraction which it exerts.

The Newtonian Constant will be known if we know the force of attraction between two bodies which we can completely measure and weigh. Employing the C. G. S. system of measurement, the Newtonian Constant is equal to the force of attraction in dynes between two balls weighing a gramme each, with their centres one centimeter apart. Of course it may be referred to pounds and inches or tons and yards, but as soon as all the quantities but G in Newton's equation

$$\text{Force} = G\,\frac{\text{Mass} \times \text{Mass}}{\text{Distance}^2}$$

are known, no matter in what units the quantities are measured, G is known. The conversion of its numerical value from one system of measurement to another is of course a mere matter of arithmetic.

Of the first method of finding G, depending on the attraction of a mountain, first attempted by Bouguer at the risk of his life in the hurricanes of snow on Chimborazo, of the experiments of Maskelyne, of Airy and of others, I cannot now find time to speak; I can only refer to Poynting's essay on the subject. It is the second method with an artificial planet that I have to describe to-night.

Now let me give some idea of the minuteness of the effect that has to be measured. Is a wall built true by the aid of a plumb-line vertical, or does it lean outwards? Newton's principle shows that the plumb-bob is attracted by the wall, yet it hangs vertically. The attraction is so small that it cannot be detected in this way. Even the attraction of a whole mountain requires the most refined apparatus to make its existence certain. Do two marbles lying on a level table rush together? According to Newton's principle they attract one another; yet if they were a thousand times smoother than they are, no movement of attraction could be detected.

Leaving matters of common experience, let us go into the physical laboratory where instruments of the highest degree of precision and delicacy (at least so they are called) are found on every table. What precautions are taken to prevent the attractions of the fixed and moving parts from interfering with the result which they are constructed to measure? None. The attractions are so small, that in no apparatus in use for the measurement of electrical, magnetic, thermal or other constants are they ever thought of, or is any provision necessary to prevent their falsifying the result. Nevertheless, the attractions exist, and if only the means are delicate enough they can be detected and measured. The Rev. John Mitchell was the first to devise a successful method. He was the first to invent the torsion balance with which Coulomb made his famous electrical researches, and which bears Coulomb's name. He devised and he made apparatus for this purpose, but he did not live to make any experiments.

After his death Cavendish remodelled Mitchell's apparatus and performed the famous Cavendish experiment. By means of the apparatus, of which for the second time I show a full-size model in this theatre, Cavendish measured the force of attraction between two balls of lead, one 12 and the other 2 inches in diameter, and with their centres 8·85 inches apart. The same experiment has since been made by Reitch, by Baily, and more recently by Cornu and Baille with greatly superior apparatus of one quarter of the size. All these observers actually determined the attraction between masses which could be weighed and measured, and thus found with different degrees of accuracy the value of G.

Let me explain now that this G, the gravitation constant, or as I prefer to call it, for the sake of distinction, the Newtonian Constant of Gravitation, has nothing to do with that other quantity generally written *g*, which represents the attraction at the earth's surface. This is a purely accidental quantity, which depends not only upon G, but also upon the size of the earth, its mean density, the latitude, the height above the sea, and finally upon the configuration and the composition of the neighbouring districts. *g* is eminently of a practical and useful character; it is the delight of the engineer and the practical man; it is not the same in all places, but that he does not mind. It is of the earth, arbitrary, incidental and vexatious. Prof. Greenhill should spell his name with a little *g*. G, on the other hand, represents that mighty principle under the influence of which every star, planet and satellite in the universe pursues its allotted course; it may possibly also be the mainspring of chemical action. Unlike any other known physical influence, it is independent of medium, it knows no refraction, it cannot cast a shadow. It is a mysterious power, which no man can explain; of its propagation through space, all men are ignorant. It is in no way dependent on the accidental size or shape of the earth; if the solar system ceased to exist it would remain unchanged. I cannot contemplate this mystery, at which we ignorantly wonder, without thinking of the altar on Mars' hill. When will a St. Paul arise able to declare it unto us? Or is gravitation, like life, a mystery that can never be solved?

Owing to the universal character of the constant G, it seems to me to be descending from the sublime to the ridiculous to describe the object of this experiment as finding the mass of the earth, or the mean density of the earth, or, less accurately, the weight of the earth. I could not lecture here under the title that has always been chosen in connection with this investigation. In spite of the courteously expressed desire of your distinguished and energetic secretary, that I should indicate in the title that, to put it vulgarly, I had been weighing the earth, I could not introduce as the object of my work anything so casual as an accidental property of an insignificant planet. To the physicist this would be equivalent to leaving some great international conference to attend to the affairs of a parish council. That is the business of the geologist. The object of this investigation is to find the value of G. The earth has no more to do with it than the table has upon which the apparatus is supported. It does interfere, and occasionally, by its attraction, breaks even the quartz fibres that I have used. The investigation could be carried on far more precisely and accurately on the moon, or on a minor planet, such as Juno; but as yet no means are available for getting there.

I shall not have time to-night to describe the work of former investigators, and for this there is little need, since it is all collected in Poynting's Adams prize essay, "On the Mean Density of the Earth," published this year. I cannot even find time to explain in more than the merest outline what I have done to develop the apparatus of Cavendish, so that he would hardly recognise in my glorified bottle-jack the balls and lever which have made his name famous. The following table, given by Poynting, however, represents the results of the labours of investigators up to the present time.

In connection with this table I cannot lose the opportunity of quoting Newton's extraordinary prophecy, marvellous in that without any direct knowledge he gave a figure which was nearer the truth than that found by many of the experimenters that came after him. The passage is as follows:—

"Unde cum Terra communis suprema quasi duplo gravior sit quam aqua, et paulo inferius in fodinis quasi triplo vel quadruplo aut etiam quintuplo gravior reperiatur; verisimile est quod copia materiæ totius in Terra quasi quintuplo vel sextuplo major sit quam si tota ex aqua constaret; præsertim cum terram quasi quintuplo densiorem esse quam Jovem jam ante ostensum sit." *

I have placed on the wall the diagram of the apparatus which I showed in action when lecturing here upon quartz fibres five years ago. With this I was able, for the first time, to show to an audience the effect of the very small attraction exerted between a 2-inch cylinder of lead and a little one weighing only a gramme or 15 grains. The apparatus which I have to describe to-night is the same in principle, the main distinction being that it is so designed and

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* Newton's 'Principia,' second edition, 1714, p. 373, line 10.

SUMMARY OF RESULTS HITHERTO OBTAINED.

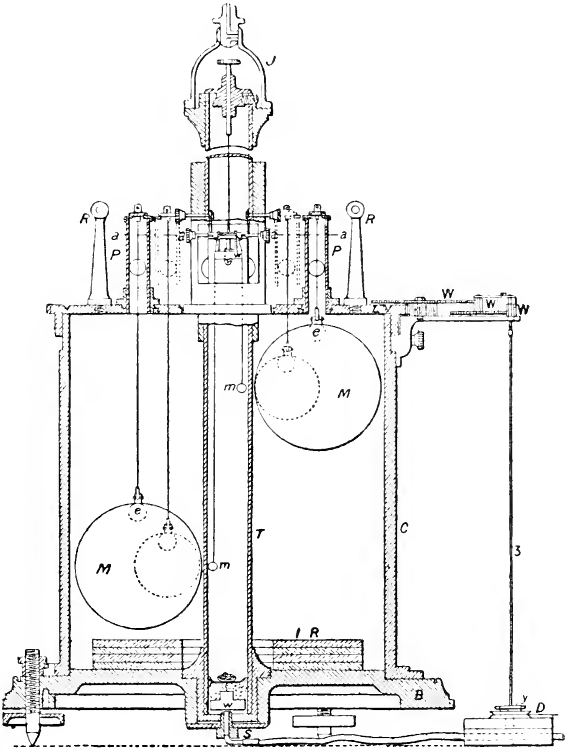
Approximate date.	Experimenter.	Method.	Result.
			Δ
1737–40	Bouguer	Plumb-line and pendulum.	Inconclusive
1774–76	Maskelyne and Hutton	Plumb-line	4·5–5
1855	James and Clarke	„	5·316
1821	Carlini	Mountain pendulum	4·39–4·95
1880	Mendenhall	„	5·77
1854	Airy	Mine pendulum	6·565
1883	Von Sterneck	„	5·77
1885	Von Sterneck	„	About 7
1797–98	Cavendish	Torsion balance	5·448
1837	Reich	„	5·49
1840–41	Baily	„	5·674
1852	Reich	„	5·583
1870	Cornu and Baille	„	5·56–5·50
1889	Boys	„	In progress
1879–80	Von Jolly	Common balance	5·692
1878–90	Poynting	„	5·493 (5·46–5·52)
1884	König, Richarz, and Krigar Menzel.	„	In progress
1886–88	Wilsing	Pendulum balance	5·579
1889	Laska	„	In progress

constructed that I can tell precisely where every gravitating particle is placed. In the design of this apparatus I have been, as every one will admit, bold—most would have preferred the word reckless; but knowing the truth of the principles which I had developed, and having faith and confidence in the quartz fibre, I deliberately chose to reduce all the dimensions to an extent which caused the forces, and especially the couples, to be insignificant in comparison with any which had been within the reach of the experimenter hitherto. The whole difficulty of Cavendish, Reich and Baily had been to measure so minute an effect; instead of increasing this I diminished it enormously, being satisfied that I should be able to make a proportionately more accurate measure by so doing. Cornu reduced the dimensions to one-quarter; I have reduced the chief one to one-eightieth. Cavendish had a force equal to 1/3650 grain's weight to measure; I have less than a five-millionth. By the use of the long lever, Cavendish had the effect of a force of 1/100 grain's weight on an arm an inch long; I have less than a twelve-millionth of a grain on an arm of that length. His forces were fourteen hundred times as great as mine; his couples or twisting forces were a hundred and twenty thousand times as great. One advantage gained by the use of small apparatus, in which alone the attracting balls can be made large compared with the length of the beam, is the increased sensibility, the greater angle of deflection produced by the attractions when the period of oscillation is the same. This is more especially the case in my apparatus where the two sides are at different levels. But the question arises whether this reduction of linear dimensions may not introduce irregularity and want of stability to such an extent as to more than counterbalance the advantages to which I have already referred. In spite of every endeavour that may be made to keep the air quiet, to exclude draughts, to keep all the apparatus at one temperature in a vault of constant temperature, infinitesimal differences must exist; one side of the apparatus must be hotter than the other, though no thermometer could be made which would detect the difference. In consequence of this difference of temperature the air circulates, and so creates a draught which blows upon the mirror and the suspended balls. I had erroneously come to the conclusion that in apparatus geometrically similar these disturbances would probably be in the proportion of the seventh power of the linear dimensions, so that greatly increased stability would be obtained by reducing the dimensions. However, I have discussed this matter with Prof. Poynting, who has shown me that in reality the disturbances would be proportional to the fifth power of the linear dimensions if, as should be the case, the circulation of the air were so extremely slow that the motion would be steady, whereas it would gradually rise from this to the eighth power as the term involving the square of the velocity increased in magnitude and the motion became irregular. So long, therefore, as the apparatus is small enough to prevent terms involving the square of the velocity from being appreciable, the stability is the same whatever the size, but as soon as the apparatus exceeds this, then the disadvantage of size very rapidly becomes evident. In addition to this, the time needed to bring the apparatus to a steady state is far greater with large apparatus. After making the geometrical measures I leave my apparatus, small as it is, three days, if possible, before observing deflections and periods.

The diagram, Fig. 1, is a vertical section through the apparatus. B and C represent an accurately turned brass box with a lid L, which can be made to turn round insensibly by the action of the wheels W W. The lid carries two tubular pillars P P, from the tops of which the balls M M hang by phosphor bronze wires, being definitely held in place by geometrical clamps on the heads of the pillars. The lid also carries two supporting pillars R R. In the centre tube the "beam mirror" N hangs by means of a quartz fibre from an adjustable torsion head surmounted by a bell jar, and from the ends of the mirror the two gold balls *m m* hang by separate quartz fibres. Four rings of india-rubber are placed on the base to prevent destruction of the apparatus in case the balls should drop by any accident. Now it is evident that if the lid is turned from the position in which it is shown, that is, with all four balls in one plane, in which position the attractions do not tend to twist the central torsion fibre at all, then these attractions will produce a couple increasing with the angle up to a certain point (65° in the particular case), after which the couple falls off again and becomes zero when it has turned 180°.

Since the effect is a maximum at 65°, very great accuracy in the measurement of this angle is of little consequence. By means of a small telescope at a distant table, and the divided edge and vernier, I can tell the angle with certainty to 1/20 degree; an uncertainty of one-quarter of a degree would be of but little consequence. Again, if the pair of gold balls twist about an axis which is not exactly that round which the lead balls are carried, if there is any small eccentricity of the gold or lead balls, then eccentricity in the common plane removes the gold balls from a position of minimum effect, eccentricity across the plane removes them from a position of

Fig. 1.

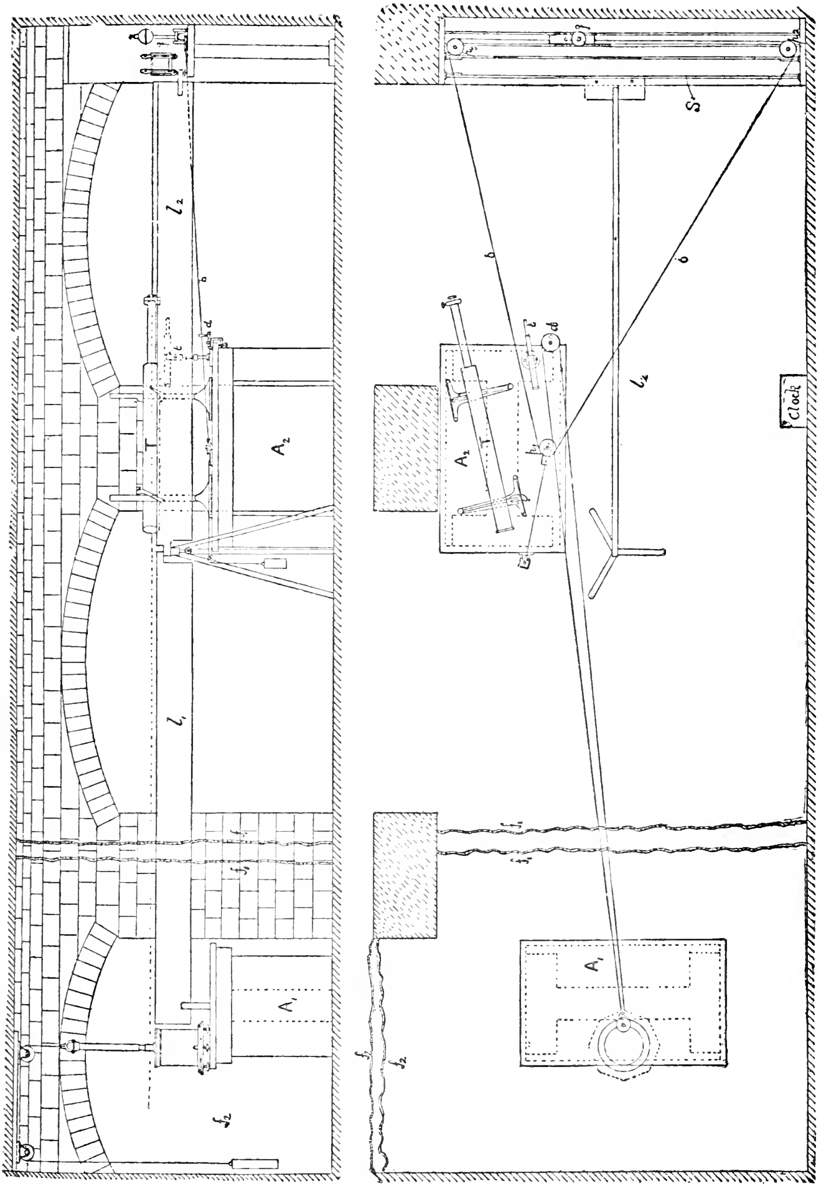


By permission of the *Engineer*.

maximum effect, and if the levels of the gold balls are not precisely the same as those of the lead balls, again the departure is from a position of maximum effect. All these three eccentricities can be determined with an accuracy of 1/1000 inch. Errors of 1/100 inch would make a barely perceptible effect upon the result. The design, therefore, is such that a great number of measures which are difficult, and can at the best only be made with a second quality degree of accuracy, are of so little consequence that this degree is more than abundant. The final result depends directly upon a few measures which, as I hope to show, can be made with facility and most accurately. These are the horizontal distance from centre to centre of the wires by which the lead balls are suspended, the horizontal distance between the centres of the quartz fibres by which the gold balls are suspended, the angle through which the mirror is deflected, the masses of the lead but not of the gold balls, and the natural time of oscillation of the mirror when the balls are suspended and when a thin cylinder of small moment of inertia, but of the same weight as the balls, is suspended axially in their stead.

Before going more into detail and showing how the operations are carried out so that all the quantities may be known with a sufficient degree of accuracy, it will be convenient to project upon the screen a drawing of the vault in which the experiments have been made. Prof. Clifton has kindly allowed me the free use of the vault under the Clarendon Laboratory at Oxford. This is shown in Fig. 2, of which the upper portion represents an elevation and the lower part a plan. The instrument itself stands upon the table A_1 in the corner, where it is screened from temperature disturbances, which my presence in the distant corner and a very small flame produce, by an octagon house of double wood lined with cotton-wool, and by double felt screens f_1 f_2. On the second table A_2 are placed a large astronomical telescope T, through which the large scale S is seen by reflection from the mirror in the apparatus, a small reading telescope *t* to read the angle of the lid and vernier, a pulley-wheel p_1 and a driving-wheel *d*. The pulley-wheel p_1 keeps the cord *b* which passes round p_2 and p_3, and is attached to the cart *g*, always tightly stretched, so that the observer at the telescope can always keep a little flame carried by the cart immediately behind the particular division under observation. The driving wheel *d* is made with a very large moment of inertia, and the handle is near the axis, so that its motion is necessarily steady. A very light cord passes round this, across the room, and after passing through a hole in the screen passes also round the little wheel D, Fig. 1, and thus serves to drive the train W W, and so carry the lid and balls round almost insensibly. Two hundred and thirty turns of *d* are required to move the lead balls from the + to the − position. I generally turn the handle a hundred and thirty times, and then when the mirror is approaching an elongation, turn the handle the remaining hundred times, finally stopping when the lid reading, as observed in the small

FIG. 2.


l₂
b
d
t
T
A₂
l₁
f₁
f₂
A₁
f₂
g
h₃
h₂
S
b
b
t
d
l₂
A₂
T
h₁
clock
f₁
f₂
A₁
f₁
f₂

telescope, is correct. The large scale S is 9 feet long, and is divided into fiftieths of an inch. There are 4800 divisions.

Two beams l_1 l_2 are seen in Fig. 2. The upper surfaces of these are straight, and are adjusted by screws until they are truly level. These are used when the true optical distance from the mirror to the scale is being measured. A steel tape, on which I engraved a fine line near each end, rests upon the beams. At one end a slider carrying a microscope is placed so as to see a fine line at the centre of the mirror accurately in focus, while at the other a corresponding slider is placed so that a projecting brass rod rests against the scale. At the same time cross lines engraved upon the plate-glass bases are placed exactly over the lines engraved on the steel tape. When afterwards the microscope is focussed upon the end of the brass rod, the distance between the cross lines, as measured by a scale, is the amount that has to be added to the distance between the engraved lines upon the tape, in order to obtain the distance from the scale to the mirror.

Overhead wheels are shown in Fig. 2, fastened to the roof above the apparatus, and again close to the end wall. These serve many purposes, as will appear later. Among others, the middle one of each carries a cord fastened at one end to a crossbar joined at its ends by guys to the pillars R of the lid, Fig. 1, and at the other to heavy balance weights to counterbalance the balls M M and part of the lid. Thus the friction is greatly reduced, and the tremor set up by rotating the lid is in a corresponding degree slight.

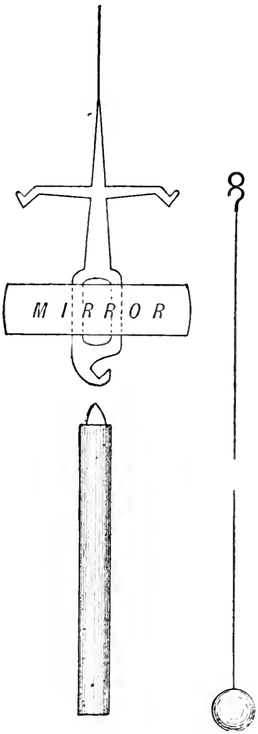
All time observations are made chronographically upon a drum by the Cambridge Scientific Instrument Company. This is placed in the adjoining vault. Two time-markers record with their points less than 1/100 inch apart, one of them marking every second of the clock with special marks for minutes and half-minutes, and the other every depression of the key at my right hand. The late Prof. Pritchard kindly lent me an astronomical clock for the purpose, to which I fitted time-marking contacts; but into the details of these I must not enter. He also allowed me to make use of one of his assistants to keep me informed of the rate of the clock from time to time.

I have up to the present spoken vaguely of the large lead balls and of the small gold balls, but have given no indication as to how they are made and how I can be sure of the truth of their form and their homogeneity. Mr. Munro, whose capacity for turning accurate spherical work is well known, made for me two moulds of hard cast iron, which I have on the table. One of these is for a 4¼-inch lead ball, and one for a 2¼-inch lead ball. Each mould is made in two halves, so truly as to shape and size that the thin steel disc that was used as a template would distinctly rattle when in its place, but when a strip of cigarette-paper was inserted on one side it could not be got in at all. The upper half of each of these moulds is provided with a cylindrical steel plunger accurately fitting a central hole in the mould, and with its end turned to the same spherical surface when it is pressed home upon its shoulder. The lower half of each mould has a $\frac{1}{4}$-inch central cylindrical hole, into which the lug of the brass ball holder exactly fits. There is also a small hole at the side which can be stopped with a brass plug. The balls are made as follows:—The interior of the mould is smoked and then screwed up as tight as possible. It is then heated until a piece of lead upon it begins to melt. The necessary quantity of pure lead melted in an earthen pot is then carefully skimmed and poured in until the cylindrical neck is full. The mould is then made to rest upon a cold iron slab, and a large blowpipe flame is directed upon the upper part so that it cools from below upwards, and not from the surface inwards; more lead is added to keep the neck full. As soon as the lead in the neck solidifies the plunger is inserted, and the whole is placed in a hydraulic press. The plunger is forced down upon its seat; the lead, already free from bubbles and vacuous cavities, is compressed until at last the excess of solid metal flows through the small side hole in the form of wire. The ball is thus made true in form, necessarily homogeneous, which no alloy is likely to be, and definite in size. When cold it can be lifted from the mould, then, after cutting off the wire which projects from its equator, it is ready for weighing.

The small gold balls are made by melting the required quantity of pure gold in a hole in a bath brick, and, as in the case of the lead, letting it cool from below upwards, so as to avoid cavities. It is then inserted in a pair of polished hemispherical hardened steel dies, which Mr. Colebrook made for the purpose, and beaten, being turned between each blow, and annealed once or twice until a perfect polished sphere, without a mark upon it, is the result. I make these in pairs of exactly the same weight, and, as in the case of the lead balls, thus obtain truth of form, accuracy of size and homogeneity, all in a very perfect—more than sufficiently perfect—degree. These are each suspended from a quartz fibre of the necessary length, to the other end of which a hook and eye is fastened. Into the very important details of these operations it is impossible, for want of time, for me to enter. The gold balls are ·2 and ·25 inch in diameter, and a pair of gold cylinders were made in a similar tool ·25 inch in diameter, and about the same length.

Perhaps the most important detail in the whole apparatus is the "beam mirror," which is of the form shown in Fig. 3. It is necessary, as far as possible, to reconcile the following incompatible conditions. It should be as light as possible, have as small a moment of inertia as possible, the optical definition should be as perfect as possible, and, almost most important of all, the form should be such that the resistance offered by the viscosity of the air should be reduced to the smallest possible degree. By cutting the middle portion out of an optically perfect round mirror all these conditions are realised in some degree, and the optical definition is actually more perfect in the horizontal direction than that due to the whole disc. This is fastened to a cross-shaped support of gilt copper. The ends of the mirror have vertical grooves of microscopic fineness cut in their thickness, so that the quartz fibre hanging from the cross-arm above may rest definitely in them. The central hook is for the purpose of hanging the "counterweight," i.e. a slender silver cylinder of exactly the same weight as the gold balls with their fibres and hooks. By this means the unknown moment of inertia of the mirror may be eliminated with the fibre equally stretched in both cases, a most necessary condition, for I have found that the torsional rigidity is seriously affected by variation in stretching.

FIG. 3.

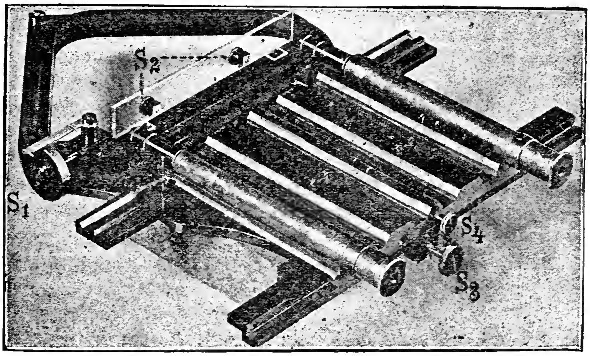


Means are provided by which I can effect the transfer of the gold balls from the beam to the side hooks or the reverse, or change their places without opening the window; but these and numerous other important details I must pass over.

Unfortunately accidents are liable to happen, and, as I know by dearly-bought experience, the gold balls may sometimes be precipitated down the central tube. I have recovered them sometimes by an india-rubber tube, let down through the window aperture, sucking at the other end until they closed the open end, when they could be drawn up. Latterly I have made use of a magnetised tuning-fork to pick up a very small fragment of iron tied to a silk line, by means of which I could draw up a diaphragm with anything that might have fallen upon it.

I have already stated that two measurements, viz. the horizontal distances between the axes of the wires which support the lead balls, and of the fibres which support the gold balls, must be made with the highest degree of accuracy attainable, for on these the result directly depends. In order to accomplish this I had to design a special instrument, an optical compass, which is illustrated in Fig. 4. This is an arrangement which rests upon the lid of the apparatus on the circular V-groove seen in Fig. 1, so that it can rotate without shake. Upon the lower framing rests the focussing slide, and on this a pair of traversing slides, each carrying a microscope in one or other of three grooves. The two traversing slides are drawn together by a spring, and can be separated by a screw cone, forming a very delicate fine adjustment. This is operated by the screw-head S_4; S_3 is a focussing screw giving a fine adjustment to the focussing slide. S_2 S_2 are two parallelising screws, the purpose of which is to bring the microscopically-divided glass scale into focus at each end simultaneously. S_1 is a micrometer screw-head, which is employed to push the scale bodily to the right by measured amounts. The two microscopes are focussed upon, say, the right sides of the wires, the focussing slide is then withdrawn, leaving them relatively unchanged, and the microscopic scale is then put in its place. The distance from wire to wire is thus transferred directly to the scale, and the fractional part of any one division of 1/100 inch is all that has to be referred to and measured by the screw. Every slide in this apparatus is geometrically arranged, so that the movements are all perfectly free, unconstrained and without shake. In measuring the distance between the fibres, which must be done while they are freely suspended, so that a force of a millionth of the weight of a grain will give them a considerable motion, means have to be provided to exclude draught, which yet must not interfere with the apparent distances of the fibres. No microscope cover-glass is any use for this purpose. It is sure to be prismatic, and when inserted between the microscope and an object it will certainly cause it to shift its apparent position. A piece of clear mica is perfect in this respect, no movement, even with a high power, being visible. I mention this as it well illustrates the sort of trap that is ever set for the experimentalist. If I had not been aware of this, and had used, as would be natural, a window of microscope cover-glass, then each fibre would have appeared as definitely in its place as before, but the place would have been wrong, perhaps by 1/1000 inch, and thus a consistent error affecting all the experiments alike would have been introduced, and no multiplication of observations or taking of means would have eliminated it. It is on this account that it is so important in experimental work to vary the conditions in every way, so as to discover unsuspected consistent errors.

FIG. 4.



The microscope scale was made by Zeiss, and is a most perfect example of scale construction. In order to test the accuracy and find the errors of the scale, I took advantage of my visit to Cardiff for the meeting of the British Association, to compare it with a series of Whitworth standard bars on Prof. Viriamu Jones's very perfect

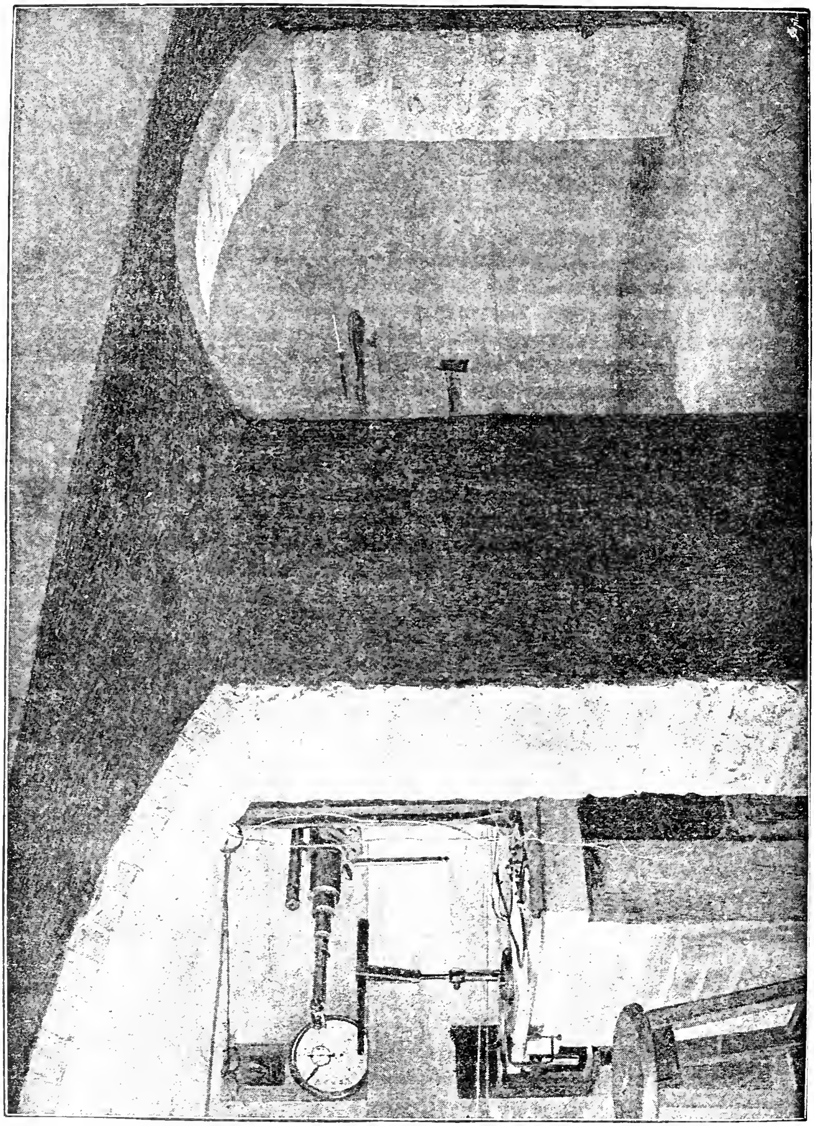
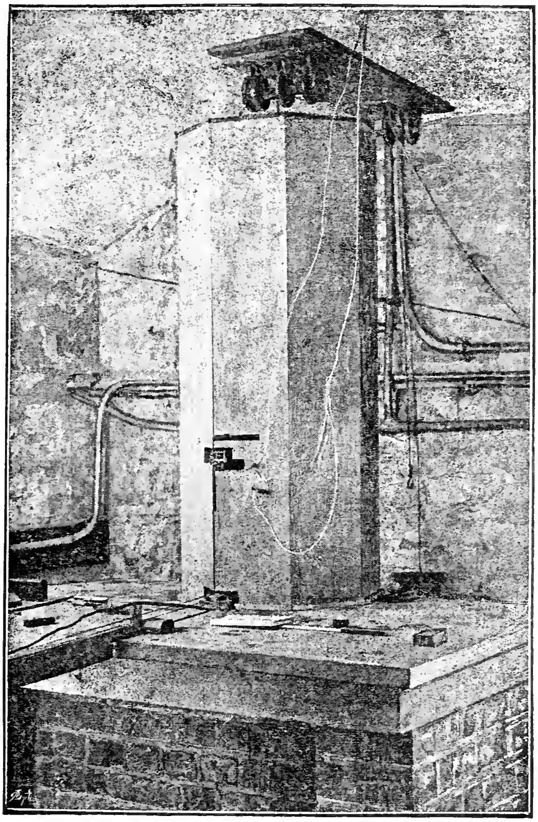


Fig. 5.

Whitworth measuring machine. For this class of work sunshine or dust give great trouble, but I was fortunate in having splendid weather for my purpose, as visitors will probably remember: it rained without ceasing during the two days I was making these measurements.

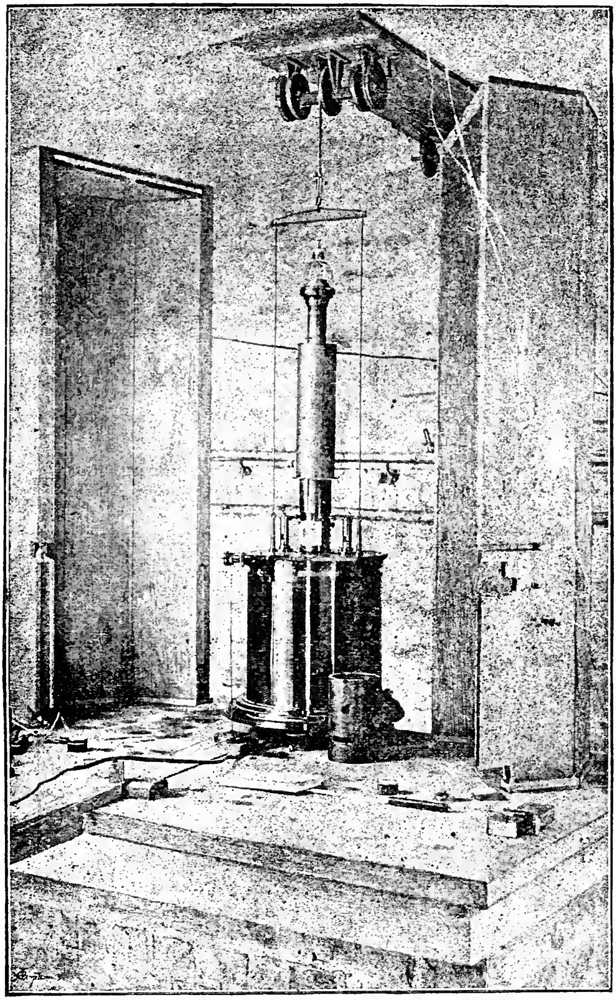
Having now very imperfectly described the apparatus and the place in which I have carried out my experiments, I will next show a series of photographs, which I took by magnesium light, to give a better idea of the appearance of the apparatus and its surroundings. Fig. 5 is a view of the vault showing the clock, the eye end of the big telescope, and the little telescope. In the distant corner is seen the felt screen with a long slit, through which the scale and telescope can be seen from the mirror of the instrument. This, of course, is

Fig. 6.



on the table behind the screen. Fig. 6 is a view of the corner itself, with the screen drawn back. The octagon protecting house, which

Fig. 7.



surrounds the apparatus, is seen in position. Here again a slit is cut large enough to allow the scale and telescope to be seen from the mirror. Fig. 7 is a view of the instrument with the two halves of the octagon house separated. Here a further system of screens consisting of concentric brass tubes may be seen, but the lower one, which surrounds the window, has been removed and placed upon the table. The driving gear is also seen in this photograph, and a pipe coming from the screw under the instrument which holds the central tube, which pipe is also seen in Fig. 1. This enables me to control the motion of the mirror from the telescope without approaching the corner in which the apparatus is set up. This is done as follows: the back window at the level of the mirror is made of metal, with a hole in it in which is screwed a metal tube lightly filled with cotton-wool. This is not central, but opposite one end of the mirror. The pipe on the table does not fit the screw, but is merely bent up and enters it loosely. By gently drawing air from the end of the pipe at the telescope a very feeble draught is produced in the apparatus, for nearly all the required air is supplied by leakage round the pipe near the screw, very little entering through the window tube, in consequence of the resistance offered by the cotton-wool. In this way, if the mirror is moving it may be gently brought to rest without impact, or it may be given a swing of any desired amplitude. So perfectly does this work, that the mirror may be steadied very quickly so as to move through less than a scale division, an amount which corresponds to six or seven seconds of arc, or to a force of less than one thousand-millionth of the weight of a grain.

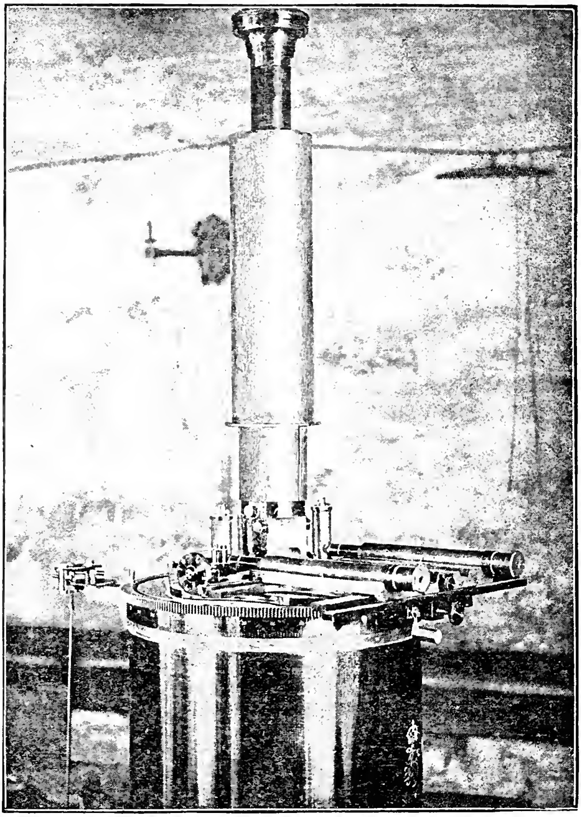
The operations for any complete experiment are fourteen in number. I do not intend to go through these seriatim, as time will not allow me to do so. It is sufficient now to say that the first eight are necessary to get the instrument and scale relatively fixed and adjusted, the vertical measures made, and generally all ready for operation 9, in which the optical compass is employed. This is a most important one, for not only are the horizontal measures made, on which so much depends, but in addition the planes of the wires and fibres are made identical, the corresponding scale reading is found, and any eccentricities are measured and may be corrected.

Fig. 8 is a view of the apparatus with the optical compass in position, and with the microscopes focussed upon the wires. They are then ready to be withdrawn by the focussing slide, so as to transfer the distances directly to the small glass scale, as already described.

When this is completed the proper windows are put in position, the screen tubes, the octagon house and the felt screens are all placed ready for operation 10, in which the deflections are measured, and the period with the balls is determined. As this is the operation in which variations of temperature produce so serious an effect, I prefer to leave everything undisturbed for three days, to quiet down. A few hours are quite useless for the purpose.

In operation 11 the period with the counter-weight in the place of the gold balls is measured; also the deflection, if any, due to the lid and lead balls upon the mirror alone. This is only 1/10 division, but its existence is certain. In the later operations the deflections, if any, due to the lid alone on the mirror alone, and to the lid alone on the mirror and gold balls, are separately determined. Neither of these can be detected. The actual elongation of the fibre may also be observed at this stage, but this is of interest only as bearing on the elastic properties of quartz fibres under longitudinal strain.

Fig. 8.



Before I come to the treatment of the observations, I should like to refer shortly to the kind of perfection of conditions which by the employment of every practicable refinement that I could devise, I have succeeded in obtaining. Taking experiment 8 as an example, favourable in that the conditions were good, i. e. I was not badly disturbed by trains, wind, or earth tremors, I give the worst and the best sets of four points of rest obtained from six elongations. They were :—

Worst set + position	Best set − position.
24493	20795·4
24493	20795·7
24493·5	20795·5
24492	20795·5
(24491·7)*	
24492·9 mean.	20795·5 mean.

Taking all the mean points of rest, as determined above, in groups of three to eliminate slow shifting, if any, of the points of rest, the series of deflections were :—

3696·5
3696·6
3696·0
3696·8

(Interval of one hour, in which oscillations of large amplitude were observed for period.)

(3697·7)
3696·0

Immediately after the oscillations of large amplitude, which in this case at the end were rather badly disturbed by trains or otherwise, a rather different deflection was observed, but not seriously different. As examination of the figures shows only one anomalous point of rest immediately after the large amplitude disturbance, I feel justified in rejecting the only discordant figure, and in taking the mean of the rest as the true deflection. The unit in this case is 1/10 division. It corresponds to an angular movement of 1/280,000, i. e. about three-quarters of a second of arc. Now a calculation of the angular twist due to a rotation of the air based upon the period, the moment of inertia, and the logarithmic decrement, shows that if the air in the tube were made to whirl round at the rate of one turn in six weeks, so that the air would blow past the gold balls at the rate of one inch in a fortnight, the deflection produced would be 1/10 division, an amount which is greater than the uncertainty of the deflection on this particular night. Again, an examination of the points of rest through the night in the positive and in the negative positions shows a very small steady creep, the same in each case. Creepage of this sort has been, I believe, mentioned as a defect of quartz fibres. When it gives trouble it is due to draughts, as already explained, or to imperfect attachment of the fibres.† In the present instance the creepage observed corresponds to a surface rate of movement on the fibre of a millionth of an inch a month.

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* Disturbed by trains.

† See my paper on 'Attachment of Quartz Fibre,' Phil. Mag. May 1894.

An examination of the mobile system of the beam and suspended gold balls, of which I exhibit a greatly enlarged and working model, at once shows that all the parts are capable of independent movement to an apparently perplexing degree. This in the theory of the instrument I have treated provisionally as a rigid system, moving all as one piece, which it certainly does not seem to be. For instance, the lead balls, by their attraction of the gold balls, pull them out of the perpendicular, so that their distance from the axis is greater than that given by measurement by the optical compass. The error amounts, in the case of the lower ball, when the lead is at its nearest points to 1/10,000,000 inch, and I have not taken any notice of it. When the beam is oscillating through so great an angle as 100,000 units the centrifugal force only makes the gold ball move out four times as much, and I have taken no notice of that. Again, when the mirror is under acceleration by the fibre, the gold balls, hanging 5 and 11 inches below, do not follow absolutely; they must lag behind, and so affect the period. Now in this case the gold balls are suspended in a manner which is dynamically equivalent to their being at the end of a pendulum $5\frac{3}{4}$ miles long, the shortest equivalent pendulum that has ever been employed in work of this kind; but short as it is, I have not thought it worth while to be perturbed by an uncertainty of a few inches. There is one point which in some of the experiments only has amounted to a quantity which I do not like to ignore. It is due to the torsional mobility of the separate fibres, about which each gold ball hangs, allowing them in their rotation to slightly lag behind the mirror. As I did not see how to allow for it, I applied to Prof. Greenhill, who immediately told me what to do, and who, with Prof. Minchin, spent a day or two in the country, covering many sheets of paper with logarithms, in finding and solving for me the resulting cubic equation. The correction on this account is 1/7850 on the stiffness of the torsion fibre.

There are four remaining corrections depending on the fact that besides the gravitating spheres there are the ball-holders and supporting wires and fibres, all of which produce small but definite disturbances in the gravitation. These are all calculated and allowed for. They are:—

Disturbances due to brass holders of lead balls	1/7320
,, ,, copper ,, gold ,, ...	1/265,000
Attraction of lead balls for quartz fibres	+1/200,000
,, gold ,, phosphor-bronze wires	−1/115,000

Then in experiment 9 gold cylinders were employed. Mr. Edser, of the Royal College of Science, calculated for me the correction to be applied if they were treated as spheres; this amounted to 1/3300.

I have already mentioned that experiment 8 was made under more than usually quiet conditions. Such extreme quiet is desirable, that

I manage to reserve Sunday nights, from midnight to six or eight in the morning, for observations of deflection and period. All the other operations can be carried on in the daytime. Sunday is the only night that is suitable, as the railway companies spend every other night shunting and making up trains about a mile away, and this causes such a continuous clatter and vibration, that hours of work may be lost. A passing train does not seem so injurious; but, fortunately for me, most of the observations were made during the coal strike, and fewer trains than usual were running. However, though I may escape from the rattling traffic of St. Giles by working at night, and on Sunday nights am not so badly affected by the trains, I am still not sure of quiet even when there is no wind. For instance, at a quarter to four on Monday morning, Sept. 10, 1893, I was recording chronographically the passage of every ten divisions. Everything was quite quiet, and at the particular moment the marks on the drum recurred at intervals of about three seconds. Suddenly there was a violent non-vibrating lurch of fifteen divisions, or 150 units, which is enormously greater than anything that either trains or traffic could produce; of course I could make no further record. The time of the last mark was, allowing for the known error of the clock, 15h. 44m. 14·3s. This was entered the same day in my note-book as an earthquake, and in Tuesday's *Standard* I read an account of a violent earthquake in Roumania at about the same time. Mr. Charles Davison informs me that the shock was recorded at Bucharest at 15h. 40m. 35s., but that the epicentrum must have been some distance from there. Exact particulars, it seems, cannot be obtained. Though it would appear that the rate of travel of the shock is unusually high, there never was any doubt that what I observed was an earthquake, and it is practically certain that it was the Roumanian earthquake.

Owing to the viscosity of the air, which limits the time during which an observation for period can be made to about 40 minutes, on account of the resistance that the slowly moving mirror and gold balls experience in their passage through it, I made one experiment with a view of reducing this difficulty, by the use of an atmosphere of pure dry hydrogen gas, which possesses a viscosity only half that of air. I found that on this account a great advantage could be gained; but this was more than counterbalanced by the difficulty of getting up a sufficient swing in the gas, and of efficiently controlling the mirror. At the same time, I think that if I had had time to provide means for feeding the gas into the tube without entering the corner, and at the same time were to prevent diffusion at the lower screw, that a little trouble in this direction would be well rewarded. Meantime I found within the limits of error, which were greater than without the hydrogen, that the deflection and the period corrected for the diminished damping were the same. The chief interest of this experiment lies in the fact that it revealed an action unknown to me, and I believe to others, that a thin plane glass mirror, silvered and lacquered on one side, definitely bends to a small extent, becoming slightly convex on the glass side when in hydrogen, and instantly recovers its form when surrounded by air again. This happened many times, producing a change of focus in the telescope of about $\frac{5}{8}$ inch. I do not offer any explanation of the fact.

There is an observation which should be of interest to elasticians. In experiments 4 to 8 the torsion fibre carried the beam mirror and the ·25-inch gold balls, weighing, with their hooks and fibres, 5·312 grammes. In experiment 9, gold cylinders were substituted, weighing, with their hooks and fibres, 7·976 grammes. The weight of the mirror was ·844 gramme. In consequence of the small increase of load the torsional rigidity of the fibre fell more than 4 per cent., an amount far too great to be accounted for by the change of dimensions, even if Poisson's ratio were as great as $\frac{1}{2}$. There is no doubt about the great reduction in stiffness, for this figure is one of the factors in the final expression for G, which does not show a change of more than 1 part in 1570.

It will not be possible at this late hour to explain how the observations are treated so as to obtain the value of G. It is sufficient to state that in one of these clips all the observed deflections and corrected periods are collected. In the second all the geometrical observations are collected and reduced, so as to obtain what I call the geometrical factor, i.e. a number which, when multiplied by the unknown G, gives the torsion on the fibre. In the third, the moments of inertia and periods are made use of to find the actual stiffness of the fibre in the several experiments, and in the fourth these are combined so as to find G. From G the density of the earth Δ immediately follows.

The annexed table contains the important particulars of each experiment. From this it will be seen that the lead balls were twisted and interchanged in every way, so as to show any want of gravitational symmetry if it should exist. For instance, after experiment 7 the ball that was high was made low, the side that was outwards was turned inwards, and their distance apart was reduced by 1/50 inch, but the change in the result was less than 1 part in 2400. The experiments 7, 8, 9, 10 were made under widely different circumstances. After experiment 8 the gold balls were changed for heavier gold cylinders, which, as has already been stated, reduced the torsion of the fibre nearly 5 per cent., but the result differs from that of experiment 7 by 1 part in 3700. I then broke the end of the torsion fibre. After keeping it in London three months I broke the other end. I then resoldered each end and put the fibre back in its place, and after making every observation afresh, found with the new shorter and stiffer fibre a result differing from that of experiment 8 by only 1 part in 60,000. These four experiments were all made under favourable circumstances, and on this account I feel more able to rely upon them than on the earlier ones, which were subject to greater uncertainty. The last experiment was made under most unfavourable conditions.

No. of Exp	Lead balls.			Gold balls.		Neutral lid reading.	Date.	Deflection.	Geometrical factor.	Stiffness of fibre.	Result.	
	Arch side.	Wall side.	Shellac spots.	Arch side.	Wall side.						G	Δ
3	2 low	1 high	Inwards	1·3 grammes each 4 low	3 high	Deg. 267	1892 Oct. 1–30	5637·3	6089·89	·00 245483	·0000000 66645	5·5213
4	2 low	1 high	Inwards	Gold balls of double weight 4 low	3 high	267	1893 Aug. 15–Sept. 3				66702	5·5167
5	1 high	2 low	Inwards	3 high	4 low	86·5	Sept. 4–11	3667·6	12423·8	772200	66711	5·5159
6	2 low	1 high	Inwards	4 low	3 high	265·9	Sept. 12–14	3667·7	12422·3	Same as No. 8	66675	5·5189
7	2 low	1 high	Outwards	4 low	3 high	265·9	Sept. 15	3664·0	12432·8		66551	5·5291
8	1 low	2 high	Inwards	4 low	3 high	265·9	Sept. 16–18	3695·4	12534·2	771664	66579	5·5268
9	1 low	2 high	Inwards	Gold cylinders 3 low	1 high	86	Sept. 27–Oct. 3	5775·5	18800·5	739988	66533	5·5306
10	1 low	2 high	Inwards	4 low	3 high	85·25	1894 Jan. 1–13	3515·4	12531·8	811011	66578	5·5269
11	1 low	2 high	Inwards	4 low	3 high	85·25	Jan. 14	Hydrogen experiment				
12	2 high	1 low	Inwards	3 high	4 low	265·2	Jan. 17–21	3520·5	12533·7	811385	66695	5·5172
									Adopted result ..		66576	5·5270

The periods and deflections were taken in the first four hours after midnight, then, after a few hours' sleep, and far too soon for the temperature to have quieted down, I took the period with the counterweight, but was only able to give ten minutes, as I had to catch a train in order to be able to give my midday lecture at South Kensington. It is not surprising that under such conditions a difference of 1 part in 600 should arise. There is a difference of about the same order of magnitude between the earlier experiments and the favourable four. There is one point about the figures that I should like to mention. No results were calculated till long after the completion of the last experiment. Had I known how the figures were coming out, it would have been impossible to have been biassed in taking the periods and deflections. Even the calculating boys would not have been quick enough to discover whether the observed elongations were such as would give a definite point of rest. I made my observations, and the figures were copied at once in ink into the books, where afterwards they left my hands and were ground out by the calculating machine. The agreement, such as it is, between my results is therefore in no way the effect of bias, for I had no notion till last May what they would be.

My conclusion is that the force with which two spheres weighing a gramme each, with their centres 1 centimeter apart, attract one another, is $6 \cdot 6576 \times 10^{-8}$ dynes, and that the mean density of the earth is 5·5270 times that of water. I do not think the fourth significant figure can be more than one or, at the outside, two parts in error.

It is evident, from what I have already said, that this work is of more than one-man power. Of necessity I am under obligations in many quarters. In the first place, the Department of Science and Art have made it possible for me to carry out the experiment by enabling me to make use of apparatus of my own design. This belongs to the Science Museum, where I hope in time to set it up so that visitors who are interested may observe for themselves the gravitational attraction between small masses. Prof. Clifton, as I have already stated, has given me undisturbed possession of his best observing room, his only good underground room, for the last four years. The late Prof. Pritchard lent me an astronomical clock. Prof. Viriamu Jones enabled me to calibrate the small glass scale on his Whitworth measuring machine; and Mr. Chaney did the same for my weights. I would specially refer to the pains that were taken by Mr. Pye, of the Cambridge Scientific Instrument Company, to carry out every detail as I wished it, and to the highly skilled work of Mr. Colebrook, to which I have already referred. Finally, I am under great obligations to Mr. Starling, of the Royal College of Science, who performed the necessarily tedious calculations.

In conclusion, I have only to say that while I have during the last five years steadily and persistently pursued this one object with the fixed determination to carry it through at any cost, in spite of any opposition of circumstance, knowing that by my discovery of the value of the quartz fibre, and my development of the design of this apparatus, I had, for the first time, made it possible to obtain the value of Newton's Constant with a degree of accuracy as great as that with which electrical and magnetic units are known; though I have up to the present succeeded to an extent which is greater, I believe, than was expected of me, I am not yet entirely satisfied. I hope to make one more effort this autumn, but the conditions under which I have to work are too difficult; I cannot make the prolonged series of experiments in a spot remote from railways or human disturbance; I cannot escape from that perpetual command to come back to my work in London; so after this I must leave it, feeling sure that the next step can only be made by my methods, but by some one more blest in this world than myself.

[C. V. B.]

GENERAL MONTHLY MEETING,

Monday, July 2, 1894.

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

George W. Field, Esq. F.R.G.S.
Mrs. Henriette Kellgren,

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 at Low Temperatures:—

Sir Douglas Galton	£25

The Chairman reported that Mr. Ludwig Mond, F.R.S. had made a proposal to the Managers of the Royal Institution of Great Britain, to convey to the Members of that Institution the freehold of No. 20 Albemarle Street, to be held by them for the purpose of a laboratory to be named "The Davy-Faraday Research Laboratory of the Royal Institution," and for the purpose of providing increased accommodation for the Institution, and that the Managers, at their Meeting held this day, had most cordially and gratefully accepted this munificent gift made in the communication from Mr. Mond, of which the following is an extract:—

"In the year 1843 a proposal was made to establish at the Royal Institution a School of Practical Chemistry, which was not only to give practical and systematic instruction to students, but was also to provide a place where original researches could be conducted by individuals skilled in manipulation, and where the professors could work out their problems by the aid of many qualified hands.

"This proposal was submitted by the Managers of the Royal Institution to Professors Faraday and Brande, who expressed their strong approval of the end proposed, and their desire that it might be carried out at the Royal Institution, 'if it could be done well.' But, on a closer examination of the limited space within the walls of the Institution, it appeared impracticable to afford accommodation for carrying out the proposed scheme.

"In 1846 the Royal College of Chemistry was founded, and since that time numerous schools for the teaching of practical chemistry have been established all over the country. These, however, only cope with the first part of the scheme recommended in 1843, while as to the second part, viz. founding a laboratory for the carrying out of independent researches, no adequate provision exists in England up to this date, although the need for it was so strongly felt so many years ago, and its importance for the advancement of science so forcibly dwelt upon by the promoters of the scheme and by such men as Faraday and Brande.

"I have felt that the need for such a laboratory has become greater and greater since the work of the scientific investigator has become more and more subtle and exact, and, in consequence, requires instruments of precision and a variety of facilities which a private laboratory can only very rarely command; and surely this need exists nowhere to a greater extent than in England, and nowhere can such a laboratory be expected to bear more abundant fruit than in this country, which possesses such an unrivalled record of great scientific researches, which have emanated from private laboratories not connected with teaching institutions, and amongst which the laboratory of the Royal Institution stands foremost, and has kept up its reputation for nearly a hundred years.

"It has been my desire for many years to found a public laboratory which is to give to the devotees of pure science, anxious and willing to follow in the footsteps of the illustrious men who have built up the proud edifice of modern science, the facilities necessary for research in chemistry, and more particularly in that branch of the science called physical chemistry.

"I have come to the same conclusion as the promoters of the scheme of 1843, viz. that such laboratory would still have the greatest prospect of success under the ægis of the Royal Institution, that in fact it would be the consummation of the work which this great Institution has been fostering in its own laboratory, with such remarkable results, by the aid of the eminent men whose services it has always been fortunate enough to procure.

"As only want of space prevented the Royal Institution undertaking this task fifty years ago, I took the opportunity which offered itself last year of acquiring the premises, No. 20 Albemarle Street, adjoining the Institution. This property I found very suitable for the purposes of such a laboratory, and large enough to afford, besides, facilities to the Royal Institution for a much needed enlargement of its present laboratory and its libraries and reception rooms, which I should with great pleasure put at the disposal of the Institution.

"Being convinced that the Managers of the Royal Institution will give all the encouragement and aid in their power in the foundation and working of such a research laboratory, I hereby offer to convey to the Royal Institution the freehold of No. 20 Albemarle Street, and also the lease I hold from the Institution of premises contiguous thereto, to be held by them for the purpose of a laboratory, to be named 'The Davy-Faraday Research Laboratory of the Royal Institution,' and also for the purpose of providing increased accommodation for the general purposes of the Royal Institution, as far as the available space will allow, after providing for the requirements of the research laboratory.

"I also offer to make, at my own expense, all structural alterations necessary to fit the premises for these purposes, and to equip the Davy-Faraday Research Laboratory with the necessary apparatus, appliances, &c., and to make such further adequate provision as will hold the Royal Institution free from all expense in connection with the premises and the working of the said laboratory. . . .

"I am aware that my offer will not provide for the third object of the scheme of 1843, viz. to enable the professors to work out their problems by the aid of many qualified hands; but I trust that if the laboratory which I offer to found proves successful, others will come forward who will supply the means for attaining this end, by the foundation of scholarships and bursaries to qualified persons willing to devote themselves to scientific work and not in a position to do so without assistance."

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

FROM

The French Government—Documents Inédits sur l'Histoire de France: Recueil des Chartes de l'Abbaye de Cluny, Tome V. 4to. 1894.

The Lords of the Admiralty—Report of Astronomer Royal. 8vo. 1894.

Report of H.M. Astronomer at the Cape of Good Hope, 1879-86. 4to. 1894.

The British Museum (Natural History)—A Monograph of Lichens found in Britain, Part 1. 8vo. 1894.

Catalogue of the Mesozoic Plants in the British Museum: The Fossil Plants of the Wealden, Part 1. 8vo. 1894.

Catalogue of Birds, Vol. XXIII. 8vo. 1894.

Accademia dei Lincei, Reale, Rôma—Classe di Scienze fisiche, matematiche e naturali. Atti, Serie Quinta: Rendiconti. 1° Semestre, Vol. III. Fasc. 9-11. 8vo. 1894.

Classe di Scienze morali, etc.: Rendiconti, Serie Quinta, Vol. III. Fasc. 3, 4. 8vo. 1894.

American Academy of Arts and Sciences—Proceedings, New Series, Vol. XX. 8vo. 1893.

American Philosophical Society—Proceedings, No. 144. 8vo. 1894.

Asiatic Society, Royal (Bombay Branch)—Journal, Vol. XVIII. No. 50. 8vo. 1894.

Prof. Peterson's Report on the search for Sanskrit MSS. (Vol. XVIII. No. 49 A). 8vo. 1894.

Asiatic Society of Bengal—Proceedings, 1893, No. 10; 1894, No. 1. 8vo. 1894.

Journal, Vol. LXII. Part I. No. 4; Part 2, No. 4. 8vo. 1893.

Annual Address by Sir C. A. Elliott. 8vo. 1894.

Astronomical Society, Royal—Monthly Notices, Vol. LIV. No. 7. 8vo. 1894.

Bankers, Institute of—Journal, Vol. XV. Part 6. 8vo. 1894.

Basel, Naturforschenden Gesellschaft—Verhandlungen, Band IX. Heft 3. 8vo. 1893.

British Architects, Royal Institute of—Journal, 3rd Series, Vol. I. Nos. 14-16. 4to.

British Association for the Advancement of Science—Report of the 63rd Meeting at Nottingham. 8vo. 1894.

British Astronomical Association—Memoirs, Vol. II. Part 3; Vol. III. Part 2. 8vo. 1894.

California, University of—Report of Work of the Agricultural Experiment Stations, 1891-2. 8vo. 1893.

Addresses, March 23, 1893. 8vo.

Annual Report, 1893. 8vo.

Department of Geology: Bulletin, Vol. I. Nos. 3, 4. 8vo. 1893.

Camera Club—Journal for June, 1894. 8vo.

Canada Meteorological Office—Report for 1889. 8vo. 1893.

Chemical Industry, Society of—Journal, Vol. XIII. No. 5. 8vo. 1894.

Chemical Society—Journal for June, 1894. 8vo.

Cooper Union Trustees, U.S.A.—35th Annual Report. 8vo. 1894.

Cornwall Polytechnic Society, Royal—Annual Report for 1893. 8vo. 1894.

Cracovie, l'Académie des Sciences—Bulletin, 1894, No. 5. 8vo.

Editors—American Journal of Science for June, 1894. 8vo.

Analyst for June, 1894. 8vo.

Athenæum for June, 1894. 4to.

Brewers' Journal for June, 1894. 8vo.

Chemical News for June, 1894. 4to.

Chemist and Druggist for June, 1894. 8vo.

Electrical Engineer for June, 1894. fol.

Electrical Engineering for June, 1894. 8vo.

Electrical Review for June, 1894. 8vo.

Electric Plant for June, 1894. 4to.

Electricity for June, 1894. 8vo.

Engineer for June, 1894. fol.

Engineering for June, 1894. fol.

Engineering Review for June, 1894. 8vo.

Horological Journal for June, 1894. 8vo.

Industries and Iron for June, 1894. fol.

Ironmongery for June, 1894. 4to.

Law Journal for June, 1894. 8vo.

Lightning for June, 1894. 4to.

Machinery Market for June, 1894. 8vo.

Nature for June, 1894. 4to.

Nuovo Cimento for May, 1894. 8vo.

Editors—continued.
Open Court for June, 1894. 4to.
Optician for June, 1894. 8vo.
Photographic News for June, 1894. 8vo.
Photographic Work for June, 1894. 8vo.
Physical Review, Vol. I. Nos. 1, 3. 8vo. 1893.
Scots Magazine for June, 1894. 8vo.
Technical World for June, 1894. 8vo.
Transport for June, 1894.
Tropical Agriculturist for June, 1894.
Work for June, 1894. 8vo.
Zoophilist for June, 1894. 4to.

Electrical Engineers, Institution of—Journal, Vol. XXIII. No. 112. 8vo. 1894.

Florence, Biblioteca Nazionale Centrale—Bolletino, Nos. 203, 204. 8vo. 1894.

Franklin Institute—Journal, No. 822. 8vo. 1894.

Geographical Society, Royal—Geographical Journal for June. 8vo.

Harvard University—University Bulletin, No. 58. 8vo. 1894.

Institute of Brewing—Transactions, Vol. VII. Nos. 7, 8. 8vo. 1894.

Johns Hopkins University—University Studies, Twelfth Series, V.–VII. 8vo. 1894.
American Chemical Journal, Vol. XVI. No. 6. 8vo. 1894.
University Circular, No. 112. 8vo. 1894.

Junior Engineering Society—Record of Transactions, Vol. III. 1892–3. 8vo. 1894.

Linnean Society—Journal, No. 177. 8vo. 1894.
Proceedings, May, 1894. 8vo.

London County Council—Report of the Technical Education Board, 1893–4. fol. 1894.

Madden, Thomas More, Esq. (the Editor)—Genealogical, Historical and Family Records of the O'Maddens of Hy Many. 8vo. 1894.

Manchester Geological Society—Transactions, Vol. XXII. Parts 16–18. 8vo. 1894.

Meteorological Society, Royal—Quarterly Journal, No. 90. 8vo. 1894.
Meteorological Record, No. 51. 8vo. 1894.

Microscopical Society, Royal—Journal, 1894, Part 3. 8vo.

Middlesex Hospital—Reports of the Registrars for 1892. 8vo. 1894.

Ministry of Public Works, Rome—Giornale del Genio Civile, 1894, Fasc. 3, 4. 8vo. And Designi. fol. 1894.

Moll, Dr. J. W. (the Author)—Papers. 8vo. 1876–93.

New York Academy of Sciences—Annals: Vol. VI. (Index); Vol. VII. Nos. 6–12. 8vo. 1894.

Odontological Society of Great Britain—Transactions, Vol. XXVI. No. 7. 8vo. 1894.

Payne, W. W. and Hale, G. E. (the Editors)—Astronomy and Astro-Physics for June, 1894. 8vo.

Pharmaceutical Society of Great Britain—Journal for June, 1894. 8vo.

Philadelphia Academy of Natural Sciences—Proceedings, 1893, Part 3. 8vo. 1893.

Photographic Society of Great Britain, Royal—Journal and Transactions, Vol. XVIII. No. 10. 8vo. 1894.

Rochechouart, Société des Amis des Sciences et Arts de—Bulletin, Tome IV. No. 1. 8vo. 1894.

Royal Dublin Society—Transactions: Vol. IV. Series II. Part 14; Vol. V. Parts 1–4. 4to. 1892–3.
Proceedings: Vol. VII. Part 5; Vol. VIII. Parts 1, 2. 8vo. 1892–3.

Royal Irish Academy—Transactions, Vol. XXX. Parts 11, 12. 4to. 1894.
Proceedings, Third Series, Vol. III. No. 2. 8vo. 1894.
Todd Lecture Series, Vol. V. 8vo. 1894.

Royal Society of London—Proceedings, No. 333. 8vo. 1894.
Catalogue of Scientific Papers, 1874–83, Vol. X. 4to. 1894.

Selborne Society—Nature Notes for June, 1894. 8vo.

Skinner, Walter R. Esq. (the Editor)—The Mining Manual for 1894. 8vo.

Society of Architects—Journal, New Series, Vol. I. No. 8. 4to. 1894.

Society of Arts—Journal for June, 1894. 8vo.

Tacchini, Professor P. Hon. M.R.I. (the Author)—Memorie della Società degli Spettroscopisti Italiani, Vol. XXIII. Disp. 5^a. 4to. 1894.

United Service Institution, Royal—Journal, No. 196. 8vo. 1894.

United States Department of Agriculture—Monthly Weather Review for March, 1894. 4to.

The Climatology and Physical Features of Maryland. 8vo. 1894.

Monthly Report of Maryland State Weather Service, Vol. III. Nos. 1, 4, 7, 8. 8vo. 1893.

Veneto, L'Ateneo—Revista: Serie XVI. Vol. I. Fasc. 1–6; Vol. II. Fasc. 1–6; Serie XVII. Vol. I. Fasc. 1–3; Vol. II. Fasc. 1–6. 8vo. 1892–3.

Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1894: Heft 5. 4to. 1894.

Very, Frank W. Esq. (the Author)—Hailstorms. 8vo. 1894.

Wilde, Henry, Esq. F.R.S. (the Author)—On the Origin of Elementary Substances and on some new Relations of their Atomic Weights. 4to. 1892.

Zoological Society of London—Proceedings, 1894, Part 1. 8vo. 1894.

GENERAL MONTHLY MEETING,

Monday, November 5, 1894.

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

Arthur Salvin Bowlby, Esq.
Sir Charles Cameron, M.D. M.P.
Baroness de Knoop,
Lord Romilly,

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 at Low Temperatures:—

W. Morris Beaufort, Esq.	£10
Mrs. Bloomfield Moore	£200
Professor Dewar	£100
Sir Frederick Abel, Bart.	£50
Edward Frankland, Esq.	£21
The Rt. Hon. Sir John Lubbock, Bart.	£20
John T. Brunner, Esq. M.P.	£50
The late Thomas Hawksley, Esq. (per Professor Dewar)	£100
Charles Hawksley, Esq.	£50
The Baroness Burdett Coutts	£100
Sir Andrew Noble, K.C.B.	£100

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

FROM

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GENERAL MONTHLY MEETING,

Monday, December 3, 1894.

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

Rev. James Oliver Bevan, M.A.
Horace T. Brown, Esq. F.R.S.
H. S. Keating, Esq.
Gabriel Lindo, Esq.
Sydney Morse, Esq.
Edward Steinkopff, Esq.
George Johnstone Stoney, Esq. M.A. D.Sc. F.R.S.
Charles Lloyd Tuckey, M.D.
Alfred Edward Western, Esq.
Charles Wightman, Esq.
G. W. Wolff, Esq. M.P.

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 at Low Temperatures:—

William S. Playfair, M.D.	£20
Ludwig Mond, Esq.	£200

The following Lecture Arrangements were announced:—

Professor J. A. Fleming, F.R.S. *M.R.I.* Six Lectures (adapted to a Juvenile Auditory) on The Work of an Electric Current. On Dec. 27 (*Thursday*), Dec. 29, 1894; Jan. 1, 3, 5, 8, 1895.

Professor Charles Stewart, M.R.C.S. Fullerian Professor of Physiology, R.I. Twelve Lectures on The Internal Framework of Plants and Animals. On *Tuesdays*, Jan. 15, 22, 29, Feb. 5, 12, 19, 26, March 5, 12, 19, 26, April 2.

William Samuel Lilly, Esq. M.A. Four Lectures on Four English Humorists of the Nineteenth Century. On *Thursdays*, Jan. 17, 24, 31, Feb. 7.

L. Fletcher, Esq. M.A. F.R.S. Three Lectures on Meteorites. On *Thursdays*, Feb. 14, 21, 28.

Samuel Rawson Gardiner, Esq. M.A. LL.D. Three Lectures on Three Periods of Seventeenth Century History: 1. The Stuart Monarchy; 2. The Commonwealth; 3. The Restoration. On *Thursdays*, March 7, 14, 21.

E. B. Tylor, Esq. D.C.L. LL.D. F.R.S. Two Lectures on Animism, as shown in the Religions of the Lower Races. On *Thursdays*, March 28, April 4.

Lewis F. Day, Esq. Three Lectures on Stained Glass Windows and Painted Glass (from the point of view of Art and Craftsmanship). On *Saturdays*, Jan. 19, 26, Feb. 2.

A. C. Mackenzie, Esq. Mus. Doc. Three Lectures on The Traditional and National in Music (with Musical Illustrations). On *Saturdays*, Feb. 9, 16, 23.

The Right Hon. Lord Rayleigh. Six Lectures on Waves and Vibrations. On *Saturdays*, March 2, 9, 16, 23, 30, April 6.

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

FROM

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Lightning for November, 1894. 4to.
Machinery Market for November, 1894. 8vo.
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Report on the Electric Light Experiments at Chatham in 1878. By Sir John Stokes. 8vo.
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Sopra un sistema di equazioni differenziali. By F. Brioschi.
Sur la distinction des intégrales des équations différentielles linéaires en sous groupes. Par F. Casorati. 4to. 1881.
Genneralizzazione di alcuni teoremi dei Sig. Hermite, Brioschi e Mittag-Leffler sulle equazioni differenziali lineari del 2° ordine. By F. Casorati.
Aggiunte a recenti Lavori dei Sig. Weierstrass e Mittag-Leffler sulle funzioni di una variabile complessa. By F. Casorati. 4to. 1882.
Sur un écrit très récent de M. Stickelberger. By F. Caso. 1881.
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WEEKLY EVENING MEETING,

Friday, January 19, 1894.

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

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

Scientific Uses of Liquid Air.

When Faraday was working on liquid gases in this Institution about 1823, with such means as were then at his command, his inquiry was limited to the determination of the specific gravities and vapour pressures of such bodies. Twenty years later, by the use of solid carbonic acid, the greatest cold then possible was obtained, and Faraday made admirable use of Thilorier's new cooling agent to extend his early investigations. Just as liquid carbonic acid produced in glass tubes was of no use as an agent for effecting the liquefaction of more resisting gaseous matters, until it could be manipulated in the solid state, so liquid air, until it could be handled, stored and used in open vessels, like any ordinary liquid, could not be said to possess scientific uses in any wide sense. Such operations become easy when double-walled vacuum vessels (such as were described in a former lecture) are employed in the conduct of experiments where substances boiling at very low temperatures have to be manipulated. The chief scientific use of liquid air consists in the facilities it gives for the study of the properties of matter at temperatures approaching the zero of absolute temperature. In this lecture the expression liquid air may mean either oxygen or air. Where a constant temperature is required oxygen is used. Liquid air made on the large scale may contain, after it is collected in open vacuum vessels, as much as 50 per cent. of oxygen. Such a liquid boils between $-192°$ and $-182°$ C., and the longer it is stored the nearer it comes to $-182°$ C. or the boiling point of pure oxygen. For a number of experiments of a qualitative character, whether it is liquid air or oxygen that is used makes no difference. In many of the experiments to be recorded, liquid oxygen made from the evaporation of liquid air was employed. In pursuing this subject in consort with Professor Fleming,* a long series of experiments, involving the use of large supplies of liquid oxygen, have been carried out on the electric resistance of metals and alloys, and

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* 'The Electrical Resistance of Metals and Alloys at Temperatures Approaching the Absolute Zero.' By James Dewar, LL.D. F.R.S. and J. A. Fleming, M.A. D.Sc. F.R.S. Professor of Electrical Engineering in University College, London. Phil. Mag. 1892.

the results warrant the conclusion that at the zero of absolute temperature all the pure metals would be perfect conductors of electricity. Under such conditions a current of electricity started in a pure metallic circuit would develop no heat, and therefore undergo no dissipation. Similarly, we infer there would be no Peltier effect at the zero. In other words, the passage of electricity from one metal to another would take place without evolution or absorption of heat.

Further investigation, along with Professor Liveing,* on the refractive index of liquid nitrogen and air, has led to the conclusion that the refractive indices of nitrogen and air are respectively for the D-ray, 1·2053 and 1·2062. In these determinations, instead of using the prisms we have employed the method of Terguem and Trannim, which consists in suspending in the liquid two plates of glass with a thin layer of air between them, and measuring the angle of incidence at which the chosen ray suffers total reflection at the surface of the air. As all the vacuum vessels are either spherical or cylindrical in form when filled with liquid, they act as lenses which are irregular and full of striations. Further, small bubbles of gas being given off in the liquid rendered any image indistinct when viewed with a telescope. In order to avoid the necessity of observing any image through the liquid, it was used simply as a lens to concentrate the light observed on the slit of a spectroscope. Under such conditions the observations were easily executed and the results satisfactory.

For some time a series of observations on the thermal opacity of liquid oxygen and nitrogen have been projected. It is, however, exceedingly difficult to experiment in such a way as to eliminate the absorbing action of the glass vessels, and as the use of rock salt is impracticable, the absorption of heat of low refrangibility remains for the present undetermined. It is possible, however, to use the glass vacuum vessels to determine approximately the relative thermal transparency for heat of high refrangibility, such as is radiated by a colza lamp. The following results represent the heat transmitted through the same vacuum vessels filled with different liquids, taking chloroform as the unit for comparison and correcting for differences of refractive index.

Chloroform	1·0	Liquid nitrous oxide ..	0·93
Carbon bisulphide ..	1·6	Liquid ethylene	0·60
Liquid oxygen	0·9	Ether	0·50

From this result it follows that liquid oxygen is nearly as transparent to high temperature heat radiation as chloroform, which is one of the most transparent liquids next to carbon bisulphide. Liquid ethylene is much more opaque. These results must, however, be considered only as an approximation to the truth, and as generally confirmatory of the inferences Tyndall drew as to the relation between gases and liquids as absorbents of radiant heat.

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* 'On the Refractive Indices of Liquid Nitrogen and Air. By Professors Liveing and Dewar. Phil. Mag. 1893.

Instead of silvering the interior and exterior of the vacuum vessels, it is found convenient when using mercury vacua to leave a little excess of liquid mercury, in order that the act of filling the inner vessel with liquid air should cause a fine silvery deposit of the metal over the exterior surface of the inner vessel. In such a vessel liquid air or oxygen shows no signs of ebullition, the surface remains as quiet and still as if it was ordinary water. The supply of heat is cut down to less than four per cent. of what it is without exhaustion and silvering in good vacuum vessels. The result is that volatile liquids can be kept thirty times longer. Such vessels do not, however, maintain indefinitely the high standard of heat isolation they possess the first time they are used. After repeated use all vacuum vessels employed in the storage and manipulation of liquid air deteriorate. Illustrations of the appearance of such vessels are given in Figs. 1 and 2. The rapidity with which a space is saturated with mercury vapour (which we know exerts a pressure of about one-millionth of an atmosphere) is easily proved by simply filling a barometer in the usual way, and then instantly applying a sponge of liquid air to a portion of the glass surface of the Torricellian vacuum space, when a mercury mirror immediately deposits. It is important to know the amount of mercury deposited from a saturated atmosphere which is maintained (containing excess of liquid mercury) at the ordinary temperature, the condensation taking place when liquid air or oxygen is discharged into a vessel surrounded by such a Torricellian vacuum. If the deposit on the cooled bulb is allowed to take place for a given time, the outer vessel can then be broken and the amount of mercury which coated the bulb ascertained by weighing. Knowing the surface of the cooled bulb, the amount deposited per unit of area can be calculated. In this way it was found that in ten minutes 2 milligrams of mercury per square centimetre of surface was deposited. Considering that one-tenth of a milligram of mercury in the form of saturated vapour at the ordinary temperature corresponds to the volume of 1 litre, this proves that the equivalent weight of 20 litres had been condensed in the space of ten minutes. This plan of cooling a portion of the surface of a vessel by the application of a liquid air sponge, enables us to test our conclusions as to the amount of matter present in certain vacua. Here is a globe of the capacity of 1 litre. It has been filled with, presumably, nothing but the vapour of mercury, by boiling under exhaustion and subsequent removal of all excess of liquid. Such a flask ought to contain mercury in the gaseous state that would weigh rather less than one-tenth of a milligram, assuming the ordinary gaseous laws extend to pressures of less than one-millionth of an atmosphere. Now we know by electric deposition that one-tenth of a milligram of gold can be made to cover one square centimetre of surface with a fine metallic deposit. Considering the general similarity in the properties of mercury and gold, we should therefore anticipate that if all the mercury vapour could be frozen out of the litre flask it would also form a mirror about one square centimetre in area. But after one such mirror is deposited, the renewed application of a second liquid air sponge to another portion of the surface would cause no visible deposit. This is exactly what takes place. If, however, two spheres, one much larger than the other, are joined together by means of a tube about 2 mill. in diameter and 50 mill. long, the whole space being a Torricellian vacuum (with some excess of mercury) then on decanting, the mercury may be transferred to the smaller sphere, as is represented in Fig. 3. Now if an air sponge is applied to a portion of the surface of the larger sphere, a mercury mirror instantly deposits, but on applying a new air sponge to another portion of the surface, no further mercury mirror is formed. The narrow glass tube prevents the excess of liquid mercury in the small bulb supplying vapour rapidly to the larger one, so that the local cooling to −180° C. of a portion of the surface has practically condensed all the mercury in the larger space, although the small one is still filled with saturated vapour and a free communication exists between them. If while in this condition the small bulb is inclined so as to allow a drop of liquid mercury to fall into the lower side of the large bulb, which has not been cooled, instant deposition of mercury takes place on the liquid air cooled portion of the upper surface. Under very small pressure of vapour, therefore, equalisation of pressure of two bulbs communicating by a narrow tube is a very slow process. There are cases, however, in which the application of a sponge of liquid air to the surface of a vessel causes no visible deposit, and yet the inference is that something has been condensed. The best arrangement to show this effect is to select highly exhausted vacuum tubes containing phosphorescent materials like alumina and other minerals, and to arrange the induction coil spark gap of a little greater resistance than the vacuum tube. On starting the coil the current passes solely by the vacuum tube, but immediately the liquid air sponge cools a portion of the surface of the bulb, the discharge shifts to the air gap. During the cooling the phosphorescence of the glass tube is greatly increased, but finally the resistance may become so great that all discharge in the vacuum tube ceases. Some old tubes belonging to the late Dr. de la Rue have given visible deposits near the electrodes, and in many the diameter and distribution of the striæ are materially changed during the local cooling to − 190° C. When large vessels containing nothing but mercury or iodine vapour as a residuum of the vacuum space are rubbed with a cotton wool sponge of liquid air in a dark room, luminous glows filling the vessel take place occasionally, or bright flashes of light which enable the shape of the vessel to be seen. The ordinary mercury vacuum vessels show the same phenomena, which is doubtless due to electric discharges caused by friction and cooling.

The optical properties of bodies cooled to the temperature of boiling liquid air will require long and patient investigation. An interesting fact easily observed is the marked change in colour of various bodies. Thus, for instance, oxide, sulphide, iodide of mercury, bichromate of potash, all become yellow or orange; while nitrate of uranium and the double chloride of platinum and ammonium become

Fig 1.

Fig. 2.

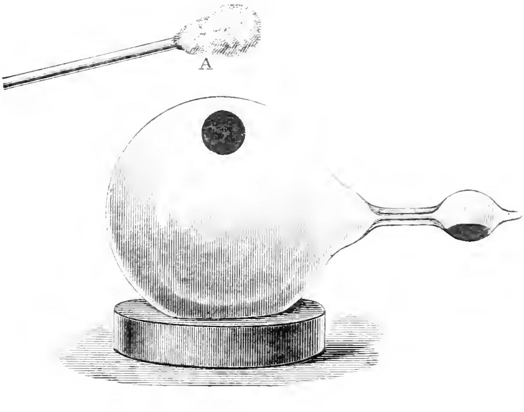

Fig. 3.

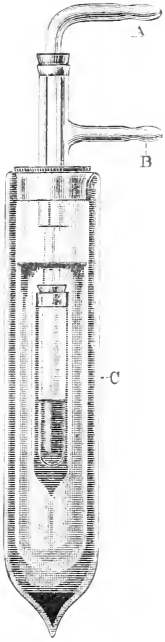

Fig. 4.

white. Chromic acid, dilute solution of iodine in alcohol, strong solutions of ferric chloride and other coloured solutions become greatly changed. Such facts are sufficient to prove that the specific absorption of many substances undergoes great changes at the temperature of – 190° C.

The tranquil atmosphere of air above the surface of the liquid in cylindrical or spherical vacuum vessels is a convenient place to cool very fragile bodies. During the slow ebullition of the fluid, gas between – 190° C. and – 180° C. is given off, which has three times the density of ordinary air, and which falls slowly over the mouth of the vessel in a heavy stream. On dipping into this atmosphere small soap bubbles, they contract rapidly and then freeze. If a soap film is made on a circle of thin wire about 2 inches in diameter, and allowed to stand until it shows the various orders of coloured bands, and is then carefully dipped into the cool air, it freezes, showing all the original colours. The black band is, however, always broken. Speaking of films, an interesting experiment may be made with a thin stretched sheet of india-rubber, such as is used for making balloons. It is well known that stretched india-rubber contracts when heated and expands when cooled. Now this can be shown very easily by covering a glass funnel or the end of a cylindrical vessel with a stretched sheet of rubber as thin as the walls of balloons. Such a surface is quite flat and fairly transparent. If a sponge of liquid air is drawn across the surface, the course is marked by a series of wrinkles, due to the temporary expansion of the rubber caused by the extreme cold. The sheet of rubber being extremely thin, soon regains the ordinary temperature, and the surface then is as flat and tense as before. During the continuous motion of the cotton wool liquid air sponge over the rubber surface, it is followed by wave-like depressions which disappear almost as quickly as they are formed. The elasticity of india-rubber, after cooling to – 182° C. and re-heating, seems unimpaired.

Organic substances that only become solid at very low temperatures may be divided into two classes: those which crystallise, and those which form glasses. Thus bisulphide of carbon, tetrachloride of carbon, methyl alcohol, hydride of amyl, all form crystals, whereas ethyl alcohol, amyl alcohol, turpentine, ethyl nitrate, chinoline, picolin, are glass-like. If a few drops of bisulphide of carbon are added to alcohol and the mixture cooled to –180° C., a white solid emulsion is formed, whereas the addition of tetrachloride of carbon to the alcohol resulted in the production of a clear solid without any separation. In the same way pure methyl alcohol crystallises easily, but the addition of a few drops of ethyl alcohol prevents crystallisation and causes a glass to be formed. Thus the examination of the behaviour of organic bodies at low temperatures may be a fruitful means of organic investigation.

For many purposes of investigation it is necessary to keep liquid air without evaporation. This is readily done by the use of two vacuum test-tubes, fitting freely one inside the other, arranged as in Fig. 4.

The smaller one is filled with liquid air, and after the insertion of an india-rubber stopper and glass tube, is completely immersed in liquid air contained in the larger vacuum vessel. In the figure the tube A connects with the inner vacuum tube and B with the outer. As the latter receives all the radiant and conducted heat, air is continuously boiling off through the tube B, but as the supply of heat is effectually cut off from the inner vacuum vessel, also containing liquid air, no air distils through tube A. This is the most convenient arrangement to use for the production of solid air. For this purpose B is connected with an air pump until the pressure is reduced to about $\frac{1}{2}$ inch, and therefore the temperature about $-200°$ C. Then a good air pump is put on to the inner vessel of liquid air (containing oxygen and nitrogen in the normal proportion of oxygen and nitrogen), by means of the tube A, while maintaining constantly the exhaustion in the outer vessel. In a short time the air in the inner vessel solidifies to a transparent jelly-like mass.

The same principle is used when the latent and specific heats have to be determined. Fig. 5 shows the general plan of the apparatus. Now a definite quantity of heat has to be conveyed into the inner vacuum vessel containing liquid air, with the object of finding the weight of liquid that distils off, on the one hand, or the elevation of temperature in the liquid that takes place on the other. For the purpose of adding a given quantity of heat it is convenient in some cases to use mercury (as represented in the figure), or to lower a piece of platinum or silver, or even glass, into the inner vessel: each unit of heat supplied evaporates a definite amount of air, which is readily ascertained by collecting the gas which comes off during the heat conveyance. In Fig. 5, A is the mercury, C the inner vessel of liquid air, D a three-way stop-cock, F a tube for collecting the air given off; E is a barometric tube for observing the pressure when the inner vessel is exhausted. In a latent heat determination all that is necessary is to weigh the mercury added and to measure the amount of air by volume which has distilled from the liquid state. If the specific heat of the liquid is wanted, then the inner vessel is exhausted (as well as the outer) through the tube F to about $\frac{1}{2}$ inch pressure, and the three-way stop-cock turned so as to shut off F and connect the inner vessel with the manometer E. Mercury is now dropped into the inner vessel until the manometer rises to the atmospheric pressure or the liquid reaches its boiling point under atmospheric pressure. Care must be taken to prevent the drops of mercury falling exactly in the same place, otherwise a mercury stalagmite grows up rapidly through the liquid, vitiating the results. Another objection to the use of mercury arises from the drops causing the rebound of small liquid air drops, which strike the cork and get evaporated away from the main body of liquid. The amount of mercury added conveys the necessary amount of heat needed to raise the given amount of liquid from its boiling point under $\frac{1}{2}$ inch pressure to its boiling point under 30 inches. The relative pressures give the temperature range, and the weight of liquid air or other

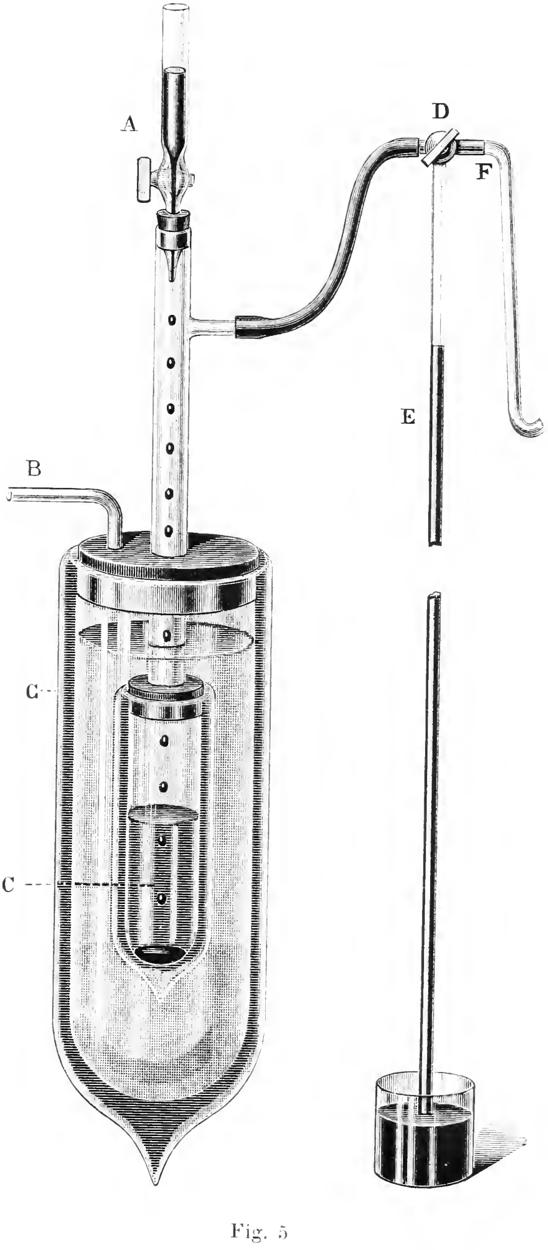

Fig. 5

gas under observation is easily ascertained, together with the weight of mercury added. In this way the latent heat of liquid oxygen at its boiling point is about 80 units, and the mean specific heat between − 198° and − 182° is 0·39.

Seeing that the most powerful chemical affinities are in abeyance at very low temperatures, it is a matter of great interest to ascertain what change comes over the physical force we name cohesion. Here we are dealing with the molecular forces which are effective in uniting together the particles of solid bodies, in contrast to the force we name chemical attraction, which exists most characteristically between dissimilar molecules. Both are alike in this respect, that they are insensible at sensible distances. If we accept the theory of matter which regards finite heterogeneousness of the most homogeneous bodies as proved, then Lord Kelvin has shown that gravitation alone would account for the so called cohesive forces. Thus, he says ('Popular Lectures,' vol. i. page 60): "But if we take into account the heterogeneous distribution of density essential to any molecular theory of matter, we readily see that it alone is sufficient to intensify the force of gravitation between two bodies placed extremely close to one another, or between two parts of one body, and therefore that cohesion may be accounted for, without assuming any other force than that of gravitation, or any other law than the Newtonian." Another view of the cohesive forces is taken by Mr. S. Tolver Preston, in his work entitled 'Physics of the Ether,' page 64. He says, "The phenomena of 'cohesion,' 'chemical union,' &c., or the general phenomena of the aggregation of molecules, being dependent on the molecular vibrations as a physical cause, it would therefore be reasonable to conclude that variation of vibrating energy (variation of 'temperature') would have a most marked influence on these phenomena, as is found to be the fact. Further, since when a physical cause ceases to exist the effect also ceases, it follows that at the absolute zero of temperature (absence of vibrating energy) the general phenomena of 'cohesion,' including the aggregation of molecules in chemical union, would cease to exist." If this theory is pressed so as to include the gaseous state, then at the temperature of −274° C. we may imagine the particles reduced to an incoherent layer of dust or powder. The experimental facts do not, however, warrant this conclusion, seeing that at the lowest temperature reached, which is about −210° C., air remains a transparent jelly. That a low temperature causes profound changes in the elastic constants of a metallic body is most easily shown by placing a rod of fusible metal in liquid air, and comparing the deflection produced by a weight when the rod is supported at one or both ends before and after cooling.

The Young modulus is increased to between four and five times its amount at ordinary temperatures. In the same way, the rigidity modulus can be shown to be greatly changed by cooling a spiral spring made of fusible metal wire. Such a spring at the ordinary temperature is quickly drawn out into a straight wire, by attempting to make it support an ounce weight. The same spiral, cooled to $-182°$ C., will support a couple of pounds, and will vibrate like a steel spring so long as it is cool. In the same way, a bell or tuning fork of fusible metal gives a distinct metallic ring at $-180°$. If two tuning-forks are taken of identical pitch, and one cooled to $-182°$, then on simultaneouly striking them beats are very distinctly heard. The simplest plan of getting some idea of the change in the cohesive force at low temperatures, is to ascertain the tenacity or breaking stress of the metals and alloys under such conditions, and to compare such results with similar experiments made at the ordinary temperature with the same metallic samples, using the same apparatus. In this way the comparative values are reliable. The only difficulty is the large quantity of liquid air or oxygen required to cool the steel supports of the wires, which have to be broken. Seeing that wires less than $\frac{1}{10}$ inch in diameter are unreliable, good strong rigid steel supports are needed, and as these have to be cooled each time a wire is broken, the experiments involve large quantities (gallons) of liquid air and oxygen. Further, as not less than three, and in many cases six experiments must be made with each sample of wire, and the stress in each case can only be applied slowly, work of this kind extends over long periods of time, and this means increased waste of liquid gases. Fig. 6 shows the general plan of the part of the testing machine which supports the wires which have to be broken. In the figure, A is the steel rod which is connected to the multiplying levers, the stress being gradually increased as usual by running in water into a vessel hung from the long end of the lever; C is the wire to be tested, B is an arrangement which measures roughly the extension of the wire, and D is a large silvered vacuum vessel holding the liquid oxygen. This latter vessel must be large, in order to avoid any part of the supports of the wire coming into contact with the sides, otherwise the shock of the wire on breaking shatters the vacuum bulb. The rupture must be made while the wire is immersed in the liquid oxygen, and the whole of the supports thoroughly cooled down. The wires must be caught in long V-shaped grooves made in the steel supports in order to avoid slipping, and change in the cross section of the wire. As a rule, the wires used were $\frac{1}{10}$ inch in diameter and 2 inches long. The following table gives the mean results of a large number of experiments:—

TABLE I.—BREAKING STRESS IN POUNDS OF METALLIC WIRES 0·098 INCHES DIAMETER.

	15° C.	−182° C.		15° C.	−182° C.
Steel (soft) ..	420	700	German silver	470	600
Iron	320	670	Gold	255	340
Copper ..	200	300	Silver	330	420
Brass	310	440			

An inspection of this table proves that all the common metals and alloys increase in tenacity at low temperatures: thus iron has doubled its breaking stress, and the other metals and alloys are all increased from a third to a half the normal amount. This increase

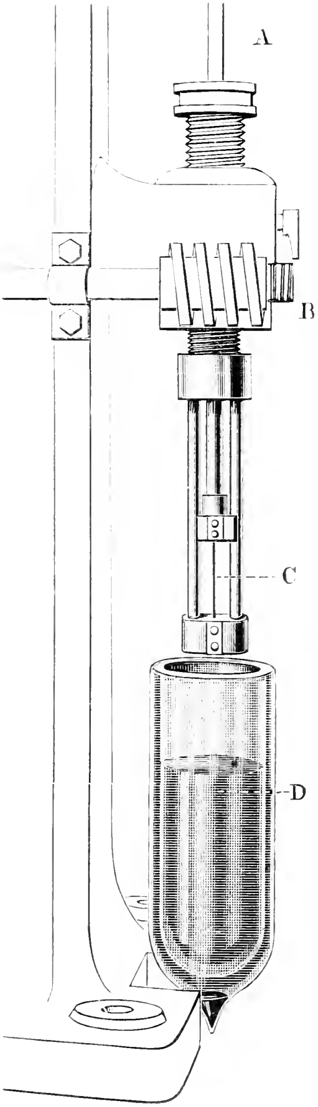

Fig. 6.

of strength is solely due to the low temperature, and persists only during its continuance. Wires that have been cooled to the temperature of − 182° C. and allowed to regain the ordinary temperature, are in no way changed as regards their breaking stress.

A second series of experiments were made with a set of cast test pieces of metals and alloys. The test pieces, all cast in the same mould, were 2 inches long with $\frac{1}{2}$ inch spherical ends, the cylindrical portion being $\frac{2}{10}$ inch diameter. The spherical ends of the test pieces rested in similar cavities made in a special set of steel supports that fitted on to the testing machine. Crystalline metals give castings that are far from uniform one with another, and it is very difficult to get even comparable results with metals like zinc, bismuth and antimony. The following table gives the experimental results :—

TABLE II.—BREAKING STRESS IN POUNDS OF CAST METALLIC TEST PIECES.
DIAMETER OF ROD 0·2 INCH.

	15° C.	−182° C.		15° C.	−182° C.
Tin	200	390	Bismuth	60	30
Lead	77	170	Antimony	61	30
Zinc	35	26	Solder	300	645
Mercury ..	0	31	Fusible metal (Woods)	140	450

It will be noted that in this list the breaking stress, by cooling to −182° C., has been increased to three times its usual value in the case of fusible metal, and to twice its usual value in the case of tin, lead and solder. The results with zinc, bismuth and antimony are exceptional, seeing they appear to be diminished in tenacity. This, however, may be only apparent, because the stresses set up in cooling such highly crystalline bodies probably weaken some set of cleavage planes so that rupture is then comparatively easy. In any case it must be admitted that no reliance can be placed on the tenacity of highly crystalline metals. The breaking stress of mercury is interesting, and turns out to be at − 182° C. nearly half that of lead at the ordinary temperatures. The percentage elongation is not given in the foregoing tables, simply because the value of such measurements is of little importance when such short pieces of the metals are under observation. The general results of such observations are, however, interesting: thus, lead and tin at ordinary temperatures elongate before breaking about the same amount, whereas if tin is cooled to −182° C. it hardly shows any extension, and lead under such conditions shows no change, stretching as much at −182° as at 15° C. Solder and fusible metal stretch less, and the cross section of the break is much less at −182° than at 15° C. The above experiments can only be considered as preliminary to a more elaborate investigation of the actual variation of the elastic constants at low temperatures. It will require complex experimental arrangements to get reliable measurements of the Young modulus and the rigidity modulus at the temperature of boiling liquid air. In the case of fusible metal, a first attempt to compare the ratio of the Young modulus at 15° and

− 182° with the ratio of the rigidity modulus between the same limits of temperature, has resulted in finding that both constants are increased in the same proportion. From this it would follow that the resistance to compression of the substance at −182° C. must be increased in a similar ratio. The comparative behaviour of strong steel spirals at 15° C. and −182° as to their elongation on the repeated addition of the same load was a subject examined on several occasions. The most careful comparison of such spirals, however, revealed no measurable differences in their elongation between the ordinary temperature and that of boiling oxygen. This may be due to the want of sufficient sensibility in the testing machine when applied to such delicate experiments. In the meantime it is reasonable to conclude that the rigidity modulus of very hard steel is not much changed by cooling it to −182° C. If balls of iron, tin, lead or ivory are cooled to −182° C. and dropped from a fixed height on a massive iron anvil the elastic rebound is markedly increased in all cases. The flat distortion surface produced on the lead sphere after impact is only one-third the diameter of the circular surface produced at the ordinary temperature when the lead ball falls from the same height.

The examination of the magnetic condition of matter at low temperatures is a subject of great interest and offers a wide field for investigation. In a former lecture the magnetic properties of liquid oxygen and air were discussed. Owing to the experimental difficulties, accurate quantitative measurements of the permeability have not yet been successful. Faraday was the first experimenter who examined the magnetic condition of matter at the lowest temperature that could be commanded in his time, viz. about −110° C. He did not succeed in making any substance which was non-magnetic at ordinary temperatures assume the magnetic state at the lowest temperature of the solid carbonic acid ether bath in vacuo. Later experimenters have directed their attention more especially to the action of high temperatures on magnetism, and the work of Professors Hopkinson and Ewing in this field of research is well known. Professor Trowbridge examined the effect of a temperature of − 80° C. on a permanent magnet, and came to the conclusion that the magnetic moment was diminished by about 50 per cent. Professor Ewing found that an increase of temperature of 150° C. above 10°, caused a reduction of the magnetic moment of a bar magnet by about 40 per cent., and that the magnet on cooling recovered its original state. This result would lead us to expect that if the same law is followed below the melting point of ice as Ewing found above it, then a bar magnet cooled to − 182° C. ought to gain in magnetic moment something like 30 to 50 per cent. The experiment of Professor Trowbridge is, however, apparently opposed to such an inference. It appears, however, that Professor Trowbridge cooled a magnet that had not reached a constant state (that is to say, one that on heating would not have completely recovered its magnetisation on cooling), because after the magnet had been cooled to − 80° on regaining the ordinary temperature, it had lost 50 per cent. of its original magnetic moment. Such a magnet would apparently diminish in magnetic moment on cooling and heating the first time the action was examined, but a repetition of the process when the action of magnetisation and temperature were strictly reversible might lead to an opposite conclusion. To settle this question a series of experiments on the magnetic moment of small magnets cooled to — 182° were carried out. Small magnets from half an inch to an inch in length were made of watch-spring or steel wire and were either used separately or in bundles; they were fixed rigidly in a block of wood by means of copper staples, and in this condition were easily clamped firmly in the field of a magnetometer. The cooling was effected by applying a cotton-wool sponge of liquid air. The relative deviations of the magnetometer are proportional to the magnetic moment of the magnet under the respective conditions of + 15° and — 182° C. After the first cooling the magnet is allowed to regain the ordinary temperature, and the operation of cooling and heating is repeated three or four times. The following table gives some of the results, and these may be taken as typical of a large additional number unrecorded.

CHANGE OF THE MAGNETIC MOMENTS OF PERMANENT MAGNETS AT + 15° AND − 182° C. per cent. of the value at the beginning of each cycle, which is always 15°.

	— 182° C.	+ 15° C.
(1) Hard steel, 0·5 inches long and 0·4 inches diameter.		
First Cycle	+ 0	− 30
Second "	+ 33	− 5
Third "	+ 36	0
(2) Soft steel.		
First Cycle	+ 12	− 28
Second "	+ 51	0
Third "	+ 51	0
(3) Hard steel, 1·03 inches long, 0·4 inches diameter.		
First Cycle	− 24	− 43·4
Second "	+ 23	0
Third "	+ 23	0
(4) Nine steel wires in bundle.		
First Cycle	+ 12·5	+ 3
Second "	+ 38	− 2
Third "	+ 33	0
Tested four days after.		
First Cycle	+ 50	0

If the experiment marked (1) is examined we find cooling to −182°, in the first cycle produced no change of magnetic moment, but that on heating to +15° C. the magnet had lost 30 per cent. of the original strength. In the second cycle cooling increased the magnetic strength of the magnet, in the condition in which it is left after the first cooling by 33 per cent., and heating diminished it by 5 per cent.; whereas in the third cycle cooling showed 36 per cent. increase and no loss in heating. It was only after three alterations of temperatures from +15° to −182° C. that the magnet reached a steady condition. In experiment (3) the first cooling shows a loss of 24 per cent., while in experiment (4) the first cooling shows a gain of $12\frac{1}{2}$ per cent.

It is clear, therefore, that according to these experiments, every magnet has individual characteristics that may either result in no change on cooling or the addition or subtraction of from 12 to 24 per cent. in the magnetic strength. All the experiments, however, show that a repetition of the cycle of heating and cooling brings the magnet to a steady state, in which cooling always causes increase in the magnetic strength of from 30 to 50 per cent., and the re-heating brings about no loss in the original magnetic moment. Such a marked alteration of magnetic strength might be used as a thermometer in low temperature research, and it is my intention to extend the inquiry to the lowest temperature that can be reached by the evaporation of nitrogen in vacua. A simple mode of showing the sudden alteration of magnetic strength on cooling, is to surround a permanent magnet made up of a bundle of steel wires with a coil of copper wire, leaving the ends of the magnet to project so that they can be dipped in liquid air. When the copper wires are attached to a galvanometer, and one of the ends of the magnet cooled, an induced electrical current occurs, due to the sudden magnetic change. Accurate observations must be made on the permeability and susceptibility of the magnetic metals at the temperature of boiling liquid air, and the above results are an indirect guarantee that this field of investigation will be fruitful in new scientific facts.

This lecture has already covered a very wide field. It is easy to put into a Friday evening discourse the work of a year. Members and friends have chiefly contributed to the Research Fund, which has enabled the Institution to extend the experimental plant needed for the prosecution of research in this field of inquiry, and they have strong claims to learn, in the first instance, the results of the general laboratory work. My object has been to illustrate the scientific uses of liquid air. To do this with any satisfaction requires what may be called a good deal of scientific prospecting. It is one thing to discover where the ore lies, it is another thing to produce the refined metal. Investigations on the properties of matter at the temperature of boiling liquid air, must be in the first instance rather qualitative than rigidly quantitative. In my opinion scientific progress is best served by conducting the inquiry on these lines. It will be easy to refine later on.

I have to acknowledge the great assistance I have received in the conduct of these experiments, from my excellent chief assistant Mr. Robert Lennox, and I must also express commendation of the way Mr. Heath has helped in the work.

[J. D.]

Royal Institution of Great Britain.

WEEKLY EVENING MEETING.

Friday, January 25, 1895.

Sir Douglas Galton, K.C.B. D.C.L. F.R.S. Vice-President, in the Chair.

Sir Colin Scott-Moncrieff, K.C.M.G. C.S.I.

The Nile.

I am to speak to you to-night of the Nile, and I think I may fairly say it is the most famous river in all the world: famous, through all the ages, for the civilisation that has existed on its banks; famous for its mystic fabulous rise, about which so many sages and philosophers have pondered; famous for its length, traversing one-fifth the distance from pole to pole; famous, and apparently destined to be famous, for the political combinations that ever centre around it. But I feel I must begin by an apology, for now that Egypt has come so completely within the tourist's range, probably many of my hearers have seen more of the Nile than I have.

If a foreigner were to lecture to his countrymen about the river Thames, and were to begin by informing them that he had never been above Greenwich, he might be looked upon as an impostor; and perhaps I am not much better, for I have never been higher up the river than Philæ, 610 miles above Cairo. For information regarding anything higher up, I must go, like you, to the works of Speke, Baker, Stanley and our other great explorers. I shall not, then, detain you to-night with any elaborate account of this upper portion of the river, but will only remind you briefly of that great inland sea, the Victoria Nyanza—in extent only a little less than the American Lake Superior—traversed by the equator, and fed by many rivers, some of them taking their rise as far as 5° S. lat. These rivers form the true source of the Nile, the mystery only solved in the present generation.

The outlet of this great lake is on its north shore, where the river rushes over the Ripon Falls, estimated by Speke at only 400 or 500 feet wide, and with a drop of 12 feet. Thence the river's course is in a north-west direction for 270 miles, to where it thunders over the Murchison Falls, a cliff of 120 feet high. Soon after that it joins the northern end of Baker's lake, the Albert Nyanza, but only to leave it again, and to pursue its course through a great marshy land for more than 600 miles, to where Bahr Gazelle joins it from the west. A little further down the great Saubat tributary comes in on the east. This is the region in which the river is obstructed by islands of floating vegetation, which, if checked in their course, at last block up its whole width, and form solid obstructions known as *sadds*, substantial enough to be used as bridges, and obstacles, of course, to navigation, until they are cleared away. The waters of the Saubat are of very light colour, and tinge the whole river, which, above its junction, is green and unwholesome from the long chain of marshes which it traverses. Hence it is called the White Nile. 600 miles further brings us to Khartoum, where the Blue Nile from the Abyssinian mountains joins it, and at 200 miles still further to the north, it is joined by the Atbara river, also from Abyssinia, a torrent rather than a river.

Baker gives a graphic account of how he was encamped by the dry bed of the Atbara on June 22, 1861. The heat was intense, the country was parched with drought. During the night the cry went forth that the floods were coming, and in the morning he found himself on the banks of a river, he says, 500 yards wide and from 15 to 20 feet deep. All nature had sprung into life. A little north of the junction of the Atbara is Berber, whence you will remember is the short cut to Suakin in the Red Sea, which so many thought would have been the true route for our army to take in relieving Gordon. From Khartoum to Assouan is a distance of 1100 miles of river, during which it makes two immense curves—for on a straight line the distance is not half so much—and it is in this part of its course that it passes over the six great cataracts or rapids which block all ordinary navigation. The first, or furthest north, cataract is just above Assouan, a distance of 750 miles from the Mediterranean, through the country known as Egypt. From the junction of the Atbara to its mouth in the Mediterranean, a distance of 1680 miles, the Nile receives no tributary. On the contrary, during every mile of its course its waters are diminished by evaporation, by absorption and by irrigation. The river gets less and less as it flows through the rainless land, and its maximum volume is to be found during the floods at the junction of the Atbara, and at other seasons at Khartoum, 1875 miles from the Mediterranean.

The whole distance by river from the Victoria Nyanza to the sea is about 3500 miles. It may not be easy to derive any clear impression from this bare recital of mileage. Let me try to convey to you in some other ways the idea of the length of the Nile. Standing on the bridge at Cairo, I used to reflect that I was just about half-way between the source of the Nile and the White Sea. Or, to put it another way, if we could suppose a river crossing our English Channel, and that the Thames should find its way out in the Euphrates and Persian Gulf, that river would be about as long as the Nile.

In this short sketch of the course of the Nile, I must not forget to mention one interesting feature. About 40 miles south of Cairo, the low Libyan chain of hills which bounds the Nile valley on the west is broken by a gap, through which the waters of the river can flow, and beyond this gap lies a saucer-shaped depression called the Fayúm, of about 400 square miles in area, sloping down to a lake of considerable size, the surface of whose waters stands about 130 feet below that of the sea. This lake is known as the Birket el Kurún.

From the time of the earliest Egyptian records, this province of the Fayúm was famed for its fertility, and to the Egyptian taste for its delightful climate. Many of the most precious monuments of antiquity have been found in the Fayúm. The famous Labyrinth is supposed to have stood just at its entrance; and, what has excited most interest for the engineer in all times, it is here that Herodotus places that wonderful lake Mœris, which, receiving for half the year the surplus supply of the Nile, rendered it back again in irrigation to Lower Egypt during the other half. Where this lake actually was, has excited discussion since any attention has been paid to ancient Egyptian history. It seems pretty clear that in earlier days the Birket el Kurún was of much greater proportions than it is now, but how it ever could have been large enough to allow of its waters flowing back into the Nile valley when the river was low, without at the same time drowning the whole Fayúm, is not very clear.

Now, what are the functions of a great river, what are the offices which it renders to man? And first of all, at least in this latitude, we would mention the carrying off to the ocean of the surplus water that descends from the skies. Nobly does the Nile fulfil this duty; but with this enormous qualification, that it transports the water from tracts where there is too much, and carries it all free of cost, not to waste it in the sea, but to bestow it on tracts where it is of priceless value, more than taking the place of rain in watering the fields.

The next function of a river is to form a highway through the land, and for most of its course the Nile fulfils this duty well, too. Gordon considered it possible for steamers to ascend the Nile, during the floods, from its mouth to the Fola rapids, a distance of about 3040 miles; but at other seasons, the six cataracts cannot be passed. Leaving out the 1100 miles which they occupy, there is an unbroken 750 miles in the lower, and nearly 1200 miles in the upper river. I cannot look on it as probable that it will ever pay to make navigable canals and locks round these cataracts, as it would entail so much hard rock-cutting.

Another function of a river is to promote industry by the employment of its water power. We know how valuable is this power even in England, and how much more in countries like Switzerland, where it abounds, and on the great rivers of America. Excepting a few very rude wooden wheels in the Fayúm, I do not know, through all the annals of the past, of a single water-wheel ever turned by the power of the Nile. But that power exists to an almost unlimited extent. And may we not prophesy that some day in the future, when that long stretch of Nubian cataracts has fallen into civilised hands, and when we know how to transmit electric energy with economy, that then our descendants will draw wealth to Egypt from its chain of barren cataracts?

As a drainage outlet to a continent, as a long highway, as a source of power, the Nile is great, but not so much so as many other rivers. Its unique position is due to the benefit it confers on Egypt in turning it from being a desert into being the richest of agricultural lands, supporting with ease a population of about six hundred to the square mile. Herodotus truly said Egypt is the gift of the Nile. It more than supplies the absence of rain, and this it does, first, by the extraordinary regularity with which it rises and falls; and secondly, by the fertilising matter which the waters carry in suspension, and bestow upon the land. Imagine what it would be to the English farmer if he knew exactly when it would rain and when it would be sunshine. When the Irrigation Department of Egypt is properly administered, the Egyptian farmer possesses this certainty, and he has this further advantage, that it is not merely water that is poured over his lands, but, during nearly half the year, water charged with the finest manure.

According to the early legend, the rise of the Nile is due to the tears shed by Isis over the tomb of Osiris, and the texts on the Pyramids allude to the night every year on which these tear-drops fall. The worship of Isis and Osiris has long passed away, but to this day every native of Egypt knows the *Lailet en Nuktah*, the night in which a miraculous drop falls into the river, and causes it to rise. It is the night of June 17. Herodotus makes no allusion to this legend of Osiris. In his time, he says, the Greeks gave three reasons for the river's rise. He believed in none of them, but considered as the most ridiculous of all that which ascribed the floods to the melting of snows, as if there could possibly be snows in such a hot region. It was many centuries after Herodotus' time when the snowy mountains of Central Africa were discovered.

The heavy rains commence in the basin of the White Nile during April, and first slowly drive down upon Egypt the green stagnant waters of that marshy region. These appear at Cairo about June 15. About a fortnight later the real flood begins, for the rains have set in in Abyssinia by May 15, and the Blue Nile brings down from the mountains its supply of the richest muddy water. It is something of the colour and nearly of the consistency of chocolate, and the rise is very rapid, as much sometimes as 3 feet per diem, for the Atbara torrent, having saturated its great sandy bed, is now in full flood also. The maximum flood is reached at Assouan about September 1, and it would reach Cairo some four days later, were it not that during August and September the water is being diverted on to the land, and the whole Nile valley becomes a great lake. For this reason the maximum arrives at Cairo about the beginning of October. The rains cease in Abyssinia about the middle of September, and the floods of the Blue Nile and Atbara rapidly decrease; but in the meantime the great lakes and marshes are replenished in the upper regions, and slowly give off their supplies, on which the river subsists, until the following June. Yearly this phenomenon presents itself in Egypt, and with the most marvellous regularity. A late rise is not more than about three weeks later than an early rise. In average years the height of the flood at Assouan is about 25½ feet above the minimum supply. If it rises 29 feet above this minimum, it means peril to the whole of Egypt, and the irrigation engineer has a hard time of it for two months. If the river only rises 20 feet above the minimum, it means that whole tracts of the valley will never be submerged. Such a poor flood has happened only once in modern times, in 1877, and the result was more serious than the devastation caused by the most violent excess.

The mean flood-discharge at Cairo is about 280,000 cubic feet per second, the maximum about 400,000. The mean lowest Nile is about 14,000 cubic feet per second at Cairo, but some years there is not more than 10,000 cubic feet per second passing Cairo in June, and within three months after this may have increased fortyfold.

Until this century, the irrigation of Egypt only employed the flood-water of the river, and it was this that made it the granary of the world. No doubt rude machines for raising Nile water were used at all seasons and from all times. But by these it was not possible to irrigate on a large scale, and in reality they were only employed for irrigating vegetables or gardens, or other small patches of land.

It must not be thought that the water of the flooded river is ever allowed to flow where it lists over the lands. The general slope of the valley on each side is away from the river, a feature which the Nile shares with all Deltaic streams. Along each edge of the river, and following its course, is an earthen embankment, high enough not to be topped by the highest flood. In Upper Egypt, the valley of which seldom exceeds six miles in width, a series of embankments have been thrown up, abutting on their inner ends against those along the river's edge, and on their outer ends on the ascending sides of the valley. The whole country is thus divided into a series of oblongs, surrounded by embankments on three sides, and by the slope of the desert hills on the fourth. In Lower Egypt, where in ancient days there were several branches of the river, this system was somewhat modified, but was in principle the same. These oblong areas vary in extent from 60,000 to 3000 or 4000 acres, and the slope being away from the river, it is easy to cut short, deep canals in the banks, which fill as the flood rises, and carry the precious mud-charged water into these great flats, or, as they are termed, basins of irrigation. There the water remains for a month or more, some 3 or 4 feet deep, depositing its mud, and then at the end of the flood it may either be run off direct into the receding river, or, more usually, passed off through sluices from one basin to another, and ultimately back into the river. In November the waters have passed off, and wherever a man and a pair of bullocks can walk over the mud, and scratch its surface with a wooden plough or even the branch of a tree, wheat or barley is sown; and so saturated is the soil that the grain sprouts and ripens in April or May without a drop of rain or any fresh irrigation. And a fine crop is reaped. One of our great brewers told me the other day, that when barley grown in this country was spread in the malting-house, about 3 per cent. of it must be counted on as not sprouting and being dead. If grain two or three years old was used, as much as 20 per cent would be found dead. With Egyptian barley, he said, even after several years, you could count on every grain germinating. The crop once reaped, the fields remain dry, and crack in the fierce summer heat until next flood comes on.

The tourist who only comes to Egypt to shun "winter and foul weather," knows nothing of the majestic glories of the Nile flood. The ancient Nilometer at the south end of the island of Roda, just above Cairo, is one of the most interesting sights of the place. The water enters from the river by a culvert into a well about 18 feet square, with a graduated stone pillar in the centre. On each side of the well is a recess about 6 feet wide and 3 feet deep, surmounted by a pointed arch, over which is carved in relief a Kufic inscription, and a similar inscription is carried all round the well, consisting of verses of the Koran. A staircase goes down the well, from the steps of which the initiated may read the height of the water on the pillar; but they are few in number, and the hereditary Sheikh of the Nilometer, whose duty it is to keep the record, is a person of some importance. The Nilometer dates from A.D. 861, and I believe in the archives of Cairo may be found the daily record for 1000 years.

I need hardly tell you that when our English engineers took the river in hand, we established a number of gauges at Wadi Halfa, Assouan, Cairo, and many other points, on more scientific principles than the venerable Nilometer of the Roda Island.

After the river has begun to rise, its height is daily chanted through the Cairo streets until it reaches 16 cubits on the gauge. At this point the Khalig el Masri, the old canal that flows through the heart of Cairo, is opened. Up to this point it is dry; and, full or empty, it is little more than a sanitary abomination at present; but in former days it occupied an important place, and when the Nile water was high enough to flow down its bed, it was looked on that the flood had fairly set in, and that the kindly fruits of the earth might be duly expected.

The head of this canal is on the right bank of the river, just south of Cairo. The water enters a channel some 30 feet wide, with a high wall on its left, and a sloping bank on its right or southern flank. The water then flows under the pointed arch of an old stone bridge. The bed of the canal is cleared so that it would flow in at a gauge of about 14½ cubits, but an earthen bank is thrown across it about 4 feet higher.

There is no more interesting ceremony in Egypt than the annual cutting of the Khalig, as the opening ceremony is called. It takes place between August 5 and 15. Days before, preparations are being made for the festival. Tents with innumerable lamps are placed along the wall on the one side; frames for all manner of fireworks are erected on the sand-banks on the other side. All the notables are there in full uniform, or in canonicals—the Khedive himself, or his representative, the Sheikh ul Islam (the highest dignitary of the

Muhammedan faith), the Sheikh el Bekri, the Sheikh es Sadat, all the learned scribes of the great university of the Azhar, the cabinet ministers and under-secretaries, the sirder of the army and his staff, the judges and the financiers.

The Egyptian troops are turned out, salutes are fired, and about eight o'clock in the warm summer night the classes all assemble under the gaily-lighted tents, the masses crowd round the frames for the fireworks, the street is lined with harem carriages full of closely-veiled figures, though it is not much that they can see from their broughams. Out in the river, just opposite the canal's mouth, is moored an old hulk of a certain sea-going outline, which has been towed up from Boulak during the day, and is an emblem of the time when the great republic of Venice sent an envoy to witness the ceremony. This boat is full of lamps, and fireworks too. As the night deepens the excitement increases. The populace on the bridge and the opposite bank are shouting, yelling and dancing wildly round the fireworks. On the other side are the gay uniforms and lighted tents, from whence we can look over the wall down on the dark water, where you see brown figures plunging in, and, waist-deep, digging with their hoes at the embankment that blocks the canal's mouth.

Long before midnight the fireworks have gone out, and left the splendid stars to themselves; the grandees have all gone to bed, but the people keep up the revelry, and in the morning, by 7.30, every one has come back. Then but little of the bank is left uncut; a few more strokes of the big hoes will do it, and the brown skins and the brown water reflect the bright sunlight from above. Then the Sheikh ul Islam solemnly thanks the Almighty, Allah the All-powerful, the All-merciful. He implores His blessing on the flood, and at a signal the bank is cut, the waters rush in, and with them a crowd of swimmers. A bag of silver piastres is scattered among them, and the ceremony is at an end.

There is a pretty legend, worth telling, of the cutting of the Khalig. Amr, the Muhammedan general, took Cairo in A.D. 640. Long before then there had been a heathen ceremony, and a virgin was yearly sacrificed to the god of the river. When the season came round, Amr was called upon as usual to sacrifice the girl. He sternly refused. That year the Nile flood was a failure. You can fancy how the indignant heathen population must have raged at the invader, and said, "We warned you what would happen if you didn't propitiate the river god." Cannot we fancy, also, how Amr's wild Arab soldiers must have had their faith sorely tried, and how they must have felt puzzled as to whether in this strange new country, with all those demon-built temples and pyramids, obelisks and sphinxes, it might not be as well to make friends of the local gods. Could Allah really help them here? Again the Nile flood came round. This time surely Amr would sacrifice the girl, and save the land? No; he would not. The people rose up in rebellion. Amr stood firm. But he wrote to the Kalif Omar for orders (Omar, whose name you will remember has come down in history as the destroyer of the Alexandrian library). Omar approved of his conduct, but sent him a paper to throw into the Nile. On the paper was written, "From Abd Allah Omar, Prince of the Faithful, to the Nile of Egypt. If thou flow of thine own accord, flow not; but if it be Allah, the one the mighty, who causeth thee to flow, then we implore him to make thee flow." Amr threw the paper into the water, and the Nile rose forthwith exactly as it was wanted. Since that day no girl has been sacrificed; but a pillar of earth is yearly left to be washed away in the middle of the canal, called the bride or the girl.

Such, as I have briefly described it, was the irrigation of Egypt until this century, when it fell under the rule of Muhammed Ali, a very sagacious and strong, if a very unscrupulous ruler. He saw that the country could produce far more valuable crops than cereals. The European market could be supplied with these from the fields of Europe, but Europe could not produce cotton and sugar-cane. Egypt had the climate, had the soil, had the teeming population; but these crops required water at all seasons; nor would it do to flood the fields to any depth, for just at the flood season the cotton crop is ripening. There was plenty of water in the river; but how was it to be got on to the land? Perennial irrigation was a fresh departure. As I have said, the Nile rises about 25½ feet. A canal, then, running 12 feet deep in flood has its bed 13½ feet above the surface of the Low Nile. Either the Nile water had to be raised, or the beds of the canals had to be lowered, in order that one should flow into the other, and after that the water had to be raised from the canal on to the land. Muhammed Ali began by lowering the canal beds of Lower Egypt, an enormous work considering the great number of the canals; and as they had been laid out on no scientific principles, but merely to suit the fancies of Turkish pashas or village sheikhs, and as those who had to excavate them to this great depth had only the slightest knowledge of levelling, the inevitable result followed—the deep channel became full of mud during the flood, and all the excavation had to be done over again. Incredible as it may seem, this great work was done year after year. It was a great serf population; if they were not fighting Muhammed Ali's battles in Arabia and Syria, they might as well be digging out the canals. No one thought of paying or feeding the workmen. The bastinado was freely applied if they attempted to run away. If they died under the labour, there were plenty more to come. But of course the work was badly done. The water might enter the canal; but, as the bed was not truly levelled, it did not follow that it would flow far. Then, as the river daily fell, the water in the canals fell too, and lessened in volume as the heat increased, and more was required. At last—in June, perhaps—the canal was dry, and the cotton crop that had been sown and watered, weeded and nurtured, since March, was lost altogether.

Then some one advised Muhammed Ali to throw a dam across the river, and so raise the water, and the result was the great Barrage.

About twelve miles north of Cairo the Nile bifurcates, and finds its way to the sea, by the Rosetta and Damietta branches. Across the heads of these two branches were built two stone bridges, one of 71, the other of 61 arches, each 5 metres or 16·4 feet span. These arches were intended to be fitted with gates; by lowering which, all the water would be dammed up, and diverted into three great trunk canals, taken out of the river just above these bridges. One to the right or east of the Damietta branch was to supply water to all the provinces of the eastern delta; one between the two bridges was to supply the splendidly fertile central delta; the third, to the left or west of the Rosetta branch, was to water all the western delta down to Alexandria.

There was no intention of water storage at the Barrage, but it was merely with the object of controlling the supply. While there was water enough in the river, by closing the gates it could be kept to a uniform level, and sent down the three trunk canals, from which it was to branch, into many minor ones. As the river went down, gate after gate would be closed, and so a constant supply could be kept in the canals. The idea was thoroughly sound. The execution was feeble.

Mougel Bey, the French engineer in charge of the work, had no doubt many difficulties to contend with. The work went fitfully on for many years, thousands of men being forced to it one year, and carried off to a campaign the next. But at last it was sufficiently finished to allow of an opening ceremonial in 1861. Gates had been fitted into the Rosetta branch arches, never into the Damietta.

The central canal had been dug in tolerably satisfactory style. The western canal, too, had been dug, but passing through a strip of desert it had become very much filled up with sand. The eastern canal was dug some five miles, and then stopped. Of course the Barrage without these canals was useless. However, they began to experiment with it, closing the gates on the Rosetta side. It was intended to hold up 4½ metres, or 14 feet 9 inches of water. It never held up 5 feet, till in 1867, it cracked across from top to bottom, on the Western side. An immense coffer-dam was built round the cracked portion, and the water was never held up again more than about 3½ feet, while the work was looked on as a deplorable failure. In 1883, all hope of making anything out of the Barrage was abandoned, and the Government were on the point of concluding a contract with a company to supply Lower Egypt with irrigation by means of an immense system of steam pumps, to cost 700,000*l.* to begin with, and 250,000*l.* a year afterwards.

That year there was a wretched serf army of 85,000 men working at canal clearances for 160 days, unfed, unpaid. The burden was nearly intolerable. The irrigation was all by fits and starts. There was no drainage; every hollow became sour and water-logged. With waterways everywhere, there was no navigation. In Upper Egypt things were better, as the system was a simpler one. But when we came to look into them, too, we found great abuse, and on an average about 40,000 acres never succeeded in obtaining water, though in the midst of abundance.

The Fayúm had long been a much-neglected province, though a most picturesque and attractive one. From carelessly allowing Nile water to flow into the lake during the floods, it had risen enough to swamp 10,000 acres of valuable land, and this mischief we found still increasing. Throughout the whole country drainage had been absolutely neglected. And here I would point out that irrigation without drainage means the sure deterioration of the land sooner or later. Considerable pains have been taken in Egypt to get the water on to the land. No sort of effort had been made to get it off. In a properly irrigated tract, between every two canals of supply, there should flow a drainage channel; the former should follow as far as possible the highest lands, the latter should follow the lowest. The canal gets smaller, till at last it is exhausted, giving itself out in innumerable branches. The drain, like a river, gets larger as it proceeds, being constantly joined by branches. But if there be no drains, and if the canals are laid out to flow into one another, so as to divide the country into, as it were, a cluster of islands, you can understand how the drainage water has no means of flowing off into the sea, and settles in unwholesome swamps. These we found prevailing to an alarming extent in the rich provinces of the delta. Such was the wretched state of Egyptian agriculture—the one single source of the country's wealth—when Lord Dufferin laid down the lines of the English administration, which have been amplified and pursued ever since.

It was in May 1883 that I took charge of the irrigation department in Egypt, having before then had some twenty years' experience of similar work in India; and I soon had the inestimable advantage of being joined by a band of the most indefatigable, energetic and able engineers, also from India, with whom it was my great privilege and happiness to be associated for the next nine years. I cannot talk too highly of these my colleagues—men who knew their work and did it, who kept constantly moving about in the provinces, badly lodged, badly fed, denied domestic comforts, constantly absent from their wives and families (they were all married men).

My friends, happy is the reformer who finds things so bad that he cannot make a movement without making an improvement. Happy the reformer who has as colleagues a staff of thoroughly loyal, duty-doing and capable men. Happy the reformer who is not pestered on all sides by the officious advice of the ignorant. Happy the reformer who has behind him a strong brave chief, as honest and truthful as he is strong. Such rare happiness fell to me in Egypt with my noble colleagues, and with Lord Cromer as our chief.

On first arrival, I was pressed, both by English and French men, to go into the question of the storage of the flood waters of the river on a large scale. I declined to do so, considering it would be time enough to think of increasing the quantity of water at our disposal when we had profitably used all that we already had, and while mighty volumes were daily flowing out to the sea, it could not be said that we were doing that. The first great work to be studied was the Barrage. We were warned on all sides to have nothing to say to it, as it was thoroughly unsound; but we felt sure we must either make it sound or build an entirely new one, and we resolved on the former. The work had failed because it was faulty in design, the floorings and foundations not being sufficiently massive, and faulty in execution from the dishonest use of bad materials and from bad workmanship. The bed of the river consists of nothing more stable than sand and alluvial mud for at least 200 feet deep. It was out of the question to think of getting down to solid rock. It was not, as we thought, very safe to excavate very deeply close to the existing works, so we decided not to try it, but merely to strengthen and consolidate the foundations, built as they were on sand. I have said that the work consisted of two great bridges over the two branches of the river. We could not shut up either branch entirely; but we decided to strengthen and complete one-half of each bridge each season, which meant four seasons' work. While the river was still in considerable flood each November, we began to throw out great embankments of earth about 200 feet from the bridge; one up-stream, the other down-stream of it, beginning at the short end, and ultimately enclosing one-half of the river as in a pond. This used to take three months' hard work. Then we pumped the water out of this enclosure, and laid bare the very bed of the river. Then we laid a massive stone flooring, 5½ feet thick, extending 100 feet up-stream, and as much down-stream, of the bridge. This was very difficult and hard work. It was kept going day and night, without intermission, from March till the end of June. Then we cut great holes in our embankment, cleared out our machinery, and prepared for the arrival of the flood at the beginning of July. Each year one-half of one bridge was finished, and the whole was completed at the end of June 1890.

In connection with the Barrage were completed the three great canals to carry off all the river supply from above it. So that practically now the Low Nile is emptied every season at the Barrage and diverted into these canals, and no water at all escapes to the sea. The natives wade everywhere across the river north of this point. Since it was completed, the Barrage has given no trouble. It holds up every year 4 metres, or 13 feet of water. The three trunk canals were all supplied with locks 160 feet by 28 feet, and adapted for navigation. The whole of these works cost about 800,000*l.* The annual increase of the cotton crop, compared to what it was before 1884, is never less than two and a-half millions sterling, which has not been a bad investment for Egypt.

Turning to Upper Egypt, my colleague, Colonel Ross, directed his attention very closely to the adjustment of canals overlapping one another, passing under and passing over one another; so that in future I trust that with the feeblest Nile flood it will be possible to pour water over every acre of the land.

The question of drainage was very thoroughly taken up. Twelve years ago it may be said that there were no drainage channels in Egypt. Two years ago there were about 1000 miles of such channels, some with beds as wide as 60 feet and flowing deep enough to carry cargo boats, others with beds only 3 or 4 feet wide. I am glad to say by these means large tracts in Lower Egypt which had been abandoned as totally ruined, have now been restored to cultivation. The level of the lake in the Fayúm was reduced by 13 feet between 1885 and 1893, and most of the inundated lands around it have been again dried.

I have already mentioned the cruel hardship of the corvée, the serf army of 85,000 men who were employed in the canal clearances from January to July, nearly half the year. I believe this institution was as old as the Pharaohs, and it was not easy to abolish it. But of course it went sorely against our British grain. Little by little we got money to enable us to pay our labour. By an annual outlay of 400,000*l.* this spring corvée has entirely ceased since 1889, and now the Egyptian labourer carries out these clearances in as free a manner as his brother in Middlesex, and gets paid for his work.

Having thus, to the best of our powers, utilised the water in the river flowing past us, we turned our attention to the storage of the surplus waters. Without some such storage it is impossible to increase the cultivation during the Low Nile. All the water is used up. During High Nile there is always a great volume escaping useless to the sea.

There are two ways in which the water may be stored; either by throwing a dam right across the river and forming a great lake above it, or, if such a place can be found, by diverting the flood water into some suitable hollow, and drawing it off from there at the season of low supply, as done by Herodotus' celebrated Lake Mœris. At one time there was a hope that such a storage basin might be found. An American gentleman, named Mr. Cope Whitehouse, in search of the real Mœris, found a very remarkable saucer-shaped depression just south of the Fayúm. We knew it could not have been Mœris, because in its bed we found no traces of a deposit of Nilotic mud, but it might be possible all the same to utilise it. The place was very carefully surveyed, and the project was estimated; but it was found that the cost of conveying the water into this basin would be so great that it was out of the question.

Attention was then turned to the possible sites where a stone dam might be built right across the river. The southern boundary of Egypt just now is near Wady Halfa, the second cataract. It is no use going to look for sites south of this, for the country is in the hands of the Mahdi and his fierce dervish soldiers. North of this point, unquestionably the best site—perhaps the only possible site—is where the Nile valley is traversed by a broad dyke of hard Syenite granite, in passing over which the river forms its first cataract just south of Assouan. It is here divided into several channels between rocky islands, and no channel is deep, so that it would be easy to divert the water from one after another, to lay bare the bed of the river, and lay the foundations of the dam in the open air. It wants no engineer to understand what an advantage this is.

And the great dam, such as was designed by Mr. Willcocks, would have been a work worthy of the land of the Pyramids and Karnak—a great wall of squared granite blocks—82 feet thick at base, of a maximum height of 115 feet, $1\frac{1}{4}$ miles long, pierced by sluices large enough to allow of the whole Nile at highest flood rushing through. The lake formed would have been 120 miles long. Would this not have been a work of some majesty to commemorate for ever the English rule in Egypt—a work one would have been proud to have had a hand in? But it was not to be. The Egyptian saw no objection to it. The money could have been found. But there was an insuperable obstacle created when, on the Island of Philæ, about 250 B.C., Ptolemy II. built a temple to Isis, on the site of older buildings long disappeared. Round this temple other buildings clustered, built by Greeks and Romans. Those of you who have not seen them, are probably familiar from pictures with the group of venerable buildings standing amidst palm trees on the rocky island, and reflected in the waters below.

Had Ptolemy only built his temple on the island of Elephantine, a few miles north, it would have been unaffected by the great dam, but Philæ is just to the south, or up-stream side of where the great dam must necessarily have come, and in consequence the island, with its temples, would be drowned for about six months every year. You probably remember the outburst of rage and indignation which the announcement of this proposed desecration created in London last summer. It was not to be tolerated that England should commit such vandalism. In vain it was answered that the place belonged to Egypt, not to England—that the Egyptian, who was to gain so much by the dam, cared absolutely nothing about Ptolemy and his temples—that he was prepared to pay a large price for a great work to benefit his country. What business was it of England to forbid him?

And it was not only the English who were indignant. For once, and only for once, I fear, since we occupied Egypt in 1882, was educated opinion in England and France at one. Both alike insisted that Philæ should not be drowned. Nor must I admit had all the engineers that were interested in the question the full courage of their opinions. While they longed to build the dam, and lamented the perverse fate that had put Philæ there, still they wished to spare Philæ—and their voice has prevailed. The majestic structure has been cut down 27 feet, and now will only be 88 feet high, and Philæ will stand henceforth in a lake, but will never be drowned.

Personally I accept the situation, for I never believed that it would be sacrificed. But yet as an engineer, I must sigh over the lost opportunity for England of making such a splendid reservoir. And as a friend to Egypt, I sigh still more that the country will not have such a splendid supply of water as would enable Upper Egypt to have the full benefits now possessed by Lower Egypt, and Lower Egypt to expand and flourish.

The reduced scheme will, however, be a great boon to the country, and I trust will now be put in hand without delay.

In 1884, when the expedition up the Nile was first being considered, I was asked by the general officer commanding in Egypt, whether I thought there was any possibility of the Mahdi diverting the river in the Soudan, and depriving Egypt of its water. The late Sir Samuel Baker was in Cairo at the time, and I consulted him as to whether he knew of any place in the Nile valley where during highest flood the water spills off to the right or left, towards the Red Sea or the Libyan Desert. He said he was sure there was no such place, and then I told the general it would be impossible for the Mahdi to divert the Nile. I was sure that with his savages he would never dam up the low supply until its surface attained the height of flood supply, and if even then during flood there was no spill channel, Egypt was safe enough.

But what the Mahdi could not do, a civilised people could do. A government official has no business to talk politics, and the Royal Institution is no place for politics; but I may be allowed to point out an evident enough fact, that the civilised possessor of the Upper Nile valley holds Egypt in his grasp.

At this moment the Italians are on the eastern edge of that valley —a nation, I must say, who have been consistently most friendly to us in Egypt. Supposing that they occupied Khartoum, the first thing they would naturally and very properly do would be to spread the waters of the Low Nile over the Soudan; and no nation in Europe understands irrigation so well. And what then would become of Egypt's cotton crops? They could only be secured by a series of the most costly dams over the river, and the fate of Philæ would surely be sealed. But more than this: a civilised nation on the Upper Nile would surely build regulating sluices across the outlet of the Victoria Nyanza, and control that great sea as Manchester controls Thirlmere. This would probably be an easy operation. Once done, the Nile supply would be in their hands; and if poor little Egypt had the bad luck to be at war with this people on the upper waters, they might flood them, or cut off their water supply at their pleasure.

Is it not evident, then, that the Nile from the Victoria Nyanza to the Mediterranean should be under one rule? That time is perhaps far off. I conclude what I have to say to-night by giving you the assurance, and I challenge contradiction, that at no time in the long history of Egypt under Pharaoh or Ptolemy, Roman or Arab, or Turk, have the people of the country been so prosperous, or so justly ruled as during the last nine years.

[C. S.-M.]

WEEKLY MEETING,

Friday Afternoon, February 1, 1895.

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

Henry Irving, Esq. D.Lit.

Acting: an Art.

My immediate purpose is not so much to deal with the existing classification of the Fine Arts as to add to the recognised number one other, the Art of Acting—that art which Voltaire spoke of as "the most beautiful, the most difficult, the most rare." The claim that I make is purely a technical one, for the thing itself has long ago been done. The great bulk of thinking—and unthinking—people accept Acting as one of the Arts; it is merely for a formal and official recognition of the fact that I ask. The people, who are the students of life, have learned their lesson, and perhaps the professors should now learn it also. In the face of the widespread influence of the stage of to-day and its place in the thoughts and hearts of the people, it would seem about as necessary to vindicate acting as an art as it would be to justify the existence of the air we breathe or the sunshine which makes life joyous; but when we find that up to now the records are deficient, we should, I think, endeavour to have them completed. Even so widely sympathetic a writer as Taine by inference excludes acting when he speaks of "the five great arts of poetry, sculpture, painting, architecture, and music"; and sometimes lesser minds than his use the general omission to classify acting as amongst the higher organized efforts of man, as a means of perpetually assailing this particular craft and those who follow it; recalling an eccentric and intolerant time, when it was said against Shakespeare that he had never been to Court, and against Molière's memory that his body had been denied full Christian burial. Official recognition of anything worthy is a good, or at least a useful thing. It is a part, and an important part, of the economy of the State; if it is not, of what use are titles and distinctions, names, ribbons, badges, offices, in fact all the titular and sumptuary ways of distinction? Systems and courts, titles and offices, have all their part in a complex and organised civilisation, and no man and no calling is particularly pleased at being compelled to remain outside a closed door.

Acting is a part of human nature. It is originally nature's own method of education in the earliest stages; and its purposeful organisation is like that of any other organisation—an Art. Out of their heightening civilisation the Greeks evolved and formulated a drama, and it was due to acting that they did so, for had there been no stage there had been no drama, for the efforts of their poets had been made in some other form and manifested in some other way. As I make a claim, I should like to justify it; and I shall therefore try to show from accepted sources that acting is in all ways and under all conditions within the bounds set down as the bounds of art; that it satisfies all conditions given; and that it has aims, purposes and objects in common with all the arts already classified. It would at the first seem that the single logical axiom "the whole is greater than its part" would be sufficient to prove to common satisfaction that systematic effort of a complex kind, which embraced all the less complex efforts already classified as arts, would be of necessity itself, an art. Let me, however, begin at the very beginning, and step by step prove that argument whose conclusions I venture to recommend to you—the place of acting amongst the cosmic arts.

Dr. Johnson, who certainly did not limit his definitions for the purposes of disputation, but made them as liberal and all-embracing as possible—and who had himself no high reverence for the playhouse, when he rather characteristically said to David Garrick, "Punch, sir, has no feelings"—defined "Art" as "the power of doing something not taught by nature and instinct"; that is, nature and instinct give power and can suggest, but art must teach how the power is to be used. Dr. Johnson also quotes South: "Properly, an habitual knowledge of certain rules and maxims by which a man is governed and directed in his actions." The philologists define the word as we have it as coming through the Latin from the Greek. In this language the root word means "to join" or "to fit"; so that broadly, the artist, in the original meaning of the word, comes under the definition of a skilled workman. Surely it is not too much to ask for the actor that he be placed within this category.

As the world progressed in power and knowledge, and as life became more complex, work became differentiated, and the terminology became enlarged; there became degrees in the skill required for the doing of work of many kinds, and there came a diminutive to the word "artist" to meet the want—the "artisan"—the artist-labourer rather than the fine-artist. Thus, by common consent, the term Artist became the distinctive right of those workers who wrought in the higher branches of their various crafts and callings. In our day, with its myriad manifestations of work, we have names for all grades generally applied—from the "hand" who works mechanically on the farm, on shipboard, or in the mill, to the "artist" who still maintains his position as the exponent of the highest organised effort—in fact, of what we call the "fine" arts. And it is to this category that I venture to affirm that acting belongs.

And here, before we consider what is the "differentia" or essential condition of a work of fine art, let me point out that art in any of its phases does not deal with the original conception or creation of ideas. The Greek language, whence we derived the word, had a completely different word to express the originator of ideas—the word creator, or maker—the poet, as we call him to this day. The functions of these two, the poet and the artist, are entirely different, and their work can be dissociated. Homer, the poet, conceived his thoughts and gave them utterance, but the fashion in which he moulded them in giving them birth was the work of art. When others disclaimed his verses so as to give forth their mighty roll and rhythm, the fashion of their speech was the work of art. When, later on, the sculptors modelled the forms of the gods and heroes, as Homer described them, translating the thoughts of the poet into graceful form, whose inner significance men could understand, this was the work of the artist too. It was no detraction from the merit of the work as work of art, that the sculptor set forth Homer's ideas and not his own. Nay, more, when a sculptor, when Homer's name was a great tradition, gave forth what appeared to be his image, was this less a work of art because it professed to represent a real man, and not a creation of the sculptor's mind?

What, then, is it which is in common with poetry, music, sculpture, painting?—It is the knowledge of the powers of nature, and the systematisation of them in such a way that effects may be recurrent as required. Hear Alexander Pope on the subject:—

> "These Rules of old discover'd not devis'd,
> Are Nature still, but Nature methodiz'd;
> Nature, like Liberty, is but restrain'd
> By the same laws which first herself ordain'd."

And again:—

> "All Nature is but Art unknown to thee."

If we, then, broadly define art as the systematisation of natural powers, wherein may we find the limitation of "fine" as applied to the arts? M. Taine, in his exhaustive treatise on *The Philosophy of Art*, says that if "we succeed in defining Nature, and in marking the conditions of existence of each art, we then possess a complete explanation of the Fine Arts, and of art in general, that is to say, a philosophy of the Fine Arts—what is called an *æsthetic* system." And he goes on to say that this æsthetic system—the science of the beautiful—"imposes no precepts, but ascertains and verifies laws." . . . That she "has sympathies for every form of art." . . . That "she accepts them as so many manifestations of human intelligence." He then proceeds:—"It is plain that a statue is meant to imitate accurately an animated human form, that a picture is intended to portray real persons in real attitudes, house interiors and landscape, such as nature provides. It is no less evident that a drama or romance attempts to represent faithfully characters, actions, and conversations, and to furnish as vivid and as accurate impressions of them as is possible."

He thus sums up his examination of the nature of a work of art:—"We have discovered a loftier aim for art, which thus becomes the work of intelligence, and no longer merely that of hand."

Here, then, we get some idea of that which constitutes a work of art as a work of "fine" art—intelligence on the part of the artist. Whereas the object of art generally and broadly is to imitate, to conform to a model, the object of fine art is to compose or select intelligently, to exercise that selective faculty which the Professor goes on to show is a higher function of nature as well as of man. And thus far it cannot be denied that acting, which certainly requires intelligence, still remains in the category of the Arts as thus limited.

But there is a fuller limitation set forth by M. Taine. It is not enough that the work imitate nature faithfully, and that the imitation be selective for particular purposes, and intelligent. It must not be content with faithfulness as to detail—it must grasp essential character. "In Nature," he says, "essential character is simply dominant; it is the aim of art to render it predominant. It moulds real objects, but it does not mould them completely. . . . Man is sensible of this deficiency, and to remove it he has invented art."

Surely this truth in Acting needs no defence! Nay, in the practice of the art it has at times grown to be an evil; for exaggeration of a type of good or evil—of passion—of emotion of any kind, has to be purposely avoided by judicious players, who realise that the expression of emotion must be complex, though it be dominated by one phase. Shakespeare, speaking in Hamlet's voice, himself pointed out the evil:—"In the very torrent, tempest, and, as I may say, whirlwind of passion you must acquire and beget a temperance that may give it smoothness." And thus far acting keeps well within the bounds of art as fixed.

But there is a still further limitation, for M. Taine, in leaving the work of art to consider the artist, says:—"There is one gift indispensable to all artists. . . . If it is wanting in them, they are nothing but copyists and mechanics. In confronting objects the artist must experience *original sensation*; the character of objects strikes him powerfully, and the result must be a strong, deep, personal impression."

"Look," says Polonius of the player, "look whether he has not turned his colour, and has tears in's eyes." Surely that which was taken by Shakespeare as typical of the poor Player of Wittenberg may be allowed to the cultured schools of England, of France, and of Germany.

Thus far we find applicable to acting the principles of art laid down by two philosophic critics—one a poet of the eighteenth century, imbued with all the culture and wisdom of classic lore, the translator of Homer, the familiar of the works of Plato and Aristotle; and the other the exponent of the modern scientific school of philosophy, a professor *par excellence* of the plastic arts. We find that what is essential to poetry, to music, to sculpture, to painting, is also essential to acting. Where, then, is the proof that acting is *not* one of the sister arts? What is there in it that disqualifies it from holding a place amongst them? To assert such a thing is to assume the attitude of Cinderella's sisters in the fairy tale. Let me offer a suggestion in the shape of a logical problem.

Hogarth painted a picture of David Garrick at a moment of his life and in such a way that all who ever saw him recognise the prototype of a certain historical character. No one denies—can deny—that this is a work of art. Now Shakespeare wrote a play in which Richard III. is a character. Can any one deny that this is a work of art? Garrick, in his playing, appeared on the stage in such wise that those who saw him knew that the man before them was the man Garrick, whilst at the same time he seemed by many signs and in many ways to be the image, copy—what you will—of Shakespeare's Richard III., though Garrick gave his Shakespeare adulterated with Cibber. Yet Garrick's work in producing this impression was, we are to be told, not a work of art. Why it was not so I leave those to say who assert that acting is not an art. But let me point out to such that they will have this difficulty to encounter—if Garrick's purposed labour was not the exercise of an art, what was it? If the product of such purposed labour was not a work of art, what was it? The poet Shakespeare conceived a thought, the artist Shakespeare worked it out into dramatic form—the actor Garrick translated the poet's thought, as given in the artist's words, into something which the public who saw and heard recognised. The painter Hogarth took the image which he saw—Shakespeare's idea and Garrick's form transmuted by something not an art to a visible and tangible shape—and fixed it on his canvas for future ages to see and admire. What, then, was it that broke the chain of intentional effort that led from Shakespeare's imagination to Hogarth's canvas? Where is the flaw in this intellectual lode? By what quip or crank of thought are we asked to deny that one alone of these varied steps in the crystallisation of a thought is not ruled by Art—and that one, man's intelligent use of the powers nature has given him? Which denial reminds one of the butcher who asked whether Edmund Kean spoke the character of Othello out of his own head or learnt it from a book; and on being told the state of the case, exclaimed against paying to hear a man repeat what every man able to read could do as well for himself.

The only reasons for acting not being an art that I have ever heard alleged, are that it is simply imitative or mimetic, that it does not create, that it does not last, and that it is not exercised with materials such as are used in the other arts. The second of these has been disposed of by the simplest examination of the word in its philological aspect. Let us examine the first. At the beginning I deny that the statement is correct. But even if it were true—true in the plainest and baldest way—this would not remove it from the category where for positive reasons we have placed it, and for negative reasons left it. All art is mimetic, and even M. Taine speaks of "the three imitative arts of sculpture, painting, and poetry." And I think that, as I shall try to show later, we may add music to the category. The function of art is to do, and not to create—it is to make to seem, and not to make to be, for to make to be is the creator's work.

Now as to this question of imitation. The artist wishes to produce—to produce what? Does the sculptor take his clay and the painter his brush and pigment and canvas, and the poet his pen and paper, and set to work to produce a vague something which will grow into the seeming of a real thing as he goes on? Such an idea is ridiculous. The artist intends, and must intend, to carry out a thought. It need not have originated in his own mind, but it must be there, howsoever begotten or received. To try to realise an image existing in one's own brain so that it may become apparent to the senses of others, is the work of all art; and it is because the outline in plastic art has to be exact—because its merit is judged by organs of mechanical accuracy, that it is necessary to reduce to exactness consonant with the realities of life, the vagueness of imagination, aided by the emotions. Inasmuch as words allow of greater complexity of thought than do tangible and visible things, so much freer is the poet or writer of any kind in the exercise of his art. When Shelley, in his *Prometheus Unbound*, describes Demogorgon "a tremendous gloom," he conveys an idea that cannot be conveyed adequately by any pictorial art. The blackest shadows of Rembrandt or Constable would be like sunshine beside the vague idea in the mind of the reader of these words who has an imagination to understand them. It is this necessity for exactness which compels the constant study of nature on the part of all artists. There can be no higher aim than to reproduce nature—nature shorn of such external accidents as would distract the mind of the spectator—nature epitomised and yielding her secret meaning. What is there in works of genius, howsoever they may be represented, which touches the heart with emotion? We feel it as we gaze on the beauty which Canova wrought in marble, which Raphael and Velasquez and Vandyke and Reynolds and Gainsborough depicted on canvas, which Michael Angelo piled up to the dome of St. Peter's—or as we listen to the tender strains of Mozart, the sad witchery of Mendelssohn, or the tempestuous force of Wagner. And yet the roots—the archetypes of all these—have lived, not perhaps in the cognate form in which they are known to us, but as elemental facts in which the skill and wisdom of man have garnered and treasured and used to these noble ends. The eyes of the sculptor and the painter beheld at some time the elements of the beauty which they reproduced. The architect found his ideals in the rising stems and sweeping branches of the forest aisles, or mayhap in the piling up of sunset clouds. And as to the music, every note of it is to be found in nature's choral forces—that mighty gamut of creation which rises from the tiniest whisper of whirring wings in the insect world, through the sighing of the night wind, the crackle of swaying corn, the roar of falling water, and the mighty voice of the sounding sea, up to the hiss of the lightning flash and the crash of the thunderbolt. Who, then, is the truest artist? He it is who best realises these myriad beauties and bounties of nature, and who best reproduces them so that others may understand the emotion which they have created in him; and beside these truths of nature all lower things must stand back abashed.

"Earth outgrows the mythic fancies
Sung beside her in her youth,
And these debonair romances
Sound but dull beside the truth.
Phœbus' chariot-course is run:
Look up, poets, to the sun!
"Truth is fair: should we forego it?
Can we sigh right for a wrong?
God Himself is the best Poet,
And the Real is His song.
Sing His truth out fair and full,
And secure His beautiful!"

So speaks the poet, Elizabeth Barrett Browning. And what says the master-poet, Shakespeare?—

"O'erstep not the modesty of nature; for anything so overdone is from the purpose of playing, whose end, both at the first and now, was and is, to hold, as 'twere, the mirror up to nature: to show virtue her own feature, scorn her own image and the very age and body of the time his form and pressure."

Perhaps I may here quote Talma's words on the actor's art, since they seem to illuminate, from an actor's standpoint, the applicability of all the rules which Taine has given:—

"The actor, in the first place, by repeated exercises, enters deeply into the emotions, and his speech acquires the accent proper to the personage he has to represent. This done, he goes to the theatre not only to give theatrical effect to his studies, but also to yield himself to the spontaneous flashes of his sensibility, and all the emotions which it involuntarily produces in him. What does he then do? In order that his inspirations may not be lost, his memory, in the silence of repose, recalls the accent of his voice, the expression of his features, his action—in a word, the spontaneous workings of his mind, which he had suffered to have free course, and, in effect, everything which, in the moments of his exultation, contributed to the effects he had produced. His intelligence then passes all these means in review, connecting them and fixing them in his memory to re-employ them at pleasure in succeeding representations. . . . By this kind of labour the intelligence accumulates and preserves all the creations of sensibility."

Let me supplement this with the words of a famous American critic of the past—Thomas R. Gould:—

"Not with his usual vision of the germs and processes of genius did Lamb write, that an actor is an imitator of the signs and terms of passion. An actor of the understanding, a *sensible* actor, indeed, always takes this method; an imaginative actor, never. One takes the words of the text, reasons upon, and *infers* the meaning, and so extracts the character. The result of this method, however carefully and comprehensively employed, is at best but an abstract induction, having something of the aspect of reality, but automatic, and without the breath of life. The other looks into a great creation, as if passing into a real presence; is filled and atmosphered by its spirit; listens to its language as to a living voice; is brought into intimate relations with the springs of its being, and conceives it in unity by the power of a brooding and recreative imagination.

"And unto this power—because 'it cometh not with observation,' but transcends the understanding, because it is vital and life-giving, and elevates Acting from a mimetic into an imaginative art, subordinating the comparative intellect to its higher and self-justified laws—we feel bound to give, with a considerate and responsible decision, the sacred name of 'Genius.'"

I have myself heard a great actress say when coming off the stage: "I could not act to-night!" Of course she had acted well—her art was too good to fail her—what she meant was that on that particular occasion she felt herself without inspiration or ecstacy—that force which must be behind every great personal effort.

Perhaps these examples may serve to point how the art of acting complies with Taine's idea as to the necessity of the artist experiencing "original sensation."

Now as to endurance of impression, is it to be seriously put forth by any one as an argument that art ceases to be art because its work does not endure? There are two questions here involved—first as to what is endurance, and second, what method of record does it require. The life of all things of the world is bounded by time, and the many accidents and disasters which are time's agents of destruction. Surely of all materials in which art can work, marble, and brick, and metal are the most enduring; and yet the works wrought in them pass away. With the Parthenon and the Colosseum in ruins, and the great temples of the gods obliterated; with the works of Praxiteles and Phidias almost unseen by any eye in their perfect beauty; with the wilderness of Benvenuto's marvels, mainly long ago reduced to chaos in the melting-pot; with Apelles a name, and even the names of the host of his compeers forgotten, who is to say that works of art need immortality in order that the labour to which they were due may be classed as art, or the labourers as artists? Where are now those mighty works of man's art which came to be known as the "Seven Wonders of the World"? Where are the Pyramids—mighty wrecks whereon Time has set his hand—the sole survivors of all the wonders of yore? Where is the mighty city Babylon, with its walls and temples and gardens?—gone! Where is the mighty statue of Olympian Jove, the triumph of Phidias?—gone! Where is the Temple of Diana?—gone! Where is the Mausoleum of Artemisia?—gone! Where is the Pharos of Alexandria?—gone! Where is the Colossus of Rhodes?—

gone! Aye, and gone with them millions of art-works by myriads of workers in countless ages—men now nameless, but once full of honour, and whose work was, and is, placed in the existing category of the arts.

So much for length of endurance; but what of its records? Are the works only to exist, and not the memory of them? Must a record be written or graven in iron or marble; or is it sufficient that it lives from mouth to mouth till the very cause of its memory be forgotten? Nay, more; we ourselves have only memories to help us in our daily life, for what is all education but organised memory? The efforts of plastic art which throughout our lives we have seen and loved are seldom before us, but take their place in memory, beside the fleeting visions which pass before us on the stage. Is not Roscius a name that lives in history, though he was neither poet, nor sculptor, nor painter, nor architect, nor musician? It would be foolish to say that a work is not a work of art because it has not permanent existence in material shape. If this be a condition of art, then on the destruction of a work the worker ceases to have been an artist. All things are comparative, and we ourselves, who have only the span of a few years to live, cannot claim immortality for our work.

As to the medium in which the actor works, I would seriously ask if there is any one so benighted as to put it forth as an argument. Was that lion which the sculptor wrought in butter for his patron's table less a work of art than the noble work of the dying lion hewn in the rock at Lucerne? Alas for the sculptor's storied pathos if this be so! for already time and weather have set their cankering hands upon the work. Was the shepherd lad's drawing on the rock, as he tended his sheep, less by its nature a work of art than when he painted altar pieces in the later years? Were the painters before the Van Eycks less artists than their successors because, in default of better mordants, they had to use unpleasant materials to fix their pigments? Why, it is the sculptor himself who works in the common clay, and it is his labourer who chisels out the marble statue, from the clay model of his master. Why, then, should it be any barrier to work being the work of art, because its elements are the most complex known to man, and the tools in use no mere work of the hands of man, but the noblest powers and qualities given by God—the power of sympathy, the force of passion, the earnestness of conscious effort?

The old professors have counted music amongst the arts. Let me ask them a few questions relating to it. Is the art confined to the composer, or is it shared by the interpreter? If the former, why is it not enough to print the score, and let men read for themselves; it would save much labour, much expense. Wherein, with regard to composition, is the limitation of art, since counterpoint is a science, and melody an inspiration? Was there no art in the interpretation of his score by Paganini, by Liszt, by Rubinstein—or is all the delicate and endless variety which an executant alone can give to pass as an artless labour? But if the term artist as applied to music be not a limitation to the composer, wherein does the interpreter of written music symbols, who can convey their meaning through quite another sense, differ from the actor, who is also an interpreter of written symbols, but of more infinite complexity, and with ever-varying hidden depth? If the actor's words and motions go forth upon the empty air artless, what becomes of the sweet vibrations of the musician's art? and if the interpreter of the composer's scrip be an artist, whosoever may be the medium of his creating the necessary vibrations by any work of man's hands, how much more artist is the singer who uses that most complete and capable instrument—the human voice. Grant the singer to be an artist, then where is the point of difference from the actor, who, also with endless modulations of voice, has to convey the myriad phases of thought and passion?

"The actor's effort is primarily to reproduce the ideas of the author's brain, to give them form, and substance, and colour, and life, so that those who behold the action of a play may, so far as can be effected, be lured into the fleeting belief that they behold reality."

Truly the actor's work embraces all the arts. He must first have the gift or faculty of acting—a power which is as much a gift as that of power to paint or to mould—and whose ordered or regulated expression is the function of art. His sympathy must then realise to himself the image in the poet's mind, and by the exercise of his art use his natural powers to the best advantage. His form and emotions are, in common with the sculptor's work, graceful and purposeful; his appearance and expression, heightened by costume and pictorial preparation, are in common with the work of the painter, and wrought in a certain degree by the same means and to the same ends: his speaking is in common with the efforts of the musician—to arouse the intelligence by the vibrations and modulations of organised sound. Was it by chance, or inspiration, or out of the experience of a life amongst the arts that the poet Campbell wrote:

"How ill can Poetry express
  Full many a tone of thought sublime;
And Painting, mute and motionless,
  Steals but a glance of time;

"But by the mighty Actor wrought
  Illusion's perfect triumph's come;
Verse ceases to be airy thought,
  And Sculpture to be dumb."

Acting may be evanescent, it may work in the media of common nature, it may be mimetic like the other arts, it may not create, any more than does the astronomer or the naturalist, but it can live, and can add to the sum of human knowledge, in the ever-varying study of man's nature by man, and its work can, like the six out of the seven wonders of the world, exist as a great memory.

[H. I.]

GENERAL MONTHLY MEETING,

Monday, February 4, 1895.

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

Lady Ashburton,
Lawrence Briant, Esq. F.C.S. F.I.C.
Henry Burton Buckley, Esq. Q.C.
Jeremiah Head, Esq. M. Inst. C.E.
Mrs. David Edward Hughes.
The Hon. Richard Clere Parsons, M.A.
Edward Stanley Mould Perowne, Esq.
Hugh Munro Ross, Esq. B.A.
Sir John Swinburne, Bart.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to the Smithsonian Institution for their Present of a Portrait of the late Mr. Thomas G. Hodgkins.

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

Professor Dewar (Rumford Prize)	..	£69 9*s.* 8*d.*

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

FROM

The Lords of the Admiralty—Nautical Almanac for 1898. 8vo.
The Secretary of State for India—Archæological Survey of India: New Imperial Series, Vols. XV. XVII. 4to. 1894.
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  Analyst for Dec. 1894 and Jan. 1895. 8vo.
  Athenæum for Dec. 1894 and Jan. 1895. 4to.
  Author for Dec. 1894 and Jan. 1895.
  Brewers' Journal for Dec. 1894 and Jan. 1895. 4to.
  Chemical News for Dec. 1894 and Jan. 1895. 4to.
  Chemist and Druggist for Dec. 1894 and Jan. 1895. 8vo.
  Electrical Engineer for Dec. 1894 and Jan. 1895. fol.
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  Machinery Market for Dec. 1894 and Jan. 1895. 8vo.
  Monist for Jan. 1895.
  Physical Review for Dec. 1894 and Jan. 1895. 8vo.
  Nature for Dec. 1894 and Jan. 1895. 4to.
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  Open Court for Dec. 1894 and Jan. 1895. 4to.
  Optician for Dec. 1894 and Jan. 1895. 8vo.
  Photographic Work for Dec. 1894 and Jan. 1895. 8vo.
  Scots Magazine for Dec. 1894 and Jan. 1895. 8vo.
  Technical World of Science and Art for Dec. 1894 and Jan. 1895.
  Transport for Dec. 1894 and Jan. 1895. fol.
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  Work for Dec. 1894 and Jan. 1895. 8vo.
  Zoophilist for Dec. 1894 and Jan. 1895. 4to.
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Microscopical Society, Royal—Journal, 1894, Part 6. 8vo.

Ministry of Public Works, Rome—Giornale del Genio Civile, 1894, Fasc. 10. 8vo. And Designi. fol.

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Richardson, Sir Benjamin Ward, M.D. F.R.S. M.R.I.—The Asclepiad, Vol. XI. No. 42. 8vo. 1894.

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Neujahrsblatt, XCVII.

WEEKLY EVENING MEETING,

Friday, February 8, 1895.

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

German Sims Woodhead, M.D.

The Antitoxic Serum Treatment of Diphtheria.

The subject with which we shall deal to-night, though at first sight of interest to the physician only, has been so fully discussed and so bitterly and irrationally opposed, perhaps also unreasonably belauded, that those who take even a general interest in the public health, or who are wishful to obtain some insight into the practical and scientific aspects of a new system of treatment, may well be interested to know something of what is being so freely written up in the columns of our daily newspapers. Beyond this, however, many take a more personal interest in a method of treatment which holds out promise of help in the cure or amelioration of the symptoms and conditions met with in diphtheria, a disease which, very justly, is looked upon as one of the most treacherous with which the physician has to deal. To begin with, I should like to make a frank confession. With that conservatism which is met with even in the most radical of natures, many, of whom I was one, felt disposed to treat antitoxic serum as belonging to the same group of substances as tuberculin, around which was constructed a theory of which the laboratory experimental basis, though apparently fair and firm, was as yet insufficient for the support of the structure of therapeutic treatment that was afterwards raised upon it. I followed the earlier experiments on this new method with great attention; I carefully analysed the principles on which the method was founded, and then with some misgivings watched the gradual development of the treatment as applied to actual cases of diphtheria. I was inclined to receive the statistics with great reserve, as I felt that this new method, like all new methods of treatment, might be making cures in the minds of the observer, and not on the bodies of the patients. Now, however, I am convinced that whatever justification my incredulity may have had from the consideration of previous experiments, none could be claimed in connection with the experiments that were carried out in the investigation of this special subject, and I am thoroughly satisfied that, although the antitoxic serum treatment may not come up to the expectations of all the rash writers on the subject—for many people seem to think that it should be a specific against diphtheria in all its stages—it promises, and this promise has in part been redeemed, to diminish the diphtheria case mortality in a very remarkable manner.

Diphtheria is primarily an inflammation of the mucous membrane (the moist skin) of the tonsils, of the soft palate, of the upper part of the gullet, and of the upper part of the windpipe. [Illustration shown.] During the course of this inflammation, which appears to be set up by the action of a special bacillus, there are usually thrown out some of the fluid elements of the blood and some of the white cells that float in the blood; these form a soft toughish layer or film which offers an excellent food and resting place for this bacillus of diphtheria, which under such favourable conditions secretes or manufactures a most virulent poison. This poison is rapidly absorbed into the blood and is carried to various parts of the body; its effects are evident at first only on the nervous system, but afterwards on the muscles.

First as to the bacillus. In 1875 Klebs described a short bacillus which he found on the surface of the greyish leather-like diphtheritic false membrane or film. [Illustration shown.] Following up these observations, Loeffler traced a definite etiological relationship between this bacillus and diphtheria. First he obtained pure cultures of the bacillus by growing it on solidified blood serum, or on a mixture of three parts of blood serum and one part of neutralised beef bouillon containing extract of beef, 1 per cent. of peptone, 0·5 per cent. of common salt, and 1 per cent. of grape sugar. This organism may be readily detached from the surface of the false membrane by pressing firmly but gently with a little bit of cotton wadding twisted round the end of an iron wire or an ordinary penholder. [Illustration shown.] When stained and examined under the microscope, the diphtheria bacilli are found to be small rods from 3 to 6 μ ($1\ \mu = \frac{1}{25000}$ of an inch) in length, fairly plump, straight, or slightly curved, sometimes wedge-shaped or pointed [Illustration shown], but usually somewhat enlarged and rounded at the ends, where also in stained specimens, the protoplasm is more deeply tinted than in the centre. This organism grows singly or in groups, or felted together to form a net-work; it may occur in irregular masses of considerable size. When these bacilli have been growing for some time on an artificial nutrient medium, they appear to be segmented, the stained material accumulating in small round nodules placed at intervals within a kind of membrane which is only very delicately tinted. During the past five weeks I have examined about 500 specimens taken from the throats of diphtheria patients, and I may say that in nearly every case where the disease has been diagnosed by the physician in charge, as being one of diphtheria, these typical bacilli have been found, whilst in those cases in which there was any doubt as to the nature of the disease, similar bacilli were found in some, but not in others.

This is of importance, because we shall find that this bacillus gives us the substance with which animals are rendered immune to the attacks of the bacillus itself, these immune animals in turn supplying the antitoxic serum. To prove that this bacillus is really the cause of the disease, Loeffler, in an elaborate series of experiments, inoculated the pure cultures of the bacillus grown on artificially prepared media, into animals; he was thus able to set up characteristic lesions, especially if he took the preliminary precaution to abrade slightly the mucous membrane, thus, as it were, ploughing the ground before scattering the seed. On such abraded surfaces the bacilli grew very luxuriantly, and false membranes were produced; in these lesions the bacilli could afterwards be found and again separated in pure cultures, whilst the characteristic toxic symptoms of diphtheria were, in each case experimented upon, repeated with the utmost fidelity. Loeffler also pointed out a most important fact in connection with the presence of the organism in the body. He found that it was strictly confined to the local wounds or lesions in the throat and posterior part of the nose, and he was also able to prove that in this position these organisms commenced to manufacture most virulent poisons, which, unlike the bacilli, can become diffused throughout the body. Klein and Sydney Martin in this country have both made very valuable contributions to our knowledge, the former concerning the bacteriology of the disease, the latter in regard to the chemical action on the tissues of the toxic or poisonous products of the bacillus.

Martin found that after the poison formed in the throat has made its way into the internal organs of the body it undergoes certain changes; it is broken down into somewhat less poisonous compounds, but these, accumulating at certain points, act especially on the nerves and muscles. It appears then that we have to deal with two sets of poisons: a very virulent poison formed by the bacilli directly from the fibrin and albuminoids of the fluids of the blood, exuded on the surface of the mucous membrane; and secondly, a less poisonous series which appear to accumulate especially in the spleen. So long as these poisons remain in the body we have the general fever, rise of temperature, and altered conditions of circulation (as evidenced by the pulse), so characteristic of the disease. At a later stage, sometimes after all the primary symptoms of diphtheria have passed away, there are often met with what are called post-diphtheritic paralyses, which are due apparently to alterations in the nerves going to muscles, especially those going to the delicate muscles of the soft palate and around the opening into the windpipe, though other groups of nerves and muscles may be similarly affected. These post-diphtheritic paralyses may be due then to the action either of the virulent poison (ferment) formed in the membrane, or of the somewhat less poisonous, but more stable toxines that are formed in the later stages of the disease. Through the kindness of Dr. Martin I am enabled to show you figures of nerves and muscles, the degeneration of which is due to the action of these poisonous substances [Illustration shown]. It is here unnecessary to enter into any detail as to the minute changes that take place in the nerve and muscle fibres, but on comparison of the affected nerve fibres with a healthy nerve fibre, it is evident that we have here grave structural alterations which must interfere most materially with the power of the nerve to conduct nerve impressions from the spinal cord to the muscle. The outer part or sheath of the nerve is in some places entirely wanting, whilst in other cases the axis cylinder or core of the nerve is either greatly attenuated or entirely absent. The poison in these cases has set up changes by which the communicating paths between the muscles and the spinal cord and brain have become thoroughly disorganised. The muscles, too, instead of being formed of cleanly striated fibres, have this striation greatly obscured, first by a kind of cloudy or ground-glass look, and later by the appearance of a number of strongly refractile granules. These, when stained with osmic acid, become black, from which we argue that they are composed of fat, and it is said that the muscle has undergone a fatty degeneration, the muscular protoplasm being partially converted into fat; ultimately the striation may be almost lost. In a case of diphtheria, then, the following stages may be traced: a sore throat (often simple enough to begin with), by which the mucous membrane is prepared for the reception of the diphtheria bacillus. The diphtheria bacillus becoming implanted on this surface, gives rise to an acute inflammatory condition, and, subsisting on the inflammatory exudate, sets up a local manufactory of a most virulent poison. This poison, absorbed into the circulation, at once acts on the nervous system, although a certain proportion seems to be broken down into a more stable, but less virulent, poison, which remains in the body, and may continue to act for a considerable time on the nerves and muscles.

Whilst these poisons are attacking the more highly organised, and therefore less stable tissues, they are stirring up or stimulating the other tissues of the body to resist their invasion and action. If this were not the case, any one attacked by diphtheria must eventually succumb to the disease; but we know that a considerable proportion of the cases of diphtheria recover even when no treatment at all is resorted to. Whatever may be the exact explanation of this recovery, we know that it depends upon the power of certain cells in the body to accommodate themselves to the presence of the toxines, and to go on doing their work of scavenging and of removing foreign substances from the body even under what originally were adverse conditions; during this process the cells become so profoundly and permanently altered that the patient is for some time protected against further attacks of the same disease. It was originally maintained that this alteration was entirely confined to the cells, but it is now generally accepted that these cells form or secrete substances which, thrown into the blood, either act directly upon the toxines so as to interfere with their activity, or so react upon the cells that they are able to continue their work in the presence of the toxine. At all events, a certain immunity against the disease is acquired. Upon these various theories is based the *rationale* of the antitoxic serum treatment of diphtheria. Ferrán claims to have been the first to obtain such a condition of immunity against diphtheria in animals; shortly afterwards, Fraenkel in Germany obtained similar results. Seeing that this immunity depends upon an alteration in the composition of the serum, should it not be possible, argued Professor Behring, to take the serum of an immunised animal and transfer it to a patient suffering from diphtheria, so as to help the tissues and cells of the patient to cope with the toxic products of the diphtheria bacillus during the earlier stages of the disease, inducing, as it were, a kind of artificial immunity to help the patient over the acute period of the attack when the poisons, though most virulent, are most unstable, and when the tissues have not yet become acclimatised to the presence of the toxic products of the bacillus; when, in fact, they are paralysed and are able to do little to protect themselves. Behring so followed up this idea, that he was able to initiate a system of treatment which promises to revolutionise our therapeutic methods in the treatment of certain specific infective diseases.

Working on the fact that an animal might be rendered more and more insusceptible to the action of the toxic products of bacteria, Behring found that he might proceed in either of two ways. He might make an artificial wound with a needle, and introduce weakened bacilli into the animal, the weakened bacilli then growing but feebly and producing a modified toxine. After the effects of the first dose had passed off, he was enabled to increase the dose and to use more active bacilli, injecting them first into the tissues and eventually directly into the circulation, with the result that enormous doses of virulent diphtheria bacilli might ultimately be introduced without giving rise to more local swelling or general febrile disturbance than was first noticed when the small dose of modified bacilli was introduced. Such a method as this, however, was attended with considerable drawbacks, as it was almost impossible to gauge, at all accurately, the number and strength of the bacilli. Not so, however, with the products of the micro-organisms the activity of which could, of course, be more accurately measured, and the dose more exactly graduated. The bacilli might multiply and continue their action on the tissues, but the poisons when injected alone would not alter in quantity or activity. As may be readily imagined, the fluid constituents of the blood can only contain those substances that are introduced into it from without, either through the vital activity of the cells of the body, the products of which must be thrown into this fluid before they can be excreted, or through artificial injection. The antitoxic substances, then, found in the blood of an immunised animal, must in the case of natural immunity following an attack of diphtheria be the result of the activity of the tissue cells, especially of the connective tissue and white blood cell groups which have been "stimulated" by the toxines introduced from without, from the false membrane in the throat. Where it is desired to produce an artificial immunity, and an "artificial" antitoxic serum, the cells are stimulated by the introduction into the body of artificially prepared toxine. The cells acted upon by the toxine elaborate the protective fluid, which is thrown into and accumulates in the blood. This substance may act in one of several, or even in several ways. (1) It may directly antagonise the diphtheria toxine, and may thus prevent the paralysing action of these poisons on the scavenging cells or phagocytes, as they are called: these, left free to perform their proper functions, can deal with the foreign elements that have got into the blood, and also with the bacilli at the seat of the local attack, for, as has been pointed out by several foreign observers, and by Ruffer in this country, immediately beneath the layer of bacilli in the false membrane there is usually a very considerable accumulation of leucocytes, especially in those cases in which recovery ultimately takes place. (2) The antitoxic substances may act on the bacilli, inhibiting their growth and interfering with their power of producing toxines. This, of course, can only be a local action should it play any part in the process. (3) These substances may act directly on the cells of the blood, lymph, and tissues, so stimulating and strengthening them that they are able to perform those functions above mentioned. It is at present difficult to state which of these processes is the one, or the most important, in protecting or curing the patient, and it may be that all play a part. It may be that the tissue cells, when acted upon by the specific diphtheria poison, become so modified that they are enabled to produce or secrete a substance which directly antagonises the action of that poison. This substance, thrown into the blood, remains there for some time, rapidly accumulates as larger and larger doses of the poison are thrown in, neutralising the poison, whose power of doing damage to the tissues is thus held in check, but remaining for some time after the toxine has disappeared; or this antitoxic substance, reacting upon the cells, may render them less susceptible to the action of the toxine.

The earlier immunising experiments were naturally performed upon the smaller animals, such as rabbits. Then Behring used sheep, and after various other animals had been tried, the horse was selected by Roux and Nocard, and Aronson, as perhaps the best of all animals from which to obtain antitoxic serum. In the first place, he is comparatively insusceptible to the action of the diphtheria bacillus—even comparatively large doses of living bacilli may be injected under the skin without producing anything more than a slight local swelling and a rise of temperature. It has also been found that horse serum, when injected, produces little or no change in the healthy human subject—that is, the serum seems to mix perfectly well with human blood plasma, and there is comparatively little danger of the extra serum being excreted by the kidneys in the form of albumen. This is a most important point, and one that no doubt influenced Roux and Nocard in their selection of the horse as an animal from which to obtain immunised serum. Beyond this, however, the blood, when drawn from the vessels, separates very perfectly into two portions—a firm clot, which if the blood be caught in a cylindrical glass jar, forms a kind of column in the centre, and a clear straw-coloured serum which accumulates around the clot, and forms a layer often several inches deep above it. [Illustration shown.] This serum contains the antitoxic substances. Lastly, considerable quantities of blood can be obtained from such a large animal as the horse, and if he be well fed, groomed and exercised, the process of bleeding may be repeated pretty frequently without causing any inconvenience to the animal: in fact, he stands bleeding as well as did our forefathers, who thought as little of being bled as we think of going to Aix or Buxton.

Let us now turn for a moment to the method of treating the horses that we wish to render immune, in order that they may supply the antitoxic serum that is to be used for the treatment of cases of diphtheria. Roux's method, which is that that has been most carefully described, and which is the one used in this country first by Dr. Ruffer at the British Institute of Preventive Medicine, and then by Professor McFadyean at the Royal Veterinary College, consists in introducing diphtheria toxine of a given strength in gradually increasing doses, until the blood of the animal so injected is found to contain a sufficient quantity of the antitoxine.

The toxine with which the animal is to be injected is made as follows:—A broth is prepared by soaking a pound of finely-minced beef in water. This is allowed to stand for twenty-four hours in the cold. To the fluid expressed from the meat fibre at the end of that time is added ½ per cent. of common salt and 2 per cent. of peptone (meat artificially digested by pepsine). This broth is then rendered faintly alkaline by the addition of soda salts or caustic soda. This is done because it is found that the diphtheria bacillus cannot grow at all vigorously, or form its poisons rapidly in an acid solution, and such poison as is formed is neutralised, or is unable to act in the presence of even a faint trace of acid. It is found that even in Roux's solution, which is always faintly alkaline to begin with, an acid reaction soon appears, but, after about ten days, this is replaced by an alkaline reaction, and as soon as this takes place, the growth of the bacilli takes on new activity, the quantity of toxine is increased, and it becomes much more virulent. Roux found that he obtained his most virulent toxines after three weeks' or a month's growth. If the growth is allowed to go on longer than this, a process of oxidation appears to take place, and I have found that the toxine from a culture carried on for two months had already lost much of its toxic activity. It should be noted that a virulent bacillus should always be taken in the first instance, otherwise the results may be very disappointing.

This nutrient broth is placed in a layer of not more than half an inch thick in a flat-bottomed flask, which is plugged with cotton wadding, and then closed with an indiarubber cork or cap. [Illustration shown.] Through this composite plug three tubes are passed into the flask; the two lateral tubes are bent at right angles, both inside and outside the flask; whilst the centre tube is fitted with a small thistle-head, which may be plugged with cotton wadding, and then closed with an indiarubber cap. The outlets of the lateral tubes are also plugged with cotton wadding, and the whole apparatus is kept for an hour or two in steam maintained at a temperature of 100° C. (Flasks so treated may be preserved for years without any change, beyond some slight evaporation, taking place in the broth.) A small quantity of a pure broth culture of the virulent diphtheria bacillus is now drawn into a long thin pipette, the indiarubber cap and the cotton wadding plug are removed from the thistle-head, and the contents of the pipette are introduced; the pipette is withdrawn, the cotton wadding is replaced, the indiarubber cap is fitted in position, and the flask is placed in an incubator which is maintained at the temperature of the body (98°·4 F., or 38°·2 C.), or better still a degree or two below this. As soon as the growth is well started (usually at the end of about 24 hours), a current of moist air is made to pass continuously over the surface of this cultivating fluid, the air being first warmed and saturated with moisture, in order as far as possible to prevent evaporation. A fine flocculent deposit soon makes its appearance on the bottom of the vessel, the supernatant fluid remaining clear. This deposit increases in thickness, much more luxuriant growth going on after the first ten days. Toxine is formed by the diphtheria bacilli so long as they can grow freely—that is, so long as they can obtain sufficient nutrient material from the fluid and from the air that is continually passing over the surface. At the end of three weeks, or even less, if all these precautions are taken, the toxine should be of such a strength that $\frac{1}{10}$ of a c.c. (about two or three drops) injected into a guinea-pig weighing 500 grammes (over 17 ounces) will kill it within 48 hours. The strength of the toxine or poison may be a little greater or a little less than this, but it is a comparatively easy matter to measure the strength, and therefore to graduate the dose to be used in immunising the horse. This only holds good, however, if the active diphtheria bacilli are removed or destroyed; these, if left in the fluid, would be a complicating and inconstant factor in the equation. In order to kill these bacilli the Germans recommend the addition of $\frac{1}{2}$ per cent. of carbolic acid to the culture; the dead bacilli falling to the bottom, leave a perfectly clear supernatant fluid. The French, on the other hand, recommend the separation of the bacilli from the fluid by means of a Pasteur-Chamberland filter. By this means a clear virulent poison which does not contain any diphtheria bacilli is obtained. With this fluid, a horse with a good constitution, and which has been proved to be free from tubercle and glanders, is injected under the skin of the side of the neck in front of the shoulder. Small doses are first injected, either pure or with the addition of $\frac{1}{3}$ of the volume of weak solution of iodide of potassium. If the fluid is of full strength, only about

1 or 2 c.c. can be given at the first injection. This is followed within 24 hours by a local swelling at the seat of injection, about the size of the palm of the hand, and the temperature may rise 1° or 2° F. ($\frac{1}{2}$° to 1° C.), otherwise the general health of the horse does not seem to suffer. He eats well, and unless regularly exercised may become very lively; of this we have had ample evidence during the recent frost and snow, when it has been unsafe to give much exercise to horses that are not very sound in limb, and as a result they have been very fresh indeed. As soon as the swelling has disappeared and the temperature has receded to the original level, a somewhat larger dose is given; the same process is repeated time after time (the dose being gradually increased to bring about the same amount of swelling and rise of temperature) for about three months, or until such time as the requisite amount of immunity is acquired, i. e. until the antitoxic action of the blood is sufficiently marked. That there is a gradually increasing immunity is evidenced by the fact that enormously large doses of the toxine in the later stages of the treatment produce even less local and constitutional disturbance than was observed after the first few injections of comparatively small quantities.

The blood is now drawn off from the jugular vein of the immunised horse by means of a metal cannula or tube to which is attached an indiarubber tube; these are first thoroughly boiled, in order that no living micro-organisms of any kind may remain on or in them, and the skin of the horse is carefully cleansed with some antiseptic lotion. The indiarubber tube leads the blood into a carefully sterilised flask or vessel provided with a double paper cap, a well-fitting cotton-wadding plug or a glass-stopper. The vessel when filled is placed in an ice-safe until the solid part, the clot, is completely separated from the fluid—the serum. From each gallon of blood about 1$\frac{1}{2}$ to 2 quarts of serum is expressed, though this varies considerably in different cases, and according to the time that the separation is allowed to continue (24 to 48 hours). This serum, a limpid straw-coloured fluid, is carefully decanted under strict antiseptic precautions, and, mixed with carbolic acid or camphor, is stored in small phials, each of which contains about a sufficient quantity for the treatment of a single patient. [Illustration shown.] In the Pasteur Institute, and in the British Institute of Preventive Medicine, the antitoxic serum is apparently brought up to such a strength that $\frac{1}{100}$ of a c.c. injected into a medium-sized guinea-pig (500 grammes, or over 17 ounces) will protect it against an injection 24 hours later of $\frac{1}{2}$ c.c. of a culture of living diphtheria bacilli strong enough, if given by itself, to kill the guinea-pig in 24 hours. It is usually recommended that 20 c.c. of this serum should be given at the first dose, and that if necessary a second 10 c.c. should be given half an hour later.

The method of testing the strength of the serum adopted by the Germans is that devised by Ehrlich, who takes ten times the lethal dose of diphtheria toxine, and in a test-tube adds a definite and known quantity of the blood to be tested. This mixture is then injected into a guinea-pig, and if the antitoxic power of the blood has been gauged aright, the animal does not suffer in the slightest degree from what under ordinary circumstances would kill ten guinea-pigs. The addition of less or weaker serum, or of more toxine, would leave the mixture still toxic.

In order to obtain a definite standard with which to compare the antitoxic power of any serum, and to determine the dose of such serum, Behring and Ehrlich have described what they term a normal antitoxic serum—that is, a serum of such a strength that $\frac{1}{10}$ of a c.c. added to ten times the lethal dose of diphtheria toxine is exactly sufficient to render it innocuous, 1 c.c. of such normal serum contains one "immunisation unit," and should be sufficient, when added to a hundred times the lethal dose and injected, to render it innocuous. In horses wholly immunised the serum may be fifty or even a hundred times as active as the normal serum above mentioned, and the dose to be given varies according to the number of immunisation units in any sample. It is not here necessary to go into the question of dose, but it may be stated that 500 of these immunisation units are usually necessary to produce the desired effects in cases of diphtheria, though in some cases still larger quantities have to be used. Behring now supplies four strengths of the serum, the weakest (marked with a yellow label) is sent out for injection of cases where the disease has not already been contracted. The next (marked with a green label) is of a strength of 600 antitoxine units, and is given to those cases in which the treatment is commenced at the very outset of the disease —that is, when the first symptoms of diphtheria manifest themselves. The next stronger antitoxic serum (white label) equals 1000 antitoxine units, and is used for cases somewhat more advanced in which the prognosis is at all grave; whilst in still graver cases, and where the symptoms have been developed for some considerable time, it is often necessary to give a serum of 1500 units; this is marked with a red label, and is, of course, highly concentrated in order that the size of the dose may not be unduly increased. In place of No. 1, healthy children and adults who are exposed to diphtheritic infection may receive a quarter of the dose of the green label flask, which Behring considers will protect against diphtheria with very great certainty. Although these general directions are laid down, it is strongly insisted upon by Behring, Kossel, Roux, and in fact by all those who have had experience of antitoxic serum, that the dose must vary according to the severity of the disease, so that much must be left to the discretion of the medical practitioner in charge of the patient. The great error into which those who first use this agent fall, is the administration of far too small a dose, especially in the case of children, for whom the dose is nearly as large as it is for adults. For this reason some of the statistics published in this country and abroad are far too unfavourable to the method. The great drawback of this method is that the dose necessary to be injected is so large; but in the loose tissue of the side of the chest, the back, or the buttock, immediately under the skin, the fluid soon disappears. It is hoped that before long, however, the active principle may be separated, and so obtained in smaller bulk.

So far we have dealt principally with the antitoxic serum as prepared by Behring and Roux and by Roux's method, which is certainly attended with comparatively few difficulties; these, however, have the disadvantage that they take from three to six months to give the desired results. In order to do away with this disadvantage, Klein has carried out a series of experiments in which he has been able to obtain serum of considerable activity in as short a period as 23 days. Instead of introducing the poison only, he adopts the plan used by Behring and Roux in their earlier experiments, of injecting living bacilli which have lost a certain degree of their activity, using for this purpose old cultures. He afterwards introduces toxine along with more virulent bacilli, and thus obtains in the animal such a degree of immunity that it is enabled to withstand, or to react very slightly to more than, a fatal dose of diphtheria bacilli. By the third week the animal will bear the injection of large quantities of virulent bacilli, and by the end of 23 or 26 days the serum has acquired such antitoxic properties that 1 c.c. of it will protect 40 to 80 guinea-pigs against a lethal dose of living diphtheria bacilli. It is difficult to compare these results with those obtained by Roux and Behring, but Klein's serum has been used with marked success in certain cases of diphtheria. It appears to have a special power of causing the membrane to clear away, and so to remove the manufactory of the poison, as on this membrane the diphtheria bacilli accumulate. This method is mentioned as one that may be used especially where it is desired to obtain antitoxic serum quickly.

Smyrnow has suggested quite a different method of preparing antitoxine. Under Nencki's advice he passed electric currents through the serum of animals, and was thus able to endow it with a certain immunising power. But he was still more successful in obtaining powerful antitoxine by electrolysing diphtheria bouillon cultures; curiously enough, the more virulent the culture the more powerful was the antitoxic substance he obtained. When this antitoxic substance was injected into a rabbit, which 24 hours before had received about ½ c.c. of a two or three days old diphtheria bouillon culture, there was a rapid rise of temperature followed by marked improvement in the condition of the animal. This observer believes that antitoxine can be obtained by this method that will be much more suitable for the treatment of the human subject than that obtained by the ordinary methods. His experiments, however, are far too few to carry any great weight, though they open up a most interesting field for future investigation.

Assuming now that the antitoxic serum is available, how is it to be used? It has been strongly recommended that it should be used not only as a curative or direct therapeutic agent, but also as a prophylactic—that is, as a protective agent against possible infection, especially during epidemics of diphtheria. It is almost too soon to consider this prophylactic property of antitoxic serum, as for some time to come the energy of those engaged in the preparation and use of this serum must be directed towards obtaining a sufficient supply for the treatment of cases of developed diphtheria.

It may be well to consider what have been the results obtained up to the present, and for this purpose the statistical method will probably carry most conviction, especially if it is possible to give full and accurate detail; and now that these statistics have been criticised not only by those who have used this treatment, but also by those who oppose it because it runs counter to their feelings and ideas, they are every day more and more trustworthy, much fuller, and more valuable.

It is first necessary to determine the average case mortality in diphtheria for some considerable period before the antitoxic treatment was introduced; then to see what has been the lowest case mortality during an equal and similar period for which we have any statistics; and lastly, to compare these with the case mortality of the period during which the antitoxic serum has been used.

In Table I. are given the mean annual death rates from diphtheria per million living in England and Wales and in London, in four periods of three years each.

TABLE I.

	1881–3.	1884–6.	1887–9.	1890–2.
England and Wales	144	166	173	192
London	213	227	315	377

Dr. Sykes gives the following statistics:—During the year 1892 there were 1962 deaths from diphtheria in London, whilst in 1893 there were 3265, or nearly twice as many deaths.

Now let us see what has been the case mortality. Statistics after correction give the following results. During 1893 there were 13,694 cases of diphtheria notified in London. The mortality amongst these cases was 3195 (*Lancet* statistics corrected), or 23·3 per cent.

Table II. gives further information, and enables us to see what is the diphtheria case mortality in large well-found hospitals.

In Table III. are given statistics dealing with the diphtheria case mortality where the serum treatment has been used. Wherever possible, the case mortality over a considerable period is given in the last column of the table, for purposes of comparison.

It is objected, however, that general statistics of this kind are of comparatively little value unless the age of the patient treated

TABLE II.

METROPOLITAN ASYLUMS BOARD: ADMISSIONS AND CASE MORTALITY, DIPHTHERIA, 1888–93.

Year.	No. of admissions.	No. of deaths.	Percentage of case mortality.
1888	99	46	46·4
1889	722	275	38·0
1890	942	316	33·5
1891	1312	397	30·2
1892	2009	583	29·0
1893	2848	865	30·3

Note.—Diphtheria cases have only been admitted into the hospitals since October 23, 1883.

is given. In order to determine the foundation upon which this certainly very legitimate objection is based, I have taken four series of cases as reported, and have placed them side by side. The percentages of deaths at certain ages in the London Asylums Board hospitals before the serum treatment are given in Table IV., the percentages of deaths of four observers who have used the serum, in Tables V. and VI.

It is very important, however, that the period of the disease at which the treatment is commenced should be taken into account, for, as already indicated, experience has taught that the later the stages of the disease at which this serum is injected, the stronger must be the dose given. It is necessary, therefore, to separate the cases in which the treatment is commenced at an early period from those in which it is commenced only when the poison has had time to disorganise the tissues, and to render them incapable of reacting to the antitoxic serum.

Table VII., p. 448, given by Kossel, brings out the great importance of this element in keeping down the case mortality. In the first column is given the day of the illness on which antitoxic serum was first injected.

For statistical purposes, too, only those cases which have been bacteriologically examined and found to be due to the action of Loeffler's diphtheria bacillus should be accepted as being cases of true diphtheria. As most of the cases in which the diphtheria bacilli are absent run a much milder course, and are much more amenable to general treatment, and as many of these have been included under diphtheria in the old statistics, such elimination will necessarily make the record tell rather against the antitoxic serum treatment than in its favour.

From a somewhat extended experience (although condensed into a very short period of time) I am satisfied that this question of the Loeffler bacillus is most important, and that every case in which the serum is used should be bacteriologically examined.

TABLE III.

		Number of cases.	Number of deaths.	Percentage of mortality.	Percentage of previous mortality.
GERMANY, AUSTRIA, HOLLAND :—					
*Kossel (up to May 1894) ..	Berlin	233	54	23·0	34·7
*Kossel (March 15 to December 1, 1894)	„	117	13	11·1	
Bokai	Buda-Pesth	35	5	14·2	53·8
Heubner	Berlin	96	37	38·5	62·5
Katz	„	128	17	13·2	38·9
*Aronson	„ &c.	255	31	12·1	32·5-41·7
Körte	„	121	40	33·1	53·8
Ranke	Munich	19	4	21·0	49·2
*Weibgen	Berlin	65	18	28·0	40·0
Börger	Greifswald	30	2	6·6	20
Kuntzen	Oscherleben	25	3	12	
Hager		25	1	4	
Möller	Magdeburg	76		39·6	55·6
Sonnenberg	Berlin	107	22	20·6	27·6
*Bagnisky (quoted by Virchow)	„	303		13·2	47·8
*Hahn	„	205	49	24	40·0
Wiederhofer	Vienna	100	24	24	52·6
	Trieste	252	45	17·8	43·8
Schüler		32	none	0·0	
Strahlmann		100	„	0·0	
Rumpf	Hamburg	26		8·0	12·0
Blumenfeld	Austria	50	2	4·0	38·0
Heim	Vienna	27	6	22·2	52·5
Gnändinger	„	27	11	40·7	
Monti	„	25	1	4	
Unterholzner	„	31	8	25·8	66·6
Ganghofner	Prague	110	14	12·7	49
Other observers		39	4	10·2	
FRANCE, ITALY, BELGIUM, SWITZERLAND :—					
Roux, Martin, and Chaillou ..	Paris	448	109	24·5	51·7
Moizard	„	231	34	14·7	50·0*
Lebreton	„	242	28	11·5	
Rabot	Lyons	47	16	34·0	50·0
Mya	Florence	17	2	11·7	
Massei	Naples	4	none	0·0	
Charon	Belgium	13	4	30·7	
Seitz	Constance	27	1	3·7	
AMERICA :—					
White	New York	32	8	25·0	42·7
Muehleck	Philadelphia	2	0		Two not treated died
Welch	Baltimore	5	1	20	
Catlin		1	0		
GREAT BRITAIN :—					
Cases reported in the *Lancet* and *British Medical Journal*		123	22	17	Average for London. 23·3
Washbourn, Goodall, and Card		72	14	19·4	Average for Hospital. 38·8

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* There is probably some overlapping, especially in the Berlin figures. This fact must be taken into account in dealing with this table as a whole.

TABLE IV.

SHOWING THE MORTALITY AT VARIOUS AGES FROM DIPHTHERIA ADMITTED INTO THE METROPOLITAN ASYLUMS BOARD'S HOSPITALS IN THE YEARS 1888–93.

Ages.	Cases admitted.	Died.	Mortality per cent.
Under 1	146	102	69·9
1 to 2	447	291	65·1
2 to 3	639	388	60·7
3 to 4	826	416	50·4
4 to 5	913	400	43·8
Totals under 5	2971	1597	53·8
5 to 10 .. .. ., ..	2462	705	28·6
10 to 15	885	93	10·5

TABLE V.

SHOWING MORTALITY FROM DIPHTHERIA AT VARIOUS AGES.

	Kossel.			Wiederhofer.			Goodall.			Total.		
	Treated.	Died.	Per cent.	Treated.	Died.	Per cent.	Treated.	Died.	Per cent.	Treated.	Died.	Per cent.
Under 1 year	3	1	33·3	8	5	62·5	4	1	25·0	15	7	46·6
1–2 years ..	4	0	0·0	24	9	37·5	10	2	20·0	38	11	28·9
2–3 " ..	18	2	11·1	20	7	35·0	7	1	14·3	45	10	22·2
3–4 " ..	14	3	21·4	14	0	0·0	9	3	33·3	37	6	16·2
4–5 " ..	20	3	15·5	16	3	18·7	10	5	50·0	46	11	23·9
Total under 5	59	9	15·2	82	24	29·2	40	12	30·0	181	45	24·3
5–10 years ..	45	3	6·6	15	0	0·0	22	2	9·1	82	5	6·0
10–15 " ..	13*	1	7·7	3	0	0·0	10	0	0·0	26	1	3·8
Grand totals..	117	13	11·1	100	24	24	72	14	19·4	289	51	17·6

* None of these were more than 13 years of age.

TABLE VI.

BAGINSKY (QUOTED BY VIRCHOW).

	Without serum treatment.			With serum treatment.		
	Treated.	Died.	Per cent.	Treated.	Died.	Per cent.
0-2 years	33	23	69·7	34	8	23·5
2–4 "	56	37	66·1	82	16	19·5
4–6 "	50	27	54·0	81	7	8·6
6–8 "	44	15	34·1	46	5	10·9
8–10 "	24	7	29·2	30	3	10·0
10–12 "	14	1	7·1	18	0	0·0
12–14 "	9	0	0·0	12	1	8·3
	230	110	47·8	303	40	13·2

TABLE VII.

Day of illness.	Treated.	Died.	Percentage.
I.	14	0	0·0
II.	30	1	3·3
III.	29	0	0·0
IV.	9	1	11·1
V.	11	2	18·1
VI.	6	3	50·0
VII.	5	2	40·0
VIII.	6	2	33·3
IX.	1	1	100·0
Unknown	6	1	16·6
	117	13	11·1

It has been said, however, and said very truly, that statistics may be made to prove anything, and I have heard it said that the observation of a few cases of diphtheria under the antitoxic treatment is worth all the statistics that could be brought together for the purpose of convincing a man of the value of the antitoxic serum treatment.

A distinguished physician, who has had charge of diphtheria wards for some time, informs me that the patients he now sees wear an entirely different aspect from those he saw before the serum treatment was adopted. Instead of being struck by the stupor, the pain, the difficulty of breathing, and the other distressing symptoms that so frequently manifest themselves during the course of this treacherous disease, he observes children with patches of membrane in the throat sitting up and playing with their toys. There is little of that distress of breathing, very little of the anxious look, and the wards altogether present a much more pleasant and genial appearance than he has ever before noticed. The other day I received a short note from another colleague, who has been going over the German hospitals to study this question, in view of taking out with him to the colonies a supply of antitoxic serum; he also states that this difference in the appearance of the diphtheria wards has impressed him far more than any statistics he has yet come across.

It has been said that most unfavourable symptoms have followed the exhibition of this serum. There can be no doubt of the fact; but after a careful study of the cases reported, I am thoroughly convinced that a very large proportion of them, at any rate, are merely *post hoc*, and not *propter hoc*. There can be no doubt that a kind of nettle-rash makes its appearance during the course of treatment, and that this may be accompanied by pains in the joints. Both these conditions, however, are usually quite transient, and seldom give rise to permanent ill effects. Albuminuria has also been ascribed to this treatment; but any one who has had to deal with children not only suffering from diphtheria, but from any form of disease, and even from none at all, is aware of the fact that albuminuria in children is of comparatively frequent occurrence. It is not striking, therefore, that those who have hitherto paid little attention to this subject should, when they come to make a careful examination of children affected with diphtheria, find a considerable number of cases in which transient albuminuria is a prominent symptom. More than this, however, it has been my duty to examine a large number of cases in which diphtheria has proved fatal, and in these cases there were certain lesions in the kidney, so distinct and so frequently present, that in describing them I used to note simply "diphtheritic condition," and then describe in detail only those features in which the appearances differed from the type that I had in my mind. This will indicate to you that alterations in the internal organs, especially in the kidneys, such as would lead to marked interference with the performance of their proper functions, were present, and had been noted long before the antitoxic serum method of treatment came into use. I may give an example of what, under certain circumstances, might have been used as a powerful argument against the use of antitoxic serum. In the *Deutsche Medizinische Wochenschrift* for December 20 of last year is reported a case of acute hæmorrhagic nephritis coming on after the use of Behring's curative serum. The patient recovered. But a similar case of acute hæmorrhagic nephritis in diphtheria, in which, however, the curative serum was not used, is reported in the same number of the same journal. The author of the second paper quotes some interesting statistics to show that albuminuria is of frequent occurrence in cases of diphtheria not treated with antitoxic serum. One observer found it in 131 out of 279 cases; another in 16 out of 53; another in 60 per cent. of all his cases; another in 227 out of 470. Suppression of urine has also been ascribed to the action of this agent; but here again, if a careful search be made of the records of diphtheria cases treated under the old method, it will be found that just as in scarlatina and acute specific infective diseases generally, but especially in those associated with rapidly supervening toxic symptoms, suppression of urine is of common occurrence; and until we have statistics on these several points, which can be compared with those above mentioned, it will be impossible and unjust to ascribe conditions to the therapeutic agent which, so far as those best able to judge can see, are to be ascribed to the disease itself.

It has been held by some that the paralysis which is so common a sequela of diphtheria should disappear entirely under the use of what they are pleased to call a specific cure for the disease. It should be remembered that the antitoxic serum cannot make good any organic damage that has been caused by the action of the toxic products of the diphtheria bacillus. It may stop their action on the tissues, and it may stimulate the tissues to react against the poison, but to the tissues themselves must be left the process of repair; the *vis medicatrix naturæ* is alone responsible for the making good of damage already done. This damage may be done at a very early stage of the disease, and if the nerves or the muscles are attacked before the antitoxine is injected, then we must expect to find degenerations and evidence of these degenerations in the various forms of post-diphtheritic paralysis; but of this we may be sure, the sooner the poison is antagonised the less will be the risk of permanent damage to the tissues. It is for this reason, I believe, that the antitoxic serum treatment of diphtheria has been so much more successful than the antitoxic serum treatment of tetanus.

The hope of success in the serum treatment of diphtheria depends upon the early application of the remedy. One word of warning. It should not be accepted that this agent can reduce the cure of diphtheria to a mere process of injection. Everything must be done to improve the conditions under which the patients are treated, to maintain their strength, to give them fresh air, cleanly surroundings and good general hygienic conditions. It will be found withal that a certain number of deaths from rapid poisoning will take place, while a number of others will succumb in the later stages of the disease. This serum can no more act as a specific in every case than can quinine cure every case of malaria; but if properly used, we believe it will reduce the mortality in a very marked degree, and if at the same time those practical sanitary reforms and improvements for which our country is so justly renowned are carried out, we may expect that diphtheria as a scourge may gradually die out from our midst. As Dr. Seaton pointed out at Buda-Pesth, we have done more in this country to improve the conditions associated with most specific infective diseases than any other nation in the world. If, now, we can graft on to our system what is best in Behring's treatment, I am convinced that we shall soon have diphtheria statistics which will compare very favourably with any that have yet been presented. The antitoxic serum treatment is only one of our lines of defence against this disease; but so much progress has already been made along this line, that within a few years, or even months, we may fairly anticipate the announcement of still greater advances and successes.

[G. S. W.]

WEEKLY EVENING MEETING,

Friday, February 15, 1895.

Hugo Müller, Esq. Ph.D. F.R.S. Vice-President, in the Chair.

Clinton T. Dent, Esq. F.R.C.S. *M.R.I.*

Influence of Science on Mountaineering.

Between mountaineering in general and climbing, which is but a special branch of mountaineering, I desire for the purpose of this discourse to draw a clear distinction, but do not wish it to be supposed that my dwelling chiefly on mountaineering implies any depreciation of simple climbing. On the latter it is well nigh impossible to break new ground, save in the geographical sense. The climbers of mountains cannot justly be accused of any exaggerated tendency to reticence as regards their adventures. The technique of climbing is really simple, and considered as a craft, the subject has been fully dealt with. Indeed, the general principles that the climber has to bear in mind have been reduced to rules so few and so simple, that many can quote, and a certain proportion can follow them.

I desire chiefly to-night to dwell for a short time on the part that science has played in developing the growth of mountaineering. This has not been adequately recognised. The popularity of mountaineering during the last thirty-five years, the period of greatest activity, has been too much laid to the credit of writers who have regarded and described the Alps as a field for the best of recreations. The more solid work was less before the world. Geologists and botanists, from the first, found in the Alps a magnificent field for pursuing their own branches of work; but in the matter of physical science the work done was speculative, not experimental. Men sought for evidence for or against the Deluge, or elaborated vast hypotheses of the earth's formation. They concerned themselves little with attempts to explain the phenomena going on under their eyes, and there was little original investigation. In old books on the Alps, statements, often of the wildest nature, are found copied from one to another without the slightest trace of acknowledgment. Men whose lines of thought led them into the direction of physical research came late into the field, but gradually their work attracted in some quarters the attention due to a new departure. So there arose men who gathered from the amassed knowledge of works of science such facts and observations as might be turned to practical account in mountain exploration. Thus was developed the scientific mountaineer, who, on the mountains, could use his head as well as his limbs. He might or might not be one who made science his prime object when among the mountains. I am far from saying that this is the ordinary type of the mountaineer of to-day; but it must be the type of the mountaineer of the future, who wishes to extend his sphere of exploration beyond the restricted field of the mid-European Alps. The pioneers were numerous. Such names as Agassiz, Studer, Rendu, Forbes, Ball, occur at once to the mind; but I must limit myself to-night to two only, De Saussure during the last century, and Tyndall in recent times.

The true value of De Saussure's work can only be estimated by considering the scientific chaos with regard to glacial phenomena that was widely prevalent before and during his time. It is not long since that avalanches, mountain falls, the bursting of glacial lakes and such like occurrences were considered generally to be the work of fiends or evil spirits. The legends that smiling Alps were converted into snowfalls and glaciers as punishment for man's wickedness were widely credited. Dragons were supposed to haunt the mountains, and were implicitly believed in by men such as Wagner, the naturalist, little more than two hundred years ago. Long after the legendary ages, of which traces enough can still be found in the Alps, and still more plainly in other mountainous countries, the state of physical science as regards mountains and glaciers was in a very primitive condition, owing largely to the terror with which mountains were generally regarded. De Saussure reduced to order by direct observation, by experiment, and by clear and impartial writing much of the confusion. It must be remembered that in the days when he travelled accurate maps were unknown. Thus, in a map of the early eighteenth century, Chamonix is depicted as some sixty miles south of the Mont Maudite, the name by which Mont Blanc was then often known. Strange views indeed are to be found in the old writers, whose desire to be credited with universal knowledge allowed them little time for accuracy of detail. Crystals were supposed to be formed by the excessive pressure to which ice was subjected. One marvels that mountains do not sink into the earth by their own weight; another believes that they would certainly do so were they not hollow. Lakes well stored with fish were imagined to be present on the top of all high mountains. Besson, who wrote in 1786, was in advance of his time, but it is to be feared that he borrowed largely from De Saussure. He advocates the determining of mountain phenomena by direct observation and experiment. Grüner, in 1760, the year De Saussure first visited Chamonix, published a treatise describing accurately the main features of the results of glacial motion. Still in De Saussure's time the progressive movement of glaciers was questioned. The very foundation of scientific mountain craft lies in knowledge of glacial phenomena and of the results of glacial motion, and De Saussure proved these convincingly enough. Previously, the regular downflow of a glacier was often confounded with the increase or diminution of the mass of ice as a whole. De Saussure independently confirmed and extended Grüner's work. He distinguished clearly between the high snowfalls and the true glacier. He explained, too, the formation of moraines. Theory thenceforth was replaced by direct observation. The principle of the progressive movement of glaciers may now seem obvious enough. Yet for ages the moraines had stretched out their long lines, the dirt bands had traced their curves, the séracs had formed, leant over, toppled and fallen, the crevasses had started, widened and closed up again, but the interpreter had been wanting.

With De Saussure's geological work I have here no concern. Most valuable and interesting are his observations on the effect of high altitudes and diminished pressure on the human frame, for these have a direct import to the modern mountaineer, and to the mountaineering question of the day. De Saussure was the true type of the scientific mountaineer. Yet had it not been for the sensational exploit of the guide Jacques Balmat, in 1786, in ascending Mont Blanc, and had it not been for the wide interest that this feat evoked, De Saussure's work might have remained comparatively unnoticed, and it may be equally true that had it not been for such work as De Saussure's, few might have passed through the door which Jacques Balmat unlocked. Unquestionably the ascent of Mont Blanc marked an epoch. Probably there were quite as many in Balmat's day who would have questioned the possibility of ascending Mont Blanc, as there are now who would question that of ascending Mount Everest.

De Saussure's observations on the law of the decrease of temperature in the atmosphere according to altitude are of the utmost value to mountaineers. The influence of cold as affecting the possibility of making higher ascents, is a factor now recognised as of the first importance. For many years after De Saussure, little more was accomplished in mountaineering than repetitions of ascents of Mont Blanc.

Modern mountaineering dates its birth in the decade 1850–60. It was in 1856 that Tyndall first visited the Alps, ascending Mont Blanc the following year. Just as De Saussure's work was emphasized and supplemented by Balmat's achievement, so Tyndall's researches came opportunely during the active revival of mountaineering when the conquest of the great Alpine peaks was proceeding apace. Though Tyndall, like De Saussure, went originally to the Alps from purely scientific motives, he at once fell under the fascination and became an enthusiastic and a highly skilled mountaineer, which De Saussure never really aimed at. To very few will it ever be given to combine so happily the qualities of man of science and mountaineer which were so conspicuously shown in Tyndall, but to many it may be possible to work on the same admirable lines. With the views which excited controversy at the time they were divulged, such as theories of glacial motion, and the viscous or non-viscous qualities of ice, I have now fortunately no concern. It need only be said, looking at the views that now obtain on this last question, that it is hard to perceive any ground for fundamental difference of opinion. The divergence of views really turned largely on the exact definition of a word. One feels almost inclined to echo John Hunter's well-known condemnation of definitions. On one point there can be little doubt; Tyndall's views fitted in admirably with practical mountaineering. He rendered clear and precise the interpretation of so many glacial phenomena that he almost made what is known as snowcraft—the most intricate, and the most valuable branch of mountaineering, for it is on excellence in snowcraft that the future of mountain exploration chiefly depends. But the great influence he had on mountaineering was through his brilliant writings and lectures. Owing largely to these the glacial world began to attract the general interest which before had been confined to the few who had frequented and climbed the high Alps. This result was due to his admirable experimental methods and to the brilliancy with which he expounded his views, and it was in this theatre mainly that the exposition was made. I may throw on the screen a slide, a view of the Weisshorn by Mr. Donkin, which almost epitomises the lectures on "Ice, Water, Vapour and Air." Imagine that from the water in the foreground rises the vapour in solution. The warm air as it rises expands. The expansion produces cooling; as a result of the cooling the vapour is condensed and the cloud is formed. Once formed, the band of cloud may remain stationary and of uniform size for a long time, constantly forming afresh on one surface, and as constantly diminishing on the other. Or the cloud may increase in volume. Following it then further in imagination till it becomes a rain cloud, the view shows the light fresh snow which has fallen on the higher flanks of the mountains. The snow sinks as the crystals part with their contained air, and so the mass by its own weight is pressed into firm snow, then into névé, then again into pure ice, which melts and flows away as a river. The circle is complete and the whole life history of a glacier is shown in this one view—not the less notable in that it is the presentment of the mountain on which Tyndall's greatest climbing feat was accomplished in 1861.

Time forbids any endeavour to repeat the more striking of the experiments shown to illustrate these processes, but I may bring before your notice once again the simple experiment first made by Faraday in 1850 to illustrate regelation. This simple observation on the properties of regelation was applied by Tyndall to the interpretation of many glacial phenomena. He showed that as the glacier passed through any narrow channel or was torn and fissured as it swept over the slopes or formed an ice-fall that the ice was subject to crushing, and he demonstrated that pressure alone was sufficient to account for the complete remoulding of the mass, the closing of the crevasses, and the re-establishment of the purity of the ice. Unless snow possess these properties all travel on the snowfields would be impossible. When below the freezing-point regelation does not take place. This fact, with regard to the highest ascents where the cold may be extreme, is of obvious significance. Under conditions of extreme cold, and where the snow contains little air, it will often be powdery, as was found on the first ascent of Chimborazo. This condition is rare in the Alps. In the latter, indeed, the worst conditions of snow seldom in the summer months turn back the mountaineer, but in higher regions, where time is of the first consequence, it would be of the greatest moment to judge beforehand in what condition the snow is likely to be found. The compass bearings with regard to the sun of the slopes up which the track lies, the prevailing winds and their temperature, the radiation from rocks in the neighbourhood and such like factors must be taken into account. With regard to the formation of crevasses Tyndall did much work, though it was limited rather to the lower portion of the glacier, and extended little above the ice-falls. He showed most clearly the method of formation of crevasses, longitudinal, transverse and oblique. Many years previously Besson had said " the ice of a glacier flows like a torrent following fluid laws," probably not appreciating the full truth of his own remark. Tyndall by careful measurement showed the situation of the point of maximum motion and demonstrated that when a glacier curved, the point of maximum motion lay nearer the convex border of the glacier. Thus in a glacier whose course is serpentine, the lines of maximum rate of motion crossed the central line at each curve. From this a practical point in mountaineering can be deduced. In descending an unknown glacier, it is generally best when the ice cannot be quitted to keep on the side of the smaller curve of the glacier where the marginal crevasses will be less numerous on this border. I can recall an occasion in the Caucasus when inattention to this point led to our being benighted on the glacier.

On the so-called dirt-bands, first noticed by Professor Forbes in 1842, Tyndall made many observations. It is matter for regret that a feature of glaciers so beautiful as these great curving stripes should have received so unpoetic a name. Tyndall clearly demonstrated their formation in the ice-falls. To the mountaineer much that is practical may be gathered from their presence. Thus the existence of dirt-bands shows conclusively that there must be an ice-fall at some part of the glacier, and that there must be rocks in the neighbourhood capable of yielding the grit of which the dirt-bands are composed. Several glaciers may coalesce and form the main stream. Thus the Mer de Glace has three tributaries; on one, the Glacier du Géant, the dirt-bands are strongly marked, on another, corresponding to the Glacier de Talèfre, they are but faintly indicated, while the third, the central stream, has no dirt-bands at all. These several streams can be distinguished one from another, to the very extremity of the Mer de Glace, by the medial moraines. It is certain then that on two of the glaciers higher up will be found ice-falls, and that the third, the central, will lead by more or less gentle declivities to the snow basins that feed it. Suppose the Mer de Glace were an unexplored field visited for the first time, such an observation might obviously be of the highest value in determining the route to be taken.

If it be true that with more accurate knowledge of glacial phenomena mountaineering skill has improved, and mountaineering possibilities extended, it would naturally be expected that the progress would be more shown in the class of amateurs, as they are termed, than in that of the professional guide. Such I think, and I have the authority of some first-rate guides of long experience to back me, is the case. Much has been said on the comparative skill of guides and travellers. The truth probably is that the best guide of to-day is fully as good a man as the best guide of any other period, while the general standard of mountaineering proficiency among travellers has greatly improved, though there will probably be not a few *laudatores temporis acti* to question such a conclusion. Mountaineering has, however, developed in such a way that no comparison is possible now between the traveller and guide, and none is needed. For the more difficult work that yet remains to be done, the qualities that the guide shows best are absolutely essential to achieve the best possible success; and so also are the qualities that the traveller has in a great measure developed. The traveller and guide can each supplement the qualities of the other, and they who are interested in the progress of mountaineering ought to be as much concerned with encouraging the development of guiding skill as of advancing their own. In one other respect Science may possibly do much for the future of mountaineering by throwing light on the problems that still environ the question of the effect of high altitudes and diminished atmospheric pressure on man. Here the mountaineer comes in direct touch with the physiologist. The evidence gathered so far has come from three sources. Some from laboratory work, some from experience on the mountain side, and a certain amount from those who have made balloon ascents. So far, it must be allowed, the laboratory work has not been fruitful in practical results; but the question as recently revived is really still young. In the very few minutes that remain I may be able very briefly to sketch how the matter now stands, and indicate what progress has been made as to its practical solution.

First, as to the contribution of the mountaineer. On this diagram are indicated certain ascents, selected chiefly because in their description special reference has been made to the effect of high altitude on the travellers. The subject has for long received occasional attention. Sometimes in the early accounts surprise is expressed at the absence of effects which have for centuries been noticed and commented on. Thus Deluc on Mont Buet (10,200 feet) seemed quite astonished that he did not suffer from mountain sickness. On Mont Blanc, De Saussure was considerably affected and gave an admirable description of the symptoms. De Saussure thought it improbable that scientific observations such as he wished to carry out could ever be properly made at so great a height—and now there is an observatory on the top, and a railway station, as I understand, is in contemplation. In the numerous accounts dealing with Mont Blanc published in the early part of this century, the effect of the rarefied air is almost uniformly mentioned. Often, it may be suspected, this was because the writer thought it proper to allude to the subject rather than because he really suffered from mountain sickness. Within the last few years the expedition has several times been made from Chamonix to the top of Mont Blanc and back to Chamonix within twenty-four hours; once I believe in about eighteen hours. The vertical height to be ascended is over 12,000 feet. In the ascent of Elbruz (Caucasus) one party experienced no discomfort at all, another party was affected. Of the more recent experiences in the Andes and Karakoram I need hardly remind you. Perhaps the Karakoram expedition shows the greatest height reached, though not much above the Schlagintweit expedition. A very curious point is brought out by the chart, viz. that heights far exceeding Mont Blanc had been reached long before the ascent of that mountain drew attention to the question. Thus the Karakoram Pass, about the height of Elbruz, has been known for centuries as a well established trade route, and another pass (the Changlung) of over 19,000 feet has long been known. Indeed, Western people were still speculating on the possibility of ascending to any higher elevation than that of Mont Blanc, while centuries before in the East men had reached points nearly 4000 feet higher. Assuming that the highest point of the earth's crust is about 30,000 feet, this other diagram shows in another form how much has been accomplished by mountaineers, and, it may be added, how little apparently remains to be done. The question of the ascent of the highest point indicated (Kabru) on the diagram is doubted by many good authorities. There is no doubt about the height of the mountain which has been triangulated, but the question is whether the travellers did not mistake the peak they actually ascended. Whether the party actually did so or not, seeing that there is conflict of opinion, must remain uncertain. But the Karakoram experience, the latest, tends to show that it was certainly not physically impossible.

Experiments in the laboratory have been conducted with apparatus on a large scale similar to that which I show you here in miniature. By means of this apparatus the atmospheric pressure can be reduced to any degree required, and the pressure can be, by an ingenious contrivance, maintained absolutely constant for any desired length of time.

This apparatus has been devised for other purposes, but essentially it could serve like M. Bert's "pneumatic cabinet." You may judge, and judge rightly, that the conditions produced in a man who shuts himself up for a time in such an apparatus and lowers the pressure are different from those on the mountaineer. At least M. Bert's pneumatic cabinet has proved the existence of other factors in the problem. M. Paul Bert, experimenting on himself, sustained a diminished pressure equal to 32,528 feet for a short time—a lower pressure than that of Mount Everest. From many experiments he was led to the conclusion that deficiency of oxygen was the main cause of the symptoms—like those of mountain sickness—experienced. He set down the limit of life as arriving when the air contains but 7 per cent. of oxygen, the normal amount being 20 per cent. He was therefore led to infer that by supplying oxygen the evil effects of diminished pressure could be warded off. To carry a sufficient supply of oxygen on the mountain side would be physically impossible. Mr. Whymper's experiences disprove M. Bert's theory, and Bert's views received a further shock in the fatal balloon in which MM. Crocé, Spinelli and Sivel lost their lives, dying from asphyxia at a height of about 28,000 feet, although they had a supply of oxygen with them. M. Bert's researches have attracted much attention, but the work of Geppert and Fränkel published in 1883 really carried the question as regards laboratory work a good deal further. And here, I hope I may be pardoned, if I turn only for a moment to some physiological details. Geppert and Fränkel found that life could be sustained, without supplying oxygen, at a far lower pressure, viz. that of 180 mm. of mercury, equivalent to a height of 36,400 feet. Yet more, they pointed out clearly that three distinct stages could be observed—that of difficulty of breathing, paralysis, and lastly, unconsciousness or coma. On the first and third much has been written. But it is the second of these three stages—the partial paralysis—which has received far less attention, that affects profoundly the question from the mountaineer's point of view. Geppert and Fränkel's results seem absolutely trustworthy. They bear out, too, even allowing for possible error in observation, the experience of those who have ascended in balloons. Life unquestionably can be maintained at far greater elevation (i. e. at a much lower pressure) than that of the highest mountain. In the pneumatic cabinet, two most important factors do not come into play. No exertion is required beyond that of breathing, and there is no lowering of temperature. In high balloon ascents again no exertion is required of the lower limbs. The same effects that are shown under diminished pressure are also shown at greatly increased pressure. The circulation in the portion of the spinal cord or marrow immediately concerned with the innervation of the lower limbs becomes greatly disturbed. The partial loss of power in the lower limbs is brought about in this wise. The blood collects and stagnates at this portion. It has been stated, but incorrectly, that the reverse condition is produced. The temperature of all the extremities is greatly lowered, not only by the surrounding cold but by change in the nerve centres themselves. The importance of this disturbance to the mountaineer who seeks to attain the greatest elevation on foot is obvious. Yet this, the most significant feature of the problem from the climber's point of view, seems to have attracted little attention. The practical effect of this partial paralysis must be to render each step which involves the raising of the weight of the body doubly or trebly as laborious as it would be at the pressure to which the individual is naturally accustomed. It is certain, however, that the effects can be completely recovered from, and this partial loss of power is, as far as can be judged from what is at present known, though a formidable obstacle and one not generally recognised, not insuperable. Possibly medical means may be discovered to combat the condition. Oxygen as a remedy has failed: other remedies may be found. Certain drugs recently introduced produce effects not unlike those which result from diminished pressure—a significant fact. One curious effect of diminished atmospheric pressure has been noted, and has been held to compensate for the diminution in the amounts of oxygen, a diminution that as Professor Roy has suggested must be increased on the mountain side when there is any melting of snow, inasmuch as water will absorb oxygen more readily than nitrogen from the air. If any stay is made at an elevation of some 13,000 feet, as Viault has shown, there is an enormous increase in the number of the red blood corpuscles, that is to say, an enormous increase in the area of the surface concerned with the absorption of oxygen. At the sea level the ratio of the body surface to the blood surface is as 1 to 2560; while at a pressure corresponding to 13,000 feet the blood corpuscles so increase in number as the ratio of the body surface to that of the corpuscles has altered to 1 to 4293. Putting the matter in another way, the actual corpuscle surface at sea level = 3840 square metres; at 13,000 (after 11 days) = 6144 square metres. But though the increase of the corpuscles may begin at once, the multiplication is a slow process. The maximum is perhaps reached in three to four days.

[C. T. D.]

WEEKLY EVENING MEETING,

Friday, February 22, 1895.

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

PROFESSOR ARTHUR SCHUSTER, Ph.D. F.R.S. F.R.A.S.

Atmospheric Electricity.

IT is hardly possible to imagine that the first experimenter who obtained an electric spark sufficiently strong to produce a sensible sound should not at once have been struck by the fact that he was in the presence of thunder and lightning on a small scale. We find, indeed, in various writings from the early days of electrical machines a number of suggestions that the thunderstorm is an electrical phenomenon; but to Benjamin Franklin belongs the merit of having perceived that a direct experiment was needed to prove what so far was only a guess. In an article entitled "Opinions and Conjectures Concerning the Properties and Effects of the Electrical Matter, arising from Experiments and Observations made at Philadelphia, 1749," the following passage occurs:—

"To determine the question whether the clouds that contain lightning are electrified or not, I would propose an experiment to be tried where it can be done conveniently. On the top of some high tower or steeple place a kind of sentry-box, big enough to contain a man and an electrical stand. From the middle of the stand let an iron rod rise and pass, bending out of the door, and then upright 20 feet or 30 feet, pointed very sharp at the end. If the electrical stand be kept clean and dry, a man standing on it, when such clouds are passing low, might be electrified and afford sparks, the rod drawing fire to him from a cloud.

"If any danger to the man should be apprehended (though I think there would be none), let him stand on the floor of his box, and now and then bring near to the rod the loop of a wire that has one end fastened to the leads, he holding it by a wax handle; so the sparks, if the rod is electrified, will strike from the rod to the wire, and not affect him."*

The experiment suggested by Franklin was successfully performed in Marly (France), by D'Alibard, on May 10, 1752,† in London by Canton, in Spital Square, on July 20, 1752, and by Wilson, in

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* 'Experiments and Observations on Electricity made at Philadelphia, in America,' by Benjamin Franklin, LL.D. and F.R.S.; London, printed for David and Henry, and sold by Francis Newbery, 1769, p. 66.

† Ibid., p. 107.

Chelmsford, Essex, on August 12th of the same year. Franklin himself describes having used a kite in Philadelphia in a letter dated October 19th, without giving the date of his observations. But this must be supplied in some passage which I have not been able to find, for Rosenberger * mentions that it was done in June.

Franklin's disbelief in the dangerous character of the experiment must have received a severe shock when he heard of the death of G. W. Richmann, who, in the year 1753, was killed by an electric discharge drawn from the clouds by means of a kite.

The thunderstorm is the most impressive effect of atmospheric electricity, though it is rivalled in beauty by the aurora, and in interest by the many phenomena of daily occurrence, which are only made perceptible to us by proper instruments. In a lecture delivered before this Institution on May 18, 1860, Lord Kelvin described the delicate electrical appliances constructed by him for the more accurate observation of atmospheric electricity. The problems then for the first time clearly stated, gave a powerful and still lasting impulse to the investigation of atmospheric electricity, and though no decisive answer can be given to all the questions raised in that lecture, recent researches have brought us somewhat nearer to their solution.

Observations which may be made every day and at every place have shown that the earth is electrified, whatever the weather may be. In the language of the older theories, which we cannot as yet altogether abandon, we say that the earth is covered with negative electricity, or, in modern phraseology, we express the same idea by the statement that we move about in an electrified field, that electric lines of force stretch through the air from the ground, from our bodies, and from everything which is exposed to the sky overhead. The strength of this electric field is not at all insignificant. If we wish to produce it artificially between two parallel plates kept at a distance of one foot, we should have to apply an electromotive-force sufficient —and sometimes more than sufficient—to light up the incandescent lamps which illuminate our dwellings. The electric force is comparatively weak in our country, but 50 volts per foot are constantly observed, and 100 volts are not uncommon; but in dryer climates the amount of the force may be considerably in excess of these figures.

If we fix our minds on the lines of force starting from the surface of the earth, we are at once led to ask, Where is their other end? Do they curve round and back again to earth? Do they end in the dust which everywhere surrounds us, or do they reach up to the clouds? Do they pass through the clouds and end where invisible particles separate the sunset red from the midday blue? Or, finally, do they leave the earth altogether, and form intangible bonds between us and the sun, the stars, the infinity of space? These are not idle questions, and we cannot be said to have solved our problem unless

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* 'Geschichte der Physik,' vol. ii. p. 316.

some definite answer is given to them. The last-mentioned view, propounded originally by Peltier, and latterly supported by Exner, is the simplest. If we could allow that the earth, once electrified negatively, could remain electrified for ever, the corresponding positive electrification being outside our atmosphere altogether, the chief difficulty of atmospheric electricity would be removed, and the normal fall of potential at the surface would be explained by the permanent negative electrification of the surface.

Unfortunately, this view, to be tenable, has to assume that the atmosphere is a complete non-conductor to the normal electric stress, and this is known not to be the case. We know of several causes which break down the insulating properties of air. If two pith balls are electrified and repel each other, and a match be lit in their neighbourhood, the pith balls come together, showing that they have lost their charge, and consequently that the flame of the match has destroyed the insulating power of air. It is not only the flame itself which conducts, but also the gases rising from the flame.* The following experiment will prove this. In Fig. 1, A represents a metallic tube bent round at the upper end, and containing at its lower end a Bunsen burner in metallic contact with the tube, which is also connected to an electroscope. The tripod T which supports the tube is insulated by blocks of paraffin. A Leyden jar, L, on a separate support, is placed so that the knob stands at about the level of the upper part of the tube, which acts as chimney to the flame. The knob of the jar may be a few inches away from the opening of the chimney, and not necessarily in a line with it. The experiment succeeds, although the gases rising from the burner may not come into contact with any part of the jar. The jar is charged, and care must be taken that no fibres of dust attach themselves either to the jar or chimney. I have found it convenient to join a piece of amalgamated zinc to the end of the chimney. Under these circumstances the charge of the jar will be found to leak across to the tube, and the leaves of it will diverge. If, as in Exner's form of electroscope, the leaves, on reaching a certain divergence, discharge by forming a contact with earth-connected plates, C C′, the charging and discharging

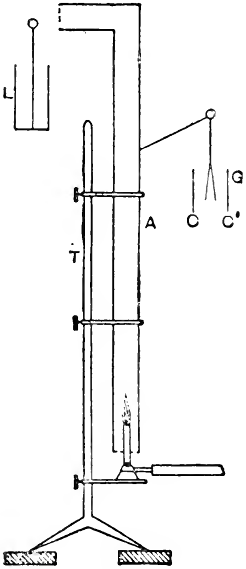

Fig. 1.

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* The most complete investigation of the conduction of gases rising from flames is contained in a series of papers by Giese ('Wiedemann's Annalen,' vol. xvii.).

can be watched for a long time. It will be noticed that the flame, being altogether surrounded by a tube of the same potential, cannot be active in this case, but the conductivity must be due to the gas as it escapes from the chimney.

It follows from these experiments that every fire burnt on the surface of the earth, and every chimney through which products of combustion pass, act like very effective lightning conductors, and would consequently discharge, slowly but surely, any electrification of the surface of the earth. The peculiar immunity of factory chimneys against damage by lightning appears from statistics collected by Hellmann in Schleswig-Holstein,* for while 6·3 churches per thousand were struck, and 8·5 windmills, the number per thousand of factory chimneys struck was only 0·3.

Franklin was acquainted with the action of flames; he also discovered that no charge could be given to a red-hot iron ball, a fact which seems to have been forgotten until re-discovered in our own times by Guthrie. Franklin also tried the action of sunlight, but obtained no result. Had he performed the experiment with carefully-cleaned zinc, he would have anticipated one of the most striking of Hertz's discoveries. We now know that a negatively-charged surface will discharge into air when illuminated by strong violet light, and sunlight will be sufficient with specially sensitive materials. This action has been investigated in detail by Elster and Geitel, who have not, however, succeeded in obtaining results with sunlight acting on such bodies as we know the earth's crust to be made of. So far, then we have no experimental evidence to include light as an active agent in the phenomenon of atmospheric electricity.

We possess in the electric discharge itself a very powerful, and probably very generally active means of breaking down the insulating power of air. Some of the experiments † which I described some years ago to prove this, were objected to on the ground that it might not be the discharge itself, but the ultra-violet light sent out by the luminosity of the discharge, which was active. The following form of the experiment conclusively shows that the discharge acts independently of light.

In Fig. 2, R represents a Rhumkorff coil entirely surrounded by a metallic box B, which is connected to earth. The terminals of the coil lead to two electrodes inside a metallic tube T, which is also kept at zero potential. This tube is arranged so that a current of air can be blown through it. The air, on escaping through the tube, is made either to impinge on or to pass near a metallic plate connected to a charged electroscope. Under these circumstances, the electroscope is not discharged either by a current of air alone, or by the coil alone. But as soon as the air is blown through the apparatus while the sparks are passing and then made to impinge on the

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* 'Veröffentl. des Kgl. Preuss. Stat. Bureaus,' 1886, p. 177, quoted by Bebber, 'Meteorologie,' p. 245.

† Proc. Roy. Soc., vol. xlii.

plate C, the electroscope is instantaneously discharged. The experiment succeeds when a plug of cotton-wool is inserted at W to stop the action of the dust; but a plug of cotton-wool at the other end diminishes the action so much that I am doubtful whether the effect then really exists there. I am, so far, not inclined to believe that the action is due to dust, but rather that the cotton-wool acts in increasing very considerably the interval which elapses between the time at which the spark acts and the time at which the sparked air passes out of the tube. The effect may be observed even though the tube L is lengthened by an addition of another piece 3 ft. or 4 ft. long.

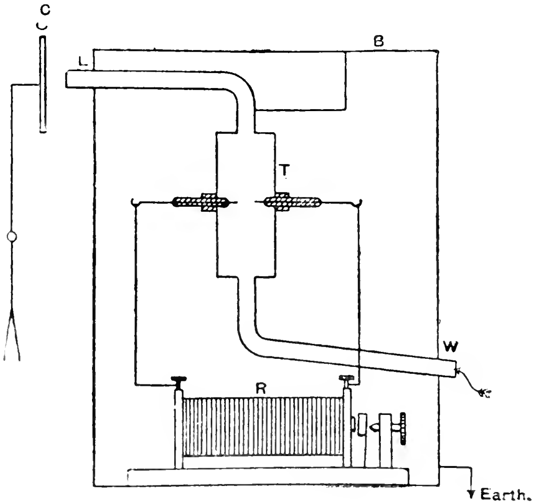

Fig. 2.

Several phenomena, one of which had been known for a long time, can be explained by the fact that the electric discharge changes the condition of the gas into a state similar to that of gases rising from flames. It is mentioned, for instance, by Faraday that electric sparks are liable to succeed each other along the same path, and it is known that the same holds for lightning flashes, facts which themselves point to a higher conductivity of air along the path of the previous discharge. A curious instance of a similar effect is afforded by lightning conductors, which are sometimes put up to protect overhead leads used for conveying a high-tension current. Owing to the obvious impossibility of connecting the leads directly to earth, a small air gap is interposed, the idea being that the air gap will act as an insulator for the current the leads are intended to carry, but that if during a thunderstorm the potential rises sufficiently high to be dangerous, equalisation may take place through the air gap to earth by means of a small spark. So far, the air gap answers its purpose, but as soon as a spark passes through the gap, it destroys the insulating power of the air, and the main current consequently takes a short-cut through the gap. At Pontresina, in the Engadine, lightning conductors put up in this way are so sensitive that a flash of lightning several miles away causes a small spark by induction, and instantaneously puts out every electric lamp in the town.

If we accept the view that an electric discharge destroys the insulating power of the gas, it follows that the outer regions of the atmosphere must conduct, for we have ample reason to suppose that electric currents are passing continuously through those regions. The aurora borealis in the arctic regions is, according to Nordenskiöld's observations, a permanent phenomenon, and the diurnal changes of terrestrial magnetism show that in our latitudes electric currents traverse the air above us. However small a conductivity we may assign to the atmosphere, the earth could not remain electrified inside such a shell of partially conducting gases. Lord Kelvin drew the same conclusion in the Royal Institution lecture, on the assumption that gases at much reduced pressures cease to insulate. We may leave it an open question whether the normal electric stress could in itself cause a discharge in the outer regions; but we cannot deny that under existing conditions these regions do not insulate, and Lord Kelvin's argument still holds good.

But the question of the ending of the lines of force—in other words, the location of the positive charge corresponding to the negative electrification of the surface of the earth—can only be solved by balloon or kite experiment, and we may briefly mention the more important results which have so far been obtained.

Observations made up to heights of about 1000 feet seem to indicate a strengthening of the electric field—i. e. the fall of potential per metre is greater at a height of, say, 200 metres than on the surface of the earth. The observations of Dr. Leonhard Weber * bring out this point clearly. In one case the fall of potential at a height of 350 metres was found to be six times that at the earth's level. This increase is in itself not surprising, if we remember that every particle of dust raised from the ground must itself be negatively electrified, and probably the observed increase in the electric force is sufficiently accounted for by the presence of electrified dust.

Observations made at greater heights in balloons, on the other hand, seem clearly to indicate that this increase soon ceases, and that a diminution already takes place at moderate heights. Thus the observations of Dr. O. Baschin † gave for the fall of potential in volts per metre the numbers 49, 28, 13 at heights of 760, 2400, 2800 metres respectively, and at a height of 3000 metres no measurable fall at all

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* 'Elektrotechnische Zeitschrift,' April 1888.

† 'Meteorologische Zeitschrift,' September 1894.

could be obtained. These observations were made in clear weather. The balloon afterwards passed over a layer of clouds, and strong electric effects were noticed. Similar observations had been previously made by others (Andrée, Le Cadet, and Bornstein), and though the subject is by no means exhausted, we may take it as provisionally established that the lines of force of the normal electric field of the earth end within the first 10,000 feet or 15,000 feet. This result is of great importance, for it shows that in fine weather there must be a layer of positively electrified air permanently above us. Currents of air in this layer must affect the field as we observe it, and possibly the daily period may be due to changes in the currents of air at a moderate height. A fact discovered by Exner is of importance in connection with this subject. Observing at three different places (in a field close to Vienna; in St. Gilgen, on the Wolfgangsee; and on the hills near Venice), he found that whenever there was a strong south wind, with a clear sky, the normal electric force was always increased, and sometimes considerably.*

The daily changes show, with few exceptions, a remarkable uniformity at different places. There are in general two maxima of potential—one at 8 or 9 o'clock in the morning, and one in the evening. The evening maximum is the most marked, while at some places, and especially near towns, the morning maximum disappears. The same general features of the daily variation have been found to hold at a number of European stations, at Cape Horn, Melbourne, and in the Northern Arctic regions. If the variation is separated into two—one having a period of 24 hours, and the other of 12 hours—the latter is found to agree in phase at widely different places on the earth's surface, while the former is found to vary to a much greater extent, and hence to be probably more affected by local circumstances. The remarkable researches of Hann have given a similar result for the diurnal variations of the barometer, and we may reasonably conclude that the semidiurnal variation of atmospheric electricity is connected with the same circulation in the upper regions of the atmosphere which shows itself in the corresponding changes of pressure.

In addition to the more regular periodic changes, the electric stress observed in fine weather shows marked differences on different days and at different seasons. With respect to these, the researches of Prof. Franz Exner † have led to the important result that there is a close connection, direct or indirect, between the amount of aqueous vapour present in the atmosphere and the fall of potential observed at the surface of the earth. If p_0 be the pressure of aqueous vapour present in centimetres, Exner ·deduces the equation for electric force P

$$P = \frac{A}{1 + k p_0},$$

where $A = 1300$, $k = 13 \cdot 1$.

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* 'Wiener Akad. Sitzungsberichte,' vol. xcvi. 1887.

† Ibid.

The formula agrees very well with observations in which the vapour pressure varied between 0·23 and 0·95, and it is especially to be remarked that it is the amount of vapour and not the humidity which determines the electric force. Observations made by Mr. E. Drory during a journey round the world fit in very well with Exner's formula, and observations made at such widely different places as Suez, Albany, Sydney, Colombo and Penang showed a fall of potential practically identical with that calculated from the above formula, though the same constants were taken and the vapour pressure varied between 0·8 cm. and 2·2 cm.

Messrs. Julius Elster and Hans Geitel * have followed up this research. Their investigations have shown a satisfactory agreement with Exner's formula, if the mean values of a number of observations in which the vapour pressure is approximately the same, is considered. But individual numbers differ very widely from the mean, so that the formula cannot be used to predict the normal fall of potential on any particular day. There is, perhaps, nothing surprising in the great divergence of such individual results if it is considered that we only observe the moisture near the surface of the earth, but are ignorant of the total amount of water in the column of air over the district in which the observations are carried out. The same authors have shown that an equally good agreement can be obtained if, instead of the amount of aqueous vapour, we take the intensity of active radiation as the determining circumstance. The light might be supposed to act on the general surface of the earth, as it does according to Hallwachs' observations on a metallic body, dissipating a regular charge. There are some difficulties in the way of this explanation, the most serious being the absence of experimental evidence that sunlight actually does act in the manner indicated on any substance forming part of the earth's surface. It is impossible at the present time to enter more fully into this subject, but attention must be drawn to the very important indirect result, that there seems to be a connection between ultra-violet radiations and the amount of aqueous vapour present in the air.

The phenomena of atmospheric electricity have been studied at the mountain observatory established on the "Sonnblick," in Salzburg, at a height of 3100 metres.

The important result has been established that the electric force is singularly constant. The great differences observed at low levels between the electric field in summer and winter, or on dry and wet days, seems to be completely absent, and these facts tend to support the conclusion derived from balloon observation, that the positive ends of the lines of force are situated at a height of something like 10,000 feet.

Brief allusion must be made to some of the causes which alter to

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* 'Wiener Akad. Sitzungsberichte,' vol. ci. 1892.

a marked extent the normal fall of potential. As the surface of the earth is negatively electrified, it follows that dust carried up by the wind must be electrified, and it is found, indeed, that in violent dust storms the laws of force near the surfaces are altogether distorted and reversed in direction. Werner Siemens* could, while standing on the top of one of the pyramids during a strong wind, charge an improvised Leyden jar sufficiently to obtain strong sparks. A casual observation of Elster and Geitel,† may prove significant. On March 7th, 1889, the temperature in Wolfenbüttel was rising from − 10° C. to + 2° C., a cirrus layer covering the sky. The fall of potential changed in the course of four hours from 1302 volts per metre to − 1200 volts, that is, from a very exceptionally high fall to an equally strong gradient in the other direction. Although the atmospheric circumstances were anomalous, they seem not in themselves sufficient to account for the anomalous electrical effects, and the authors suggest that a possible explanation may be found in a violent dust storm which on the previous day was observed in Alexandria.

Fogs are generally found to increase the normal fall considerably, so that the drops of water must be taken as positively electrified.

Waterfalls considerably disturb the electric condition in their neighbourhood, the air surrounding the fall being found charged negatively sometimes to considerable distances.

Whether clouds in themselves are always electrified is very doubtful; they no doubt disturb and generally weaken the fall of potential at the earth's surface, but this may only be due to a displacement of the positively-electrified layer which balloon observations have shown to exist at a height of from 10,000 feet to 20,000 feet. While a cloud discharges rain, the electrical effects in the neighbourhood of the place are the same as that in the neighbourhood of a waterfall. The explanation is probably the same in the two cases, and by means of experiments, alluded to further on, we may reproduce the negative electrification of air under similar circumstances.

Measurements of the electrification of falling rain or snow, simple as they appear at first sight, are beset with very serious difficulty. We owe the most complete investigation on the point to Messrs. Elster and Geitel.‡ They find no regularity in the electrification, though positive signs slightly preponderate with snow and negative signs with rain.

The approach of a thunderstorm announces itself by characteristic cumuli clouds, and the general atmospheric condition favourable to their formation is felt by many persons of nervous temperament. Many of us are accustomed to hear that "there is thunder in the

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* 'Pogg. Ann.,' cix. 1860; 'Meteorologische Zeitschrift,' 1890, p. 252.

† 'Ziele und Methoden,' p. 11.

‡ 'Wiener Sitzungsberichte,' vol. xcix. 1890.

air." Whatever the special feeling of "thunder" may be due to, it cannot be an electrical effect, for electrical instruments delicate enough to detect a small fraction of the normal force, give no indications of the approach of a thunderstorm, and it is only when the cloud has begun to discharge rain or hail that strong electrical effects are noticed. During the thunderstorm the electroscope is, of course, much disturbed, and there are frequent and violent reversals of its indications.* The fact that no effects are observed at the surface of the earth during the approach of a thunder-cloud does not prove that there is no electrical separation, for we may imagine two oppositely electrified layers at different levels producing a strong electric field between them, but only weak effects outside. That some such thing may possibly occur is indicated by observations made in mountain districts, where violent electrical disturbances are observed previous to the formation of clouds.† The cumulus cloud, from which the lightning strikes out, is nearly always associated with a cirrus layer above it, and the flash occurs more frequently upwards or sidewards between the clouds than down to earth. Under such circumstances it is clear that instruments on the surface of the earth can only very partially indicate the nature and distribution of electrical stress in the neighbourhood of the cloud.

Thunderstorms seem always to be connected with a vortex motion, and meteorologists distinguish two kinds of thunderstorms. The first kind forms in the outlying portions of a large cyclonic system. The storms which occur in winter are mostly of this nature, and the vortex necessary for its formation is of the nature of a secondary disturbance. The thunderstorm which forms in summer, on the other hand, makes its own vortex, and is of a much more local character than that which is produced round a previously-established barometric depression. The summer storm is much influenced by the character of a district. There are certain configurations apparently favourable to its formation, as is clearly brought out by the charts which have been made representing their frequency.

The route travelled over by the storm is affected by mountain ridges, and rivers also seem to offer a peculiar impediment. Many of them are brought to an end either along their whole front, or only part of it, when they reach the banks of a large river.‡

Some curious problems are presented by the detailed structure of lightning flashes. Although these lie outside the range of the present lecture, reference must be made to the very beautiful photographs of lightning flashes taken both in this country and abroad. The ordinary forms which lightning takes are familiar to all; but a good deal of mystery still surrounds the so-called globular lightning. The manner

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[*] Weber, 'Elektrotechnische Zeitschrift,' vol. x.; Elster and Geitel, 'Ueber einige Ziele und Methoden Luftelektrischer Untersuchungen,' Wolfenbüttel, 1891.

[†] Trabert, 'Meteorologische Zeitschrift,' 1889, p. 342.

[‡] Bebber, 'Meteorologie,' p. 255; Bornstein, 'Archiv der Seewart,' viii. 1885.

in which this form appears is best described in the words of eye-witnesses.

Dr. A. Wartmann gives to the Physical Society of Geneva the following account of what he saw :*—"At half-past six o'clock in the evening I drove from Versoix to Genthoud. On the Malagny Road I heard the coachman say he did not know where he was. His eyes were so much fatigued by the frequent and intense lightning discharges, that he was blinded, and could not, even in the intervals, see the road, in spite of the good lanterns alongside. I stepped on to the box and took the reins. We had barely passed the principal gate of the grounds of Dr. Marcet, when I became conscious of a bright and lasting luminosity behind me. Thinking it was a fire, I turned round, and saw, at a distance of, roughly, 300 metres, a ball of fire of about 40 cm. diameter. It travelled in our direction with a velocity about equal to that of a bird of prey, and left no luminous trail behind. Just as the ball had overtaken us, about 24 metres to our right, it burst with a terrific noise, and it seemed to me as if lines of fire started from it. We felt a violent shaking, and remained blinded a few seconds. As soon as I regained power of distinguishing objects, I saw that the horses had turned at a right angle to the carriage, with their chests in the hedge, with drooping ears and all signs of great terror. I returned on the following day to the place where I had seen the ball explode, but could find no sign of any damage. At a distance of 100 metres I found that a group of three trees, bordering a wood, had their upper branches singed, but it is not possible to say whether this was due to the discharge which I had seen."

The following is a translation of an account given by Mr. H. W. Roth :†—"During the thunderstorm of May 19, 1888, at about 6 p.m., a flash of lightning took effect which seems to me remarkable from a physiological point of view. The dealer Werner, from Ellerbruch, and his son (16 years old), with a one-horse conveyance containing rags, were on the road which leads from here to the village of Ottensen, about three miles away in a south-westerly direction. The father had been left a little behind, and the son was occupied in giving bread to the horses, when he found himself suddenly surrounded by light, and noticed a fiery ball, about the size of his fist, move towards him along the back of the horse. Then he lost consciousness. He felt no concussion. The father, on approaching, saw the horses' limbs still contracting, and at first he thought his son was dead, but succeeded, after considerable efforts, in bringing him back to life in about three-quarters of an hour. The horse was dead."

Some curious statistics have been collected, especially in Germany, as to the damage done by lightning flashes. That damage seems to

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* 'Arch. des Sci. Phys. et Nat.' (3) vol. xxi. 1889. The above account is translated from the 'Meteorologische Zeitschrift,' 1889.

† 'Meteorologische Zeitschrift,' 1889, p. 231.

have increased to an enormous extent within the last 50 years, and although in cases of this kind statistics may easily be at fault, there seems no doubt about the reality of the fact, which may find an explanation in the partial cutting down of forests in those parts where thunderstorms chiefly occur. When lightning strikes into forests, it selects certain trees by preference. Thus, in the principality of Lippe, taking the percentage of beeches struck by lightning as unity, that for other trees is as follows:—Oak 48, spruce fir 5, Scotch fir, 33.

The St. Elmo's fire, a continuous discharge from points and sharp angles, is often observed on board ship and in mountain districts during a storm. Its appearance was considered a sign of the approaching end of the lightning, and was looked upon with favour by the ancient sailors in the Mediterranean Sea, who gave to it the name of Castor and Pollux. There was another appearance called Helena, a bad omen, which by many is believed to have been another form of the St. Elmo's fire, and the present name has been stated to be a corruption of the word Helena. Some support is given to this view by the fact that the Emperor Constantine built a castle in the Pyrenees, which he named after his mother, Helena, and this castle seems to be referred to occasionally as St. Elne or St. Elme. But it is much more probable, as argued by Dr. F. Piper,* that the word is derived from St. Erasmo, a bishop who came from Antiochia, and suffered a martyr's death at the beginning of the fourth century. He seems to have been specially considered the patron of Italian sailors. Churches and castles in Naples and Malta were called St. Erasmo and St. Ermo, and Ariosto describes St. Elmo's fires as St. Ermo's fires. The electric discharge which goes under this name has a different appearance according as it is the positive or negative electricity which escapes, and both kinds occur with about equal frequency.

Although we have not yet arrived at any satisfactory theory of atmospheric electricity, some progress has been made, and this account would not be complete without a short account of the views taken by men of science on the subject. The number of theories proposed is very considerable. Dr. Suchsland,† in a pamphlet published in 1886, gives an account of twenty-four, to which he adds one —his own. The year 1884 alone has given birth to four theories.

We may group the theories according to the origin they assign to the source of energy which is involved in the formation of the electric field. All the work we can perform is either derived from the sun or from the earth's rotation. There is, as far as I know, only one theory—that of Edlund—which makes the earth's rotation in space responsible for the separation of electricities in the atmosphere. But Edlund's views are not tenable in theory, and, even granting his

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* F. Piper, 'Pogg. Ann.,' vol. lxxxii. p. 317.

† 'Die Gemeinschaftliche Ursache der Elektrischen Meteore und des Hagels,' H. W. Schmidt, Halle-a.-S.

deductions, the normal fall of potential should, according to the views of the author, have a different sign in the polar and equatorial regions, which is contrary to the observed fact. This theory does not, however, exhaust the possibility of explaining atmospheric electricity as a phenomenon of electromagnetic induction, and it is not disproved that in some form or other the rotation of the earth's magnetic field may play a part in the origin of the electric field. The theories which take solar radiation as the source of the energy divide themselves into several groups. We may think of a direct thermo-electric or actinic action, but there is, so far, no experimental support to such views. One of the earliest and most natural suppositions is the belief in evaporation as a source of electrification. This was Volta's theory, and experiments have at various times been produced in its support; but, so far, no one has been able to invalidate Faraday's conclusion that whenever electrification seemed to appear as a consequence of evaporation, it was really due to secondary causes, such as the friction of the liquid spray against the sides of the containing vessel. Rejecting Volta's theory, there is nothing left but the belief in some form of contact or frictional electricity either between drops of water and air, or water and ice, or any two of the various bodies present in the atmosphere. The possibility of contact electricity between a solid or liquid and a gas, is not quite easy to submit to the test of experiment. If we rub two solid bodies together, we may, by separating them, investigate the electric field produced; but, supposing we have a drop of water surrounded on all sides by air, the water may be covered with an electric layer of, say, positive electricity, the air in contact with the water with the opposite kind, and it is not at all clear how we could experimentally demonstrate the difference of potential between the air and the drop which is thus produced. A current of air flowing past the drop might carry away some of the negative layer, and in this way an electric field may be established while clouds are forming, but the conditions necessary for an experimental demonstration would be very difficult to realise. Two methods have been devised which practically demonstrate some form of contact electricity between gases and water.

Lenard, wishing to imitate the electric field observed in the neighbourhood of waterfalls, has established by careful experiment a number of important facts, which are all consistent with the following explanation. If we imagine two oppositely electrified layers at the surface of a drop of water such as has been referred to, and if the drop falls on to a layer of the same liquid, or if similar drops impinge on each other, the difference of potential produced by the fusion of the surface layers becomes greater than is consistent with equilibrium. For, taking the case of drops falling into a mass of water contained in a cylindrical vessel, the extent of surface between air and water is not increased by the falling drops, and we must imagine that surface to be already covered with a sufficient electrical sheet to establish the required difference of potential. The electrification of the drops is, therefore, not wanted, and a change in the distribution takes place. The natural supposition would be, that this equilibrium would be restored very quickly through the surface of the water, but a certain time seems to be required for this. Meanwhile, the strong current of air which in Lenard's experiments is brought down with the water drops carries some of the electricity away, the water remaining positive. More recent experiments of Lord Kelvin's, with air bubbling through water, point similarly to contact forces between gases and liquids, and in these experiments also it appears that a considerable time is required to establish electric equilibrium between a gas and a solid. Lenard finds very important differences caused by small impurities in the water, the water acting much more strongly when it is pure. If it contains as much salt as is contained in the sea, the effect is reversed, and the air becomes positively electrified. The explanation which is given above is practically that of Lenard, whose observations have been confirmed and further extended by Prof. J. J. Thomson. These experiments, no doubt, account for the behaviour of air in the neighbourhood of waterfalls, and they probably also explain the negative electrification of air in the neighbourhood of districts in which rain is falling. The strong positive electrification of mist may also be due to the same cause.

There seems to be no doubt that the formation of a cloud is often accompanied by electrical effects. A few years ago, descending from the Dent Blanche, I found myself, after sunset, at a height of about 12,000 feet. A current of air was apparently blowing up the valley which stretches from Evolena towards Ferpecle, and I could observe a cloud condensing below me at a height a little less than the snow-line. As night came on, and we continued our descent over the glacier and down the valley, a series of electric discharges were noticed between the cloud, which was lying in a deep-cut valley, the sides of the mountain, and the blue sky overhead. Here the moist air was evidently streaming through the cloud, depositing its moisture in the form of drops, and it seemed the most natural explanation at the time that the air left the cloud in an electrified state.

But while by means of experiments we have been able to produce some of the phenomena of atmospheric electricity, we have other important effects which cannot be accounted for in so simple a way. The electric discharges during a thunderstorm give evidence of electric fields, which could hardly be explained by contact electricity between drops of water and air alone. The fact that thunderstorms are nearly always connected with the formation of hail, and Faraday's experiments showing that water rubbing against ice becomes negatively electrified, is made use of in the theories of Sohnke and Luvini. It is quite likely that there is some truth in these theories. Their weak point lies in the difficulty of seeing how particles of ice and water can be first sufficiently mixed to allow of friction, and then become sufficiently separated to produce an electric field of such magnitude as we know must exist in a thunder cloud.

It is to be remarked, however, that the laws of contact electricity must be applicable to gases as well as to solids, and that if water becomes positive when rubbing against air, and negative when rubbing against ice, there must be a strong contact difference between ice and air. In other words, it does not matter whether there is direct friction between ice and water, or whether the air forms an intermediate body. We may imagine air rising through a cloud containing drops of water negatively electrified, and then passing through an ice cloud having its negative electricity increased, thus leaving the ice and water particles at a difference of potential which may, by a fusion of the drops, increase sufficiently to produce a lightning discharge. This seems to the writer the most plausible theory which, in the present state of our knowledge, can be formed. As regards the permanent negative charge of the earth's surface, the time has not yet arrived for forming a definite opinion. Although we know that the earth, once electrified, would gradually lose its charge into the atmosphere, yet we can express no opinion as to the rate at which the loss is going on. That loss may be exceedingly slow, and consequently equilibrium might be attained by a very small preponderance of negative electricity brought back to its surface through some cause or other. Rain, as has already been mentioned, is more frequently electrified negatively than positively in our own climate, and though we do not know how far this holds in the tropical belt, it is at any rate possible that the surface of the earth may in this way alone make up for the loss. We may also reasonably think that Lenard's observation on salt water may account for the permanent charge. Every wave that breaks into spray under the action of a strong wind would leave the water negatively electrified, the air carrying away the positive charge. It would be of great interest to possess observations on atmospheric electricity on board ship while waves are breaking in the neighbourhood. So far we have only Exner's observations to guide us, who found, while observing at Lavinia, in Ceylon, that the spray from breaking waves affected the indications of the electrometer, proving its positive electrification.*

But although the loss of electricity from the earth's surface may be very slow, it is equally possible that it is considerable. We shall not be able to treat this question satisfactorily until we have some clearer notion of the causes of the aurora. We know that the aurora implies electric currents, and the circuit of these currents may lie completely within the earth's atmosphere, and have nothing to do with the observed fall of potential near the ground. It is also possible that the body of the earth forms part of the electric circuit, and if that is the case, there must be across different parts of the surface an outward and inward flow of positive electricity. Such a discharge could not fail to influence the phenomena we have discussed, and it seems probable that we should have some evidence derived

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* 'Wiener Akad. Sitzungsberichte,' vol. xcviii.

from observation if the aurora was always accompanied by discharges through the earth's surface. Except in the polar regions, these auroræ do not seem to affect the normal fall of potential. There is a third view we may take as to the circulation of electric currents indicated by the aurora: the return current may take place in space outside the earth's atmosphere. A good deal might be said in favour of this view, and the rotation of the earth's magnetic field in space might be a sufficient cause for the production of these currents; but this is not the place to enter further into this question.

Calculations made from observation on the height of the aurora have generally resulted in an altitude of from 100 to 200 miles, except in the polar regions, where the aurora seems occasionally to descend to a much lower level. It has also been noticed that auroræ are associated with certain bands of cirrus clouds, and this seems to indicate that although the luminous phenomenon is sufficiently intense to be observed at only great heights, yet the electric phenomena may descend to the level of the cirrus.

As regards the connection between the aurora and the sunspot period, further observations in the polar regions are needed. On the one hand, we have Paulsen's * statement, derived from observations in Greenland, to the effect that the greatest number of auroræ are seen when sunspots are at their minimum, that is, at a time when in our own latitudes the number is smallest; and, on the other hand, we have Nordenskiöld's observations, which seem to point in the opposite direction. In a publication which contains much important matter on the geographical distribution and form of the aurora borealis, Nordenskiöld contrasts the appearances he has observed in the *Vega* during the winter of 1878–79, passed in the Behring Straits, with that previously observed in 1872–73 to the north of Spitzbergen. According to this author, the auroræ, during the minimum sunspot period in 1878–79, were "hardly worthy of his notice by the side of those observed in 1872–73." But although only faintly luminous, the auroræ of 1879 were persistent and regular in shape. They did not affect the magnetic field, and seem to show a regular and continuous, though weak, electric discharge. The arc and streamers in 1872 were much more brilliant and much more irregular. Some objection may be raised against these observations, in so far as they refer to different places, and local circumstances may have affected the phenomenon; but in the face of the very careful description he gives us, we cannot as yet accept Paulsen's results without further confirmation.

The problem of atmospheric electricity, like that of terrestrial magnetism, presents special features in the arctic regions, and until we possess a greater number of observations in those little accessible

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* Paulsen, 'Danske Videnskab. Selskabs Forhand.,' 1889. (I have not seen the original memoir, but only an abstract in the 'Jahrbuch der Astronomie und Geophysik,' 1890.)

parts of the earth's surface, many important problems cannot be satisfactorily solved. Arctic and antarctic expeditions are of interest to scientific men, not because they care much whether we get a few miles nearer the pole, but because a well-conducted party collects invaluable information on its journey. Although much remains to be done in the regions surrounding the north magnetic pole, our knowledge in the southern hemisphere is almost disgracefully inadequate, and it is to be hoped that before long a well-equipped expedition may fill up to a certain extent the large gaps in our electrical and magnetical knowledge which at present stop so many of our researches.

But although investigations to be conducted in the Arctic regions are of primary importance, we may do much nearer home in extending and completing existing information. Instrumental appliances and methods of observation, originally put into a satisfactory state by Lord Kelvin, have been improved, especially by Mascart, Exner, Elster and Geitel. One of our most crying wants at present is a series of continuous observations by means of self-registering instruments in places where the neighbourhood of a town, or other local circumstances, do not interfere with the normal changes. The Greenwich Observatory, to whom we look for help in such matters, is placed in the difficulty that the daily variations there observed are markedly different from those in the majority of places, and it is probable that the nearness of London is fatal to any generally useful series of observations of atmospheric electricity being conducted in our national Observatory.

[A. S.]

WEEKLY EVENING MEETING,

Friday, March 1, 1895.

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

The Rev. Canon Ainger, M.A. LL.D.

The Children's Books of a Hundred Years Ago.

(No Abstract.)

GENERAL MONTHLY MEETING,

Monday, March 4, 1895.

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

George S. Albright, Esq.
Edward George Betts, Esq. M.R.C.S.
Miss Catherine Emily Bradshaw.
Reginald Arthur Bray, Esq.
Matthew Bulloch, Esq.
George Bywaters, Esq.
G. Felix N. Clay, Esq. B.A.
Charles Scott Dickson, Esq. M.A.
Joseph J. Elliott, Esq.
James Garvie, Esq.
Mrs. Alexander Goschen.
Frederick Leverton Harris, Esq. M.A.
E. G. Harrison, Esq.
Stapleton C. Hogg, Esq.
Ernest Law, Esq.
His Grace The Duke of Newcastle.
George Henry Ogston, Esq.
Bertram Percy Portal, Esq.
Spencer John Portal, Esq.
Marmaduke Prickett, M.D. M.A.
Lieut.-Colonel William W. Rawes, R.A.
William Thomas Shaw, Esq.
John Henry Skelton, Esq.
James J. Walker, Esq.
William Hall Walker, Esq.
Arthur Fraser Walter, 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 at Low Temperatures :—

Sir William J. Farrer	£50
John Douglas Fletcher, Esq.	£100

The Special Thanks of the Members were returned to Mr. Hugh Spottiswoode, for a portrait of his father, the late Mr. William Spottiswoode, *M.R.I.*

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

PROFESSOR GEORGE FORBES, M.A. F.R.S. M. INST. C.E.—Three Lectures on ALTERNATING AND INTERRUPTED ELECTRIC CURRENTS; on *Tuesdays*, April 23, 30, May 7.

PROFESSOR E. RAY LANKESTER, M.A. LL.D. F.R.S.—Four Lectures on THIRTY YEARS' PROGRESS IN BIOLOGICAL SCIENCE; on *Tuesdays*, May 14, 21, 28, June 4.

PROFESSOR DEWAR, M.A. LL.D. F.R.S. *M.R.I.* Fullerian Professor of Chemistry, R.I.—Four Lectures on THE LIQUEFACTION OF GASES; on *Thursdays*, April 25, May 2, 9, 16.

WILLIAM HUGGINS, ESQ., D.C.L. LL.D. F.R.S. *M.R.I.*—Three Lectures on THE INSTRUMENTS AND METHODS OF SPECTROSCOPIC ASTRONOMY (The Tyndall Lectures); on *Thursdays*, May 23, 30, June 6.

ARNOLD DOLMETSCH, ESQ.—Three Lectures on MUSIC AND MUSICAL INSTRUMENTS OF THE 16TH, 17TH AND 18TH CENTURIES; 1. English. 2. French. 3. Italian. (With Illustrations upon Original Instruments.) On *Saturdays*, April 27, May 4, 11.

SEYMOUR LUCAS, Esq. A.R.A.—Two Lectures on PICTURE MAKING; on *Saturdays*, May 18, 25.

PROFESSOR EDWARD DOWDEN, D.C.L. LL.D. Professor of English Literature, Trinity College, Dublin.—Two Lectures on ELIZABETHAN LITERATURE. 1. The Pastoral. 2. The Masque; on *Saturdays*, June 1, 8.

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

FROM

Abney, Captain W. de W. C.B. D.C.L. F.R.S. (the Author)—Colour Vision, being the Tyndall Lectures delivered in 1894 before the Royal Institution. 8vo. 1895.

Accademia dei Lincei, Reale, Roma—Classe di Scienze Fisiche, Matematiche e Naturali. Atti, Serie Quinta: Rendiconti. 1° Semestre, Vol. IV. Fasc. 1-3. 8vo. 1895.

Classe di Scienze Morali, etc.: Rendiconti, Serie Quinta, Vol. III. Fasc. 10-12. 8vo. 1894.

American Academy—Proceedings, New Series, Vol. XXI. 8vo. 1894.

American Association—Proceedings, August Meeting, 1893, at Madison, Wisconsin. 8vo. 1894.

Asiatic Society of Bengal—Proceedings, 1894, No. 9. 8vo. 1894.

Journal, Vol. LXIII. Part 1, No. 3; Part 2, No. 3. 8vo. 1894.

Astronomical Society, Royal—Monthly Notices, Vol. LV. No. 3. 8vo. 1894.

Bankers, Institute of—Journal, Vol. XVI. Part 2. 8vo. 1895.

Basel Naturforschenden Gesellschaft—Verhandlungen, Band X. Heft 2. 8vo. 1894.

Bombay, the Under Secretary of Government, General Department—Progress Report of the Archæological Survey of Western India, 1893-94. 8vo. 1894.

Boston Public Library—Bulletin, New Series, Vol. V. No. 4. 8vo. 1895.

Boston Society of Natural History—Proceedings, Vol. XXVI. Parts 2, 3. 8vo. 1894.

Geology of the Boston Basis. By W. O. Crosby. Vol. I. Part 2. 8vo. 1894.

Memoirs, Vol. III. No. 14. 4to. 1894.

British Architects, Royal Institute of—Journal, 3rd Series, Vol. II. No. 3. 4to.

British Association—Report of Meeting at Oxford, 1894. 8vo. 1894.

British Astronomical Association—Journal, Vol. IV. No. 11; Vol. V. Nos. 3, 4. 8vo. 1895.
Camera Club—Journal for February, 1895. 8vo.
Chemical Industry, Society of—Journal, Vol. XIV. No. 1. 8vo. 1895.
Chemical Society—Journal for February, 1895. 8vo.
  Proceedings, Nos. 147, 148. 8vo. 1894.
Cracovie, l'Académie des Sciences—Bulletin, 1895, No. 1. 8vo.
Donisthorpe, Wordsworth, Esq. (the Author)—A System of Measures. 8vo. 1895.
Editors—American Journal of Science for February, 1895. 8vo.
  Analyst for February, 1895. 8vo.
  Athenæum for February, 1895. 4to.
  Brewers' Journal for February, 1895. 8vo.
  Chemical News for February, 1895. 4to.
  Chemist and Druggist for February, 1895. 8vo.
  Electrical Engineer for February, 1895. fol.
  Electrical Engineering for February, 1895. 8vo.
  Electrical Review for February, 1895. 8vo.
  Electric Plant for February, 1895. 4to.
  Electricity for February, 1895. 8vo.
  Engineer for February, 1895. fol.
  Engineering for February, 1895. fol.
  Engineering Review for February, 1895. 8vo.
  Horological Journal for February, 1895. 8vo.
  Industries and Iron for February, 1895. fol.
  Ironmongery for February, 1895. 4to.
  Law Journal for February, 1895. 8vo.
  Lightning for February, 1895. 8vo.
  Machinery Market for February, 1895. 8vo.
  Nature for February, 1895. 4to.
  Nuovo Cimento for February, 1895. 8vo.
  Open Court for February, 1895. 4to.
  Optician for February, 1895. 8vo.
  Photographic News for February, 1895. 8vo.
  Photographic Work for February, 1895. 8vo.
  Rasa-Ranga-Rahasya, Vol. I. No. 6. 8vo. 1894.
  Scots Magazine for February, 1895. 8vo.
  Technical World for February, 1895. 8vo.
  Transport for February, 1895. fol.
  Tropical Agriculturist for February, 1895.
  Work for February, 1895. 8vo.
  Zoophilist for February, 1895. 4to.
Florence, Biblioteca Nazionale Centrale—Bolletino, Nos. 219, 220. 8vo. 1894.
Franklin Institute—Journal, No. 830. 8vo. 1894.
Geographical Society, Royal—Geographical Journal for February, 1895. 8vo.
Geological Society—Quarterly Journal, No. 201. 8vo. 1895.
Gladstone, J. H. Esq. F.R.S. M.R.I. &c.—Records of the Tercentenary Festival of the University of Dublin in 1892. 4to. 1894.
  Tijdschrift van het Nederlandsch Aardrijkskundig Genootschap, Tweede Serie, Deel IX. X. 8vo. 1892–93.
Huggins, Mrs. M.R.I. (the Author)—The Astrolabe. 8vo. 1894.
Johns Hopkins University—American Chemical Journal, Vol. XVII. No. 2. 8vo. 1895.
Linnean Society—Journal, No. 210. 8vo. 1894.
London County Council (Technical Education Board)—London Technical Education Gazette, No. 5. 8vo. 1895.
Meteorological Society, Royal—Quarterly Journal, No. 93. 8vo. 1895.
  Meteorological Record, No. 54. 8vo. 1895.
Microscopical Society, Royal—Journal, 1895, Part 1. 8vo.

Middlesex Hospital—Reports for 1893. 8vo. 1894.

New South Wales, The Agent-General for—A Statistical Account of the Seven Colonies of Australasia. By G. A. Boulenger. 8vo. 1894.

New York Academy of Sciences—Transactions, Vol. XIII. 1893–94. 8vo.

New Zealand, The Registrar-General of—Statistics of the Colony of New Zealand for 1893. 8vo. 1894.

North of England Institute of Mining and Mechanical Engineers—Transactions, Vol. XLIV. Part 2. 8vo. 1895.

Numismatic Society—Chronicle and Journal, 1894, Part 4. 8vo.

Pharmaceutical Society of Great Britain—Journal for February, 1895. 8vo.

Philadelphia, Academy of Natural Sciences—Proceedings, 1894, Part 2. 8vo. 1894.

Photographic Society of Great Britain, Royal—Journal and Transactions, Vol. XIX. No. 6. 8vo. 1894.

Physical Society of London—Proceedings, Vol. XIII. Part 3. 8vo. 1895.

Prince, C. L. Esq. F.R.A.S. &c.—Summary of a Meteorological Journal. 8vo. 1894.

Roma, Ministero di Agricultura, Industria e Commercio—Statistica delle Biblioteche. 2 vols. 8vo. 1893–94.

Royal Colonial Institute—Catalogue of the Library. 8vo. 1895.

Royal Irish Academy—Cunningham Memoirs, No. X. 4to. 1894.

Royal Society of London—Proceedings, Nos. 341, 342. 8vo. 1894.

Saxon Society of Sciences,
Mathematisch-physische Classe, Band XXI. No. 3. 8vo. 1895.

Selborne Society—Nature Notes for February, 1895. 8vo.

Society of Antiquaries—Proceedings, 2nd Series, Vol. XV. No. 2. 8vo. 1894.

Society of Arts—Journal for February, 1895. 8vo.

St. Pétersbourg, Académie Impériale des Sciences—Bulletin, V^e Serie, Tome II. No. 1. 8vo. 1895.

Tacchini, Prof. P. Hon. Mem. R.I. (the Author)—Memorie della Società degli Spettroscopisti Italiani, Vol. XXIII. Disp. 12^a. 4to. 1895.

United Service Institution, Royal—Journal, No. 204. 8vo. 1895.

United States Patent Office—Official Gazette, Vol. LXIX. Nos. 10–12. 8vo. 1894.

Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1895, Heft. 1. 4to. 1895.

WEEKLY EVENING MEETING,

Friday, March 8, 1895.

Sir Frederick Abel, Bart. K.C.B. 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.*

The Physical Work of von Helmholtz.

The career we are to consider this evening was a career of singular distinction. In days when the range of "natural knowledge" is so vast that most workers are compelled to be content if they can add something to one or two of the sub-divisions of one of the main branches of science, von Helmholtz showed that it is not impossible to be at once a great mathematician, a great experimental physicist, and, in the widest sense of the term, a great biologist.

It was but eight months yesterday since he delivered his last lecture; it is six months to-day since he died, and the interval is too short for us to attempt to decide on the exact place which will be assigned to him by posterity; but making all allowance for the fact that each age is apt to place its own great among the greatest, making all allowance for the spell which his name cast over many of us in the lecture rooms where we ourselves first gained some knowledge of science, I am sure that I only express the views of all those who know his work best, when I say that we place him in the very front rank of those who have led the great scientific movement of our time. This opinion I have now to justify. I must try to convey to you in some sixty minutes an outline of the work of more than fifty strenuous years, to give you some idea of the wide range of the multifold activities which were crowded into them, of the marvellous insight with which the most diverse problems were attacked and solved, and, if it may be, some image of the man himself. The task is impossible, and I can but attempt some fragments of it.

The history of von Helmholtz is in one respect a simple tale. There are no life and death struggles with fate to record. His work was not done with the wolf at the door, or while he himself was wrestling with disease. He passed through no crises in which success or failure, immortality or oblivion, seemed to depend on the casting of a die. He suffered neither from poverty nor riches. He was a hale, strong man on whom external circumstances neither imposed exceptional disabilities, nor conferred exceptional advantages, but who, by sheer force of the genius that was in him, passed on from success to success till he was recognised by all as the admirable Crichton of modern science, the most widely cultivated of all students of nature, the acknowledged leader of German science, and one of the first scientific men in the world.

It is the more fitting that this evening should have been set aside for the consideration of the work of Helmholtz, in that England may claim some share in his greatness. Before her marriage his mother bore an English name—Caroline Penn; she was, as her name implied, of English descent. His father was a Professor of Literature in the Gymnasium at Potsdam, so that his early days were passed amid that plain living and high thinking which are characteristic of intellectual circles in Germany. The boy did well at school, and when the time came for choosing a profession, his passion for mathematics and physics had already developed itself. The course of his love for these sciences did not run quite smooth. The path of his ambition was crossed by the hard necessity which in some cases checks, in others fosters, but in all chastens the aspirations of youth. He had to make his livelihood. Science must be to him what the Germans happily call a "bread-study." Medicine offered a fair prospect of prosperity. Physics, in those days, was but an intellectual pastime. And so the young man took his father's advice, and became an army doctor. In this, as in so many other cases, "the path of duty was the way to glory."

It is possible that if von Helmholtz had been what—with a sad consciousness of the limitations it implies—I may call a mere physicist, he would have played a greater part in the development of some of those subjects, the study of which he initiated or helped to initiate, but did not thereafter pursue. It is possible that had he been a biologist, and nothing more, he would have followed up the early investigation in which he dealt a blow at the theory that putrefaction and fermentation are chemical processes only, clearly indicating, if he did not actually demonstrate, that the decay which follows death is due to an outburst of low forms of life.

He might thus under other circumstances have done work for which he showed his competence, but which is now chiefly associated with other names; but it is certain that without the unusual combination of wonderful mathematical power and a professional knowledge of anatomy, he would never have accomplished the special tasks which it is his special glory to have achieved.

His first three papers, however, hardly displayed the fusion between his various powers which was afterwards so remarkable a characteristic of his work. The first two were on biological subjects. The third was the famous essay on the 'Conservation of Force.' I have told elsewhere the story of the dramatic circumstances under which it was given to the world, of the interest it excited among the members of the Physical Society of Berlin, the refusal of the editor of *Poggendorff's Annalen* to publish it, and the final triumph of the author and his views.* Helmholtz was not, and did not claim to be an original author of the doctrine of the conservation of energy; but two young men, Sir William Thomson in England, and Helmholtz in Germany, independently, and within a month of each other, were the first persons who compelled the scientific world to regard it seriously.

There is one interesting fact which connects this essay directly with the Royal Institution. Four years after it was published, it was placed by Du Bois Reymond in the hands of one who was lost to science in the same year as von Helmholtz himself—the late Prof. Tyndall. He was much impressed, and has spoken of the incident as bringing him face to face with the great doctrine of the "Conservation of Energy." † He translated the essay into English, and for many years made it his habit to place every physical paper published by Helmholtz within the reach of English readers.

And now, having brought you to the point at which Helmholtz may be said to have been fairly started on his life's work, let me first describe briefly his official career, before I consider his achievements in greater detail.

When his extraordinary abilities became evident, he was permitted to sever his connection with the army. At twenty-seven years of age he became Teacher of Anatomy in the Academy of Arts at Berlin. In the next year he was appointed Professor of Anatomy and Physiology at Königsberg, and he held similar posts in the Universities of Bonn (1855–58) and Heidelberg (1858–71). It was not till 1871 that his early love for physics was finally rewarded. When the chair of Physics was to be filled in the University of the newly-founded German Empire, in Berlin, it was felt that even in Germany—the land of specialists—no better occupant could be found than one who was then in his fiftieth year, and who had been all his life a teacher of anatomy and physiology. The choice was universally approved and completely justified, and von Helmholtz held this post till his death.

In this connection I am, by the kindness of Sir Henry Roscoe, enabled to show to you a relic of remarkable interest. It is a photograph of the great teacher and investigator, taken at the very last lecture that he delivered—that, namely, on July 7, 1894.

For some years, that is, from the date of its foundation, von Helmholtz was the president of the Physikalisch-Technische Reichs-Anstalt in Charlottenburg. This institution, founded partly by the munificence of the late Dr. Werner Siemens, partly by funds supplied by the State, has no precise analogue in this country. It is devoted to the carrying out of systematic researches on questions of fundamental importance to which a long time must be devoted.

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* *Fortnightly Review*, November, 1894.

† 'Introduction to Popular Lectures by Helmholtz,' translated by E. Atkinson, 1873.

The most characteristic work of Helmholtz was, as I have already hinted, that in which his knowledge of physics and his knowledge of anatomy were both directed to a common end. He dealt in turns with the external physical phenomena, with the mechanism of the organs which the phenomena affect, with the relations between the mechanical effect on the organ and the sensations which it excites, and, lastly, with the connection between the sensations in those simple cases which can alone be investigated in the laboratory, and the complex laws of æsthetics and art.

The two books in which these problems were chiefly treated were the 'Physiological Optics,' and the 'Sensations of Sound.' It is impossible to do more than lay before you a sample which may afford some idea of the intricacy of the problems with which he dealt, and of the pitfalls amongst which he walked so warily. For this purpose I have chosen one branch of his work on 'Sound.'

I have deliberately selected that particular portion which has been most questioned, that on which the verdict of most of those who have sat in judgment on his views has been against him.

In discussing this question I must give a general description of the principle phenomena; but if I were to attempt an exhaustive catalogue of all the facts disputed and undisputed, and of all the theories which have been based upon or upset by them, not only would time fail me, but those who have not given special attention to the subject would, I fear, become hopelessly confused amid the chaos of opposing statements and views. Another reason which urges me to be brief, is that a few years ago Prof. Silvanus Thompson explained the whole subject to the members of the Royal Institution, having kindly consented to act as the mouthpiece of the celebrated instrument maker, König, who has played so large a part in these controversies.

Among the chief achievements of Helmholtz was an explanation of the physical difference between pairs of notes which we recognise as concords and discords respectively. When two neighbouring notes are sounded, alternate swellings and fallings off of the intensity are heard which are called beats. These produce an unpleasant effect, which depends partly on their number, partly on the relative pitches of the beating notes. When two notes beat badly, they form an intolerable discord. When they become separated by a wider interval, the beats are so rapid that they cease to be unpleasant.

The sense of dissonance produced by many of these wider intervals, such as the seventh (4 : 7), requires further explanation. In general, the fundamental musical note is only the first and loudest of a series of so-called partials, whose vibration frequencies are 2, 3, 4, &c. times that of the fundamental, and the consonance and dissonance of two notes is shown to depend on the presence or absence of beats between important members of these series. Thus in the case of the seventh the frequencies of the octave of the lower note and that of the upper note would be in the proportion 8 : 7, which are sufficiently near to make the beats very prominent and disturbing.

In cases where the notes are pure, that is, are not accompanied by upper partials, the explanation of dissonance is based upon another phenomenon.

When two notes are sounded simultaneously a third tone is often perceived, the frequency of which is equal to the difference of their frequencies. The number of vibrations of this tone is equal to the number of beats, and as there has been controversy as to whether the beats when they become rapid can produce a note, and if so, whether this note is or is not the same thing as the difference tone, it is necessary to distinguish between the two. This distinction is to be found in the mode of their production; but for the moment it is sufficient to remember that they may be distinguishable, and to reserve for them two names, viz. the beat-note, and the first difference tone respectively.

Helmholtz drew attention to the fact that besides the difference tone there is also produced a note, the frequency of which is equal to the sum of those of the two primaries, and this he called the first summation tone.

Together with these he believed that there existed summation and difference tones of higher orders, the whole series being included under the name of combination tones. Our sense of dissonance between pure notes was explained as dependent on beats produced by the combination tones.

Up to the time of Helmholtz it was generally thought that these tones were produced in the ear itself, and had no objective existence in the external air. They are thus often called subjective, but as that adjective is usually reserved for impressions produced in the brain itself, it is better to say that they were regarded as *ear-made*. Helmholtz himself gave a theory, which showed that it is probable that a membrane like the drum-skin of the ear, which is forced out of shape by pressure, and that bones, like those in the ear, which can rattle, would, if acted upon by two notes, manufacture by their own proper movements all the varied combinational tones which his theory postulated. He therefore believed that combinational tones were largely ear-made.

You will observe that his theory of discord is quite unaffected by the question whether the combination tones are or are not sometimes objective. Provided only they are produced at all, it is immaterial whether they are produced in the ear itself. Von Helmholtz admitted that the phenomena we observe are in most cases ear-made tones; but he also asserted that they were sometimes objective, and could set bodies tuned to vibrate with them in resonant motion. This latter statement has been denied with singular unanimity, sometimes, I think, without due regard to the limitations which Helmholtz himself placed on the conditions under which the objective character of the notes can be realised.

All ordinary calculations as to the production and mingling of different waves of sound are based upon the supposition that the displacements of the particles of air, or other body through which the sound is travelling, are very small. If this is so, the force which tends to restore each disturbed particle to its ordinary position of equilibrium is accurately proportional to the amount of the displacement.

In von Helmholtz' view, objective combination tones were in general produced when the disturbance was so great that this condition was no longer fulfilled. Violence is of the essence of the explanation. Hence the siren, where both sets of holes open into the same small wind chest—the harmonium, in which two reeds alternately close and open slits in the same enclosure, are the instruments best suited to produce them. Of these the siren is the more efficient. Von Helmholtz convinced himself that the combination tones produced by the harmonium are for the most part ear-made. He expressly stated that " when the places in which the two tones are struck are entirely separate and have no mechanical connection, as, for example, if they come from two singers, two separate wind instruments, or two violins "—to which we may add two tuning-forks —" the reinforcement of the combinational tones by resonators is small and dubious." *

Now this reinforcement by resonators has been altogether denied by most of those who have taken an interest in the matter, while, if an exception is allowed, it is in favour of the beats of a disturbed unison, the observed effects being ascribed to the beats, and not to the difference tone.

Some writers make no exception whatever in their denial of the objective reality of what may be broadly termed secondary tones. Thus Mr. Bosanquet, who made a most careful series of experiments some fourteen years ago, stated that " the ordinary first difference tone . . . is not capable of exciting a resonator. . . . In short, the difference tone of Helmholtz . . . as ordinarily heard, is not objective in its character." †

Prof. Preyer, too, using very sensitive tuning-forks, found that the differential tone given by two forks did not affect a fork the frequency of which corresponded with its own, except in cases where the difference tone was itself a partial of one of the forks.

It must be remembered that the assertions of Helmholtz as to the experimental proof of the objective nature of the tones were made with reference to those instruments which he regarded as most likely to produce objective notes, viz. the siren and the harmonium, and that, therefore, experiments with forks hardly affect his position.

Let us now try with the siren whether it is possible to confirm or to disprove the validity of his views.

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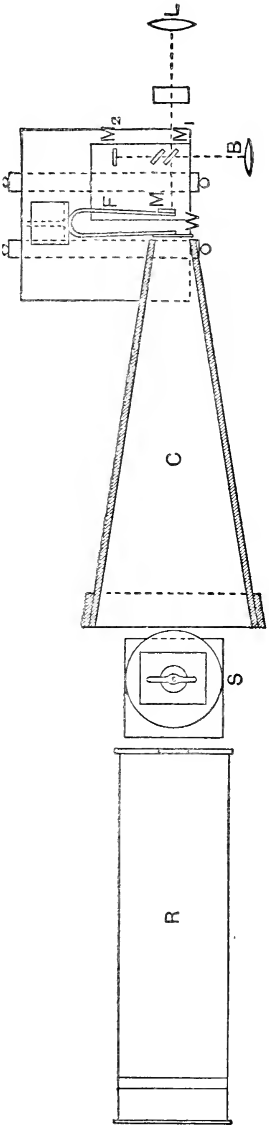
* 'Sensations of Tone,' translated by Ellis, p. 157.

† Proc. Phys. Soc., iv. 1881, p. 233.



For this purpose the rather bulky apparatus which you see before you has been constructed. I should hardly have been able to realise the idea embodied in it, at all events in time to show it to you this evening, if I had not been favourably situated in two respects. In the first place I have had the zealous co-operation of one of my assistants, Mr. Edwin Edser, who has not only made all the parts of the apparatus that required to be newly made, but has thrown himself into the investigation with the utmost energy, working at it late and early, and making many valuable suggestions and improvements. In our joint work we have been helped by some of my senior students, and notably by Messrs. Cullen and Forsyth. In the second place, I have had at my disposal the magnificent collection of acoustical apparatus in the National Museum at South Kensington, some of which I am allowed, by the kindness of the Department of Science and Art, to bring here this evening.

The essential part of the apparatus is a tuning-fork F, to one prong of which is attached a mirror M, and to the other a square of thin wood, strengthened by ribs, which is of the same weight as the mirror. The fork thus loaded has been compared with one of König's large standards by means of Lissajous' figures. Its frequency does not differ from 64 complete vibrations per second by more than one vibration in two minutes. The shank is supported by a mass of lead, which in turn is placed upon a paving-stone. Upon this stone also rest the other mirrors necessary for producing Michelson's interference bands. The mirror M_1 is silvered so thinly that half the light which falls upon it is reflected, and half is transmitted.

A ray proceeding from the lantern L will be divided at M_1 into two, which follow the paths $LM_1M_2M_1B$ and LM_1MM_1B respectively. Interference bands are thus produced which can be projected on to a screen, so as to be rendered visible to a large audience.

If the prong of the tuning-fork moves through the eighty-thousandth of an inch, that is, through a distance equal to a half wave-length of light, the path of the ray which falls upon it is shortened by a whole wave-length, and the position of each band is shifted to that previously held by its neighbour. If the fork vibrates with an amplitude of this almost infinitesimal amount, the bands will disappear, or will alternately appear and disappear according to circumstances. The fork may therefore be used to detect by resonance the presence of vibrations, the frequency of which is 64 *per second.*

A priori, there were two difficulties of opposite kinds which made it doubtful whether the fork would be an efficient weapon for the purpose for which it was to be used.

In the first place it would feel tremors of any sort, and it was doubtful whether it would be possible to discriminate between mere shakes and the vibrations which were to be studied. This difficulty has been very largely overcome.

The table on which the apparatus stands rests on india-rubber. On the table are a pair of library steps; these support two pieces of wood, which are heavily weighted and rest on india-rubber balls. From these two beams hang steel wires, which carry india-rubber door-fasteners, and these in turn support two rods on which the paving-stone is placed. By this alternation of elastic and of heavy bodies we can make the bands absolutely steady, unless the disturbances are violent. The quiet movements necessary for working the apparatus, the blowing of the bellows, and the like, produce no effect. On the other hand, the shutting of a door in a distant part of the building, the rumble of a cart in the street, will cause the bands to disappear. A great deal of the work on which we rely has been done at South Kensington between midnight and three o'clock in the morning. Trustworthy observations have indeed been made at other times, but it is only in the still small hours that the apparatus is at its best.

The second doubt was of a different kind. It was certain that the instrument would be more or less shaken; it was not quite certain whether the fork would respond to vibrations of the given period. It is easy to set a tuning-fork in vibration by resonance when it is mounted on a sounding box, but in that case the vibrations of the enclosed mass of air are communicated through the box to the fork. When the stalk of the fork is held rigidly, a tuning-fork is notoriously difficult to excite by resonance. This objection is, of course, to some extent counterbalanced by the extraordinary sensitiveness of the means of detecting the vibrations, but it is necessary to supplement this by other devices. The instrument used is a siren (S). In front of it is placed a hollow wooden pyramid, the narrow end of which is near to, and is of the same area as the wooden plate attached to the tuning-fork. This serves to collect the waves of sound and to concentrate them on the fork. Behind the siren is a large resonator by König, timed to respond to 64 vibrations per second.

In some respects the apparatus requires careful handling. Of course if you blow down the connecting cone the fork may be disturbed, and sometimes a particular note of the siren appears to affect the fork for no very obvious reason. Probably the resonance of the air in the cone, or the vibrations of the wooden disc, may at times be the causes of such effects. We have, however, found that whatever they may be due to, they differ in appearance from those produced by vibrations synchronous with the periodic time of the fork, and they can in general be got rid of by a very slight readjustment of the apparatus. The fact that our main conclusions do not depend on any such nicety, is proved by the fact that the instrument has been set up twice in the laboratory, and once in the lecture room in the College. In each case all the experiments have been successful, and on one occasion only were we troubled by a disturbance due to a note (of about 253 vibrations) when sounded alone. A slight readjustment of the cone, however, eliminated this effect entirely.

Such difficulties make it no easy matter to set up the apparatus in a hurry, and the most I can hope to do this evening is to demonstrate to you the methods of using it. I cannot undertake to make the actual measurements before you.

It is, however, desirable to illustrate the sensitiveness of the apparatus to vibrations of 64 per second, and its insensitiveness to other sounds.

Provided the current of air does not travel directly down the cone, organ pipes may be blown just outside it without producing any effect. One of König's large tuning-forks may be bowed strongly without effect.

If, however, the exciting fork be tuned to 64 vibrations per second, and if it be struck as lightly as possible with the handle of a small gimlet, used as a hammer, the handle having been previously covered with india-rubber, the bands will imemdiately vanish, though the note produced is often quite inaudible, even to a person whose ear is placed close to the fork.

Let the weights on the fork be shifted so that it makes 63·5 vibrations per second, then the resonating fork beats, and the bands regularly appear and disappear every two seconds.

Having thus explained the construction and working of the apparatus, let me show you how we have tested whether it responds to a different tone. When the proper rows of holes are opened, the siren will give simultaneously the c' of 256 and the e' of 320 vibrations. The interval is a major third, the difference tone is 64 vibrations. The pitch is determined by the beats between the upper note and a standard tuning-fork which gives e'. Sounding the upper note alone no effect is produced on the interference bands, as the beats first appear, then die out, and are finally heard again when the note given by the siren is too high.

It could be shown in like manner that the 256-note alone produces no effect, but if, when the standard fork of 320 vibrations and the upper note of the siren are judged to be in exact accord, the 256-note be also produced, the bands immediately disappear. Sometimes, of course, a small error is made in the estimate of the pitch, and the effect is not instantaneous, but in every case the bands disappear when the beats between the two notes are so slow that they cannot be distinguished.

It is therefore evident that Helmholtz was right when he asserted that the difference tone given by the siren is objective. It exists outside the ear, for it can move a tuning-fork.

König has shown that in many cases, when two notes are sounded simultaneously beats are heard, as though the most prominent phenomenon was the production of beats not between the two fundamental notes, but between the upper of these and the nearest partial of the lower note. Inasmuch as these beats are heard when the lower note (as far as can be tested) is free from upper partials, this rule is not the explanation of the phenomenon, but it is a convenient way of expressing the results. In the experiment just described, the frequencies of the two notes were in the ratio 12 to 15. The first partial of the lower note (12) is therefore the nearest to the higher tone; that is to say, König's beat tone and the first difference tone are identical.

It is easy to arrange an experiment in which these conditions are not fulfilled. Thus let the notes be in the ratio 9 : 15. The second partial of the lower note is 18, which is nearer to 15 than to 9; hence the König beat-tone would have a relative frequency of $18 - 15 = 3$. If the siren rotates 10·6 times per second, the frequencies of the two fundamental notes are $9 \times 10{\cdot}6 = 96$ and $15 \times 10{\cdot}6 = 160$ respectively. As before, the difference tone is 64.

In this case we can use another method of determining the speed of the siren. In 1880 Lord Rayleigh constructed an instrument in which the mass of air enclosed in a tube is excited by resonance, and the fact of the excitation is indicated by a light mirror, which is set where the motion is greatest, inclined at 45° to the direction of the air currents. In accordance with the general law that a lamina tends to place itself perpendicular to the direction of a stream, the mirror moves when the air vibrates. In the original apparatus the amount of the movement was controlled by magnets. Since that date Prof. Boys has modified the instrument by substituting a quartz thread suspension for a silk fibre, and using the torsion of the thread instead of the directing force of the magnets. In a lecture delivered before the British Association, in Leeds, he exhibited the apparatus, which is sometimes called a mirror resonator. Prof. Boys has been good enough to make two of these instruments for me, and for reasons which I will not at the moment enter into, we decided that one of them should respond to 161 vibrations per second. It so happens that this coincides almost exactly with the frequency of one of the notes in the experiment under discussion (160). It is thus possible to use the mirror resonator as an auxiliary instrument to test the speed of the siren. When the proper note is reached the spot of light will move, and if the difference tone is objective the interference bands ought to disappear simultaneously. We tried this experiment several times. An observer so placed that he could not see the interference bands, lifted his hand when the spot of light moved. It was quite extraordinary to note the absolute agreement between his movements and the behaviour of the bands.

By throwing the spot of light and the bands near together on the screen, the coincidence can be watched by a number of persons. We have tried whether the difference tone is objective in four cases, and in all have detected it by the disappearance of the interference bands. The details of the experiments are collected in the following table. In the first two experiments the first difference tone is, and in the last two it is not, coincident with König's lower beat note.

DIFFERENCE TONES.

Number of holes in siren.	Interval.	Frequencies.		Difference—and König's beat tones.	
15 and 12	Major third	320	256	64	64
16 „ 12	Fourth	256	192	64	64
15 „ 9	Major sixth	160	96	64	32
18 „ 8	An octave and a major tone	115·2	51·2	64	12·8

Of course the question at once arises whether, when it can be distinguished separately, König's beat tone is also objective. I do not wish to express a final opinion on this point, but I may say that when the rows of eight and eighteen holes were opened, the speed of the siren was increased till the notes corresponding to 256 and 576 vibrations were produced. König's note would in that case have a frequency of $576 - 2 \times 256 = 64$. We tried twice to obtain this. On the second occasion, especially, all the conditions were favourable, and the experiment was carried on for a long time. On neither occasion did we obtain the smallest sign of an effect on the fork and interference bands.

We must next turn to the summation tone which Helmholtz discovered. It has been almost universally denied that this note is objective. Without going into details, it is only necessary to remark that the late Mr. Ellis, the translator of the 'Tonempfindungen,' who took a dispassionate view of the controversy, thought that the position assumed by Helmholtz had been disproved. To the statement of Helmholtz that "it was formerly believed that the combinational tones were purely subjective and were produced in the ear itself," Ellis appended the note: "the result of Mr. Bosanquet's and Prof. Preyer's quite recent experiments is to show that they are so."

In an experiment on the summation tone, as the total number of vibrations must not exceed 64, the notes will be too low to be well heard. I shall therefore use a third method of determining the rate of speed of the siren. A mirror attached to the lower plate of the instrument rotates with it. Concentric with, and lying on this, is a circle of paper with eighteen cogs. Light reflected from the mirror passes through holes in two pieces of tinfoil attached to the prongs of a tuning-fork. When the fork is at rest, these holes are superposed; but when the fork vibrates, they move apart, are closed by the tinfoil, and only cross each other twice in each complete vibration. The tuning-fork makes 27·2 vibrations per second, and thus allows the light to pass 54·4 times per second. But when the siren makes 3·048 revolutions per second, the rows of nine and twelve holes give a summation tone of 64 vibrations, and each cog moves over $18 \times 3{\cdot}048 = 54{\cdot}9$, or say 55 times the distance between two consecutive cogs. If the wheel were viewed 55 times a second, the cogs would appear stationary, as in that interval each would be replaced by the next. As they are really seen about 54·4 times a second, they appear to move slowly forwards at the rate of about one interspace in two seconds. When this speed is attained the bands disappear, thus proving the objective exsistence of the summation tone.

We have repeated this observation in various ways, and always with success. The results are summed up in the table.

It is, perhaps, a drawback that all the notes in these experiments are very low. In order to remedy this, and also to put the matter to the test by means of another instrument, we have employed a mirror resonator which responds to 576 vibrations per second.

The rows of 15 and 12 holes being opened, notes of 320 and 256 vibrations were produced. When they were sounded separately, the noise seemed just to make the resonator move. When they were sounded together, the spot of light was driven off the scale, when the upper note coincided with that of a 320-vibration fork, but immediately returned when this pitch was lost.

SUMMATION TONES.

Numbers of holes in siren.	Interval.	Frequencies.		Sum.
10 and 8	Major third	$35{\cdot}\dot{5}$	$28{\cdot}\dot{4}$	64
12 „ 9	Fourth	36·57	27·43	64
16 „ 9	Minor seventh	40·96	23·04	64

The summation tone of 576 vibrations was also obtained by two other combinations of holes. The 320-fork was used, and the disturbance occurred in the one case when the pitch of the upper note given by the siren was nearly the same as before, and in the other case when it was about a tone higher.

The results are summed up in the table.

SUMMATION TONES.

Numbers of holes in siren.	Interval.	Frequencies.		Sum.
15 and 12	Major third	320	256	576
16 „ 12	Fourth	329·15	246·85	576
16 „ 9	Major sixth	360	216	576

I venture to think that these experiments prove the accuracy of von Helmholtz. They show that the siren, at all events, does produce objective tones, the frequencies of which coincide with those of the first difference and summation tones, and that this statement is valid as regards the difference tone, whether it is or is not coincident with König's beat tone.

I have now in one single case tried to convey to you some idea of the complexity of the problems with which von Helmholtz dealt. He was the first man who detected a relation between the surging mass of partials and combination tones and our sensations of concord and discord. The main facts of his theory are, I believe, generally accepted. On some points modern opinion has tended to stray from his views; one of these we have studied afresh this evening.

It was the fact that I had to deliver this discourse which led me to investigate the question anew, and therefore I felt bound to tell you the results we have at present attained. Had it not been for this, I should not have published them as yet. We have several improvements of the apparatus in view. We do not pretend to have covered the field. I do not, therefore, wish to generalise. My object has been to refute hasty generalisations. I am content if I have convinced you, as I have convinced myself, that Helmholtz was correct in stating that the siren produces objective tones whose frequencies are equal to the sum and difference of their primaries, and that the methods we have employed have brought to light no facts opposed to his view that these notes cannot be explained as secondary effects of partials, but as phenomena of the first order—in other words, as real combination tones.

But brief space now remains to discuss the vast remainder of his work, and as I have already published an appreciation of that,* I must content myself with trying to give you, in a few sentences, some idea of the range of his intellect.

His investigations on optics were not less important than those

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* 'Fortnightly Review,' November 1894.

on sound. He invented the ophthalmoscope, by which the oculist can study the inmost recesses of the eye. The theory of colour vision, the theory of binocular vision, the curious subjective effects which are produced when we deliberately deceive our own senses by the stereoscope; these subjects he made especially his own.

In the field of mathematics, he was the first to define the peculiar rotatory motion of a liquid known as vortex-motion. Great men had laid the foundations of hydrodynamics before him, but all had overlooked the importance and laws of the vortex. Since the memoir of Helmholtz was published, the subject has been widely studied. Lord Kelvin has originated the famous vortex-ring theory of matter; Prof. Fitzgerald has suggested that the ether may be a complex of vortices, or, as it has been called, a vortex-sponge.

On electricity he wrote much—on the theory of the galvanic cell, on electrolysis, on electromagnetism.

In England, at all events, we give the preference, as regards the last subject, to the theory and writings of our own Maxwell.

As I have already said, von Helmholtz, in an age of specialists, was a universal genius. His intellect could light on nothing which it did not illuminate. Hence, his opinions on side issues are of more than ordinary importance, his "obiter dicta" are worth attention, his popular lectures acquire a special interest. Let us for a few moments turn to these.

The watchword of Helmholtz in dealing with educational problems, is "freedom." Freedom for the student, freedom for the teacher.

In England, we are fond of insisting that there are certain things which everybody who aspires to academic rank must know; of hedging in our students by prescribed courses of study. We make them feel that general culture is an iron-bound safe, which they must wrench open before they can attain the gem of real knowledge, rather than a setting, without which the most profound acquirements seem unattractive and dull. Yet von Helmholtz, one of the most highly educated men, one of the most comprehensive geniuses of the latter end of the century, will have no set courses, except as a preparation for a definite profession, is proud that Germany has "retained the old conception of students, as that of young men responsible to themselves, striving after science of their own free will, to whom it is left to arrange their own plan of studies as they think best." Not content with having made the attainment of this ideal almost impossible for English students, doctrinaire educationalists are now beginning to throw their net around the teacher. It is claimed that as the student must go through a prescribed course of study in order to learn, so the teacher must be drilled and examined before he is allowed to teach. Whatever can be said for this plan as regards the less advanced class of teachers, who are to devote themselves to the instruction of children—and in this case I believe there is something to be said for it—it is quite opposed to von Helmholtz' view of what is best when the teaching is of university rank, and the students are men and women. Make it easy for whoever has given some proof of knowledge, and wants to teach, to try his hand; make it easy for the student to go to the teacher from whom he gains the most. Look for the best educational results, not necessarily from the best lecturer, but from the man who is in closest contact with his subject. Do not force your teacher on his audience, but do all you can to establish a bond of sympathy between them. Trust, in a word, to the free play of living forces, and not to the hampering restrictions of "necessary subjects" and "compulsory lectures." This is a paraphrase of the views which Helmholtz held, and he illustrated them by the history of this Institution itself.

"I have often," he said, "wondered that the Royal Institution of London, a private society which provides for its members and others short courses of lectures on the progress of natural science, should have been able to retain permanently the services of men of such scientific importance as Humphry Davy and Faraday. It was no question of great emoluments; these men were manifestly attracted by a select public, consisting of men and women of independent mental culture." And then he goes on to show that in a German university the teacher is attracted to his work, because he has to deal with a body of students who are capable of forming opinions, and of judging what is best for themselves.

And this leads us to another point. Von Helmholtz insisted that it is useless and dangerous to crowd the universities with students who are not capable of taking advantage of the opportunities they offer. "The majority of students," he says, "must come to us with a sufficiently logically trained judgment, with a sufficient habit of mental exertion, and with a taste sufficiently developed on the best models to be able to discriminate truth from the bubbling appearance of truth. . . . It would be very dangerous for the universities if large numbers of students frequented them who were less developed in [these] respects. The general self-respect of the students must not be allowed to sink. If that were the case, the dangers of academic freedom would choke its blessings. It must, therefore, not be looked upon as pedantry, or arrogance, if the universities are scrupulous in the admission of students of a different style of education. It would be still more dangerous if, for any extraneous reasons, teachers were introduced into the faculty who have not the complete qualifications of an independent academcal teacher." *

It would be out of place on this occasion to attempt to apply these views to existing circumstances in London; but with the knowledge that the final constitution of a Teaching University for the metropolis may be decided within the next few months, I cannot but feel that London will be happy if it escapes from the fetters which some of its so-called friends are forging for learning; and if, on the other hand,

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* 'Popular Lectures,' vol. ii. 1881, p. 264–5.

the wise determination of the Gresham Commissioners to include in the university only institutions of university rank, can be maintained against the attacks which will be made upon it.

Lastly, I wish to defend the memory of von Helmholtz from a possible misconception. Those who cultivate art may perhaps look upon him as the poet or the master of style look upon the grammarian; as a mere gerund-grinder, occupied with the study of the dead materials which they alone can use. Of course Helmholtz was not a great artist in the sense that he was a great scientific man, but it would be most unfair to picture him as interested only in the study of law, and as insensitive to beauty; as occupied with sound and light, but careless as to music and painting. I could quote passage after passage from his works to prove his keen sense of the loveliness as well as of the order of nature, to show the homage that he paid, and the freedom he accorded to art. His object was not to lead art captive to science, but rather to unite them in an alliance of mutual confidence and support.

"The horizons of physics, philosophy, and art," he said, "have been too widely separated, and, as a consequence, the language, the methods, and the aims of any one of these studies presents a certain amount of difficulty for the student of any other of them." To smooth away these difficulties, to bridge over the separating gulf, to supply the common language, were the objects of the life work of von Helmholtz. It was a noble ideal, nobly pursued, and crowned with as much success as could reward the efforts of one man. It is an ideal akin to that which dominates this Institution, where science, literature and art are all heard in turns.

If it is possible to sum up in a sentence the teaching of von Helmholtz, and the work of his life, it is that, in spite of the apparent diversities between science and science, between science, philosophy and art there is a fundamental unity, and that the future is for those who detect, amid the seeming discords of the schools, the true harmony which underlies and dominates them all.

[A. W. R.]

WEEKLY EVENING MEETING,

Friday, March 15, 1895.

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

Professor W. Chandler Roberts-Austen, C.B. F.R.S. *M.R.I.*

The Rarer Metals and their Alloys.

> " For reason is not the only attribute of man, nor is it the only faculty which he habitually employs for the ascertainment of truth."—G. J. Romanes.
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> " Appreciation . . . . by æsthetic and intellectual faculties which are not senses, and which are not unfrequently sadly wanting where the senses are in full vigour."—T. H. Huxley.

The study of metals possesses an irresistible charm for us, quite apart from its vast national importance. How many of us made our first scientific experiment by watching the melting of lead, little thinking that we should hardly have done a bad life's work if the experiment had been our last, provided we had only understood its full significance. How few of us forget that we wistfully observed at an early age the melting in an ordinary fire of some metallic toy of our childhood; and such an experiment has, like the " Flat iron for a farthing," in Mrs. Ewing's charming story, taken a prominent place in literature which claims to be written for the young. Hans Andersen's fairy tale, for instance, the " History of a Tin Soldier," has been read by children of all ages and of most nations. The romantic incidents of the soldier's eventful career need not be dwelt upon; but I may remind you that at its end he perished in the flames of an ordinary fire, and all that could subsequently be found of him was a small heart-shaped mass. There is no reason to doubt the perfect accuracy of the story recorded by Andersen, who at least knew the facts, though his statement is made in popular language. No analysis is given of the tin soldier; in a fairy tale it would have been out of place, but the latest stage of his evolution is described, and the record is sufficient to enable us to form the opinion that he was composed of both tin and lead, certain alloys of which metals will burn to ashes like tinder. His uniform was doubtless richly ornamented with gold lace. Some small amount of one of the rarer metals had probably—for on this point the history is silent—found its way into his constitution, and by uniting with the gold, formed the heart-shaped mass which the fire would not melt, as its temperature could not have exceeded 1000° C.; for we are told that the golden rose, worn by the *artiste* who shared the soldier's fate, was also found unmelted. The main point is, however, that the presence of one of the rarer metals must have endued the soldier with his singular endurance, and in the end left an incorruptible record of him.

This incident has been taken as the starting-point of the lecture, because we shall see that the ordinary metals so often owe remarkable qualities to the presence of a rarer metal which fits them for special work.

This early love of metals is implanted in us as part of our "unsquandered heritage of sentiments and ideals which has come down to us from other ages," but future generations of children will know far more than we did; for the attempt will be made to teach them that even psychology is a branch of molecular physics, and they will therefore see much more in the melted toy than a shapeless mass of tin and lead. It is really not an inert thing; for some time after it was newly cast, it was the scene of intense molecular activity. It probably is never molecularly quiescent, and a slight elevation of temperature will excite in it rapid atomic movement anew. The nature of such movement I have indicated on previous occasions when, as now, I have tried to interest you in certain properties of metals and alloys.

This evening I appeal incidentally to higher feelings than interest, by bringing before you certain phases in the life-history of metals which may lead you to a generous appreciation of the many excellent qualities they possess.

Metals have been sadly misunderstood. In the belief that animate beings are more interesting, experimenters have neglected metals, while no form of matter in which life can be recognised is thought to be too humble to receive encouragement. Thus it is that bacteria, with repulsive attributes and criminal instincts, are petted and watched with solicitude, and comprehensive schemes are submitted to the Royal Society for their development, culture, and even for their "education,"* which may, it is true, ultimately make them useful metallurgical agents, as certain micro-organisms have already proved their ability to produce arseniuretted hydrogen from oxide of arsenic.†

It will not be difficult to show that methods which have proved so fruitful in results when applied to the study of living things, are singularly applicable to metals and alloys, which really present close analogies to living organisms. This must be a new view to many, and it may be said, "it is well known that uneducated races tend to personify or animate external nature," and it is strange, therefore, to attempt, before a cultured audience, to trace analogies which must appear to be remote, between moving organisms and inert alloys, but "the greater the number of attributes that attach to anything, the more real that thing is." ‡ Many of the less known metals are very

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* Dr. Percy Frankland especially refers to the "education" of bacilli for adapting them to altered conditions. Roy. Soc. Proc. vol. lvi. 1894, p. 539.

† Dr. Brauner. Chem. News, Feb. 15, 1895, p. 79.

‡ Lotze, 'Metaphysic,' § 49, quoted by Illingworth. 'Personality, Human and Divine.' Bampton Lectures, 1894, p. 43.

real to me, and I want them to be so to you; listen to me, then, as speaking for my silent metallic friends, while I try to secure for them your sympathy, esteem and intuitive perception of their beauty.

First, as regards their origin and early history. I fully share Mr. Lockyer's belief as to their origin, and think that a future generation will speak of the evolution of metals as we now do of that of animals, and that observers will naturally turn to the sun as the field in which this evolution can best be studied.

To the alchemists metals were almost sentient; they treated them as if they were living beings, and had an elaborate pharmacopœia of "medicines" which they freely administered to metals in the hope of perfecting their constitution. If the alchemists constantly drew parallels between living things and metals, it is not because they were ignorant, but because they recognised in metals the possession of attributes which closely resemble those of organisms. "The first alchemists were gnostics, and the old beliefs of Egypt blended with those of Chaldea in the second and third centuries. The old metals of the Egyptians represented men, and this is probably the origin of the *homunculus* of the middle ages, the notion of the creative power of metals and that of life being confounded in the same symbol." *

Thus Albertus Magnus traces the influence of congenital defects in the generation of metals and of animals, and Basil Valentine symbolises the loss of metalline character, which we now know is due to oxidation, to the escape from the metal of an indestructible spirit which flies away and becomes a soul. On the other hand, the "reduction" of metals from their oxides was supposed to give the metals a new existence.† A poem of the thirteenth century well embodies this belief in the analogies between men and metals, in the quaint lines:—

> "Homs ont l'estre comme metaulx,
> Vie et augment des vegetaulx,
> Instinct et sens comme les bruts,
> Esprit comme ange en attributs."

"Men have being"—constitution—like metals; you see how closely metals and life were connected in the minds of the alchemists, and we inherit their traditions.

"Who said these old renowns, dead long ago, could make me forget the living world?" are words which Browning places in the lips of Paracelsus, and we metallurgists are not likely to forget the living world; we borrow its definitions, and apply them to our metals. Thus nobility in metals as in men, means freedom from

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* Berthelot, 'Les origines de l'alchimie,' 1885, p. 60.

† 'Les Remonstrances ou la complainte de nature a l'alchymiste errant.' Attributed to Jehan de Meung, who with Guillaume de Lorris wrote the 'Roman de la Rose.' M. Meon, the editor of the edition of 1814 of this celebrated work, doubts, however, whether the attribution of the *complainte de nature*, to Meung is correct.

liability to tarnish, and we know that the rarer metals are like rarer virtues, and have singular power in enduing their more ordinary associates with firmness, elasticity, strength and endurance. On the other hand, some of the less known metals appear to be mere "things" which do not exist for themselves, but only for the sake of other metals to which they can be united. This may, however, only seem to be the case because we as yet know so little about them. The question naturally arises, how can the analogies between organic and inorganic bodies now be traced? I agree with my colleague at the École des Mines of Paris, Prof. Urbain Le Verrier, in thinking that it is possible * to study the biology, the anatomy, and even the pathology of metals.

The anatomy of metals—that is, their structure and framework—is best examined by the aid of the microscope, but if we wish to study the biology and pathology of metals, the method of autographic pyrometry, which I brought before you in a Friday Evening lecture delivered in 1892, will render admirable service, for, just as in biological and pathological phenomena vital functions and changes of tissue are accompanied by a rise or fall in temperature, so molecular changes in metals are attended with an evolution or absorption of heat. With the aid of the recording pyrometer we now "take the temperature" of a mass of metal or alloy in which molecular disturbance is suspected to lurk, as surely as a doctor does that of a patient in whom febrile symptoms are manifest.

It has, moreover, long been known that we can submit a metal or an alloy in its normal state to severe stress, record its power of endurance, and then, by allowing it to recover from fatigue, enable it to regain some, at least, of its original strength. The human analogies of metals are really very close indeed, for, as is the case with our own mental efforts, the internal molecular work which is done in metals often strengthens and invigorates them. Certain metals have a double existence, and according to circumstances, their behaviour may be absolutely harmful or entirely beneficial. The dualism we so often recognise in human life becomes allotropism in metals, and they, strangely enough, seem to be restricted to a single form of existence if they are absolutely free from contamination, for probably an absolutely pure metal cannot pass from a normal to an allotropic state. Last, it may be claimed that some metals possess attributes which are closely allied to moral qualities, for, in their relations with other elements, they often display an amount of discrimination and restraint that would do credit to sentient beings.

Close as this resemblance is, I am far from attributing consciousness to metals, as their atomic changes result from the action of external agents, while the conduct of conscious beings is not determined from without, but from within. I have, however, ventured to offer the introduction of this lecture in its present form, because any

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* 'La Métallurgie en France,' 1894, p. 2.

facts which lead us to reflect on the unity of plan in nature, will aid the recognition of the complexity of atomic motion in metals upon which it is needful to insist.

The foregoing remarks have special significance in relation to the influence exerted by the rarer metals on the ordinary ones. With the exception of the action of carbon upon iron, probably nothing is more remarkable than the action of the rare metals on those which are more common; but their peculiar influence often involves, as we shall see, the presence of carbon in the alloy.

Which, then, are the rarer metals, and how may they be isolated? The chemist differs somewhat from the metallurgist as to the application of the word "rare." The chemist thinks of the "rarity" of a compound of a metal; the metallurgist, rather of the difficulty of isolating the metal from the state of combination in which it occurs in nature.

The chemist in speaking of the reactions of salts of the rarer metals, in view of the wide distribution of limestone and pyrolusite, would hardly think of either calcium or manganese as being among the rarer metals. The metallurgist would consider pure calcium or pure manganese to be very rare: I have only recently seen comparatively pure specimens of the latter.

The metals which, for the purposes of this lecture, may be included among the rarer metals are: (1) those of the platinum group, which occur in nature in the metallic state; and (2) certain metals which in nature are usually found as oxides or in an oxidised form of some kind, and these are chromium, manganese, vanadium, tungsten, titanium, zirconium, uranium and molybdenum (which occurs, however, as sulphide). Incidental reference will be made to nickel and cobalt.

Of the rare metals of the platinum group I propose to say but little; we are indebted for a magnificent display of them in the library to my friends Messrs. George and Edward Matthey and to Mr. Sellon, all members of a great firm of metallurgists. You should specially look at the splendid mass of palladium, extracted from native gold of the value of 2,500,000*l*., at the melted and rolled iridium, and at the masses of osmium and rhodium. No other nation in the world could show such specimens as these, and we are justly proud of them.

These metals are so interesting and precious in themselves, that I hope you will not think I am taking a sordid view of them by saying that the contents of the case exhibited in the library are certainly not worth less than ten thousand pounds.

As regards the rarer metals which are associated with oxygen, the problem is to remove the oxygen, and this is usually effected either by affording the oxygen an opportunity for uniting with another metal, or by reducing the oxide of the rare metal by carbon, aided by the tearing effect of an electric current. In this crucible there is an intimate mixture, in atomic proportions, of oxide of chromium and finely divided metallic aluminium. The thermo-junction (A, Fig. 1) of the pyrometer which formed the subject of my last Friday Evening lecture here, is placed within the crucible B, and the spot of light C, from the galvanometer D, with which the junction is connected, indicates on the screen that the temperature is rising. You will observe that as soon as the point marked 1010° is reached, energetic action takes place: the temperature suddenly rising above the melting-point of platinum, melts the thermo-junction, and the spot of light swings violently; but if the crucible be broken open, you will see that a mass of metallic chromium has been liberated.

The use of alkaline metals in separating oxygen from other metals is well known. I cannot enter into its history here, beyond saying that if I were to do so, frequent references to the honoured names of Berzelius, Wöhler and Winkler would be demanded.*

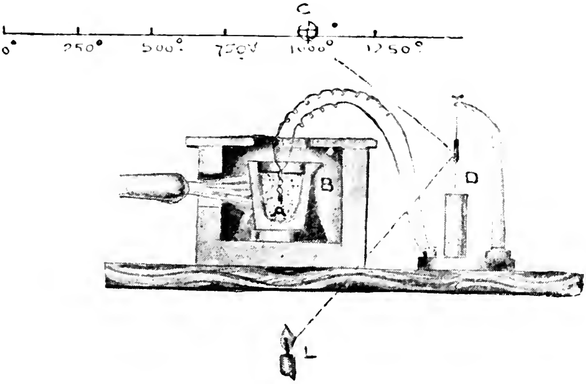

Fig. 1.

Mr. Vautin has recently shown that granulated aluminium may readily be prepared, and that it renders great service when employed as a reducing agent. He has lent me many specimens of rarer metals which have been reduced to the metallic state by the aid of this finely-granulated aluminium; and I am indebted to his assistant, Mr. Picard, who was lately one of my own students at the Royal School of Mines, for aid in the preparation of certain other specimens which have been isolated in my laboratory at the Mint.

The experiment you have just seen enables me to justify a statement I made respecting the discriminating action which certain metals appear to exert. The relation of aluminium to other metals is very singular. When, for instance, a small quantity of aluminium

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* An interesting paper, by H. F. Keller, on the reduction of oxides of metals by other metals, will be found in the 'Journal of the American Chemical Society,' December 1894, p. 833

is present in cast iron, it protects the silicon, manganese and carbon from oxidation.* The presence of silicon in aluminium greatly adds to the brilliancy with which aluminium itself oxidises and burns.† It is also asserted that aluminium, even in small quantity, exerts a powerful protective action against the oxidation of the silver zinc alloy, which is the result of the desilverisation of lead by zinc.

Moreover, heat aluminium in mass to redness in air, where oxygen may be had freely, and a film of oxide which is formed will protect the mass from further oxidation. On the other hand, if finely-divided aluminium finds itself in the presence of an oxide of a rare metal, at an elevated temperature, it at once acts with energy and promptitude, and releases the rare metal from the bondage of oxidation. I trust, therefore, you will consider my claim that a metal may possess moral attributes has been justified. Aluminium, moreover, retains the oxygen it has acquired with great fidelity, and will only part with it again by electrolytic action, or at very high temperatures under the influence of the electric arc in the presence of carbon.

[A suitable mixture of red-lead and aluminium was placed in a small crucible heated in a wind furnace, and in two minutes an explosion announced the termination of the experiment. The crucible was shattered to fragments.]

The aluminium loudly protests, as it were, against being entrusted with such an easy task, as the heat engendered by its oxidation had not to be used in melting a difficultly fusible metal like chromium, the melting point of which is higher than that of platinum.

It is admitted that a metal will abstract oxygen from another metal if the reaction is more exothermic than that by which the oxide to be decomposed was originally formed. The heat of formation of alumina is 391 calories, that of oxide of lead is 51 calories; so that it might be expected that metallic aluminium, at an elevated temperature, would readily reduce oxide of lead to the metallic state.

The last experiment, however, proved that the reduction of oxide of lead by aluminium is effected with explosive violence, the temperature engendered by the reduction being sufficiently high to volatilise the lead. Experiments of my own show that the explosion takes place with much disruptive power when aluminium reacts on oxide of lead *in vacuo*, and that if coarsely ground, fused litharge be substituted for red-lead, the action is only accompanied by a rushing sound. The result is, therefore, much influenced by the rapidity with which the reaction can be transmitted throughout the mass. It is this kind of experiment which makes us turn with such vivid interest to the teaching of the school of St. Claire Deville, the members of which have rendered such splendid services to physics and metallurgy. They do not advocate the employment of the

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* Bull. Soc. Chim. Paris, vol. xi. 1894, p. 377.

† Ditte, 'Leçons sur les Métaux,' part ii. 1891, p. 206.

mechanism of molecules and atoms in dealing with chemical problems, but would simply accumulate evidence as to the physical circumstances under which chemical combination and dissociation take place, viewing these as belonging to the same class of phenomena as solidification, fusion, condensation and evaporation. They do not even insist upon the view that matter is minutely granular, but in all cases of change of state, make calculations on the basis of work done, viewing changed "internal energy" as a quantity which should reappear when the system returns to the initial state.

A verse, of some historical interest, may appeal to them. It occurs in an old poem to which I have already referred as being connected with the 'Roman de la Rose,' and it expresses nature's protest against those who attempt to imitate her works by the use of mechanical methods. The "argument" runs thus:—

"Comme Nature se complaint,
Et dit sa douleur et son plaint
A ung sot souffleur sophistique
Qui n'use que d'art méchanique."

If the "use of mechanical art" includes the study of chemistry on the basis of the mechanics of the atoms, I may be permitted to offer the modern school the following rendering of nature's plaint:—

How nature sighs without restraint,
And grieving makes her sad complaint
Against the subtle sophistry
Which trusts atomic theory.

An explosion such as is produced when aluminium and oxide of lead are heated in presence of each other, which suggested the reference to the old French verse, does not often occur, as in most cases the reduction of the rarer metals by aluminium is effected quietly.

Zirconium is a metal which may be so reduced. I have in this way prepared small quantities of zirconium from its oxide, and have formed a greenish alloy of extraordinary strength by the addition of $\frac{2}{10}$ per cent. of it to gold, and there are many circumstances which lead to the belief that the future of zirconium will be brilliant and useful. I have reduced vanadium and uranium from its oxide by means of aluminium as well as manganese, which is easy, and titanium, which is more difficult. Tungsten, in fine specimens, is also before you, and allusion will be made subsequently to the uses of these metals. At present I would draw your attention to some properties of titanium which are of special interest. It burns with brilliant sparks in air; and, as few of us have seen titanium burn, it may be well to burn a little in this flame. [Experiment performed.] Titanium appears to be, from the recent experiments of M. Moissan, the most difficultly fusible metal known; but it has the singular property of burning in nitrogen—it presents, in fact, the only known instance of vivid combustion in nitrogen.*

Titanium may be readily reduced from its oxide by the aid of aluminium. Here are considerable masses, sufficiently pure for many purposes, which I have recently prepared in view of this lecture.

The other method by which the rarer metals may be isolated is that which involves the use of the electrical furnace. In this connection the name of Sir W. Siemens should not be forgotten. He described the use of the electric arc furnace in which the carbons were arranged vertically, the lower carbon being replaced by a carbon crucible; and in 1882 he melted in such a furnace no less than ten pounds of platinum during an experiment at which I had the good fortune to assist. It may fairly be claimed that the large furnaces with a vertical carbon in which the bath is maintained fluid by means of the electric current, the aluminium and other metals being reduced by electrolytic action, are the direct outcome of the work of Siemens.

In the development of the use of the electric arc for the isolation of the rare, difficultly fusible metals Moissan stands in the front rank. He points out † that Despretz ‡ used in 1849 the heat produced by the arc of a powerful pile; but Moissan was the first to employ the arc in such a way as to separate its heating effect from the electrolytic action it exerts. This he does by placing the poles in a horizontal position, and by reflecting their heat into a receptacle below them. He has shown, in a series of classical researches, that employing 800 ampères and 110 volts a temperature of at least 3500° may be attained, and that many metallic oxides which until recently were supposed to be irreducible may be readily made to yield the metal they contain.§

A support or base for the metal to be reduced is needed, and this is afforded by magnesia, which appears to be absolutely stable at the utmost temperatures of the arc. An atmosphere of hydrogen may be employed to avoid oxidation of the reduced metal, which, if it is not a volatile one, remains at the bottom of the crucible almost always associated with carbon—forming, in fact, a carbide of the metal. I want to show you the way in which the electric furnace is used, but,

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* Lord Rayleigh has since stated that titanium does not combine with argon; and M. Guntz points out that lithium in combining with nitrogen produces incandescence. M. Moissan has also shown that uranium does not absorb argon.

† Ann. de Chim. et de Phys. vol. iv. 1895, p. 365.

‡ Comptes Rendus, vol. xxviii. p. 755, and vol. xxix. 1849, pp. 48, 545, 712.

§ The principal memoirs of M. Moissan will be found in the Comptes Rendus, vol. cxv. 1892, p. 1031; ibid. vol. cxvi. 1893, pp. 347, 349, 549, 1222, 1225, 1429; ibid. vol. cxix. 1894, pp. 15, 20, 935; ibid. vol. cxx. 1895, p. 290. The more important of the metals he has isolated are uranium, chromium, manganese, zirconium, molybdenum, tungsten, vanadium and titanium. There is an important paper by him on the various forms of the electric furnace in the Ann. de Chim. et de Phys., vol. iv. 1895, p. 365.

unfortunately, the reductions are usually very tedious, and it would be impossible to actually show you much if I were to attempt to reduce before you any of the rarer metals; but as the main object is to show you how the furnace is used, it may be well to *boil* some silver at a temperature of some 2500°, and subsequently to melt chromium in the furnace (Fig. 2). This furnace consists of a clay receptacle A, lined with magnesia B. A current of 60 ampères and 100 volts is introduced by the carbon poles C, C; an electro-magnet M is provided to deflect the arc on to the metal to be melted. [By means of a lens and mirror D E the image of the arc and of the molten metal was projected on to a screen. For this purpose it was found convenient to make the furnace much deeper than would ordinarily be the case.]

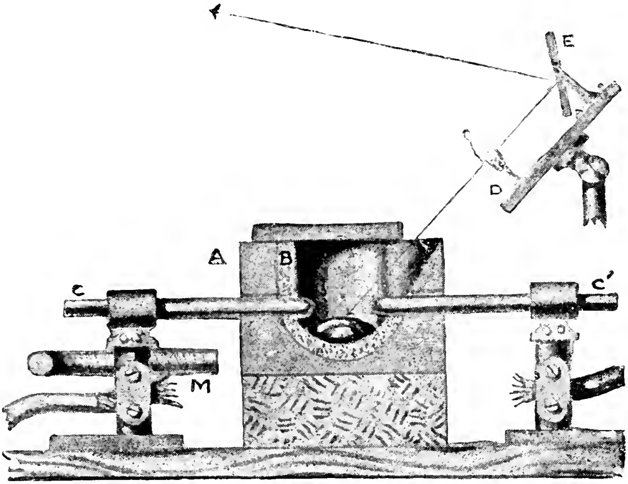

Fig. 2.

The result is very beautiful, but can only be rendered in dull tones by the accompanying illustrations (Figs. 3, 4). It may be well, therefore, to state briefly what is seen when the furnace is arranged for the melting of metallic chromium. Directly the current is passed, the picture reflected by the mirror E, Fig. 2, shows the interior of the furnace (Fig. 3) as a dark crater, the dull red poles revealing the metallic lustre and grey shadows of the metal beneath them. A little later these poles become tipped with dazzling white, and in the course of a few minutes the temperature rises to about 2500° C. Such a temperature will keep chromium well melted, though a thousand degrees more may readily be attained in a furnace of this kind. Each pole is soon surrounded with a lambent halo of

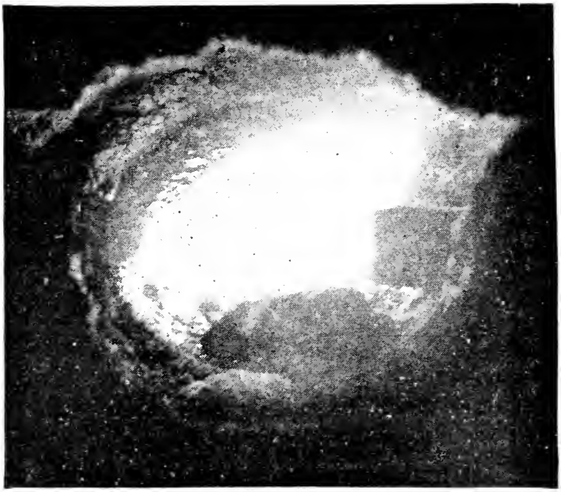

Fig. 3.—This represents the interior of the furnace containing molten chromium, as is seen either by reflection on a screen or by looking into the furnace from above, the eyes being suitably protected by deeply tinted glasses.

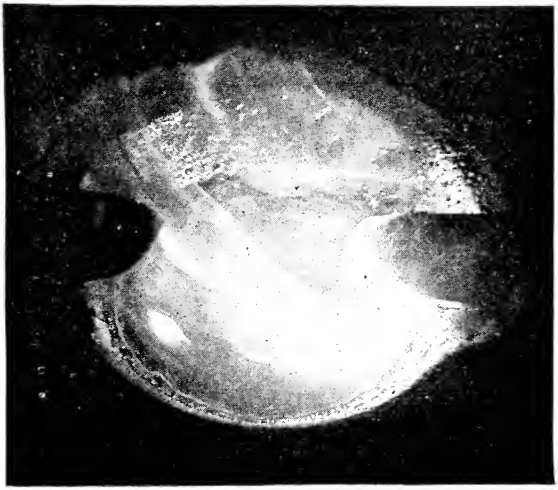

Fig. 4.—In this case the arc was broken the instant before the photograph was taken. The furnace contained a bath of silver just at its boiling point. The reflection of the poles in the bath, the globules of distilled silver, and the drifting cloud of silver vapour, are well shown.

the green-blue hue of the sunset, the central band of the arc changing rapidly from peach-blossom to lavender and purple. The arc can then be lengthened, and as the poles are drawn further and further asunder, the irregular masses of chromium fuse in silver droplets below an intense blue field of light, passing into green of lustrous emerald; then the last fragments of chromium melt into a shining lake, which reflects the glowing poles in a glory of green and gold, shot with orange hues. Still a few minutes later, as the chromium burns, a shower of brilliant sparks of metal are projected from the furnace, amid the clouds of russet or brown vapours which wreath the little crater; whilst if the current is broken, and the light dies out, you wish that Turner had painted the limpid tints, and that Ruskin might describe their loveliness.

The effect when either tungsten or silver (Fig. 4) replaces chromium is much the same, but, in the latter case, the glowing lake is more brilliant in its turbulent boiling, and blue vapours rise to be condensed in the iridescent beads of distilled silver which stud the crater walls.

Such experiments will probably lend a new interest to the use of the arc in connection with astronomical metallurgy, for, as George Herbert said long ago—

> "Stars have their storms even in a high degree,
> As well as we;"

and Lockyer has shown how important it is, in relation to such storms, to be able to study the disturbances in the various strata of the stellar or solar atmosphere. Layers of metallic vapour which differ widely in temperature can be more readily obtained by the use of the electrical furnace, than when a fragment of metal is melted and volatilised by placing it in the arc on the lower carbon.

It must not be forgotten that the use of the electric arc between carbon poles renders it practically impossible to prepare the rare metals without associating them with carbon, often forming true carbides; but it is possible in many cases to separate the carbon by subsequent treatment. Moissan has, however, opened up a vast field of industrial work by placing at our disposal practically all the rarer infusible metals which may be reduced from oxides, and it is necessary for us now to consider how we may best enter upon our inheritance. Those members of the group which we have known long enough to appreciate are chromium and manganese, and these we have only known free from carbon for a few months. In their carburised state they have done excellent service in connection with the metallurgy of steel; and may we not hope that vanadium, molybdenum, titanium, and uranium will render still greater services? My object in this lecture is mainly to introduce you to these metals, which hitherto few of us have ever seen except as minute cabinet specimens, and we are greatly indebted to M. Moissan for sending us beautiful specimens of chromium, vanadium, uranium, zirconium, tungsten, molybdenum and titanium. [These were exhibited.]

The question naturally arises: Why is the future of their usefulness so promising? Why are they likely to render better service than the common metals with which we have long been familiar? It must be confessed that as yet we know but little what services these metals will render when they stand alone; we have yet to obtain them in a state of purity, and have yet to study their properties, but when small quantities of any of them are associated or alloyed with other metals, there is good reason to believe that they will exert a very powerful influence. In order to explain this, I must appeal to the physical method of inquiry to which I have already referred.

It is easy to test the strength of a metal or of an alloy; it is also easy to determine its electrical resistance. If the mass stands these tests well, its suitability for certain purposes is assured; but a subtle method of investigation has been afforded by the results of a research entrusted to me by a committee of the Institution of Mechanical Engineers, over which Dr. Anderson, of Woolwich, presides. We can now gather much information as to the way in which a mass of metal has arranged itself during the cooling from a molten condition, which is the necessary step in fashioning it into a useful form; it is possible to gain insight into the way in which a molten mass of a metal or an alloy, molecularly settles itself down to its work, so to speak, and we can form conclusions as to its probable sphere of usefulness.

The method is a graphic one, such as this audience is familiar with, for Prof. Victor Horsley has shown in a masterly way that traces on smoked paper may form the record of the heart's action under the disturbing influence caused by the intrusion of a bullet into the human body. I hope to show you by similar records the effect, which though disturbing is often far from prejudicial, of the introduction of a small quantity of a foreign element into the "system" of a metal, and to justify a statement which I made earlier, as to the applicability of physiological methods of investigation to the study of metals. In order that the nature of this method may be clear, it must be remembered that if a thermometer or a pyrometer, as the case may be, is plunged into a mass of water or of molten metal, the temperature will fall continuously until the water or the metal begins to become solid; the temperature will then remain constant until the whole mass is solid, when the downward course of the temperature is resumed. This little thermo-junction is plunged into a mass of gold, an electric current is, in popular language, generated, and the strength of the current is proportional to the temperature to which the thermo-junction is raised; so that the spot of light from a galvanometer to which the thermo-junction is attached enables us to measure the temperature, or, by the aid of photography, to record any thermal changes that may occur in a heated mass of metal or alloy.

It is only necessary for our purpose to use a portion of the long scale, which may be traced across the end of the room by the spot of light from the galvanometer, but we must make that portion of the scale movable. Let me try to trace before you the curve of the freezing of pure gold. It will be necessary to mark the position occupied by the movable spot of light at regular intervals of time during which the gold is near 1045° C., that is, while the metal is becoming solid. Every time a metronome beats a second, the white screen A (Fig. 5), a sheet of paper, will be raised a definite number of inches by the gearing and handle B, and the position successively occupied by the spot of light C will be marked by hand.

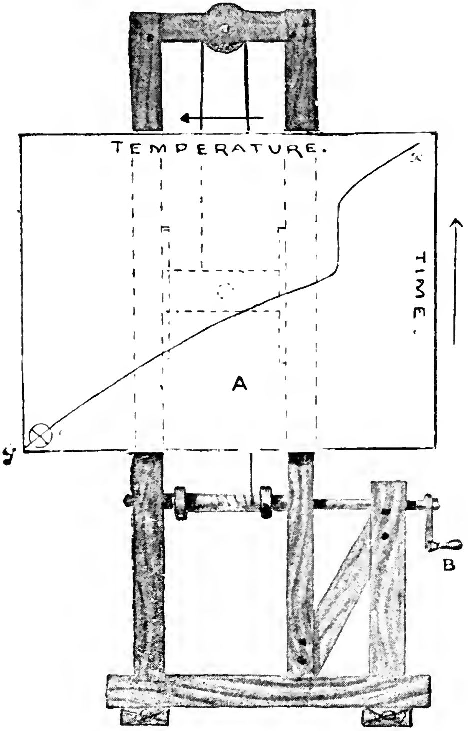

Fig. 5.

You see that the time-temperature curve, x y, so traced is not continuous. The freezing point of the metal is very clearly marked by the vertical portion. If the gold is very pure the angles are sharp, if it is impure they are rounded. If the metal had fallen below its freezing point without actually becoming solid, that is, if superfusion, or surfusion, had occurred, then there would be, as is often the case, a dip where the freezing begins, and then the temperature curve rises suddenly.

If the metal is alloyed with large quantities of other metals, then there may be several of these freezing points, as successive groups of alloys fall out of solution. The rough diagrammatic method is not sufficiently delicate to enable me to trace the subordinate points, but they are of vital importance to the strength of the metal or alloy, and photography enables us to detect them readily.

Take the case of the tin-copper series; you will see that as a mass of tin-copper alloy cools, there are at least two distinct freezing points. At the upper one the main mass of the fluid alloy became solid; at the lower, some definite group of tin and copper atoms fall out, the position of the lower point depending upon the composition of the mass.

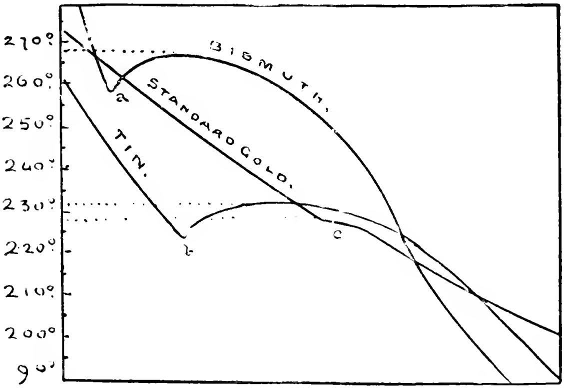

FIG. 6.

Now turn to more complex curves taken on one plate by making the sensitised photographic plate seize the critical part of the curve, the range of the swing of the mirror from hot to cold being some sixty feet. The upper curve (Fig. 6) gives the freezing point of bismuth, and you see that surfusion, *a*, is clearly marked, the temperature at which bismuth freezes being 268° C. The lower curve, marked "tin," represents the freezing point of that metal, which we know is 231° C., and in it surfusion, *b*, is also clearly marked. The curve marked standard gold contains a subordinate point, *c*, which you will observe is lower than the freezing point of tin, and it is caused by the solidification of a small portion of bismuth, which alloyed itself with some gold atoms, and remained fluid below the freezing point not only of bismuth itself but of tin. Now gold with a low freceing point in it like this is found to be very brittle, and we are in a fair way to answer the question why $\frac{2}{10}$ per cent. of zirconium doubles the strength of gold, while $\frac{2}{10}$ per cent. of thallium, another rare metal, halves the strength. In the case of the zirconium the subordinate point is very high up, while in the case of the thallium it is very low down. So far as my experiments have as yet been carried, this seems to be a fact which underlies the whole question of the strength of metals and alloys. If the subordinate point is low, the metal will be weak; if it is high in relation to the main setting point, then the metal will be strong, and the conclusion of the whole matter is this: The rarer metals which demand for their isolation from their oxides either the use of aluminium or the electric arc, never, so far as I can ascertain, produce low freezing points when they are added in small quantities to those metals which are used for constructive purposes. The difficultly fusible rarer metals are never the cause of weakness, but always confer some property which is precious in industrial use. How these rarer metals act, why the small quantities of the added rare metals permeate the molecules, or, it may be the atoms, and strengthen the metallic mass, we do not know; we are only gradually accumulating evidence which is afforded by this very delicate physiological method of investigation.

As regards the actual temperatures represented by points on such curves, it will be remembered that the indications afforded by the recording pyrometer are only relative, and that gold is one of the most suitable metals for enabling a high, fixed point to be determined. There is much trustworthy evidence in favour of the adoption of 1045° as the melting point hitherto accepted for gold. The results of recent work indicate, however, that this is too low, and it may prove to be as high as 1061·7, which is the melting point given by Heycock and Neville * in the latest of their admirable series of investigations to which reference was made in my Friday Evening lecture of 1892.

It may be well to point to a few instances in which the industrial use of such of the rarer metals, as have been available in sufficient quantity, is made evident. Modern developments in armour plate and projectiles will occur to many of us at once. This diagram (Fig. 7) affords a rapid view of the progress which has been made; and in collecting the materials for it from various sources, I have been aided by Mr. Jenkins. The effect of projectiles of approximately the same weight, when fired with the same velocity against 6-inch plates, enables comparative results to be studied, and illustrates the fact that the rivalry between artillerists who design guns, and metallurgists who attempt to produce both impenetrable armour-plates and irresistible projectiles, forms one of the most interesting pages in our national history. When metallic armour was first applied to the sides of war vessels, it was of wrought iron, and proved to be of very great service by absolutely preventing the passage of ordinary cast-iron shot into the interior of the vessel, as was demonstrated through

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* Trans. Chem. Soc., vol. lxvii. 1895, p. 160.

the American Civil War in 1866. It was found to be necessary, in order to pierce the plates, to employ harder and larger projectiles than those then in use, and the chilled cast-iron shot with which Colonel Palliser's name is identified proved to be formidable and effective. The point of such a projectile was sufficiently hard to retain its form under impact with the plate, and it was only necessary to impart a moderate velocity to a shot to enable it to pass through the wrought-iron armour (A, Fig. 7).

It soon became evident that in order to resist the attack of such projectiles with a plate of any reasonable thickness, it would be necessary to make the plate harder, so that the point of the projectile should be damaged at the moment of first contact, and the reaction to the blow distributed over a considerable area of the plate. This object could be attained by either using a steel plate in a more or less hardened condition, or by employing a plate with a very hard face of steel, and a less hard but tougher back. The authorities in this country during the decade, 1880–90, had a very high opinion of plates that resisted attack without the development of through-cracks, and this led to the production of the compound plate. The backs of these plates (B, Fig. 7) are of wrought iron, the fronts are of a more or less hard variety of steel, either cast on, or welded on by a layer of steel of an intermediate quality cast between the two plates. Armour-plates of this kind differ in detail, but the principle of their construction is now generally accepted as correct.

Such plates shown by the plate B, resisted the attack of large Palliser shells admirably, as when such shells struck the plate they were damaged at their points, and the remainder of the shell was unable to perforate the armour against which it was directed. An increase in the size of the projectiles led, however, to a decrease in the resisting power of the plates, portions of the hard face of which would at times be detached in flakes from the junction of the steel and the iron. An increase in the toughness of the projectiles by a substitution of forged chrome-steel for chilled iron (see lower part of plate B), secured a victory for the shot, which was then enabled to impart its energy to the plate faster than the surface of the plate itself could transmit the energy to the back. The result was that the plate was overcome, as it were, piecemeal; the steel surface was not sufficient to resist the blow itself, and was shattered, leaving the projectile an easy victory over the soft back. The lower part of plate B (in Fig. 7) represents a similar plate to that used in the *Nettle* trials of 1888.* It must not be forgotten, in this connection, that the armour of a ship is but little likely to be struck twice by heavy projectiles in the same place, although it might be by smaller ones.

Plates made entirely of steel, on the other hand, were found, prior to 1888, to have a considerable tendency to break up com-

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* Proceedings Institution of Civil Engineers, 1889, vol. xcviii. p. 1 *et seq.*

pletely when struck by the shot. It was not possible, on that account, to make their faces as hard as those of compound plates; but while they did not resist the Palliser shot nearly as well as the rival compound plate, they offered more effective resistance to steel shot (see lower part of plate C, Fig. 7).

ATTACK OF 6-INCH ARMOUR-PLATES BY 4·72-INCH SHELLS, WEIGHING 57·2 LBS.

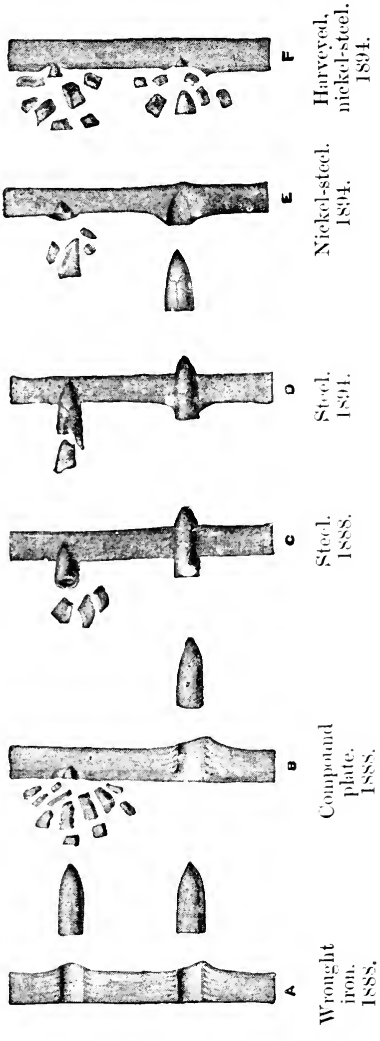

FIG. 7.—The upper series of projectiles are Palliser chilled-iron shells, and the lower are chrome steel. In each case the velocity of the projectile is approximately 1610 foot-seconds, and the energy 1070 foot-tons.

It appears that Berthier recognised, in 1820, the great value of chromium when alloyed with iron; but its use for projectiles, although now general, is of comparatively recent date, and these projectiles now commonly contain from 1·2 to 1·5 per cent. of chromium, and will hold together even when they strike steel plates at a velocity of 2000 feet per second * (see lower part of plate D); and unless the armour-plate is of considerable thickness, such projectiles will even carry bursting charges of explosives through it. [The behaviour of a chromium-steel shell, made by Mr. Hadfield, was dwelt upon, and the shell was exhibited.]

It now remained to be seen what could be done in the way of toughening and hardening the plates so as to resist the chrome-steel shot. About the year 1888, very great improvements were made in the production of steel plates. Devices for hardening and tempering plates were ultimately obtained, so that the latter were hard enough throughout their substance to give them the necessary resisting power without such serious cracking as had occurred in previous ones. But in 1889, Mr. Riley exhibited, at the meeting of the Iron and Steel Institute, a thin plate that owed its remarkable toughness to the presence of nickel in the steel. The immediate result of this was that plates could be made to contain more carbon, and hence be harder, without at the same time having increased brittleness; such plates, indeed, could be water-hardened and yet not crack.

The plate E (Fig. 7) represents the behaviour of nickel-steel armour. It will be seen that it is penetrated to a much less extent than in the former case; at the same time there is entire absence of cracking.

Now as to the hardening processes. Evrard had developed the use of the lead bath in France, while Captain Tressider † had perfected the use of the water-jet in England for the purpose of rapidly cooling the heated plates. The principle adopted in the design of the compound plates has been again utilised by Harvey, who places the soft steel or nickel-steel plate in a furnace of suitable construction, and covers it with carbonaceous material such as charcoal, and strongly heats it for a period, which may be as long as 120 hours. This is the old Sheffield process of cementation, and the result is to increase the carbon from 0·35 per cent. in the body of the plate to 0·6 per cent. or even more at the front surface, the increase in the amount of carbon only extending to a depth of 2 or 3 inches in the thickest armour.

The carburised face is then "chill-hardened," the result being that the best chrome-steel shot are shattered at the moment of impact, unless they are of very large size as compared with the thickness of the plate. The interesting result was observed lately ‡ of shot doing less harm to the plate and penetrating less, when its

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* 'Journal U.S. Artillery,' 1893, vol. ii. p. 497.

† Weaver, "Notes on Armour," 'Journal U.S. Artillery,' vol. iii. 1894, p. 417.

‡ Brassey's 'Naval Annual,' 1894, p. 367.

velocity was increased beyond a certain value, a result due to a superiority in the power of the face of the plate to transmit energy over that possessed by the projectile, which was itself damaged, when a certain rate was exceeded. At a comparatively low velocity the point of the shot would resist fracture, but the energy of the projectile is not then sufficient to perforate the plate, which would need the attack of a much larger gun firing a projectile at a lower velocity.

The tendency to-day is to dispense with nickel, and to use ordinary steel, "Harveyed";[§] this gives excellent 6-inch plates, but there is some difference of opinion as to whether it is advantageous to omit nickel in the case of very thick plates, and the problem is now being worked out by the method of trial. Probably, too, the Harveyed plates will be much improved by judicious forging after the process, as is indicated by some recent work done in America. The use of chromium in the plates may lead to interesting results.

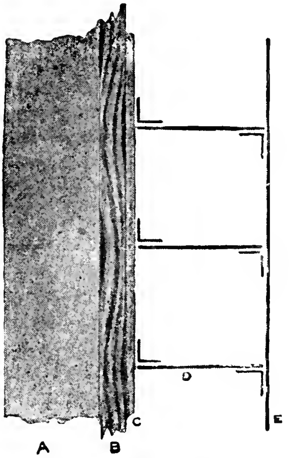

FIG. 8.—Section of Barbette of the *Majestic*.

Turn for a moment to the *Majestic* class of ships, the construction of which we owe to the genius of Sir William White, to whom I am indebted for a section representing the exact size of the protection afforded to the barbette of the *Majestic*. [This section was exhibited and is shown as reduced to the diagram, Fig. 8.] Her armour is of the Harveyed steel, which has hitherto proved singularly resisting to chromium projectiles.

In this section A represents a 14-inch Harveyed steel armour-plate, B a 4-inch teak backing, C a $1\frac{1}{4}$-inch steel plate, D $\frac{1}{2}$-inch steel frames, and E $\frac{1}{2}$-inch steel linings.

It will, I trust, have been evident that two of the rarer metals, chromium, in the projectiles, and nickel in the armour, are playing a very important part in our national defences; and if I ever lecture to you again, it may be possible for me to record similar triumphs for molybdenum, titanium, vanadium, and others of these still rarer metals.

Here is another alloy, for which I am indebted to Mr. Hadfield. It is iron alloyed with 25 per cent. of nickel, and Hopkinson has shown that its density is permanently reduced by 2 per cent. by an exposure to a temperature of -30°, that is, the metal expands at this temperature.

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[*] 'Engineering,' vol. lvii. 1894, pp. 465, 530, 595.

Supposing, therefore, that a ship-of-war was built in our climate of ordinary steel, and clad with some 3000 tons of such nickel-steel armour, we are confronted with the extraordinary fact that if such a ship visited the Arctic regions, it would actually become some 2 feet longer, and the shearing which would result from the expansion of the armour by exposure to cold would destroy the ship. Before I leave the question of the nickel-iron alloys, let me direct your attention to this triple alloy of iron, nickel and cobalt in simple atomic proportions. Dr. Oliver Lodge believes that this alloy will be found to possess very remarkable properties; in fact, as he told me, if nature had properly understood Mendeléef, this alloy would really have been an element. As regards the electrical properties of alloys, it is impossible to say what services the rarer metals may not render; and I would remind you that "platinoid," mainly a nickel-copper alloy, owes to the presence of a little tungsten its peculiar property of having a high electrical resistance which does not change with temperature.

One other instance of the kind of influence the rarer metals may be expected to exert is all that time will permit me to give you. It relates to their influence on aluminium itself. You have heard much of the adoption of aluminium in such branches of naval construction as demand lightness and portability. During last autumn Messrs. Yarrow completed a torpedo-boat which was built of aluminium alloyed with 6 per cent. of copper. Her hull is 50 per cent. lighter, and she is 3½ knots faster than a similar boat of steel would have been, and, notwithstanding her increased speed, is singularly free from vibration.

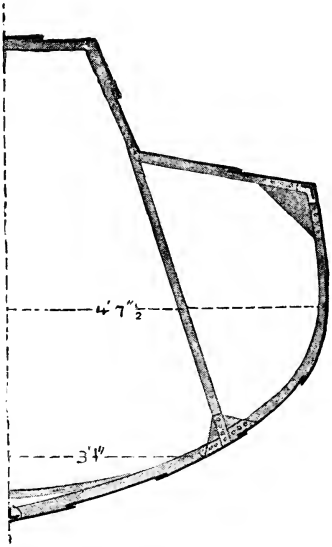

Fig. 9.—Half-section Midship of Aluminium Torpedo-boat.

Her plates are $\frac{1}{10}$ inch thick, and $\frac{1}{6}$ inch where greater strength is needed. It remains to be seen whether copper is the best metal to alloy with aluminium. Several of the rarer metals have already been tried, and among them titanium. Two per cent. of this rare metal seems to confer remarkable properties on aluminium, and it should do so according to the views I have expressed, for the cooling curve of the titanium-aluminium alloy would certainly show a high subordinate freezing point.

Hitherto I have appealed to industrial work, rather than to abstract science, for illustrations of the services which the rarer metals may render. One reason for this is that at present we have but little knowledge of some of the rarer metals apart from their association with carbon. The metals yielded by treatment of oxides in the electric arc are always carburised. There are, in fact, some of the rarer metals which we, as yet, can hardly be said to know except as carbides. As the following experiment is the last of the series, I would express my thanks to my assistant, Mr. Stansfield, for the great care he has bestowed in order to ensure their success. Here is the carbide of calcium which is produced by heating lime and carbon in the electric arc. It possesses great chemical activity, for if it is placed in water the calcium seizes the oxygen of the water, while the carbon also combines with the hydrogen, and acetylene is the result, which burns brilliantly. [Experiment shown.] If the carbide of calcium be placed in chlorine water, evil-smelling chloride of carbon is formed.

In studying the relations of the rarer metals to iron, it is impossible to dissociate them from the influence exerted by the simultaneous presence of carbon; but carbon is a protean element—it may be dissolved in iron, or it may exist in iron in any of the varied forms in which we know it when it is free. Matthiessen, the great authority on alloys, actually writes of the "carbon-iron alloys." I do not hesitate, therefore, on the ground that the subject might appear to be without the limits of the title of this lecture, to point to one other result which has been achieved by M. Moissan. Here is a fragment of pig iron highly carburised; melt it in the electric arc in the presence of carbon, and cool the molten metal suddenly, preferably by plunging it into molten lead. Cast iron expands on solidification, and the little mass will become solid at its surface and will contract; but when, in turn, the still fluid mass in the interior cools, it expands against the solid crust, and consequently solidifies under great pressure. Dissolve such a mass of carburised iron in nitric acid to which chlorate of potash is added; treat the residue with caustic potash, submit it to the prolonged attack of hydrofluoric acid, then to boiling sulphuric acid, and finally fuse it with potash, to remove any traces of carbide of silicon, and you have carbon left, but—in the form of *diamonds*.

If you will not expect to see too much, I will show you some diamonds I have prepared by strictly following the directions of M. Moissan. As he points out, these diamonds, being produced under stress, are not entirely without action on polarised light, and they have, sometimes, the singular property of flying to pieces like Rupert's drops when they are mounted as preparations for the microscope. [The images of many small specimens were projected on the screen from the microscope, and Fig. 10, E, shows a sketch of one of these. The largest diamond yet produced by M. Moissan is 0·5 millimetre in diameter.]

A (Fig. 10) represents the rounded, pitted surface of a diamond, and B a crystal of diamond from the series prepared by M. Moissan, drawings of which illustrate his paper.* The rest of the specimens, C to F, were obtained by myself by the aid of his method as above described: C represents a dendritic growth apparently composed of hexagonal plates of graphite, while D is a specimen of much interest, as it appears to be a hollow sphere of graphitic carbon, partially crushed in. Such examples are very numerous, and their surfaces are covered with minute round graphitic pits and prominences of great brilliancy. Specimen E (which, as already stated, was one of a series shown to the audience) is a broken crystal, probably a tetrahedron, and is the best crystallised specimen of diamond I have as yet succeeded in preparing. Minute diamonds, similar to A, may be readily produced, and brilliant fragments, with the lamellar structure shown in F, are also often met with.

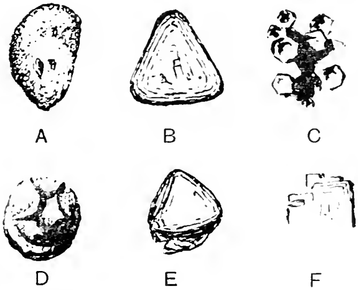

Fig. 10.—Preparations for the microscope of diamonds and other forms of carbon obtained from carburised iron.

The close association of the rarer metals and carbon and their intimate relations with carbon, when they are hidden with it in iron, enabled me to refer to the production of the diamond, and afford a basis for the few observations I would offer in conclusion. These relate to the singular attitude towards metallurgical research maintained by those who are in a position to promote the advancement of science in this country. Statements respecting the change of shining graphite into brilliant diamond are received with appreciative interest; but, on the other hand, the vast importance of effecting similar molecular changes in metals is ignored.

We may acknowledge that "no nation of modern times has done so much practical work in the world as ourselves, none has applied itself so conspicuously or with such conspicuous success to the indefatigable pursuit of all those branches of human knowledge which give to man his mastery over matter." † But it is typical of our

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* Comptes Rendus, vol. cxviii. 1894, p. 324.

† 'The Times,' February 22, 1895.

peculiar British method of advance to dismiss all metallurgical questions as "industrial," and leave their consideration to private enterprise.

We are fortunately to spend, I believe, eighteen millions this year on our Navy, and yet the nation only endows experimental research in all branches of science with four thousand pounds. We rightly and gladly spend a million on the *Magnificent*, and then stand by while manufacturers compete for the privilege of providing her with the armour-plate which is to save her from disablement or destruction. We as a nation are fully holding our own in metallurgical progress, but we might be doing so much more. Why are so few workers studying the rarer metals and their alloys? Why is the crucible so often abandoned for the test tube? Is not the investigation of the properties of alloys precious for its own sake, or is our faith in the fruitfulness of the results of metallurgical investigation so weak that, in its case, the substance of things hoped for remains unsought for and unseen in the depths of obscurity in which the metals are left?

We must go back to the traditions of Faraday, who was the first to investigate the influence of the rarer metals upon iron, and to prepare the nickel-iron series of which so much has since been heard.* He did not despise research which might possibly tend to useful results, but joyously records his satisfaction at the fact that a generous gift from Wollaston of certain of the "scarce and more valuable metals" enabled him to transfer his experiments from the laboratory in Albemarle Street to the works of a manufacturer at Sheffield.

Faraday not only began the research I am pleading for to-night, but he gave us the germ of the dynamo, by the aid of which, as we have seen, the rarer metals may be isolated. If it is a source of national pride that research should be endowed apart from the national expenditure, let us, while remembering our responsibilities, rest in the hope that metallurgy will be well represented in the Laboratory which private munificence is to place side by side with our historic Royal Institution.

[W. C. R.-A.]

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* In the development of the use of these alloys, the Société Ferro-Nickel and Les Usines du Creuzot, deserve special mention.

WEEKLY EVENING MEETING,

Friday, March 22, 1895.

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

Sir Wemyss Reid, LL.D.

Emily Brontë.

(Abstract deferred.)

WEEKLY EVENING MEETING,

Friday, March 29, 1895.

Hugo Müller, Esq., Ph.D. F.R.S. Vice-President, in the Chair.

Professor H. E. Armstrong, LL.D. Ph.D. F.R.S. Pres. C.S.

The Structure of the Sugars and their Artificial Production.

(Abstract deferred.)

GENERAL MONTHLY MEETING,

Monday, April 1, 1895.

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

Lockett Agnew, Esq.
Julius Althaus, M.D.
Harry Hankey Dobree, Esq.
James Marsh Johnstone, 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

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WEEKLY EVENING MEETING,

Friday, April 5, 1895.

Sir Frederick Bramwell, Bart. D.C.L. LL.D. 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. Professor of Natural Philosophy, R.I.

Argon.

It is some three or four years since I had the honour of lecturing here one Friday evening upon the densities of oxygen and hydrogen gases, and upon the conclusions that might be drawn from the results. It is not necessary, therefore, that I should trouble you to-night with any detail as to the method by which gases can be accurately weighed. I must take that as known, merely mentioning that it is substantially the same as is used by all investigators nowadays, and introduced more than fifty years ago by Regnault. It was not until after that lecture that I turned my attention to nitrogen; and in the first instance I employed a method of preparing the gas which originated with Mr. Vernon Harcourt, of Oxford. In this method the oxygen of ordinary atmospheric air is got rid of with the aid of ammonia. Air is bubbled through liquid ammonia, and then passed through a red-hot tube. In its passage the oxygen of the air combines with the hydrogen of the ammonia, all the oxygen being in that way burnt up and converted into water. The excess of ammonia is subsequently absorbed with acid, and the water by ordinary desiccating agents. That method is very convenient; and, when I had obtained a few concordant results by means of it, I thought that the work was complete, and that the weight of nitrogen was satisfactorily determined. But then I reflected that it is always advisable to employ more than one method, and that the method that I had used—Mr. Vernon Harcourt's method—was not that which had been used by any of those who had preceded me in weighing nitrogen. The usual method consists in absorbing the oxygen of air by means of red-hot copper; and I thought that I ought at least to give that method a trial, fully expecting to obtain forthwith a value in harmony with that already afforded by the ammonia method. The result, however, proved otherwise. The gas obtained by the copper method, as I may call it, proved to be one-thousandth part heavier than that obtained by the ammonia method; and, on repetition, that difference was only brought out more clearly. This was about three years ago. Then, in order, if possible, to get further light upon a discrepancy which puzzled me very much, and which, at that time, I regarded only with disgust and impatience, I published a letter in 'Nature' inviting criticisms from chemists who might be interested in such questions. I obtained various useful suggestions, but none going to the root of the matter. Several persons who wrote to me privately were inclined to think that the explanation was to be sought in a partial dissociation of the nitrogen derived from ammonia. For, before going further, I ought to explain that, in the nitrogen obtained by the ammonia method, some—about a seventh part—is derived from the ammonia, the larger part, however, being derived as usual from the atmosphere. If the chemically-derived nitrogen were partly dissociated into its component atoms, then the lightness of the gas so prepared would be explained.

The next step in the enquiry was, if possible, to exaggerate the discrepancy. One's instinct at first is to try to get rid of a discrepancy, but I believe that experience shows such an endeavour to be a mistake. What one ought to do is to magnify a small discrepancy with a view to finding out the explanation; and, as it appeared in the present case that the root of the discrepancy lay in the fact that part of the nitrogen prepared by the ammonia method was nitrogen out of ammonia, although the greater part remained of common origin in both cases, the application of the principle suggested a trial of the weight of nitrogen obtained wholly from ammonia. This could easily be done by substituting pure oxygen for atmospheric air in the ammonia method, so that the whole, instead of only a part, of the nitrogen collected should be derived from the ammonia itself. The discrepancy was at once magnified some five times. The nitrogen so obtained from ammonia proved to be about one-half per cent. lighter than nitrogen obtained in the ordinary way from the atmosphere, and which I may call for brevity "atmospheric" nitrogen.

That result stood out pretty sharply from the first; but it was necessary to confirm it by comparison with nitrogen chemically derived in other ways. The Table before you gives a summary of such results, the numbers being the weights in grams actually contained under standard conditions in the globe employed.

ATMOSPHERIC NITROGEN.

By hot copper (1892)	2·3103
By hot iron (1893)	2·3100
By ferrous hydrate (1894)	2·3102
Mean	2·3102

CHEMICAL NITROGEN.

From nitric oxide	2·3001
From nitrous oxide	2·2990
From ammonium nitrite purified at a red heat	2·2987
From urea	2·2985
From ammonium nitrite purified in the cold	2·2937
Mean	2·2990

The difference is about 11 milligrams, or about one-half per cent.; and it was sufficient to prove conclusively that the two kinds of nitrogen—the chemically-derived nitrogen and the atmospheric nitrogen—differed in weight, and therefore, of course, in quality, for some reason hitherto unknown.

I need not spend time in explaining the various precautions that were necessary in order to establish surely that conclusion. One had to be on one's guard against impurities, especially against the presence of hydrogen, which might seriously lighten any gas in which it was contained. I believe, however, that the precautions taken were sufficient to exclude all questions of that sort, and the result, which I published about this time last year, stood sharply out, that the nitrogen obtained from chemical sources was different from the nitrogen obtained from the air.

Well, that difference, admitting it to be established, was sufficient to show that some hitherto unknown gas is involved in the matter. It might be that the new gas was dissociated nitrogen, contained in that which was too light, the chemical nitrogen—and at first that was the explanation to which I leaned; but certain experiments went a long way to discourage such a supposition. In the first place, chemical evidence—and in this matter I am greatly dependent upon the kindness of chemical friends—tends to show that, even if ordinary nitrogen could be dissociated at all into its component atoms, such atoms would not be likely to enjoy any very long continued existence. Even ozone goes slowly back to the more normal state of oxygen; and it was thought that dissociated nitrogen would have even a greater tendency to revert to the normal condition. The experiment suggested by that remark was as follows: to keep chemical nitrogen—the too light nitrogen which might be supposed to contain dissociated molecules—for a good while, and to examine whether it changed in density. Of course it would be useless to shut up gas in a globe and weigh it, and then, after an interval, to weigh it again, for there would be no opportunity for any change of weight to occur, even although the gas within the globe had undergone some chemical alteration. It is necessary to re-establish the standard conditions of temperature and pressure which are always understood when we speak of filling a globe with gas, for I need hardly say that filling a globe with gas is but a figure of speech. Everything depends upon the temperature and pressure at which you work. However, that obvious point being borne in mind, it was proved by experiment that the gas did not change in weight by standing for eight months—a result tending to show that the abnormal lightness was not the consequence of dissociation.

Further experiments were tried upon the action of the silent electric discharge—both upon the atmospheric nitrogen and upon the chemically-derived nitrogen—but neither of them seemed to be sensibly affected by such treatment; so that, altogether, the balance of evidence seemed to incline against the hypothesis of abnormal lightness in the chemically-derived nitrogen being due to dissociation, and to suggest strongly, as almost the only possible alternative, that there must be in atmospheric nitrogen some constituent heavier than true nitrogen.

At that point the question arose, what was the evidence that all the so-called nitrogen of the atmosphere was of one quality? And I remember—I think it was about this time last year, or a little earlier—putting the question to my colleague, Professor Dewar. His answer was that he doubted whether anything material had been done upon the matter since the time of Cavendish, and that I had better refer to Cavendish's original paper. That advice I quickly followed, and I was rather surprised to find that Cavendish had himself put this question quite as sharply as I could put it. Translated from the old-fashioned phraseology connected with the theory of phlogiston, his question was whether the inert ingredient of the air is really all of one kind; whether all the nitrogen of the air is really the same as the nitrogen of nitre. Cavendish not only asked himself this question, but he endeavoured to answer it by an appeal to experiment.

I should like to show you Cavendish's experiment in something like its original form. He inverted a U tube filled with mercury, the legs standing in two separate mercury cups. He then passed up, so as to stand above the mercury, a mixture of nitrogen, or of air, and oxygen; and he caused an electric current from a frictional electrical machine like the one I have before me to pass from the mercury in the one leg to the mercury in the other, giving sparks across the intervening column of air. I do not propose to use a frictional machine to-night, but I will substitute for it one giving electricity of the same quality of the construction introduced by Mr. Wimshurst, of which we have a fine specimen in the Institution. It stands just outside the door of the theatre, and will supply an electric current along insulated wires, leading to the mercury cups; and, if we are successful, we shall cause sparks to pass through the small length of air included above the columns of mercury. There they are; and after a little time you will notice that the mercury rises, indicating that the gas is sensibly absorbed under the influence of the sparks and of a piece of potash floating on the mercury. It was by that means that Cavendish established his great discovery of the nature of the inert ingredient in the atmosphere, which we now call nitrogen; and, as I have said, Cavendish himself proposed the question, as distinctly as we can do, is this inert ingredient all of one kind? and he proceeded to test that question. He found, after days and weeks of protracted experiment, that, for the most part, the nitrogen of the atmosphere was absorbed in this manner, and converted into nitrous acid; but that there was a small residue remaining after prolonged treatment with sparks, and a final absorption of the residual oxygen. That residue amounted to about $\frac{1}{120}$ part of the nitrogen taken; and Cavendish draws the conclusion that, if there be more than one inert ingredient in the atmosphere, at any rate the second ingredient is not contained to a greater extent than $\frac{1}{120}$ part.

I must not wait too long over the experiment. Mr. Gordon tells me that a certain amount of contraction has already occurred; and if we project the U upon the screen, we shall be able to verify the fact. It is only a question of time for the greater part of the gas to be taken up, as we have proved by preliminary experiments.

In what I have to say from this point onwards, I must be understood as speaking as much on behalf of Professor Ramsay as for myself. At the first, the work which we did was to a certain extent independent. Afterwards we worked in concert, and all that we have published in our joint names must be regarded as being equally the work of both of us. But, of course, Professor Ramsay must not be held responsible for any chemical blunder into which I may stumble to-night.

By his work and by mine the heavier ingredient in atmospheric nitrogen which was the origin of the discrepancy in the densities has been isolated, and we have given it the name of "argon." For this purpose we may use the original method of Cavendish, with the advantages of modern appliances. We can procure more powerful electric sparks than any which Cavendish could command by the use of the ordinary Ruhmkorff coil stimulated by a battery of Grove cells; and it is possible so to obtain evidence of the existence of argon. The oxidation of nitrogen by that method goes on pretty quickly. If you put some ordinary air, or, better still, a mixture of air and oxygen, in a tube in which electric sparks are made to pass for a certain time, then in looking through the tube, you observe the well-known reddish-orange fumes of the oxides of nitrogen. I will not take up time in going through the experiment, but will merely exhibit a tube already prepared (image on screen).

One can work more efficiently by employing the alternate currents from dynamo machines which are now at our command. In this Institution we have the advantage of a public supply; and if I pass alternate currents originating in Deptford through this Ruhmkorff coil, which acts as what is now called a "high potential transformer," and allow sparks from the secondary to pass in an inverted test tube between platinum points, we shall be able to show in a comparatively short time a pretty rapid absorption of the gases. The electric current is led into the working chamber through bent glass tubes containing mercury, and provided at their inner extremities with platinum points. In this arrangement we avoid the risk, which would otherwise be serious, of a fracture just when we least desired it. I now start the sparks by switching on the Ruhmkorff to the alternate current supply; and, if you will take note of the level of the liquid representing the quantity of mixed gases included, I think you will see after, perhaps, a quarter of an hour that the liquid has very appreciably risen, owing to the union of the nitrogen and the oxygen gases under the influence of the electrical discharge, and subsequent absorption of the resulting compound by the alkaline liquid with which the gas space is enclosed.

By means of this little apparatus, which is very convenient for operations upon a moderate scale, such as for analyses of "nitrogen" for the amount of argon that it may contain, we are able to get an absorption of about 80 cubic centimetres per hour, or about 4 inches along this test tube, when all is going well. In order, however, to obtain the isolation of argon on any considerable scale by means of the oxygen method, we must employ an apparatus still more enlarged. The isolation of argon requires the removal of nitrogen, and, indeed, of very large quantities of nitrogen, for, as it appears, the proportion of argon contained in atmospheric nitrogen is only about 1 per cent., so that for every litre of argon that you wish to get you must eat up some hundred litres of nitrogen. That, however, can be done upon an adequate scale by calling to our aid the powerful electric discharge now obtainable by means of the alternate current supply and high potential transformers.

In what I have done upon this subject I have had the advantage of the advice of Mr. Crookes, who some years ago drew special attention to the electric discharge or flame, and showed that many of its properties depended upon the fact that it had the power of causing, upon a very considerable scale, a combination of the nitrogen and the oxygen of the air in which it was made.

I had first thought of showing in the lecture room the actual apparatus which I have employed for the concentration of argon; but the difficulty is that, as the apparatus has to be used, the working parts are almost invisible, and I came to the conclusion that it would really be more instructive as well as more convenient to show the parts isolated, a very little effort of imagination being then all that is required in order to reconstruct in the mind the actual arrangements employed.

First, as to the electric arc or flame itself. We have here a transformer made by Pike and Harris. It is not the one that I have used in practice; but it is convenient for certain purposes, and it can be connected by means of a switch with the alternate currents of 100 volts furnished by the Supply Company. The platinum terminals that you see here are modelled exactly upon the plan of those which have been employed in practice. I may say a word or two on the question of mounting. The terminals require to be very massive on account of the heat evolved. In this case they consist of platinum wire doubled upon itself six times. The platinums are continued by iron wires going through glass tubes, and attached at the ends to the copper leads. For better security, the tubes themselves are stopped at the lower ends with corks and charged with water, the advantage being that, when the whole arrangement is fitted by means of an indiarubber stopper into a closed vessel, you have a witness that, as long as the water remains in position, no leak can have occurred through the insulating tubes conveying the electrodes.

Now, if we switch on the current and approximate the points sufficiently, we get the electric flame. There you have it. It is, at present, showing a certain amount of soda. That in time would burn off. After the arc has once been struck, the platinums can be separated; and then you have two tongues of fire ascending almost independently of one another, but meeting above. Under the influence of such a flame, the oxygen and the nitrogen of the air combine at a reasonable rate, and in this way the nitrogen is got rid of. It is now only a question of boxing up the gas in a closed space, where the argon concentrated by the combustion of the nitrogen can be collected. But there are difficulties to be encountered here. One cannot well use anything but a glass vessel. There is hardly any metal available that will withstand the action of strong caustic alkali and of the nitrous fumes resulting from the flame. One is practically limited to glass. The glass vessel employed is a large flask with a single neck, about half full of caustic alkali. The electrodes are carried through the neck by means of an indiarubber bung provided also with tubes for leading in the gas. The electric flame is situated at a distance of only about half an inch above the caustic alkali. In that way an efficient circulation is established; the hot gases as they rise from the flame strike the top, and then as they come round again in the course of the circulation they pass sufficiently close to the caustic alkali to ensure an adequate removal of the nitrous fumes.

There is another point to be mentioned. It is necessary to keep the vessel cool; otherwise the heat would soon rise to such a point that there would be excessive generation of steam, and then the operation would come to a standstill. In order to meet this difficulty the upper part of the vessel is provided with a water-jacket, in which a circulation can be established. No doubt the glass is severely treated, but it seems to stand it in a fairly amiable manner.

By means of an arrangement of this kind, taking nearly three horse-power from the electric supply, it is possible to consume nitrogen at a reasonable rate. The transformers actually used are the "Hedgehog" transformers of Mr. Swinburne, intended to transform from 100 volts to 2400 volts. By Mr. Swinburne's advice I have used two such, the fine wires being in series so as to accumulate the electrical potential and the thick wires in parallel. The rate at which the mixed gases are absorbed is about seven litres per hour; and the apparatus, when once fairly started, works very well as a rule, going for many hours without attention. At times the arc has a trick of going out, and it then requires to be restarted by approximating the platinums. We have already worked 14 hours on end, and by the aid of one or two automatic appliances it would, I think, be possible to continue operations day and night.

The gases, air and oxygen in about equal proportions, are mixed in a large gasholder, and are fed in automatically as required. The argon gradually accumulates; and when it is desired to stop operations the supply of nitrogen is cut off, and only pure oxygen allowed admittance. In this way the remaining nitrogen is consumed, so that, finally, the working vessel is charged with a mixture of argon and oxygen only, from which the oxygen is removed by ordinary well-known chemical methods. I may mention that at the close of the operation, when the nitrogen is all gone, the arc changes its appearance, and becomes of a brilliant blue colour.

I have said enough about this method, and I must now pass on to the alternative method which has been very successful in Professor Ramsay's hands—that of absorbing nitrogen by means of red-hot magnesium. By the kindness of Professor Ramsay and Mr. Matthews, his assistant, we have here the full scale apparatus before us almost exactly as they use it. On the left there is a reservoir of nitrogen derived from air by the simple removal of oxygen. The gas is then dried. Here it is bubbled through sulphuric acid. It then passes through a long tube made of hard glass and charged with magnesium in the form of thin turnings. During the passage of the gas over the magnesium at a bright red heat, the nitrogen is absorbed in a great degree, and the gas which finally passes through is immensely richer in argon than that which first enters the hot tube. At the present time you see a tolerably rapid bubbling on the left, indicative of the flow of atmospheric nitrogen into the combustion furnace; whereas, on the right, the outflow is very much slower. Care must be taken to prevent the heat rising to such a point as to soften the glass. The concentrated argon is collected in a second gasholder, and afterwards submitted to further treatment. The apparatus employed by Professor Ramsay in the subsequent treatment is exhibited in the diagram, and is very effective for its purpose; but I am afraid that the details of it would not readily be followed from any explanation that I could give in the time at my disposal. The principle consists in the circulation of the mixture of nitrogen and argon over hot magnesium, the gas being made to pass round and round until the nitrogen is effectively removed from it. At the end that operation, as in the case of the oxygen method, proceeds somewhat slowly. When the greater part of the nitrogen is gone, the remainder seems to be unwilling to follow, and it requires somewhat protracted treatment in order to be sure that the nitrogen has wholly disappeared. When I say "wholly disappeared," that, perhaps, would be too much to say in any case. What we can say is that the spectrum test is adequate to show the presence, or at any rate to show the addition, of about one-and-a-half per cent. of nitrogen to argon as pure as we can get it; so that it is fair to argue that any nitrogen at that stage remaining in the argon is only a small fraction of one-and-a-half per cent.

I should have liked at this point to be able to give advice as to which of the two methods—the oxygen method or the magnesium method—is the easier and the more to be recommended; but I confess that I am quite at a loss to do so. One difficulty in the comparison arises from the fact that they have been in different hands. As far as I can estimate, the quantities of nitrogen eaten up in a given time are not very different. In that respect, perhaps, the magnesium method has some advantage; but, on the other hand, it may be said that the magnesium process requires a much closer supervision, so that, perhaps, fourteen hours of the oxygen method may not unfairly compare with eight hours or so of the magnesium method. In practice a great deal would depend upon whether in any particular laboratory alternate currents are available from a public supply. If the alternate currents are at hand, I think it may probably be the case that the oxygen method is the easier; but, otherwise, the magnesium method would, probably, be preferred, especially by chemists who are familiar with operations conducted in red-hot tubes.

I have here another experiment illustrative of the reaction between magnesium and nitrogen. Two rods of that metal are suitably mounted in an atmosphere of nitrogen, so arranged that we can bring them into contact and cause an electric arc to form between them. Under the action of the heat of the electric arc the nitrogen will combine with the magnesium; and if we had time to carry out the experiment we could demonstrate a rapid absorption of nitrogen by this method. When the experiment was first tried, I had hoped that it might be possible, by the aid of electricity, to start the action so effectively that the magnesium would continue to burn independently under its own developed heat in the atmosphere of nitrogen. Possibly, on a larger scale, something of this sort might succeed, but I bring it forward here only as an illustration. We turn on the electric current, and bring the magnesiums together. You see a brilliant green light, indicating the vaporisation of the magnesium. Under the influence of the heat the magnesium burns, and there is collected in the glass vessel a certain amount of brownish-looking powder which consists mainly of the nitride of magnesium. Of course, if there is any oxygen present it has the preference, and the ordinary white oxide of magnesium is formed.

The gas thus isolated is proved to be inert by the very fact of its isolation. It refuses to combine under circumstances in which nitrogen, itself always considered very inert, does combine—both in the case of the oxygen treatment and in the case of the magnesium treatment; and these facts are, perhaps, almost enough to justify the name which we have suggested for it. But, in addition to this, it has been proved to be inert under a considerable variety of other conditions such as might have been expected to tempt it into combination. I will not recapitulate all the experiments which have been tried, almost entirely by Professor Ramsay, to induce the gas to combine. Hitherto, in our hands, it has not done so; and I may mention that recently, since the publication of the abstract of our paper read before the Royal Society, argon has been submitted to the action of titanium at a red heat, titanium being a metal having a great affinity for nitrogen, and that argon has resisted the temptation to which nitrogen succumbs. We never have asserted, and we do not now assert, that argon can under no circumstances be got to combine. That would, indeed, be a rash assertion for any one to venture upon; and only within the last few weeks there has been a most interesting announcement by M. Berthelot, of Paris, that, under the action of the silent electric discharge, argon can be absorbed when treated in contact with the vapour of benzine. Such a statement, coming from so great an authority, commands our attention; and if we accept the conclusion, as I suppose we must do, it will follow that argon has, under those circumstances, combined.

Argon is rather freely soluble in water. That is a thing that troubled us at first in trying to isolate the gas; because, when one was dealing with very small quantities, it seemed to be always disappearing. In trying to accumulate it we made no progress. After a sufficient quantity had been prepared, special experiments were made on the solubility of argon in water. It has been found that argon, prepared both by the magnesium method and by the oxygen method, has about the same solubility in water as oxygen—some two-and-a-half times the solubility of nitrogen. This suggests, what has been verified by experiment, that the dissolved gases of water should contain a larger proportion of argon than does atmospheric nitrogen. I have here an apparatus of a somewhat rough description, which I have employed in experiments of this kind. The boiler employed consists of an old oil-can. The water is supplied to it and drawn from it by coaxial tubes of metal. The incoming cold water flows through the outer annulus between the two tubes. The outgoing hot water passes through the inner tube, which ends in the interior of the vessel at a higher level. By means of this arrangement the heat of the water which has done its work is passed on to the incoming water not yet in operation, and in that way a limited amount of heat is made to bring up to the boil a very much larger quantity of water than would otherwise be possible, the greater part of the dissolved gases being liberated at the same time. These are collected in the ordinary way. What you see in this flask is dissolved air collected out of water in the course of the last three or four hours. Such gas, when treated as if it were atmospheric nitrogen, that is to say after removal of the oxygen and minor impurities, is found to be decidedly heavier than atmospheric nitrogen to such an extent as to indicate that the proportion of argon contained is about double. It is obvious, therefore, that the dissolved gases of water form a convenient source of argon, by which some of the labour of separation from air is obviated. During the last few weeks I have been supplied from Manchester by Mr. Macdougall, who has interested himself in this matter, with a quantity of dissolved gases obtained from the condensing water of his steam engine.

As to the spectrum, we have been indebted from the first to Mr. Crookes, and he has been good enough to-night to bring some tubes which he will operate, and which will show you at all events the light of the electric discharge in argon. I cannot show you the spectrum of argon, for unfortunately the amount of light from a vacuum tube is not sufficient for the projection of its spectrum. Under some circumstances the light is red, and under other circumstances it is blue. Of course when these lights are examined with the spectroscope—and they have been examined by Mr. Crookes with great care—the differences in the colour of the light translate themselves into different groups of spectrum lines. We have before us Mr. Crookes' map, showing the two spectra upon a very large scale. The upper is the spectrum of the blue light; the lower is the spectrum of the red light; and it will be seen that they differ very greatly. Some lines are common to both; but a great many lines are seen only in the red, and others are seen only in the blue. It is astonishing to notice what trifling changes in the conditions of the discharge bring about such extensive alterations in the spectrum.

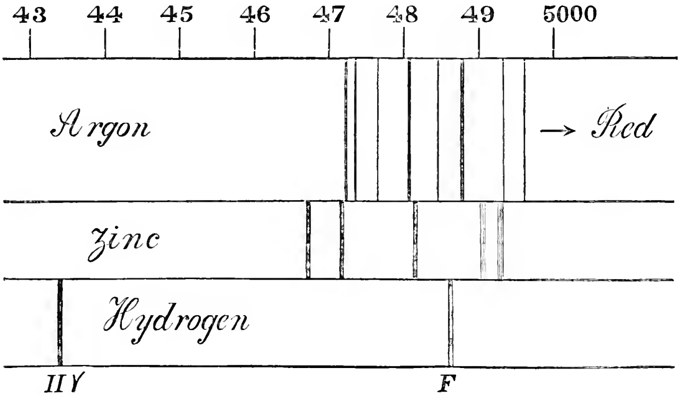
One question of great importance, upon which the spectrum throws light is, is the argon derived by the oxygen method really the same as the argon derived by the magnesium method? By Mr. Crookes' kindness I have had an opportunity of examining the spectra of the two gases side by side, and such examination as I could make revealed no difference whatever in the two spectra, from which, I suppose, we may conclude either that the gases are absolutely the same, or, if they are not the same, that at any rate the ingredients by which they differ cannot be present in more than a small proportion in either of them.

My own observations upon the spectrum have been made principally at atmospheric pressure. In the ordinary process of sparking, the pressure is atmospheric; and, if we wish to look at the spectrum, we have nothing more to do than to include a jar in the circuit, and to put a direct-vision prism to the eye. At my request, Professor Schuster examined some tubes containing argon at atmospheric pressure prepared by the oxygen method, and I have here a diagram of a characteristic group. He also placed upon the sketch some of the lines of zinc, which were very convenient as directing one exactly where to look. See figure on page 535.

Within the last few days, Mr. Crookes has charged a radiometer with argon. When held in the light from the electric lamp, the vanes revolve rapidly. Argon is anomalous in many respects, but not, you see, in this.

Next, as to the density of argon. Professor Ramsay has made numerous and careful observations upon the density of the gas prepared by the magnesium method, and he finds a density of about 19·9 as compared with hydrogen. Equally satisfactory observations upon the gas derived by the oxygen method have not yet been made, but there is no reason to suppose that the density is different, such numbers as 19·7 having been obtained.

One of the most interesting matters in connection with argon, however, is what is known as the ratio of the specific heats. I must not stay to elaborate the questions involved, but it will be known to many who hear me that the velocity of sound in a gas depends upon the ratio of two specific heats—the specific heat of the gas measured at constant pressure, and the specific heat measured at constant volume. If we know the density of a gas, and also the velocity of sound in it, we are in a position to infer this ratio of specific heats; and, by means of this method, Professor Ramsay has determined the ratio in the case of argon, arriving at the very remarkable result that the ratio of specific heats is represented by the number 1·65, approaching very closely to the theoretical limit, 1·67. The number 1·67 would indicate that the gas has no energy except energy of translation of its molecules. If there is any other energy than that, it would show itself by this number dropping below 1·67. Ordinary gases, oxygen, nitrogen, hydrogen, &c., do drop below, giving the number 1·4. Other gases drop lower still. If the ratio of specific heats is 1·65, practically 1·67, we may infer then that the whole energy of motion is translational; and from that it would seem to follow by arguments which, however, I must not stop to elaborate, that the gas must be of the kind called by chemists monatomic.



I had intended to say something of the operation of determining the ratio of specific heats, but time will not allow. The result is, no doubt, very awkward. Indeed, I have seen some indications that the anomalous properties of argon are brought as a kind of accusation against us. But we had the very best intentions in the matter. The facts were too much for us; and all we can do now is to apologise for ourselves and for the gas.

Several questions may be asked, upon which I should like to say a word or two, if you will allow me to detain you a little longer. The first question (I do not know whether I need ask it) is, have we got hold of a new gas at all? I had thought that that might be passed over, but only this morning I read in a technical journal the suggestion that argon was our old friend nitrous oxide. Nitrous oxide has, roughly, the density of argon; but that, as far as I can see, is the only point of resemblance between them.

Well, supposing that there is a new gas, which I will not stop to discuss, because I think the spectrum alone would be enough to prove it, the next question that may be asked is, is it in the atmosphere? This matter naturally engaged our earnest attention at an early stage of the enquiry. I will only indicate in a few words the arguments which seem to us to show that the answer must be in the affirmative.

In the first place, if argon be not in the atmosphere, the original discrepancy of densities which formed the starting point of the investigation remains unexplained, and the discovery of the new gas has been made upon a false clue. Passing over that, we have the evidence from the blank experiments, in which nitrogen originally derived from chemical sources is treated either with oxygen or with magnesium, exactly as atmospheric nitrogen is treated. If we use atmospheric nitrogen, we get a certain proportion of argon, about 1 per cent. If we treat chemical nitrogen in the same way we get, I will not say absolutely nothing, but a mere fraction of what we should get had atmospheric nitrogen been the subject. You may ask, why do we get any fraction at all from chemical nitrogen? It is not difficult to explain the small residue, because in the manipulation of the gases large quantities of water are used; and, as I have already explained, water dissolves argon somewhat freely. In the processes of manipulation some of the argon will come out of solution, and it remains after all the nitrogen has been consumed.

Another wholly distinct argument is founded upon the method of diffusion introduced by Graham. Graham showed that if you pass gas along porous tubes you alter the composition, if the gas is a mixture. The lighter constituents go more readily through the pores than do the heavier ones. The experiment takes this form. A number of tobacco pipes—eight in the actual arrangement—are joined together in series with indiarubber junctions, and they are put in a space in which a vacuum can be made, so that the space outside the porous pipes is vacuous or approximately so. Through the pipes ordinary air is led. One end may be regarded as open to the atmosphere. The other end is connected with an aspirator so arranged that the gas collected is only some 2 per cent. of that which leaks through the porosities. The case is like that of an Australian river drying up almost to nothing in the course of its flow. Well, if we treat air in that way, collecting only the small residue which is less willing than the remainder to penetrate the porous walls, and then prepare "nitrogen" from it by removal of oxygen and moisture, we obtain a gas heavier than atmospheric nitrogen, a result which proves that the ordinary nitrogen of the atmosphere is not a simple body, but is capable of being divided into parts by so simple an agent as the tobacco pipe.

If it be admitted that the gas is in the atmosphere, the further question arises as to its nature.

At this point I would wish to say a word of explanation. Neither in our original announcement at Oxford, nor at any time since, until the 31st of January, did we utter a word suggesting that argon was an element; and it was only after the experiments upon the specific heats that we thought we had sufficient to go upon in order to make any such suggestion in public. I will not insist that that observation is absolutely conclusive. It is certainly strong evidence. But the subject is difficult, and one that has given rise to some difference of opinion among physicists. At any rate this property distinguishes argon very sharply from all the ordinary gases.

One question which occurred to us at the earliest stage of the enquiry, as soon as we knew that the density was not very different from 21, was the question of whether, possibly, argon could be a more condensed form of nitrogen, denoted chemically by the symbol N_3. There seem to be several difficulties in the way of this supposition. Would such a constitution be consistent with the ratio of specific heats (1·65)? That seems extremely doubtful. Another question is, can the density be really as high as 21, the number required on the supposition of N_3? As to this matter, Professor Ramsay has repeated his measurements of density, and he finds that he cannot get even so high as 20. To suppose that the density of argon is really 21, and that it appears to be 20 in consequence of nitrogen still mixed with it, would be to suppose a contamination with nitrogen out of all proportion to what is probable. It would mean some 14 per cent. of nitrogen, whereas it seems that from one-and-a-half to two per cent. is easily enough detected by the spectroscope. Another question that may be asked is, would N_3 require so much cooling to condense it as argon requires?

There is one other matter on which I would like to say a word —the question as to what N_3 would be like if we had it. There seems to be a great discrepancy of opinions. Some high authorities, among whom must be included, I see, the celebrated Mendeleef, consider that N_3 would be an exceptionally stable body; but most of the chemists with whom I have consulted are of opinion that N_3 would be explosive, or, at any rate, absolutely unstable. That is a question which may be left for the future to decide. We must not attempt to put these matters too positively. The balance of evidence still seems to be against the supposition that argon is N_3, but for my part I do not wish to dogmatise.

A few weeks ago we had an eloquent lecture from Professor

Rücker on the life and work of the illustrious Helmholtz. It will be known to many that during the last few months of his life Helmholtz lay prostrate in a semi-paralysed condition, forgetful of many things, but still retaining a keen interest in science. Some little while after his death we had a letter from his widow, in which she described how interested he had been in our preliminary announcement at Oxford upon this subject, and how he desired the account of it to be read to him over again. He added the remark, "I always thought that there must be something more in the atmosphere."

WEEKLY EVENING MEETING,

Friday, April 26, 1895.

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

John Hopkinson, Esq. M.A. D.Sc. F.R.S. *M.R.I.*

The Effects of Electric Currents in Iron on its Magnetisation.

Let us recall a well-known experiment of Faraday's. Upon a ring of iron is wound a few turns of copper wire, through which may be passed a current. On it is a so wound a second coil, the ends of which are connected to a galvanometer. It was known that when a current is passed through the first-named or primary coil, the ring becomes a magnet, and that if the current is reversed in direction the magnetisation is also reversed. Faraday showed that when the magnetism is reversed, a transient current is caused in the second coil. The current which is reversed is a measure of the magnetising force; the total amount of current caused in the second coil by the reversal is a measure of the induction of the magnet. The current in the second coil is opposed in direction to the current in the primary after reversal—that is, it is in the direction of the current before reversal. If the changes in magnetisation in the iron produce a current in the coil which is connected to the galvanometer, it is clear that they will also tend to produce a current in the coil which in like manner surrounds the magnet, and which has been connected to the battery. This current is what used to be known as the extra current. It continues for a short time the current in the battery coil in the direction which it had before reversal, and retards its change to the opposite direction. If the battery which gives the current is a battery of many cells, and its extra E.M.F. is taken up by ordinary non-inductive resistances, there will be an E.M.F. in the battery to overcome these currents produced by the change of magnetisation, and the reversal of the current will be effected speedily. If, however, the number of cells is only sufficient to produce a current through the resistances of the electromagnet, the change will be much slower.

I have here an ordinary Westinghouse transformer which has an electromagnet in the ring form, but the iron of its core is divided into thin plates for the purpose of preventing currents in the iron itself. There is upon it a primary coil connected to the battery and a secondary coil connected to the galvanometer. I will first show you the effect produced when a battery current is produced by a battery of one cell giving about two volts. You observe that the current induced in the secondary coil takes a somewhat long time for its production and for its subsequent diminution—about 15 seconds. I will now connect it to a battery of high E.M.F. or rather to the mains which supply current to the building, and we will repeat the experiment with the same current in the primary coil. Observe the difference. The current increases more rapidly and dies away very much more quickly, taking in all about three seconds instead of 15 seconds, as before. I should perhaps here state that the sensibility of the galvanometer has been altered between the two experiments, so as to give a deflection of convenient size to be observed upon the screen.

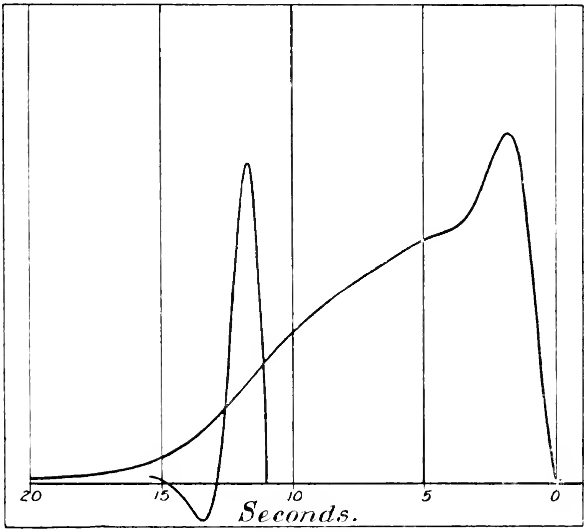
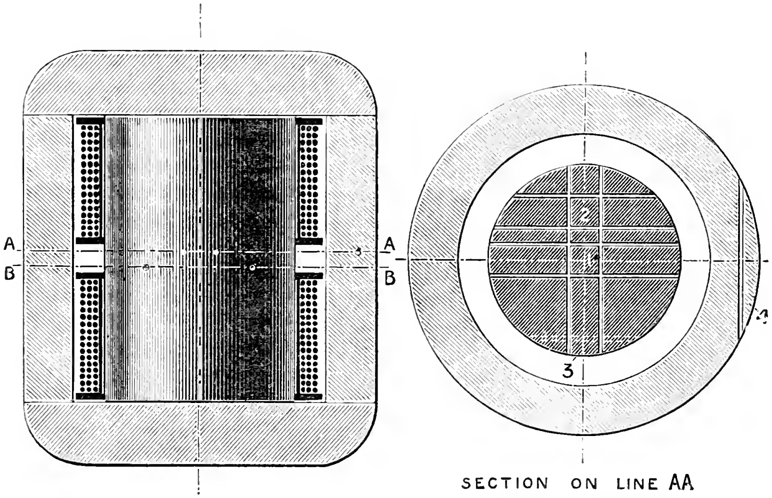


Fig. 1.

In the first case it is very much more sensitive than in the second. Exactly the same experiment can be shown to you in another form. These galvanometer deflections can be exhibited in the form of curves, in which the abscissæ represent times and the ordinates represent the deflections. The curve which I am showing you (Fig. 1) has actually been taken as a photograph, the plate being moved across the field with uniform velocity. On the same plate you see the curves which have been obtained with a larger E.M.F. and with a smaller one. The times at which the reversal occurred in each case are marked by the sudden change at the beginning of the curve. You see exactly the same thing as you saw upon the screen: the big battery makes the changes occur rapidly, the small battery allows them to occur more slowly. The dip of the curve for the large battery beyond the axes of the ordinates is not a real part of the phenomena. It is caused by the momentum of the moving part of the galvanometer.

In these experiments the iron has been divided in order to get rid of the particular effect about which I wish to speak this evening. Iron, as everybody knows, is a good conductor of electricity, not so good as copper, but still much better than any other substance than the metals. In a magnet, then, with a solid iron core, the outer portions of the iron are in a similar position to the copper coils surrounding the iron. On reversing the magnetising current, currents will be induced in the iron, and these currents will delay the changes of magnetic induction within them, and they will delay them the more the deeper in the iron is the point under consideration. I have in the room below a large magnet, which has been expressly constructed for the purpose of investigating the changes of induction which occur at different depths in the iron when the magnetising current is reversed. It is shown diagrammatically in Fig. 2. One view represents the vertical section of the magnet, the other the horizontal section. The magnet consists of a central cylinder of iron surrounded by an annulus with a large slab of iron at each end for the purpose of completing the magnetic circuit. Between the central cylinder and the annulus are placed the magnetising copper coils In



Scale 1 in. to 1 foot.

Fig. 2.

the solid cylinder holes are drilled at right angles to each other, and these holes meet within the iron. A strand of copper wire has been drawn in through the holes, and has subsequently been connected up so that the coils of the strand are in series with each other. In this way we have a coil surrounding a portion of the iron at 3, at 2, and at 1, of approximately the same area. If these coils then are connected to a galvanometer they will measure the changes of induction which occur in the areas 3, 2 and 1 enclosed, and we shall be able to show you the times at which the currents in the iron permit the effect of reversal in the copper coils to be manifest. We have, in addition, a coil, marked 4, in the outer magnet. Now, when the currents in the large copper coils are reversed, the magnetism or the induction in the coils 1, 2, 3 and 4 will also be reversed, but more slowly, and currents will be induced in these coils. I propose to show you the way in which these currents occur and are modified by the currents in the iron. We shall see that the changes in induction induce currents in the iron, and that these in their turn delay the changes in induction.

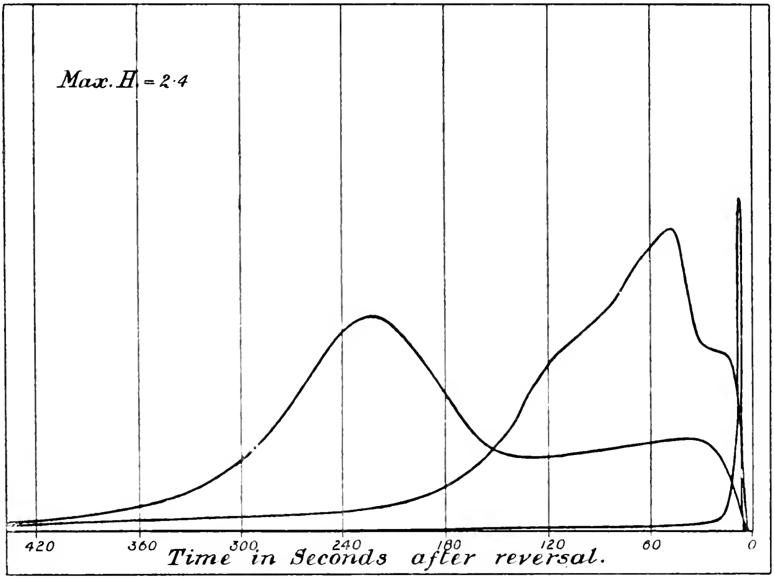


FIG. 3.

In order to give you a better idea of what is happening, we have provided three galvanometers. The galvanometers throw their images upon the screen at different altitudes. Now, what I want you to observe is this—that the disturbance of induction in the outermost coil occurs immediately upon reversal of the current, and it occurs after some time in No. 2 coil, and after a much longer time in No. 1 coil, and also to observe the difference of the effects according as the magnetising current is large or small. In all these experiments a considerable battery power is used, and its excess of E.M.F. is taken up with non-inductive resistances consisting of lamps; that is in order to confine the effects to the currents in the iron, and to get rid as far as possible of the effects of induction in the copper coils themselves. We will begin with a somewhat low force, 2·4. You notice that, as shown in Fig. 3, the galvanometer of No. 3 coil is promptly deflected, and as promptly returns to zero, because there is no depth of iron outside it in which currents can circulate. No. 2 coil soon moves off to a substantial deflection, and presently, after 50 seconds have elapsed, it moves off to a greater deflection. The behaviour of No. 1 coil is not dissimilar, but it takes much longer. As in the case of the transformer, the same thing can be shown as a curve (Fig. 3). Observe the relation of currents in the exploring coils to time. No. 3 coil rises to a maximum, and at once ceases. No. 2 goes off fairly rapidly, dwells for a time, and then rises to a full maximum at about 50 seconds; whilst No. 1 coil goes through the same events, but more slowly, as is shown by the increased abscissæ taking four minutes to reach its maximum. Here is a curve (Fig. 4) for a lower force, 1·2, half of the last: notice how great the difference—the maximum is reached much more speedily and the currents drop more slowly. Take a somewhat higher force, 6·0, to see its effect on the galvano-

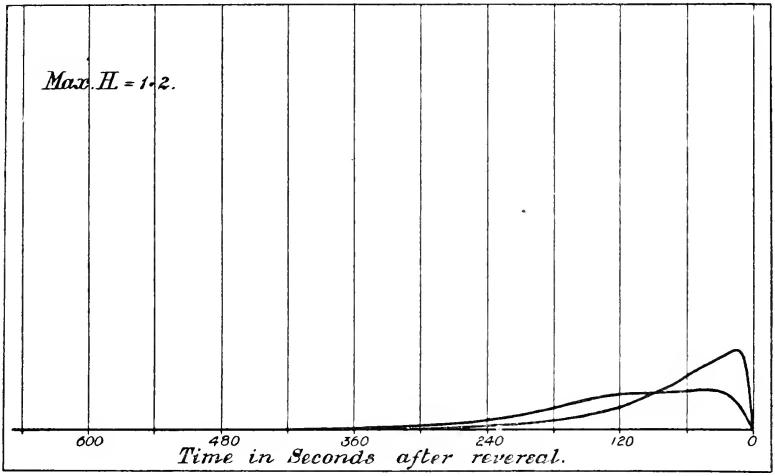


Fig. 4.

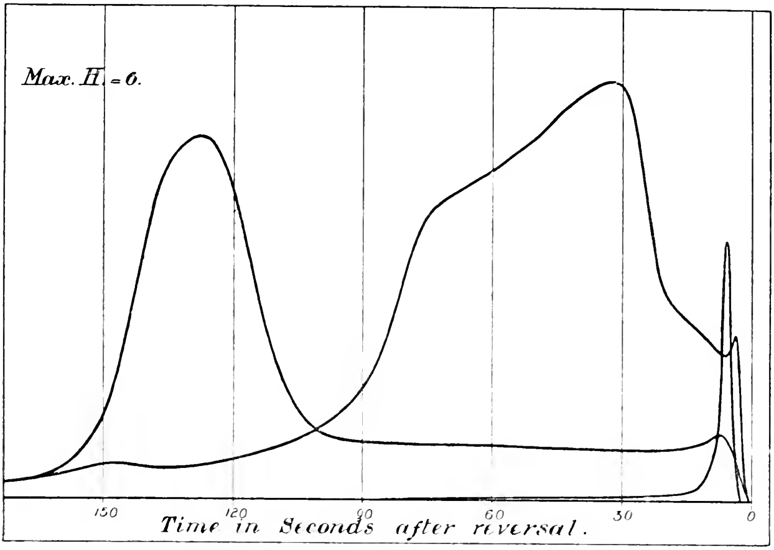

Max. H. = 6.
150
120
90
60
30
0
Time in Seconds after reversal.

FIG. 5.

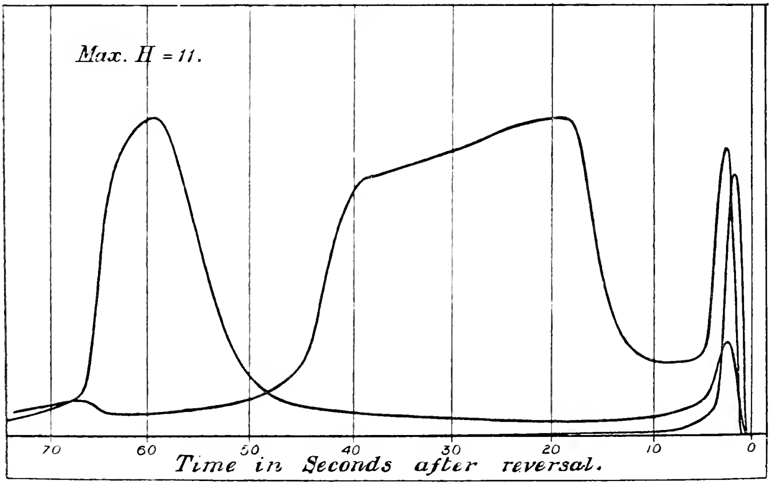

Max. H = 11.
70
60
50
40
30
20
10
0
Time in Seconds after reversal.

FIG. 6.

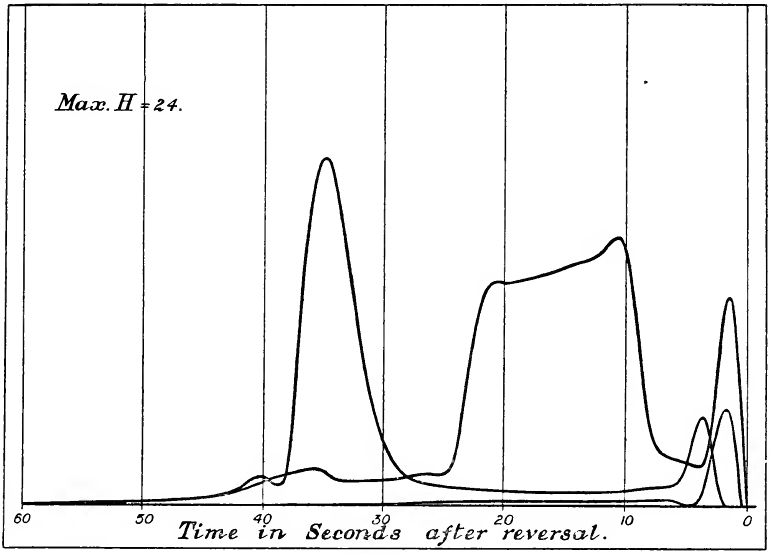

Max. H = 24.
60
50
40
30
20
10
0
Time in Seconds after reversal.

FIG. 7.

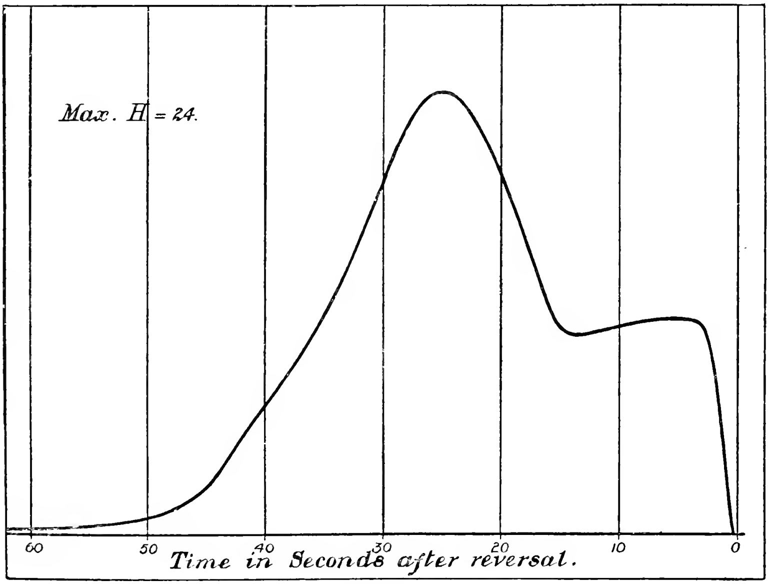

Max. H = 24.
60
50
40
30
20
10
0
Time in Seconds after reversal.

FIG. 8.

meters. The sensibility of No. 3 galvanometer is much less than that of Nos. 2 and 1, which are about the same. You see a new phenomenon develop: there is a first maximum in Nos. 2 and 1 coils, followed by a dwell and then a rise to a second maximum. Look at the corresponding curves (Fig. 5). The maxima are occurring earlier than with the force of 2·4—that for No. 2 coil at 30 seconds instead of 50 seconds, and of No. 1, the centre coil, at 130 seconds instead of 240. Again take a higher force, this time 11 (Fig. 6): see how marked the first rise has become. On the curve notice that the maximum of No. 1 coil is as great as No. 2—with lower forces it has always been less. The highest force I shall show you is 24 (Fig. 7), and it is really the prettiest. The first maxima occur markedly at once. No. 2 coil comes down to a small value, then rises to a substantial maximum in about 10 seconds, remains near its maximum value for 10 seconds, and passes away. Whilst No. 2 has been at its maximum, No. 1 has been quietly remaining undisturbed, and only when No. 2 has quite finished does No. 1 suddenly move off to a maximum greater than No. 2 at 35 seconds and then very suddenly disappear. We must not leave this part of the subject without showing one experiment with No. 4 coil, which is placed in the annular part of the magnet. We will use the maximum force (24). The general character of the changes is the same as before, but in this case the magnetism changes at once in the interior of the ring, and later at parts nearer to the outside. You see the same thing from the curve, Fig. 8.

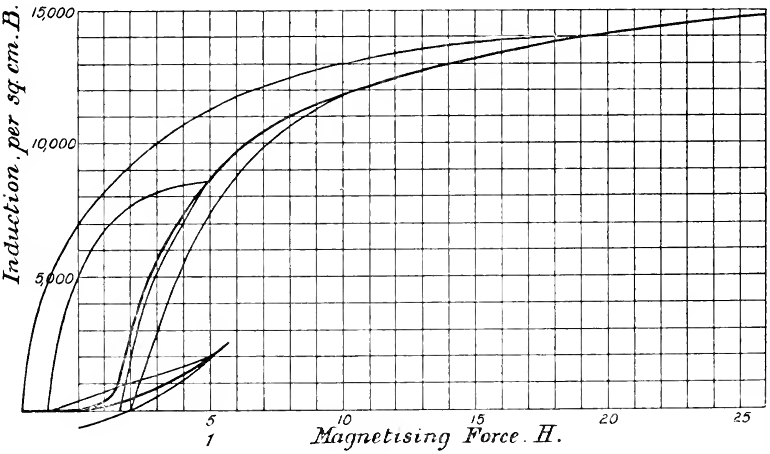


Fig. 9.

I am afraid I can only give you a very general explanation of the peculiarities of these curves. I will first of all throw upon the screen a diagram (Fig. 9) showing what are known as cyclic curves of magnetisation. The vertical ordinates represent the induction, and the horizontal abscissæ the magnetising force. You will observe that when the magnetising force is small the ratio of induction to magnetising force is also small; that as the magnetising force increases this ratio is also increased for a time, but that, finally, it again becomes small. You will observe, further, that when the magnetising force is great the curve of magnetisation begins with a small rate of change, which then becomes large, and which again becomes finally small. Now the slowness of changes of induction depends upon the largeness of the ratio of induction to magnetising force, and, therefore, we should expect that when this ratio is small the changes would occur with comparative rapidity, and that when it is large they would occur more slowly. This is exactly what we have seen. With a small force the change shows itself almost at once; with an intermediate force the principal maximum occurs much later, and with a large force it is continuously being accelerated as the force becomes larger. The first rise that occurs with a large force is due to the early part of the curve of induction, where changes of induction are comparatively small, and where consequently the effects rush in rapidly. This first maximum is followed by a time of comparative quiescence, which is again succeeded by a second and large maximum caused by the full effect of the great changes of induction which have occurred, but which have passed in comparatively slowly; and then, finally, the change drops away to zero quite suddenly, owing to the great rate at which the last part of the change has rushed in and pressed upon the parts which had occurred before it.

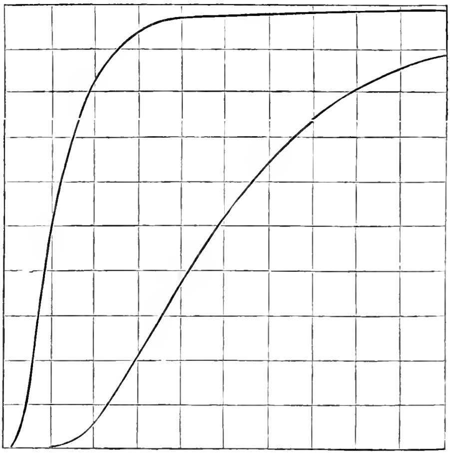


FIG. 10.

These phenomena of the times at which changes of induction occur in the cores of electromagnets have a close analogy to the retardation of signals upon cables. Many of you know if a battery is connected to a submarine telegraph cable, and if its connection is suddenly reversed, that the current throughout that cable is not reversed at once, but that it takes an appreciable time before the effect is perceptible at the far end of the cable. This retardation of the signal, as it is called, rapidly increases with the length of the cable. I will first of all show you two curves of arrival, as they are called, of the current in a cable (Fig. 10). In these curves the ordinates represent the currents, and the abscissæ represent the times. You will observe that there are two curves shown; one is for a length of cable double the other, the longer cable giving the slower times. Now all that the curves in this form are capable of showing you is that with the longer cable the events are more retarded. In order to make them really comparable with the magnetic experiments, we must show a curve in which the abscissæ represent, not the currents, but the rate of change of current. We have such a curve here (Fig. 11). In this case the abscissæ represent the rate at which the current is changed, and the ordinates represent the time. This curve has a remarkable similarity to the magnetic curve with the lowest current. The similarity of the curves, as shown in Fig. 12, is quite sufficient to suggest an analogy between the phenomena which actually exist. I can show you the same thing in another way. There are in the library one of Lord

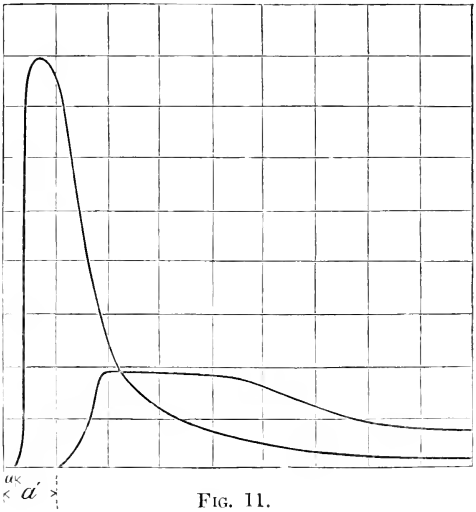


Fig. 11.

Kelvin's syphon recorders, made by Messrs. Muirhead & Co., which will be at work after the lecture, and also an artificial cable, which has also been made by Messrs. Muirhead & Co. Curves can be obtained from the end and middle of the cable.

Magnetic experiments such as these have really a wide application. They are not restricted to the particular size of the cylinder upon which the experiment is made. If the cylinder is larger or smaller similar magnetic events will happen, but they will happen at times shorter or longer in direct proportion to the square of the linear dimensions of the cylinders—that is, to the areas of the cylinders. Hence we may boldly infer what will happen in magnet cores of dimensions too small to experiment upon or so large that they would be very costly to make. For example, suppose a core is made up of wires $\frac{1}{100}$ of an inch in diameter, we may expect that everything will go on with the same currents around the core but 1,440,000 times as fast; with a force of 24, everything will be over at the centre of the core if the current is instantaneously reversed in $\frac{1}{40000}$ of a second.

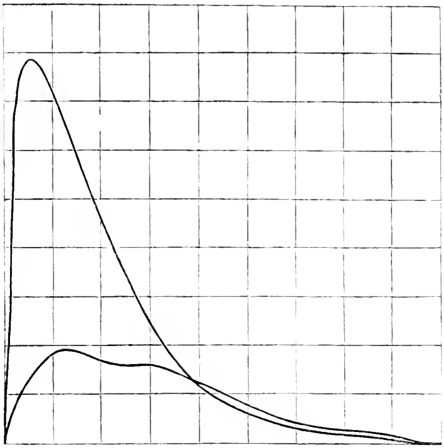


Fig. 12.

One very practical application of these results is to the cores of transformers, but in this case we have not the sudden reversal of the magnetising current, but the current continuously varies from positive to negative and from negative to positive. It is not difficult to imitate these conditions on the slow scale which is suitable for our large magnet, but if I were to show you an experiment at an appropriate rate on the magnet which we used for reversals, I fear you would find the results intolerably tedious. It would take some minutes to get through a single reversal. I will, therefore, use a smaller magnet, 4 inches instead of 12 inches diameter, which will get through the business nine times as speedily. I will first of all throw upon the screen a diagram (Fig. 13) which shows the method of the experiment. Here is the source of electricity, marked "dynamo"; here are resistances for regulating the current, marked "rheostat"; and here is a liquid reverser intended to reverse the direction of the current by continuous steps. It consists of two copper plates, to which the current is taken, and of two moving plates revolving between these plates, which plates are connected to the electromagnet. As the moving plates revolve, the current will diminish from a maximum in one direction to nothing, and will then increase to a maximum in the opposite direction. The electromagnet is marked "magnet," the secondary coils of which are not indicated in the diagram. Only one coil in the centre of the magnet is connected. We will now turn the reverser faster. You see, the variations of inductions diminish greatly—indeed, the induction at the centre of the core is but little affected by the changing current in the magnetising coils (change leads). We might have tried three coils instead of one, but the experiment is a little

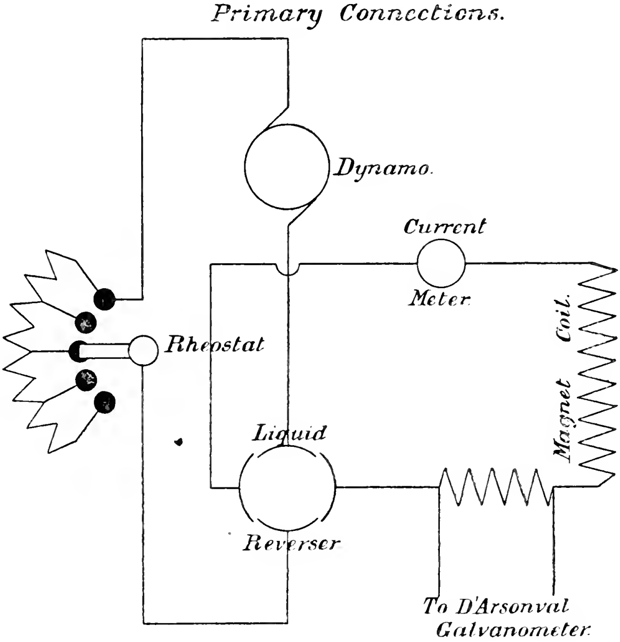


FIG. 13.

confusing. If we had done so, we should have seen that the diminution of disturbance with increase of speed was less with No. 2 coil than with No. 1 coil, and that it had disappeared entirely with No. 3 coil, and we should further see that the current in No. 2 coil lay behind No. 3, and in No. 1 behind No. 2. I can show you the contrast more effectively with the Westinghouse transformer with a divided core. This is now connected to the lowest galvanometer. We turn slowly: notice the deflection. We turn faster: you see the deflection is increased instead of diminished, as it was with the central coil of the solid electromagnet. Experiments such as these are at once applicable to transformer cores and the cores of dynamo machines. They show that in practice manufacturers have divided the iron about enough, and not too much.

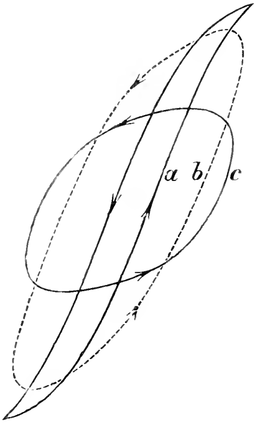


Fig. 14.—Magnetic Curve-Tracer Curves for Soft Iron Bars.

(*a*) Cycle performed slowly.
(*b*) Period of cycle 3 seconds.
(*c*) Period of cycle 0·43 second.

I have here on the table an instrument designed by Prof. Ewing for the purpose of describing the curves which express the relation between induction and magnetising force in iron. It served in Prof. Ewing's hands to illustrate the point which we have been discussing. I will throw upon the screen curves taken from Prof. Ewing's paper read before the Royal Society. The curves have been taken from solid samples of iron—I mean iron which is continuous, and not divided for the purpose of annulling the current in it. In the first curve (*a*, Fig. 14) we have the result in which the cycle has been passed through very slowly, and is the true curve of magnetisation. We have *b*, where the cycle has passed through in three seconds—and you will observe that the amplitude of the induction has diminished, and we have, lastly, the curve *c*, where the period of the cycle is four-tenths of a second, and you will observe that it has much more diminished. This is owing to the fact that changes of induction in the centre of the iron hardly exist, and that therefore the total effect is materially diminished. The diagram (Fig. 15) is for steel. We have *a* the natural curve of induction; *b* the case when the period of the cycle is three seconds; and *c* when the period is six-tenths of a second. Curves such as these, of course, very readily give you the average effects upon the whole mass of the iron. The curves which I have shown you give the particular effects of different parts of the mass.

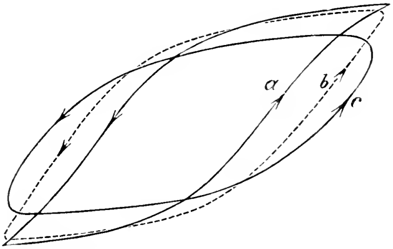


FIG. 15.—MAGNETIC CURVE-TRACER CURVES OF STEEL.

(*a*) Cycle performed slowly.
(*b*) Period of cycle 3 seconds.
(*c*) Period of cycle 0·6 second.

In conclusion, let us indulge in a little wild speculation, not because it is probable that it is in any sense true, but because it is interesting. Suppose a magnet were made exactly like the one on which we experimented, but of the size of the earth, and that some mighty electrician generated such a current in its copper coils as would give a magnetising force of 2·5, and then reversed it, it would take some thousands of millions of years before the rate of disturbance at the centre attained its maximum value. The speculation I suggest is this: is it not conceivable that the magnetism of the earth may be due to currents in its material sustained by its changing induction but slowly dying away?

[J. H.]

ANNUAL MEETING,

Wednesday, May 1, 1895.

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 1894, testifying to the continued prosperity and efficient management of the Institution, was read and adopted. The Real and Funded Property now amounts to above 102,000*l.* entirely derived from the Contributions and Donations of the Members and of others appreciating the value of the work of the Institution.

Sixty-two new Members were elected in 1894.

Sixty-three Lectures and Nineteen Evening Discourses were delivered in 1894.

The Books and Pamphlets presented in 1894 amounted to about 242 volumes, making, with 578 volumes (including Periodicals bound) purchased by the Managers, a total of 820 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. LL.D. F.R.S. M. Inst. C.E.

MANAGERS.

Sir Frederick Abel, Bart. K.C.B. D.C.L. LL.D. F.R.S.
Captain W. de W. Abney, C.B. D.C.L. F.R.S.
The Right Hon. Lord Amherst, F.S.A.
William Anderson, Esq. D.C.L. F.R.S.
Sir Benjamin Baker, K.C.M.G. LL.D. F.R.S.
John Birkett, Esq. F.R.C.S.
William Crookes, Esq. F.R.S.
Edward Frankland, Esq. D.C.L. LL.D. F.R.S.
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WEEKLY EVENING MEETING,

Friday, May 3, 1895.

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

VETERINARY-CAPTAIN F. SMITH, F.R.C.V.S. F.I.C.

The Structure and Function of the Horse's Foot.

THE structure and function of the horse's foot is a subject which is not only of theoretical interest but of supreme practical importance. When I tell you that half the unsoundness and at least half the lameness amongst horses in this kingdom are due to trouble either in or near the foot, you will, I am sure, agree with me that it is impossible to over-estimate the importance of the subject which I have to bring before you this evening.

The reason why the foot should be such a frequent seat of unsoundness is not difficult to understand, when we remember the unnatural conditions under which horses both in town and country have to work, and, further, the risk they incur from shoeing.

Shoeing is a necessary evil, but the harm resulting from the application of a shoe to the foot is not in itself great; it is the *abuse* of shoeing which constitutes the danger. The serious and senseless mutilations which are practised on the foot rob shoeing of much of its value, and constitute it a standing reproach to our civilisation.

I regret that the time at my disposal will not admit of the question of shoeing being touched on; but during this discourse, as opportunity occurs, I will allude to some of the great evils which are practised in this indispensable art, evils which I may at once say might in a few months be swept away throughout the length and breadth of the land, if the horse-owning community possessed even an elementary knowledge of the manner in which the horse's foot is built up, and the use of its various parts.

It is probable that the majority of laymen regard the foot as a solid block of horn placed at the end of the limb, and on which the horse stands. I shall hope to show you that the foot is a highly specialised structure, endowed with tissues possessing acute sensation, mechanisms by which concussion is warded off, a blood supply unequalled in any other part of the body, the whole being enclosed within a covering of horn known as the hoof.

The foot is therefore divided into two parts, a core consisting of bones, blood-vessels, tendons and other tissues, which in shape resembles a miniature hoof, and enveloping this a covering of horn possessing neither blood-vessels nor nerves; the first is called the sensitive, the other the insensitive foot, and the two fit together much as a finger fits into a glove.*

Various names have been given to the different parts of the foot: for instance, the wall is the portion visible when the foot is on the ground; the position of the sole is obvious; whilst a wedge-shaped piece of horn placed in the central and posterior part of the foot is vulgarly known as the "frog"—we shall speak of it as the foot-pad; finally, a portion of the wall inflected at the heels forms a part known as the bars.

It is impossible to completely grasp the function and structure of the foot unless we possess some information as to the nature of horn.

If horn be examined under the microscope it is found to consist of cells which resemble the scales found on the skin; in fact, hoof is modified skin, the cells forming which have, by a process of compression and chemical change, become converted from scales of skin to scales of horn.

The essential microscopical feature is the presence of canals, around and between which the cells are arranged, uniting and knitting the parts together in such a way as to produce the tough yielding material known as horn. The tubes run through the entire length of the structure; they are not completely hollow, as the name might imply, but are lightly packed with very soft cells.

Horn which is dry, as in any of the feet on this table, is as brittle as glass, and fractures like a piece of glue. Horn which is moist cannot be broken; it can be twisted and torn, but only with difficulty. Under pressure the moist horn yields, the dry horn breaks.

The moisture in the foot is something very remarkable: in the foot-pad it amounts to 42 per cent., whilst in the wall, which is the driest, it falls to 24 per cent. The use of this moisture is to keep the foot pliable and yielding; where, therefore, the greatest yielding is required there the moisture is the largest, and where resistance is most needed there the moisture is the least. I shall constantly have to refer to the moisture in horn, for, as you will see later, it is the essential factor in the foot around which all the others work.

It is through the cells and tubes in the horn that a constant passage of water occurs, by which means the foot maintains, in spite of the evaporation which is taking place from it, the amount of moisture it requires.

We spoke of the sensitive being buried within the insensitive foot; it is from this sensitive foot that the horn is secreted, the process being a slow and gradual one. If we examine a horn-secreting surface, it will be found covered with delicate projections known as papillæ, about one-quarter to one-half inch in length; these papillæ fit into

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* A working model of the foot, twelve times its natural size, was kindly built for this lecture by Captain Gillespie, Army Service Corps, to whom I am very greatly indebted.

holes in the horn, and the tubular formation of horn is due to the fact that it is pierced at its origin for the reception of papillæ.

The wall of the horse's foot is divided, for convenience of description, into the toe, quarters and heels. The thickness is greatest at the toe, and decreases gradually towards the heels where it is thinnest; but the wall at the heels, instead of being continued so as to complete the circle of the foot, suddenly turns in and travels in a forward direction between the sole and foot-pad. This portion is called the bar, and the practical lesson which has to be learned is that the bar is part of the wall, is intended to bear weight, and should not be cut away in shoeing as is so commonly practised. In a foot of a wild horse shot in Thibet—of which a plaster cast is placed on this table—the most extraordinary development of the bars is shown.

It is obvious that by the inflection of the wall the heels of the foot are considerably strengthened; and this is especially necessary, as the circle of the wall is only an imperfect one.

The amount of moisture in the wall varies, depending upon its position relative to the horn-secreting surface. The horn-secreting surface of the wall lies immediately under the upper edge of the hoof; the nearer the horn is taken to this upper edge the more moisture it contains, the further from the edge the less the moisture. It is obvious, therefore, that as the wall grows longer it becomes drier; and moderate dryness of horn is only another name for toughness, so that the portion of wall in contact with the ground is much harder than the portion above the ground.

The growth of the wall under normal conditions is the same at any part of its surface: if it grows an inch at the toe it grows an inch at the quarters and heels. You will observe that the wall at the heels is, roughly, only half the height of the wall at the toe, and, bearing in mind what has been previously said about horn becoming drier as it increases in length, you will have no difficulty in understanding that the horn at the toe is older and tougher than the horn at the heel, which, from being much younger and shorter contains more moisture, and is therefore elastic and yielding. If, for example, we assume the length of the wall at the toe to be four inches, and that at the heel to be two, it is obvious that the wall at the toe is double the age of that at the heel; and if we continue this investigation further by drawing lines around the wall parallel with the upper edge of the hoof, it will readily be seen that the portion in contact with the ground is of varying age, being oldest at the toe and gradually decreasing in age to the heel, in other words, being hardest at the toe and softest at the heel.

There is an object in all this which we must now enquire into. When a horse's foot comes to the ground in either the trot, canter or gallop, viz. in any pace which causes concussion, the heel is the first to make contact with the ground; by this means, as we shall hope to show, the shock of impact is considerably reduced, for the soft tissues of the posterior part of the foot yield slightly under the strain instead of offering rigid opposition, and this yielding, which we shall have to deal with more fully later on, is permitted to occur through the young moist horn which exists at this part.

From the heel the weight is transmitted along the foot from rear to front, and finally the heel becomes raised, the toe alone bearing on the ground. This is the position in which the greatest wear and tear of the foot occurs, for the toe is now engaged in giving the propulsion to the body, the friction is therefore considerable, and to meet this the horn at this part is comparatively dry and very tough.

We can see, therefore, that the variations in the amount of moisture in the wall are intended to meet the wear and tear of the foot.

The use of the wall is to support the weight of the body: the horse's weight is literally slung inside its foot. This slinging apparatus is infinitely stronger than if the weight were imposed, as we might imagine, on the sole of the foot, and, in addition, it is distributed over a larger surface than it otherwise would be.

When we remember that the mean weight of a horse is 10 cwt., and there are many which weigh 15 cwt. or more, there is no difficulty in observing that the foot is really an extremely small base on which to impose this enormous weight. The area of the human foot appears to be greater than that afforded by the horse's foot, but I shall now have to show you that the slinging apparatus previously spoken of, increases in a remarkable manner the internal surface of the horse's foot without adding to its circumference.

Found on the inside of the wall are 500 or 600 plates or leaves of horn, which run in the direction of the fibres of the foot—they may be seen in this model. In length they nearly correspond to the wall, whilst they are so thin as to be perfectly transparent. Regarded by themselves their function is not very evident; but if we examine the exterior of the sensitive foot, it will be observed that it is covered with a very large number of delicate sensitive leaves, also of extreme thinness, and so full of blood-vessels as to give a bright red colour to the part.

These sensitive leaves or laminæ correspond in number and position to their insensitive counterpart, and the two sets are found to be fitted into each other in such a way as to form the most perfect dovetail.

This dovetailing of the laminæ produces immense strength: by no ordinary process is it possible to destroy the union of these two surfaces, even after death; special methods have to be adopted in the study of anatomy if we wish to separate the horny from the sensitive laminæ.

But the dovetailing is further increased in strength by a remarkable arrangement. If we make a horizontal section of the two sets of laminæ in position, and examine them microscopically,* we find that

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* A number of microscopical preparations were placed on the library table illustrating the structure of the foot. The majority were kindly prepared for this lecture by Professor Mettam, B.Sc. Dick Veterinary College, Edinburgh.

each lamina, both horny and sensitive, possesses secondary laminæ or lamellæ; of these there are about 150 to each primary lamina, so that we may say the union between the sensitive and insensitive wall is brought about by the dovetailing of 1000 primary and 150,000 secondary laminæ.

Here is a model of a single lamina 450 times larger than normal. The structure rather reminds one of a fern leaf or feather, the stem being the primary lamina, and the lateral projections the secondary leaves.

So much for the slinging apparatus. If time permitted I could tell you much more of interest about it; and the undoubted evidence we possess that by it, and it alone, is the enormous weight of a horse's body solely supported.

We have one more point to discuss in connection with the laminæ, and that is the increase in the surface which they afford to the foot. The simplest method of explaining my meaning is to take the commonplace example of a book consisting say of 500 pages, which when bound in the ordinary manner is easily compressed into a body having a small surface, yet if each of the 500 pages be removed and placed side by side the area they cover would be considerable. Much the same arrangement exists in the foot. By the folding up of horny and sensitive material a very large surface is disposed within a very small circumference, and careful measurements of the primary and secondary laminæ have led to the conclusion that the surface thus contained within each foot of the horse is not less than eight to ten square feet.

The next part of the foot to receive attention is the sole. This, as may be seen from the model and diagrams, is concave in shape towards the ground, which is evidence, if any were required, that it is not intended to support the horse's weight; that margin of it, however, in contact with the wall is doubtless capable of sustaining pressure.

The function of the sole is to save the sensitive parts situated above it from injury, and that it is eminently qualified for this purpose is evident to any one who has witnessed the intense lameness which arises from a stone getting wedged in the foot.

The sole grows from the sensitive sole, which may be seen in the diagram to be scarlet in colour and covered with numerous projections, or papillæ, which fit into minute holes on the upper surface of the horny sole.

A peculiarity in the horn of the sole is the fact that it only grows a certain thickness before it breaks off. The object of this is, that as the sole over its general surface is not in contact with the ground, it is exposed to little or no friction like that of the wall, which in a state of nature is maintained of proper length by the friction to which it is exposed. The sole is therefore shed on attaining a certain thickness, but no shedding occurs until a new sole of suitable thickness has been produced to take its place.

One of the common evils of shoeing is cutting away the sole of the foot. If we bear in mind the use of the sole I am sure the ruin produced by this barbarous practice will be very evident to you. The sole cannot be too thick, and I have shown you that nature provides for its exfoliation. Under the weight of the horse's body the sole slightly yields: but this we will discuss presently.

The foot-pad, or, as it is commonly known, the frog, is peculiar both from its shape and the nature of its horn. The horn forming this body is very soft, and resembles rubber; it can be cut, but offers considerable resistance to friction, and when exposed to friction it wears away with a ragged surface in much the same way as rubber. Its pliability is due to the considerable amount of moisture it contains, which you may remember I stated was as high as 42 per cent., or about double that found in the wall.

This foot-pad has a sensitive counterpart, a body composed of fibrous material containing fat, and so like fat in colour that it has been termed the fatty frog. This sensitive foot-pad fills up the entire space between the heels of the foot, and forms a dense cushion exactly resembling in shape the foot-pad, and it is from the surface of this cushion that the horny foot-pad grows.

There is no part of the horse's foot which has been exposed to more mutilation in shoeing than the foot-pad; probably there is no part of the equine less understood, or one where more ignorance has been shown.

The impression amongst laymen is that the foot-pad is a dangerous excrescence, which regularly at every monthly shoeing must be cut away to prevent the horse from becoming lame. This practice, I regret to say, is countenanced by people of intelligence, who in the matter of horse-shoeing place themselves entirely in the hands of their servants.

The use of the pad is to save the foot and limb from concussion: its position in that part where I previously told you the largest amount of concussion is inflicted is evidence of this; further, the rubber-like nature of its horn is suggestive of a mechanism for the prevention of jar and shock. The shape of the pad, and the fact that in the unshod or carefully-shod foot it is in contact with the ground over a large surface, is evidence that it must assist in providing a firm foot-hold and prevent slipping. Finally, from its position and use it keeps the heels apart and maintains the proper width of the foot.

All these facts can be absolutely demonstrated. Take, for instance, the last function accorded the pad, viz. maintaining the proper width of the heels of the foot, a simple experiment will demonstrate this to perfection. If we take a foot with a large well-developed pad, and so shoe the horse that it does not come in contact with the ground, the heels of the foot become narrower every day, and in three months' time the part is beyond recognition, the heels have curled in, the pad has folded in on itself so that it is not one-half its original width, and the fibrous cushion previously mentioned as lying above the foot-pad wastes away as it is thrown out of use.

We may now reverse the experiment, and shoe the horse in such a manner that what is left of the foot-pad is made to rest on the ground: in a month, or even less, the most marked changes have occurred, the pad commences to unfold itself like a bud, the cushion becomes larger, the foot wider, and in three months the transformation may be complete. Such is an experiment which may be performed on any horse with absolutely identical results, and proves to demonstration that the pad is intended by nature to rest on the ground.

We must now take a cursory glance at the internal foot, as our time will not admit of a complete examination.

The bones found in the foot are three in number: two wholly belong to the foot, one belongs partly to the foot and partly to the portion of the limb above the hoof known as the coronet. Dealing only with the foot bones, one is found to resemble a miniature hoof in shape, is very porous in its structure, and has growing from each extremity a plate of cartilage which extends superiorly above the hoof and posteriorly as far back as the heels. The bone is porous to admit of the innumerable blood-vessels for which the sensitive foot is remarkable, while the introduction of the plates of cartilage is to allow of lateral movement in the posterior part of the foot, such as would not be possible if bone existed in its place.

The second bone of the foot is one of the smallest, but practically one of the most interesting in the body. Its position can be seen in this model; and it is unfortunately the seat of the most incurable lameness to which the horse is liable. Beneath this small bone is a tendon which flexes the foot and keeps the bone in position.

Surrounding all these are the sensitive structures to which previous reference has been made. But before passing on to the final subject for our consideration, I must draw your attention to the remarkable vascularity of the foot; in few parts of the body do we find so many blood-vessels. These diagrams can give you but a faint notion of the number of vessels in the foot, and even they deal only with the veins; to have introduced the arteries would have complicated the drawing too much. Practically the whole of the sensitive foot is scarlet in colour, from the amount of blood it contains, and the sole use of this blood is to manufacture the horny covering.

We alluded just now to two plates of cartilage found in the foot; they occupy the position shown in the diagram, and their use is connected with the important lateral movement or expansion which the foot undergoes when weight is placed on it. If it were not for these elastic plates, expansion of the foot would be rendered very difficult. The plates also assist the circulation of the blood in the foot, by exercising, during their elastic movements, pressure on the veins, and thus pumping the blood out of the part.

Perhaps the greatest interest in the foot is centred in the mechanisms which prevent concussion, these are as follows: lateral expansion of the foot, descent of the vascular within the horny foot, flattening of the sole, and sinking of the heels.

The expansion of the foot has been known for many years, but has always found more opponents than supporters; it was not until the introduction of foot apparatus which was capable of making delicate measurements that it was possible to convince the incredulous. Lungwitz in Germany has made some valuable observations on the expansion of the foot. Independently and unknown to each other we were both reinvestigating the phenomenon with improved apparatus, and obtained results which were practically identical. I show you on the screen the apparatus employed by Lungwitz, which consists chiefly of a shoe to which can be fitted an arm carrying a screw. To this arm one pole of the battery of an electric bell is attached; the wall of the foot is covered with tinfoil carefully secured in its place, and to it is attached the other pole of the battery; the contact screw is so adjusted that if the foot widens when the weight is placed on it, the tinfoil touches the screw and so closes the circuit, of which the bell gives the indication. With this and other apparatus Lungwitz investigated the movements of the foot not only at rest, but during work.

I have been unable to investigate the movements of the foot during work, but on the table may be seen a piece of apparatus constructed on the same lines as that employed by Lungwitz, and with it I shall be able to show you, even on the dead foot, that there is marked lateral expansion. There is another piece of apparatus which I have employed, not only to indicate lateral movement in the foot, but to register the amount. The apparatus is constructed on the lines of a well-known form of steam gauge; a pin is connected with a series of wheels which multiply its movement, and convey this for the purpose of registration to a hand working on a dial; a very small amount of movement in the pin gives rise to a considerable excursion of the hand on the dial; by dividing the dial into a certain number of parts and carefully estimating their value by means of a vernier, an apparatus capable of registering the $\frac{1}{125}$ of an inch is readily obtained. I have this instrument on the table; it is placed against the wall of the foot at any desired spot, and, by lifting up the opposite leg, and so throwing extra weight on its fellow, the foot expands. A large number of observations carried out on these lines, demonstrated that during rest simply imposing extra weight on one fore-foot by lifting up its fellow, caused it to expand $\frac{1}{25}$ of an inch.

It may be asked what is the value of this trifling increase? My answer is that this "give" makes all the difference between a rigid and a yielding mass, the slight yielding saves the foot from jar and concussion. It is obvious that the amount of "give" depends upon the force with which the foot comes to the ground, viz. on the pace, but under no circumstance is it likely to be more than $\frac{1}{8}$ of an inch. The only part of the foot which expands is that portion of the wall situated towards the heels.

On this large model I have shown you in section the arrangement of the bones within the foot, and I pointed out the existence of a small but exceedingly important bone, the seat of an incurable lameness. The question arises as to the use of the navicular bone? My reply to a rather complex question must be brief—it is to increase the area of the pedal joint. It is obvious that an increase in the area of the joint could have been obtained by making the pedal bone itself larger, instead of introducing a third bone into the joint for the purpose. You will, however, observe from this model that the navicular bone has a movement quite independent of that of the pedis, it gives slightly when I press upon it and then returns to its place. Bearing in mind what I told you early in this lecture that the heel of the foot comes to the ground first in all fast paces, you will be able to see that this small and troublesome bone really forms a yielding articulation, on to which the first force of impact is imparted, and in this way concussion is prevented. When this bone becomes diseased the animal, as we might imagine, goes on its toes and loses all freedom in its gait.

The next mechanism to be described is the descent of the vascular within the horny foot, a process which can be perfectly seen in this model. The whole of the vascular foot under the influence of the body weight sinks or becomes depressed within the hoof, to rise again to its position when the weight is taken off the limb: to revert to our original simile, the finger slides up and down within the glove. The amount of this movement is about $\frac{1}{16}$ of an inch. The effect of it is that the foot offers an elastic and not a rigid resistance to the concussion of impact, and in this way neutralises the jar which would otherwise be felt, in the same way that it is easier to catch a cricket-ball by a retreating movement of the hand than by rigid opposition.

At the moment of the descent of the internal foot, the horny sole, which you will remember is concave towards the ground surface, becomes slightly flattened, as the result of which no bruising of the delicate structures covering the sensitive foot is incurred.

If we place a foot rule in such a position that one arm is resting on the ground, while the other is lying parallel to the wall of the foot at the toe, and in this position lift up the opposite foot so as to throw double weight on the one under investigation, it will be found that at the moment the extra weight comes on the limb the upper or coronary edge of the hoof slightly recedes from the foot rule; when the extra weight is taken off the foot, the edge advances into its original place. This phenomenon is associated with a sinking of the upper edge of the hoof at the heels, and an increase in the width of the foot.

The change in shape just described follows as the result of a temporary rearrangement in position of the parts within.

We have previously drawn attention to the very vascular nature of the horse's foot; time will not admit of stopping to enquire into the causes of this vascular condition, but what chiefly strikes the physiologist is that a part lying furthest from the heart should be able to have such a complex circulation carried on with comparative ease. Does the foot in any way assist in its own circulation? The experiment which I am about to show you proves this very conclusively, and demonstrates that a pumping mechanism exists, by which the blood is forced out of the foot every time the weight comes on it.

Into the veins of this foot I have placed two glass tubes, and both are filled with water; by projecting these tubes on the screen you will be better able to observe that at the moment I press on the foot joint—and thereby, as you will remember, depress the internal foot and at the same time cause the whole part to slightly expand—the fluid rises considerably in the manometer tubes; when I remove the pressure the fluid falls. Now in the living foot when the weight comes on the limb, the blood is pumped with considerable force up the veins of the leg, and at every movement this is repeated. That the living foot behaves like our dead one, is proved by the fact that if a vein be divided in the living animal, a jet, as if from a syringe, comes from it every time the foot comes to the ground.

The pumping action in the foot is due to the various movements occurring in this organ, and without their aid it is probable that the circulation in the foot would be carried on with extreme difficulty.

Finally, let us briefly pass in review the changes occurring in the foot from the time it makes contact with the ground until it leaves it.

The weight is received on the posterior part of the foot and foot-pad, by which means the plantar cushion resting above the foot-pad is altered in shape: the foot-pad and plantar cushion being compressed and widened, each exerts pressure on the part of the foot with which it is in contact, so that both the wall of the hoof and the elastic cartilages are pressed outwards, and expansion of the foot occurs. Concurrently with this the weight has been received on the posterior part of the pedal joint with its yielding articulation formed by the navicular bone. By the time the whole foot is flat on the ground, the entire sensitive foot has become depressed within the horny envelope, the heels of the hoof have sunk, and the coronary edge travelled backwards. The body now passes over the foot, the limb revolves as it were around one point, viz. the foot joint, and finally the heels leave the ground, their width becomes decreased, while the final propulsion to the body is given by the toe, which is the last part of the foot to leave the ground.

The hour allotted to this discourse has expired. I have had to take you very hurriedly, and I fear very imperfectly, over a considerable amount of ground, such, indeed, as might have occupied our attention for several lectures, but I trust I have awakened an interest in a very important subject, and that something may have fallen from me which will be of use to you and to that animal to which we are all so much attached.

[F. S.]

GENERAL MONTHLY MEETING,

Monday, May 6, 1895.

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

Ernest H. Fry. Esq. B.Sc.
Henry Irving, Esq. D.Lit.
Thomas Muir, Esq.
Henry Perigal, Esq. F.R.A.S. F R.M.S.
Harold Smith, Esq. F.R.Met.S.
William Stanley Smith, Esq.
Mrs. Slingsby Tanner,

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 at Low Temperatures:—

George Matthey, 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.:—

FROM

The Secretary of State for India—Report on Public Instruction in Bengal for 1893–4. 8vo. 1894.
The Governor-General of India—Geological Survey of India: Records, Vol. XXVIII. Part 1. 8vo. 1895.
Accademia dei Lincei, Reale, Roma—Classe di Scienze Fisiche, Matematiche e Naturali. Atti, Serie Quinta: Rendiconti. 1° Semestre, Vol. IV. Fasc. 6, 7. 8vo. 1895.
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Asiatic Society of Great Britain, Royal—Journal for April, 1895. 8vo.
Astronomical Society, Royal—Monthly Notices, Vol. LV. No. 5. 8vo. 1895.
Bankers, Institute of—Journal, Vol. XVI. Part 4. 8vo. 1895.
Batavia Observatory—Rainfall in East Indian Archipelago, 1893. 8vo. 1894.
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Proceedings, No. 150. 8vo. 1894-5.
Cracovie, l'Académie des Sciences—Bulletin, 1895, No. 3. 8vo.
Cuervo, Don Francisco Ceron T. (the Author)—Projecto de Cartucho de Foguio para Armamento Mauser. 8vo. Madrid, 1895.
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Athenæum for April, 1895. 4to.
Brewers' Journal for April, 1895. 8vo.
Chemical News for April, 1895. 4to.
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Electrical Engineering for April, 1895. 8vo.
Electrical Review for April, 1895. 8vo.
Electric Plant for April, 1895. 4to.
Electricity for April, 1895. 8vo.
Engineer for April, 1895. fol.
Engineering for April, 1895. fol.
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Horological Journal for April, 1895. 8vo.
Industries and Iron for April, 1895. fol
Ironmongery for April, 1895. 4to.
Law Journal for April, 1895. 8vo.
Lightning for April, 1895. 8vo.
Machinery Market for April, 1895. 8vo.
Monist for April, 1895. 8vo.
Nature for April, 1895. 4to.
Nuovo Cimento for April, 1895. 8vo.
Open Court for April, 1895. 4to.
Optician for April, 1895. 8vo.
Photographic News for April, 1895. 8vo.
Photographic Work for April, 1895. 8vo.
Rasa-Ranga-Rahasya, Vol. I. No. 7. 8vo. 1894.
Scots Magazine for April, 1895. 8vo.
Technical World for April, 1895. 8vo.
Transport for April, 1895. fol.
Tropical Agriculturist for April, 1895.
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WEEKLY EVENING MEETING,

Friday, May 10, 1895.

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

The Hon. G. N. Curzon, M.P.

A Recent Journey in Afghanistan.

Direct British interest in Afghanistan dates only from the commencement of the present century, when in consequence of the expanding ambitions of the heirs of the Durani sovereignty, established by Ahmed Shah Abdali in the middle of the last century, the rulers of India found it requisite to enter into political relations with the Amirs of Afghanistan. Since then we have twice fought serious campaigns in that country, which have been in each case clouded with almost irreparable calamity before they terminated in a triumphant vindication of British arms. In the intervals between these two campaigns, and still more since the last, we have entered into diplomatic and treaty relations with the rulers of that country, which relations have now happily terminated in a position where the sovereign of Afghanistan, though subsidised by us, is yet an independent monarch, than whom there is not in Asia any more loyal or convinced ally of the British Crown.

His country is one whose external characteristics do not differ greatly from those of Persia, from which it is divided by no physical separation. A succession of valleys or plains, in which irrigation from streams or underground wells produces a verdure that is as startling in spring and summer as are the corresponding sterility and dearth of colour in the late autumn and winter, are severed from each other by mountain ranges, the normal features of which are brown and unattractive, but which in parts of the Suleiman range on the eastern frontier, and still more, of course, in the main range of the Hindu Kush, which runs from east to west through the centre of Afghanistan like a great vertebral cord, rise to lofty altitudes, scaled with difficulty in summer, and in winter blocked by impassable snows. Cultivation throughout the country is dependent on water, naturally or artificially brought. The inhabitants are in no sense of the term a nation. They have neither common blood, common traditions, common feelings, nor common history. Modern Afghanistan is indeed a purely accidental geographical unit, which has been carved out of the heart of Central Asia by the swords of conquerors or the genius of individual statesmen. The bulk of the people commonly called Afghan belong to the Pathan stock, which is claimed by some as of Semite, but which I believe to be of Aryan origin. But there are also in Afghanistan many people of Persian origin, notably on the Herat side; a large Turki population in the Cis-Oxian provinces of Afghan Turkestan; and very numerous relics of Mongolian emigration in the shape of the Hazâras, who inhabit the centre of the country. These people are bound together by no very strong common feeling, except that of their religion, which is Mohammedan, mainly of the Sunni persuasion; by their love of independence, which is immemorial; by their turbulence, which is unique, even in Asia; and by their enforced respect for a strong sovereign like the present Amir.

My own visit to Afghanistan occurred in November and December last, and was in response to an invitation from the Amir, Abdur Rahman Khan, to visit him at his capital. Though I happened to be the first Englishman who had ever been honoured by such an invitation from an Afghan sovereign, I preferred to regard it, and I think I am justified in regarding it, as a proof of the friendly feelings of the present Amir toward the British people, rather than as a personal compliment towards myself. It was certainly in such a character that I was received and entertained while at Kabul; and whilst this may have added to the responsibilities of the position that I temporarily occupied, it did not diminish either the charm of the compliment or the pleasure of the visit.

I will briefly describe to you the incidents of my journey up to Kabul, my stay there, my interviews with the Amir, and my march down from Kabul to Kandahar, before re-entering India by way of Beluchistan.

At 30 miles from Peshawur the Khyber Pass, after climbing to the elevation of 3600 feet at Lundi Kotal, debouches at Lundi Khana upon Afghan soil. Here I was received by Gholam Haider Khan, the Sipah Salar, or commander-in-chief of the Afghan army, on behalf of the Amir, and by a considerable escort of cavalry. I was alone here and during the whole of my journeys in the country, having only with me a Kashmerian body servant and a Persian jemadar of the Indian Corps of Guides. The Sipah Salar escorted me as far as Jellalabad, so famous for the heroic defence of Sale's Brigade in 1841–42, where I was accommodated in one of the summer palaces of the Amir. Proceeding by marches of about 24 miles a day, and resting at night in tents provided for me by the Amir, it was on the eighth morning after leaving Peshawur that I came in sight of Kabul, and entered that city amid a considerable military display, being escorted to the Ark, or Palace, by large numbers of the mounted bodyguard of the sovereign. There I was accommodated in a building erected by the present Amir at the edge of the palace moat, and adjoining the Salam Khana, or great Durbar Hall.

The city and surroundings of Kabul, with the exception of the indestructible natural features, which remain unchanged, have been very much altered since the experiences of the first, and even of the second Afghan War. The Balar Hissar, or fort in which the British Residency was situated, and in which Sir Louis Cavagnari perished, has been completely dismantled; scarcely a stone stands one upon another; only a few bullet-riddled walls remain to revive the melancholy tale, and the whole is a scene of pitiful desolation. The walls on the Sher Darwaza and Asmai heights are rapidly falling to ruin, and only on the tops of the hills are the forts, which were erected by Lord Roberts, preserved as arsenals or magazines. The city of Kabul is one of the least interesting and the most squalid of the Eastern cities that I have seen. Its situation marks it out as the natural capital of Afghanistan, but it has in it neither external nor interior evidences of grandeur. The public buildings are few and mean. There is the tomb of the Emperor Baber, whose favourite residence was at Kabul, and who died here, which had fallen into complete ruin before it was patched up by the present Amir. There is the brick mausoleum of Timur Shah, the son and successor of Ahmed Shah, which is in a scarcely less lamentable state of decay. The buildings of the native town are of mud, the streets are narrow and dirty, and the bazaars have no beauty. Nevertheless a large population, estimated at 150,000, is congregated within the walls. Whatever of mark or distinction there is in the city it owes to the present Amir. He would like, had he the time and opportunity, to rebuild it on a different and a healthier site. In the meantime, he has removed his official quarters and his residence outside the precincts of the old native town.

The Ark or Palace is situated on the north of the city, between it and the Sherpur cantonment, where Lord Roberts was besieged, and is a large quadrilateral structure, with a parapet, big projecting bastions at the corners and centres of the sides, surrounded by a broad moat. The Amir does not, as a rule, reside here himself, but within it are the Treasury and the Armoury, as well as the Harem Serai, where the ladies reside. The Amir moves about from one to another of the various country residences that he has built on favourable spots in the neighbourhood. It was in the garden of Baber that he nearly died last October, and at the time of my visit he was residing in the Bostan Serai, a European-looking villa, which he has built immediately outside the Palace enclosure.

Before I pass to an account of my interviews with the Amir I may give you some idea of the manner of man he is, at any rate in respect of enlightenment and breadth of view, by pursuing the subject of the changes which Kabul has experienced under his rule. You have all of you probably heard of the workshops and the English employés, and the miniature railway and the European establishments of the Afghan capital. These have been the growth exclusively of the last ten years, and have been due to the initiative of the Amir, assisted by Sir Salter Pyne, the enterprising and capable Englishman who has personally supervised the introduction of these improvements, and who has done so much to familiarise the Afghans with the mechanical implements and inventions of the West.

The workshops are a big range of buildings situated on the banks of the Kabul river, outside the town, where are employed some 1000 or 1100 native men and boys, under the superintendence of 100 Hindoo artificers, who have been trained in the factories and workshops of India. These shops have in the main been designed and are mostly used by the Amir for the output of military material in one form or another, and upon them he expends yearly the greater part of the subsidy which he receives from the Indian Government, instead, as many other Oriental potentates would have done, of retaining it in bullion or converting it into hoarded treasure. There are turned out the most modern and scientific implements of warfare, from Hotchkiss and Gatling guns, made on the exact pattern of the original, down to Martini rifles and Snider carbines. There are manufactured shot and shell, cartridges, cartridge boxes, swords, knives and sabres; saddlery, boots, straps, stirrups, bits, bridles, bugles, scabbards and trunks. You may almost turn in a naked Afghan at one end, like the fabled pig in the stockyards at Chicago, and expect to see him come out at the other end fully accoutred and equipped as a nineteenth century fighting machine.

Nor is this all, for in an adjoining building silver bars are being cut up into the Amir's smart new currency of Kabuli rupees at the rate of 20,000 a day. Hard by, soap is being manufactured at the rate of 10 tons a week, though I very much doubt if any Afghan has yet applied the native composition to his personal toilette. And candles emerge at the rate of 100,000 in the same period. Elsewhere English stallions, one of them a Derby runner, bought from the Prince of Wales, are employed, under the superintendence of an English stud groom, in improving the various native breeds of horses. English sheep and cattle are similarly engaged with the native flocks and herds; while an English tailor makes beautiful uniforms, resplendent with gold lace and epaulettes, with which to deck the bodies of the ministers and officers and grandees. From the street outside the palace the little miniature railway of Kabul starts, and runs for a distance of seven miles to some limestone quarries in the hills, whence it brings down the stone which is employed by the Amir in these and kindred works for the beautification or the strengthening of his capital.

I next turn to my interviews and conversation with the Amir Abdur Rahman Khan. During the fortnight of my stay at Kabul I saw him six times, and rarely for less than three hours at a time. The Amir is a great conversationalist, and as the whole business of the court and the government is conducted by him personally, for the most part by word of mouth, he and his surroundings are equally accustomed to long sittings, or rather standings (for in his presence no Afghan, not even his own sons, sit), extending over the greater part of the day. The audiences were invariably fixed for 1 P.M. At the first and last of these I wore uniform; on the intervening occasions, dressed in London morning costume, I was driven in one of the

Amir's landaus, with a cavalry escort, to the gates of the Bostan Serai, or pavilion, in which he was then residing. At the doors I was met by the Master of the Ceremonies—a very handsome and charming individual, who I hope will come to London next week—by the Captain of the Bodyguard, and by the Amir's private secretary and right-hand-man, bearing the title of Mir Munshi, and by other officials. Escorted by these personages, always in uniform, I walked through the garden to the building, passing through a room in which was a piano, with a case painted by Burne-Jones, and came to an inner apartment where the Amir, who had not then completely recovered from his recent severe illness, and who was, moreover, a victim to chronic gout in his lower limbs, was seated at the upper end on a *charpoy* or low bedstead overspread with silken quilts. I was invited to a chair at the right hand of the Amir. The only other persons seated were Sir S. Pyne, Miss Hamilton, the lady doctor who was at that time looking after the health of the Amir; and on one occasion his eldest son, Prince Habibulla Khan. I have often been asked by what means our conversations were conducted. Persian is the society and court language of Afghanistan, as of all the neighbouring Central Asian states, and though the Amir can also speak Turki and Pushtu (which is the dialect of the ordinary Afghan and Pathan), it is in Persian that he transacts all business and correspondence. My own knowledge of Persian is not adequate either to initiate or to understand a sustained conversation, and our interchange of views was effected through the capable agency of the Mir Munshi before mentioned, who has a thorough acquaintance with both Persian and English. I may here say that in prolonged interviews with an Oriental potentate, where time is of no great importance, and in which words have to be weighed rather carefully, I regard it as of great service not to be too closely acquainted with the language of the country. Such knowledge of an Oriental tongue as a European, even by considerable study, is likely to acquire, is not adequate to enable him to conduct a political or abstruse discussion with any ease, while he is apt to be betrayed by unfamiliarity into blunders of which he is himself unconscious. Given a competent and faithful interpreter, I would sooner converse with any Asiatic potentate in this circuitous way. During the time that he is speaking one has ample opportunity for reflection and consideration of the proper reply; while it is in the power of the interpreter, if skilful, to present the questions or answers of a European in a light which commends itself to the Oriental intellect. The Amir is extraordinarily precise and exacting in his own demands upon the accuracy of the translator, pausing after nearly every word or group of words, to see that it is exactly rendered, and fixing his deep and searching eyes upon the person who is answering or arguing with him.

I regard the Amir Abdur Rahman Khan as a firm and convinced and loyal ally of the British Government. Though he has often had differences with the Indian Government, and though there have been moments when the relations between them were strained, I do not think that on the broad issues of Imperial policy his fidelity has ever been or can be impugned. He is the friend of Great Britain, and he is the foe of Great Britain's foes. He knows that we neither covet nor desire to annex his country. To us he looks in the last resort for the protection of Afghan territory, and for the safeguarding of Afghan freedom. As an independent sovereign, he is compelled, for sake of appearances with his own subjects, to exhibit an independence that to sensitive officialdom is sometimes galling. But at a crisis it is to British advice and arms that he will invariably turn. I may mention, as an instance of this, his intense anxiety to visit England in the present summer. Though when I started from India no one deemed it likely that he would accept the invitation of the British Government to visit this country in the present year, or even if he accepted it that he had any real desire to come, not only before I left Kabul had he unreservedly accepted that invitation, but I do not hesitate to say that he had no keener or more absorbing wish than to carry out this resolve. He told me himself that if he had to be carried he would yet endeavour to come. That he has not been able to do so, and has been compelled to depute a younger son in his place, is due neither to any intrinsic reluctance, nor to any change of plan, but simply to the condition of his own health. If well and strong the Amir could, in my judgment, have left Afghanistan with perfect impunity, and have returned without risk. But in the somewhat precarious state of his health, he has not thought it right either himself to leave his country under physical circumstances which might have developed into a crisis, or to allow his eldest son to go abroad on a long journey at a time when untoward events might have rendered his presence essential at home. I grieve very much for this decision, because I believe that the Amir, by his personality and appearance and character, would have made a profound impression on the British Government and people, and because I believe that the reciprocal influence upon himself would have been not less salutary. And I regret also that his eldest son, Prince Habibulla Khan is not coming, because he is a young man of considerable ability, as well as amiability of character, and is a devoted adherent of the British alliance, and because, if an actual Afghan sovereign could not, it would have been well if a potential Afghan sovereign could have visited English shores. In their joint absence, the Amir will be worthily represented by his second son, Prince Nasrullah Khan, who is a young man of excellent manners and education, and who will, I am sure, be received by the British people with distinction and honour as the representative of his illustrious father. Be it remembered that no Afghan prince has ever yet visited Europe, and that the voluntary presence of such a one as the invited guest of the British sovereign is a conclusive answer to those ill-instructed and timorous persons who are always decrying an interference which they would not themselves have the courage to undertake, much less to carry to a successful issue, but whose unanswerable justification is its final success.

I shall here say nothing of the history of the Amir or of the precarious fortunes of his dynasty. He has occupied the throne since placed there by the British in 1880, after the last Afghan war, and during these fifteen years he has ruled the most turbulent people in Asia with a hand that is only imperfectly described as one of iron. I do not imagine that in the world there is a potentate more feared by such of his people as have reason to fear him than is Abdur Rahman Khan. On the other hand, he is generous and just towards those in whom he confides, and over his personal *entourage* it was obvious that he exercised an influence, one might almost say a fascination, that was supreme.

In Mohammedan countries, as my hearers will be aware, monogamy is exceptional among private individuals, and is still more rarely practised in courts. Abdur Rahman Khan has had several wives, and his two eldest sons are the offspring of a mother who was not of royal blood. Nevertheless, their seniority and priority of claim have never been seriously contested; experience and age, and paternal descent, being, as the Amir explained to me, more valid credentials than the genealogical pedigree or the personal attractions of a mother. By his eldest son, Habibulla Khan, the Amir will, humanly speaking, be succeeded. The prince is a young man of twenty-two years of age, of great charm of manner, very generally popular in the country, and of good abilities. He speaks English a little, and has for years been trained by his father in the functions and practice of government. Should the Amir completely recover his health, Habibulla Khan will still endeavour to come to England in a future year. Perhaps, anyhow, he may one day come as a reigning sovereign. The second son of the Amir, Prince Nasrullah Khan, who will land on these shores next week, is a young man of about twenty years of age, of refined and studious appearance. The Amir told me that he was the scholar of the family, and is devoted to books. In the subdivision of administrative functions among his sons the Amir has entrusted Nasrullah Khan with the study and superintendence of finance, in which he will one day be the minister and adviser of his elder brother. I need not here speak of the younger sons, of whom there are two or three, still boys. During my stay at Kabul I dined on several occasions with Princes Habibulla and Nasrullah Khan, and I look back with great pleasure upon the long evenings, relieved by interesting conversation upon their family history and personal experiences, which I spent in their company. As strict Mohammedans, the Princes ate from separate dishes, but they were indifferent to the qualms occasioned by the convivial presence of an unbeliever.

It was after a fortnight spent in intercourse with this capable and remarkable sovereign and his amiable sons, in riding about the capital and its neighbourhood, and in viewing whatever of interest was there to be seen, that by the Amir's invitation I started on my southward ride of nearly 400 miles through Afghanistan, and rejoined British territory at Chaman in Beluchistan.

It is now nearly fifteen years since, for the space of three weeks, in the month of August 1880, the eyes of all England, and one might well-nigh say of Europe, were riveted in anxious suspense upon that portion of the distance—320 miles in length—that intervenes between Kabul and Kandahar. A brave English general, with 10,000 fighting men, had vanished from all sight or ken within that gap; and not until he reappeared at the other end, and the wires were set going under the ocean and over the land with the news of a signal victory, gained on the very morrow of the completed march, could the world be certain of the wisdom or the foolhardiness of the venture. English readers then learned to be tolerably familiar with the features of the route from Kabul to Kandahar; and Sir F. Roberts' march, because unattended with incident or, until its close, with fighting, did not lose the credit attaching to admirable organisation and leadership, or the romance of its dramatic plunge into the unknown.

Lord Roberts was, however, not the first British general, nor did he command the first British army that marched that way. Only a few months earlier Sir Donald Stewart, the gallant commander-in-chief, had marched up in the reverse direction from Kandahar to Kabul, to relieve Roberts and to open the road. His advance had been not unattended with fighting, and it was the success with which it was crowned that prepared the way and smoothed the path for the returning and punitive column of the younger general. The modern honours of the Kabul-Kandahar road were thus shared by the two commanders.

The physical difficulties of the route are small; and the figures of elevation will show how relatively modest, for a country of mountains like Afghanistan, are the altitudes that require to be crossed. Kabul itself is 5780 feet above the sea. Thence the road ascends by easy slopes towards Ghuzni, which is 7280 feet, crossing on the way the single pass of any height or seriousness that is to be found in the entire distance. This is the Sher Dahan or Lion's Mouth, the *kotal* or crest of which is 8500 feet above the sea. From Ghuzni the track descends by a still less perceptible slope, following the broad valley of the Tarnak river, until at Kelat-i-Ghilzai it again touches the same elevation as Kabul. From there the descent is a little more rapid, conducting to the fertile and well-watered plain from the midst of which, embosomed in trees, rises, at a height of only 3400 feet, the walled quadrilateral of Kandahar. But few rivers require to be crossed in the interval, though occasional small *nullahs* must be bridged for the passage of wheeled artillery. The track, though sometimes encumbered with dust and sand, runs for the most part over a hard and gravelly soil, and is equally accommodated to the movements of infantry and horse. In the autumn and winter an agreeable and inspiriting climate prevails; and although snow may be expected as far south as Kelat-i-Ghilzai in December and January, and sometimes lies thickly at Ghuzni, the road is rarely, if ever, blocked. When I rode along it in December last, no snow had yet fallen, except upon the higher hills; and the glory of these sparkled like a white battlement built skywards on either flank of my line of march. The journey of 320 miles is divided into thirty-one caravan marches. Lord Roberts' army covered it between August 8 and 31, 1880, i.e. in 24 days, including halts. I occupied 11 days on the march, being well-mounted on relays of horses sent on for use by the Amir. But this, which is child's-play for a cavalier decently provided, was a more severe task for my mounted escort of 50 men, who rode the same animals, without change, from Kabul to Kandahar; and on to Chaman—a further distance of 65 miles—whence they turned back again, and, I doubt not, returned to the capital in much the same time. In the East, no one thinks these long and continuous marches astonishing. But at home either our horseflesh is less robust or we treat it more tenderly.

The only places upon this march of which I need here speak are Ghuzni, Kelat-i-Ghilzai and Kandahar, all of which are familiar names in English ears, and are associated with many memories of heroism or of suffering. Ghuzni has been three times captured or occupied by British arms; and 53 years ago a British force endured within the walls of its citadel the horrors of a prolonged midwinter siege. The town occupies a remarkable position, with a lofty acropolis at its north end, and may be compared in more than one particular with the external appearance of Athens. I was lodged in the Bala Hissar, or citadel, from the summit of which is a wonderful outlook over the flat roofs of the Afghan mud houses, on which grain is outspread, or fuel is stacked to dry, over the crumbling walls of the city to the irrigated plots outside, where the Ghuzni river meanders in slow coils, and to the more distant belt of gravelly desert. The defences, both of the city and of the citadel, which have been more than once destroyed by the British, are in a state of irretrievable decay, and would not stand a bombardment of ten minutes. Those who were visiting the East for the first time might perhaps be shocked at the slatternly ruin of places so famous in history as Kabul, Ghuzni and Kandahar, and might read in their tottering parapets and crumbling walls the evidence of a national decline. This would not be a wholly fair inference. Life in the East is a perpetual oscillation between the splendid and the squalid. Things, places and buildings are sometimes repaired. They are never kept in repair. Repair in Asia is the offspring of a temporary emergency or of a passing whim. It is never either a sustained effort or a permanent condition.

About 2½ miles from the Kabul gate of Ghuzni is the tomb of the famous Mahmud of Ghuzni, one of those appalling human visitations who in a single lifetime wrought more suffering to the world than was ever compensated for by a century of peace and prosperity, but who have fortunately been eliminated from the drama of human possibilities by the more organised structure of modern states and by the application of mechanical science to the pursuit of war. It was from this tomb that were carried off by General Nott, in 1842, the famous sandal-wood gates of Somnath, about which Lord Ellenborough issued his absurd and pompous proclamation to the princes and people of India, which were not the gates of Somnath at all, and which now repose in the meritorious obscurity of the magazine at Agra.

Kelat-i-Ghilzai is also a fortified enclosure, erected on the summit of a long and narrow elevation which rises abruptly from the valley of the Tarnak. Like Ghuzni, its walls have witnessed both the heroism of British assault and the sufferings of an arduous siege. Like the defences of Ghuzni, also, they are now in a state of almost complete decay.

In the centre of the fort is a lofty artificial mound or *miri*, on the top of which used to be planted a big gun; but the muzzle was blown off by a young British officer in 1879, and the gun was rolled down the slopes, from which it has since entirely disappeared.

Similar conditions, as regards external repair, or rather the lack of it, prevail in the case of Kandahar. More than any site in Afghanistan is this place associated with British recollections, for it has been in British occupation on two occasions for a longer period than any other Afghan city, and its proximity to the British frontier has at times led some persons to favour the idea of a more complete absorption. To most English readers the name will perhaps suggest the successful battle fought outside its walls by Lord Roberts on September 1, 1880, at the termination of his brilliant march, and it was with no small emotion that I rode over every yard of that historic battlefield. But to me, Kandahar is at least equally interesting as having been the scene of the labours from 1839 to 1842, of that which to my mind was the most gifted and far-seeing English intellect that has in the present century been applied either to the learned study, or to the practical direction of Central Asian affairs, viz., the late Sir Henry Rawlinson. Those of us who tread at a distance in his footsteps, will always regard him as our master, and will lose no opportunity of paying honour to his respected name.

I had a great reception in Kandahar, riding into the city at the head of a large cavalry escort, and being saluted on the Topkhaneh or parade ground, outside the Ark or citadel, by the entire garrison. I was accommodated in the Dowlet Serai, one of the few buildings in the enclosure of the Ark which is in any state of repair; the remainder of the houses and walled enclosures, which 50 years ago used to accommodate the Barukzai Sirdars, having almost completely fallen to pieces. Kandahar is a very busy place, owing to its close proximity to the Indian frontier, and to the great volume of trade that passes through, and the bazaars were always crowded in the morning. I spent several days there, visiting every scene or site of interest in the neighbourhood. But as a warning to possible visitors to Asiatic cities or countries, I would add that unless one knows pretty well beforehand what to ask for and to inspect, one is liable to see nothing at all. No one at Kandahar appeared to know anything about his native city, and I honestly believe that during my stay I imparted much more reliable topographical information to the Kandaharis than I received from them.

From Kandahar I rode in two days to the British frontier at Chaman, in Beluchistan, a distance of 65 miles, where I said good-bye to my Afghan escort and my friendly hosts, and when I again saw British uniforms, heard British voices, and enjoyed the dubious consolations of a railway train.

If I be asked what is the prevailing attitude of the Afghan people towards the British, I should be far from drawing, from the friendly and even cordial nature of my own reception, the illegitimate inference that the Afghans are devotedly attached to the English, or that they have wholly abandoned the suspicions or dislike of an earlier time. The Afghans are not merely, for the most part, Sunni Mohammedans of a somewhat pronounced and prejudiced type, liable to waves of fanaticism and imbued with a natural abhorrence of unbelievers, but they are also a singularly turbulent people, who accept any control, even that of their own ruler, with impatience, who are split up amongst themselves by racial and tribal differences which only a man of blood and iron can temporarily reconcile; addicted, moreover, to violence and bloodshedding, and affording, therefore, about as unfavourable a field for European arts and influences as can anywhere be found. Add to this that their chief historical knowledge of England is derived from two campaigns in which we have invaded their country, have killed many thousands of their people, and have partially destroyed their principal cities, and it will be readily understood that it is as yet early in the day to expect any very extravagant or boisterous evidence of affection. On the other hand, I have not a doubt that Englishmen and English officers are personally liked and respected by the Afghans —because we have courage, which all Orientals respect; because they have learnt from experience to rely on English justice and good faith; and because they find that Englishmen are apt to speak the truth, which they themselves are not. Above all, they realise fully, from the sequel of more than one campaign, and, from the entire events of the past fifteen years, that we entertain no hostile designs upon their country, of which we do not want to possess ourselves of one single yard ; but, on the contrary, that it is to England that they must look for protection and defence against exterior encroachments, to which they would otherwise be exposed. The Amir was constantly saying to me that England and Afghanistan are members of one house, and that conviction, though it may only have originated in the brain of a ruler who is, in many respects, generations ahead of his own people and age, will in time percolate through the various strata of Afghan society. That it will be very much encouraged by the visit of an

Afghan prince to England, and by the complimentary reception which I am sure will await him here, I have no doubt. Thus, by slow degrees, the suspicion and antagonism that have hitherto kept the two people apart will melt; the barriers that have severed Afghanistan from the world will be thrown down, and that state will pass from the category of barbarian to that of civilised communities, under the ægis of the power against whom it once fought with such bitter intensity, but whom it will have learned, from a later and happier experience, to regard as its protector and friend.

[G. N. C.]

WEEKLY EVENING MEETING,

Friday, May 17, 1895.

BASIL WOODD SMITH, Esq. F.R.A.S. F.S.A. Vice-President, in the Chair.

Professor WALTER RALEIGH.

Robert Louis Stevenson.

WHEN a popular writer dies, the question it has become the fashion with a nervous generation to ask, is the question, "Will he live?" There is no idler question, none more hopelessly impossible and unprofitable to answer. It is one of the many vanities of criticism to promise immortality to the authors that it praises, to patronise a writer with the assurance that our great-grandchildren, whose time and tastes are thus frivolously mortgaged, will read his works with delight. But "there is no antidote against the opium of time, which temporally considereth all things: our fathers find their graves in our short memories, and sadly tell us how we may be buried in our survivors." Let us make sure that our sons will care for Homer before we pledge a more distant generation to a newer cult.

Nevertheless, without handling the prickly question of literary immortality, it is easy to recognise that the literary reputation of Robert Louis Stevenson is made of good stuff. His fame has spread, as lasting fame is wont to do, from the few to the many. Fifteen years ago his essays and fanciful books of travel were treasured by a small and discerning company of admirers; long before he chanced to fell the British public with 'Treasure Island' and 'Dr. Jekyll and Mr. Hyde' he had shown himself a delicate marksman. And although large editions are nothing, standard editions, richly furnished and complete, are worthy of remark. Stevenson is one of the very few authors in our literary history who have been honoured during their lifetime by the appearance of such an edition; the best of his public, it would seem, do not only wish to read his works, but to possess them, and all of them, at the cost of many pounds, in library form. It would be easy to mention more voluminous and more popular authors than Stevenson whose publishers could not find five subscribers for an adventure like this. He has made a brave beginning in that race against Time which all must lose.

It is not in the least necessary, after all, to fortify ourselves with the presumed consent of our poor descendants, who may have a world of other business to attend to, in order to establish Stevenson in the position of a great writer. Let us leave that foolish trick to the politicians, who never claim that they are right—merely that they will win at the next elections. Literary criticism has standards other than the suffrage; it is possible enough to say something of the literary quality of a work that appeared yesterday. Stevenson himself was singularly free from the vanity of fame; "the best artist," he says truly, "is not the man who fixes his eye on posterity, but the one who loves the practice of his art." He loved, if ever man did, the practice of his art; and those who find meat and drink in the delight of watching and appreciating the skilful practice of the literary art, will abandon themselves to the enjoyment of his masterstrokes without teasing their unborn and possibly illiterate posterity to answer solemn questions. Will a book live? Will a cricket match live? Perhaps not, and yet both be fine achievements.

It is not easy to estimate the loss to letters by his early death. In the dedication of 'Prince Otto' he says, "Well, we will not give in that we are finally beaten. . . . I still mean to get my health again; I still purpose, by hook or crook, this book or the next, to launch a masterpiece." It would be a churlish or a very dainty critic who should deny that he has launched masterpieces, but whether he ever launched *his* masterpiece is an open question. Of the story that he was writing just before his death he is reported to have said that "the goodness of it frightened him." A goodness that frightened him will surely not be visible, like Banquo's ghost, to only one pair of eyes. His greatest was perhaps yet to come. Had Dryden died at his age, we should have had none of the great satires; had Scott died at his age, we should have had no Waverley Novels. Dying at the height of his power, and in the full tide of thought and activity, he seems almost to have fulfilled the aspiration and unconscious prophecy of one of the early essays:—

"Does not life go down with a better grace foaming in full body over a precipice, than miserably straggling to an end in sandy deltas?

"When the Greeks made their fine saying that those whom the gods love die young, I cannot help believing that they had this sort of death also in their eye. For surely, at whatever age it overtake the man, this is to die young. Death has not been suffered to take so much as an illusion from his heart. In the hot-fit of life, a-tiptoe on the highest point of being, he passes at a bound on to the other side. The noise of the mallet and chisel is scarcely quenched, the trumpets are hardly done blowing, when, trailing with him clouds of glory, this happy starred, full-blooded spirit shoots into the spiritual land."

But we on this side are the poorer—by how much we can never know. What strengthens the conviction that he might yet have surpassed himself and dwarfed his own best work is, certainly no immaturity, for the flavour of wisdom and old experience hangs about his earliest writings, but a vague sense awakened by that brilliant series of books, so diverse in theme, so slight often in structure and occasion, so gaily executed, that here was a finished literary craftsman, who had served his period of apprenticeship and was playing with his tools. The pleasure of wielding the graving tool, the itch of craftmanship, was strong upon him, and many of the works he has left are the overflow of a laughing energy, arabesques carved on the rock in the artist's painless hours.

All art, it is true, is play of a sort; the "sport-impulse" (to translate a German phrase) is deep at the root of the artist's power; Sophocles, Shakespeare, Molière, and Goethe, in a very profound sense, made game of life. But to make game of life was to each of these the very loftiest and most imperative employ to be found for him on this planet; to hold the mirror up to Nature so that for the first time she may see herself, to "be a candle-holder and look on" at the pageantry which, but for the candle-holder, would huddle along in the undistinguishable blackness, filled them with the pride of place. Stevenson had the sport-impulse at the depths of his nature, but he also had, perhaps he had inherited, an instinct for work in more blockish material, for lighthouse-building and iron-founding. In a 'Letter to a Young Artist,' contributed to a magazine years ago, he compares the artist in paint or in words to the keeper of a booth at the world's fair, dependent for his bread on his success in amusing others. In his volume of poems he almost apologises for his excellence in literature:—

"Say not of me, that weakly I declined
The labours of my sires, and fled the sea,
The towers we founded, and the lamps we lit,
To play at home with paper like a child;
But rather say: *In the afternoon of time*
A strenuous family dusted from its hands
A sand of granite, and beholding far
Along the sounding coasts its pyramids
And tall memorials catch the dying sun,
Smiled well content, and to this childish task
Around the fire addressed its evening hours."

Some of his works are, no doubt, best described as paper-games. In 'The Wrong Box,' for instance, there is something very like the card-game commonly called "Old Maid"; the odd card is a superfluous corpse, and each dismayed recipient in turn assumes a disguise and a pseudonym and bravely passes on that uncomfortable inheritance. It is an admirable farce, hardly touched with grimness, unshaken by the breath of reality, full of fantastic character; the strange funeral procession is attended by shouts of glee at each of its stages, and finally melts into space.

But, when all is said, it is not with work of this kind that Olympus is stormed; art must be brought closer into relation with life, these airy and delightful freaks of fancy must be subdued to a serious scheme if they are to serve as credentials for a seat among the immortals. The decorative painter, whose pencil runs so freely in limning these half-human processions of outlined fauns and wood-nymphs, is asked at last to paint an easel-picture.

Stevenson is best where he shows most restraint, and his peculiarly rich fancy, which ran riot at the suggestion of every passing whim, gave him, what many a modern writer sadly lacks, plenty to restrain, an exuberant field for self-denial. Here was an opportunity for art and labour; the luxuriance of the virgin forests of the West may be clipped and pruned for a lifetime with no fear of reducing them to the trim similitude of a Dutch garden. His bountiful and generous nature could profit by a spell of training that would emaciate a poorer stock. From the first, his delight in earth and the earth-born was keen and multiform; his zest in life

> " . . . put a spirit of youth in everything,
> That heavy Saturn laughed and leaped with him;"

and his fancy, light and quick as a child's, made of the world around him an enchanted pleasance. The realism, as it is called, that deals only with the banalities and squalors of life, and weaves into the mesh of its story no character but would make you yawn if you passed ten minutes with him in a railway carriage, might well take a lesson from this man, if it had the brains. Picture to yourself (it is not hard) an average suburb of London. The long rows of identical bilious brick houses, with the inevitable lace curtains, a symbol merely of the will and power to wash; the awful nondescript object, generally under glass, in the front window—the shrine of the unknown god of art; the sombre invariable citizen, whose garb gives no suggestion of his occupation or his tastes—a person, it would seem, only by courtesy; the piano-organ the music of the day, and the hideous voice of the vendor of halfpenny papers the music of the night; could anything be less promising than such a row of houses for the theatre of romance? Set a realist to walk down one of these streets: he will inquire about milk bills and servants' wages, latch-keys and Sunday avocations, and come back with a tale of small meannesses and petty respectabilities written in the approved modern fashion. Yet Stevenson, it seems likely, could not pass along such a line of brick bandboxes without having his pulses set a-throbbing by the imaginative possibilities of the place. Of his own Lieutenant Brackenbury Rich he says:—

"The succession of faces in the lamplight stirred the lieutenant's imagination; and it seemed to him as if he could walk for ever in that stimulating city atmosphere and surrounded by the mystery of four million private lives. He glanced at the houses and marvelled what was passing behind those warmly lighted windows; he looked into face after face, and saw them each intent upon some unknown interest, criminal or kindly."

It was that same evening that Prince Florizel's friend, under the name of Mr. Morris, was giving a party in one of the houses of West Kensington. In one at least of the houses of that brick wilderness human spirits were being tested as on an anvil, and most of them tossed aside. So also, in 'The Rajah's Diamond,' it was a quiet suburban garden that witnessed the sudden apparition of Mr. Harry Hartley and his treasures precipitated over the wall; it was in the same garden that the Rev. Simon Rolles suddenly, to his own surprise, became a thief. A monotony of bad building is no doubt a bad thing, but it cannot paralyse the activities or frustrate the agonies of the mind of man.

To a man with Stevenson's live and searching imagination, every work of human hands became vocal with possible associations. Buildings positively chattered to him; the little inn at Queensferry, which even for Scott had meant only mutton and currant jelly, with cranberries "vera weel preserved," gave him the cardinal incident of 'Kidnapped.' How should the world ever seem dull or sordid to one whom a railway station would take into its confidence, to whom the very flagstones of the pavement told their story; in whose mind, "the effect of night, of any flowing water, of lighted cities, of the peep of day, of ships, of the open ocean," called up "an army of anonymous desires and pleasures"? To have the "golden-tongued Romance with serene lute" for a mistress and familiar is to be fortified against the assaults of tedium.

His attitude towards the surprising and momentous gift of life was one prolonged passion of praise and joy. There is none of his books that reads like the meditations of an invalid. He has the readiest sympathy for all exhibitions of impulsive energy; his heart goes out to a sailor, and leaps into ecstasy over a generous adventurer or buccaneer. Of one of his earlier books he says: "From the negative point of view I flatter myself this volume has a certain stamp. Although it runs to considerably upwards of two hundred pages, it contains not a single reference to the imbecility of God's universe, nor so much as a single hint that I could have made a better one myself." And this was an omission that he never remedied in his later works. Indeed, his zest in life, whether lived in the back gardens of a town or on the high seas, was so great that it seems probable the writer would have been lost had the man been dowered with better health.

> "Whereas my birth and spirit rather took
> The way that takes the town,
> Thou didst betray me to a ling'ring book,
> And wrap me in a gown,"

says George Herbert, who, in his earlier ambitions, would fain have ruffled it with the best at the court of King James. But from Stevenson, although not only the town, but oceans and continents, beckoned him to deeds, no such wail escaped. His indomitable cheerfulness was never embarked in the cock-boat of his own prosperity. A high and simple courage shines through all his writings. It is supposed to be a normal human feeling for those who are hale to sympathise with others who are in pain. Stevenson reversed the position, and there is no braver spectacle in literature than to see him not asking others to lower their voices in his sick-room, but raising his own voice that he may make them feel at ease and avoid imposing his misfortunes on their notice. "Once when I was groaning aloud with physical pain," he says in the essay on 'Child's Play,' "a young gentleman came into the room and nonchalantly inquired if I had seen his bow and arrow. He made no account of my groans, which he accepted, as he had to accept so much else, as a piece of the inexplicable conduct of his elders; and, like a wise young gentleman, he would waste no wonder on the subject." Was there ever a passage like this? The sympathy of the writer is wholly with the child, and the child's absolute indifference to his own sufferings. It might have been safely predicted that this man, should he ever attain to pathos, would be free from the facile, maudlin pathos of the hired sentimentalist.

And so, also, with what Dr. Johnson has called "metaphysical distresses." It is striking enough to observe how differently the quiet monasteries of the Carthusian and Trappist brotherhoods affected Matthew Arnold and Robert Louis Stevenson. In his well-known elegiac stanzas Matthew Arnold likens his own state to that of the monks:—

"Wandering between two worlds, one dead,
The other powerless to be born,
With nowhere yet to rest my head,
Like these on earth I wait forlorn.
Their faith, my tears, the world deride,—
I come to shed them at their side!"

To Stevenson, on the other hand, our Lady of the Snows is a mistaken divinity, and the place a monument of chilly error,—for once in a way he takes it upon himself to be a preacher, his temperament gives voice in a creed:—

"And ye, O brethren, what if God,
When from Heaven's top He spies abroad,
And sees on this tormented stage
The noble war of mankind rage,
What if His vivifying eye,
O monks, should pass your corner by?
For still the Lord is Lord of might;
In deeds, in deeds, he takes delight;
The plough, the spear, the laden barks,
The field, the founded city, marks;
He marks the smiler of the streets,
The singer upon garden seats;
He sees the climber in the rocks;
To Him, the shepherd folds his flocks;
For those He loves that underprop
With daily virtues Heaven's top,
And bear the falling sky with ease,
Unfrowning Caryatides.
Those He approves that ply the trade,
That rock the child, that wed the maid,
That with weak virtues, weaker hands,
Sow gladness on the peopled lands,
And still with laughter, song, and shout
Spin the great wheel of earth about.

But ye?—O ye who linger still
Here in your fortress on the hill,
With placid face, with tranquil breath,
The unsought volunteers of death,
Our cheerful General on high
With careless looks may pass you by!"

And the fact of death, which has damped and darkened the writings of so many minor poets, does not cast a pallor on his conviction. Life is of value, only because it can be spent, or given; and the love of God coveted the position, and assumed mortality. If a man treasure and hug his life, one thing only is certain, that he will be robbed some day, and cut the pitiable and futile figure of one who has been saving candle-ends in a house that is on fire. Better than this to have a foolish spendthrift blaze and the loving cup going round. Stevenson speaks almost with a personal envy of the conduct of the four marines of the *Wager*. There was no room for them in the boat, and they were left on a desert island to a certain death. "They were soldiers, they said, and knew well enough it was their business to die; and as their comrades pulled away, they stood upon the beach, gave three cheers, and cried, 'God bless the king!' Now, one or two of those who were in the boat escaped, against all likelihood, to tell the story. That was a great thing for us"—even when life is extorted it may be given nobly, with ceremony and courtesy. So strong was Stevenson's admiration for heroic graces like these that in the requiem that appears in his poems he speaks of an ordinary death as of a hearty exploit, and draws his figures from lives of adventure and toil:—

"Under the wide and starry sky
Dig the grave and let me lie.
Glad did I live and gladly die,
And I laid me down with a will.
This be the verse you grave for me:
Here he lies where he longed to be,
Home is the sailor, home from the sea,
And the hunter home from the hill."

This man should surely have been honoured with the pomp and colour and music of a soldier's funeral.

The most remarkable feature of the work he has left is its singular combination of style and romance. It has so happened, and the accident has gained almost the strength of a tradition, that the most assiduous followers of romance have been careless stylists. They have trusted to the efficacy of their situation and incident, and have too often cared little about the manner of its presentation. By an odd piece of irony, style has been left to the cultivation of those who have little or nothing to tell. Sir Walter Scott himself, with all his splendid romantic and tragic gifts, often, in Stevenson's perfectly just phrase, "fobs us off with languid and inarticulate twaddle." He wrote carelessly and genially, and then breakfasted, and began the business of the day. But Stevenson, who had romance tingling in every vein of his body, set himself laboriously and patiently to train his other faculty, the faculty of style.

I. STYLE.

Let no one say that "reading and writing comes by nature," unless he is prepared to be classed with the foolish burgess who said it first. A poet is born, not made—so is every man—but he is born raw. Stevenson's life was a grave devotion to the education of himself in the art of writing.

> "The lyf so short, the craft so long to lerne,
> Thassay so hard, so sharp the conquering."

Those who deny the necessity, or decry the utility, of such an education, are generally deficient in a sense of what makes good literature—they are "word-deaf," as others are colour-blind. All writing is a kind of word-weaving; a skilful writer will make a splendid tissue out of the diverse fibres of words. But to care for words, to select them judiciously and lovingly, is not in the least essential to all writing, all speaking; for the sad fact is this, that most of us do our thinking, our writing, and our speaking in phrases, not in words. The work of a feeble writer is always a patchwork of phrases, some of them borrowed from the imperial texture of Shakespeare and Milton, others picked up from the rags in the street. We make our very kettle-holders of pieces of a king's carpet. How many overworn quotations from Shakespeare suddenly leap into meaning and brightness when they are seen in their context! "The cry is still, 'They come!'"; "More honoured in the breach than the observance"—the sight of these phrases in the splendour of their dramatic context in 'Macbeth' and 'Hamlet' casts shame upon their daily degraded employments. But the man of affairs has neither the time to fashion his speech, nor the knowledge to choose his words, so he borrows his sentences ready made, and applies them in rough haste to purposes that they do not exactly fit. Such a man inevitably repeats, like the cuckoo, monotonous catchwords, and lays his eggs of thought in the material that has been woven into consistency by others. It is a matter of natural taste, developed and strengthened by continual practice, to avoid being the unwitting slave of phrases.

The artist in words, on the other hand, although he is a lover of fine phrases, in his word-weaving experiments uses no shoddy, but cultivates his senses of touch and sight until he can combine the raw fibres in novel and bewitching patterns. To this end he must have two things: a fine sense, in the first place, of the sound, value, meaning, and associations of individual words, and next, a sense of harmony, proportion, and effect in their combination. It is amazing what nobility a mere truism is often found to possess when it is clad with a garment thus woven.

Stevenson had both these sensitive capabilities in a very high degree. His careful choice of epithet and name have even been criticised as lending to some of his narrative-writing an excessive air of deliberation. His daintiness of diction is best seen in his earlier work; thereafter his writing became more vigorous and direct, fitter for its later uses, but never unillumined by felicities that cause a thrill of pleasure to the reader. Of the value of words he had the acutest appreciation. 'Virginibus Puerisque,' his first book of essays, is crowded with happy hits and subtle implications conveyed in a single word. "We have all heard," he says in one of these, "of cities in South America built upon the side of fiery mountains, and how, even in this tremendous neighbourhood, the inhabitants are not a jot more impressed by the solemnity of mortal conditions than if they were delving gardens in the greenest corner of England." You can feel the ground shake and see the volcano tower above you at that word "*tremendous* neighbourhood." Something of the same double reference to the original and acquired meanings of a word is to be found in such a phrase as "sedate electrician," for one who in a back office wields all the lights of a city; or in that description of one drawing near to death, who is spoken of as groping already with his hands "on the face of the *impassable*." The likeness of this last word to a very different word, "*impassive*," is made to do good literary service in suggesting the sphinx-like image of death. Sometimes, as here, this subtle sense of double meanings almost leads to punning. In 'Across the Plains' Stevenson narrates how a bet was transacted at a railway station, and subsequently, he supposes, "*liquidated* at the bar." This is perhaps an instance of the excess of a virtue, but it is an excess to be found plentifully in the works of Milton.

His loving regard for words bears good fruit in his later and more stirring works. He has a quick ear and appreciation for live phrases on the lips of tramps, beach-combers, or Americans. In 'The Beach of Falesá' the sea captain who introduces the new trader to the South Pacific island, where the scene of the story is laid, gives a brief description of the fate of the last dealer in copra. It may serve as a single illustration of volumes of racy, humorous, and imaginative slang:—

"'Do you catch a bit of white there to the east'ard?" the captain continued. 'That's your house. . . . When old Adams saw it he took and shook me by the hand. "I've dropped into a soft thing here," says he. "So you have," says I. . . . Poor Johnny! I never saw him again but the once . . . and the next time we came round there he was dead and buried. I took and put up a bit of a stick to him: "John Adams, *obit* eighteen and sixty-eight. Go thou and do likewise." I miss that man. I never could see much harm in Johnny.'

"'What did he die of?' I inquired.

"'Some kind of sickness,' says the captain. 'It appears it took him sudden. Seems he got up in the night, and filled up on Pain Killer and Kennedy's Discovery. No go—he was booked beyond

Kennedy. Then he had tried to open a case of gin. No go again: not strong enough. . . . Poor John!'"

There is a world of abrupt, homely talk like this to be found in the speech of Captain Nares and of Jim Pinkerton in 'The Wrecker'; and a wealth of Scottish dialect, similar in effect, in 'Kidnapped,' 'Catriona,' and many other stories. It was a delicate ear and a sense trained by practice that picked up these vivid turns of speech, some of them perhaps heard only once, and a mind given to dwell on words, that remembered them for years, and brought them out when occasion arose.

But the praise of Stevenson's style cannot be exhausted in a description of his use of individual words or his memory of individual phrases. His mastery of syntax, the orderly and emphatic arrangements of words in sentences, a branch of art so seldom mastered, was even greater. And here he could owe no great debt to his romantic predecessors in prose. Dumas, it is true, is a master of narrative, but he wrote in French, and a style will hardly bear expatriation. Scott's sentences are, many of them, shambling, knock-kneed giants. Stevenson harked further back for his models, and fed his style on the most vigorous of the prose writers of the seventeenth and early eighteenth centuries, the golden age of English prose. "What English those fellows wrote!" says Fitzgerald in one of his letters; "I cannot read the modern mechanique after them." And he quotes a passage from Harrington's 'Oceana':—

"This free-born Nation lives not upon the dole or Bounty of One Man, but distributing her Annual Magistracies and Honours with her own hand, is herself King People."

It was from writers of Harrington's time and later that Stevenson learned something of his craft. Bunyan and Defoe should be particularly mentioned, and that later excellent worthy, Captain Charles Johnson, who compiled the ever-memorable 'Lives of Pirates and Highwaymen.' Mr. George Meredith is the chief of those very few modern writers whose influence may be detected in his style.

However it was made, and whencesoever the material or suggestion borrowed, he came by a very admirable instrument for the telling of stories. Those touches of archaism that are so frequent with him, the slightly unusual phrasing, or unexpected inversion of the order of words, show a mind alert in its expression, and give the sting of novelty even to the commonplaces of narrative or conversation. A nimble literary tact will work its will on the phrases of current small-talk, remoulding them nearer to the heart's desire, transforming them to its own stamp. This was what Stevenson did, and the very conversations that pass between his characters have an air of distinction that is all his own. His books are full of brilliant talk—talk real and convincing enough in its purport and setting, but purged of the languors and fatuities of actual commonplace conversation. It is an enjoyment like that to be obtained from a brilliant exhibition of fencing, clean and dexterous, to assist at the talking bouts of

David Balfour and Miss Grant, Captain Nares and Mr. Dodd, Alexander Mackellar and the Master of Ballantrae, Prince Otto and Sir John Crabtree, or those wholly admirable pieces of special pleading to be found in 'A Lodging for the Night' and 'The Sire de Malétroit's Door.' But people do not talk like this in actual life—"'tis true, 'tis pity; and pity 'tis, 'tis true." They do not; in actual life conversation is generally so smeared and blurred with stupidities, so invaded and dominated by the spirit of dulness, so liable to swoon into meaninglessness, that to turn to Stevenson's books is like an escape into mountain air from the stagnant vapours of a morass. The exact reproduction of conversation as it occurs in life can only be undertaken by one whose natural dulness feels itself incommoded by wit and fancy as by a grit in the eye. Conversation is often no more than a nervous habit of body, like twiddling the thumbs, and to record each particular remark is as much as to describe each particular twiddle. Or in its more intellectual uses, when speech is employed, for instance, to conceal our thoughts, how often is it a world too wide for the shrunken nudity of the thought it is meant to veil, and thrown over it, formless, flabby, and black—like a tarpaulin! It is pleasant to see thought and feeling dressed for once in the trim, bright raiment Stevenson devised for them.

There is an indescribable air of distinction, which is, and is not, one and the same thing with style, breathing from all his works. Even when he is least inspired, his bearing and gait could never be mistaken for another man's. All that he writes is removed by the width of the spheres from the possibility of commonplace, and he avoids most of the snares and pitfalls of genius with noble and unconscious skill.

If he ever fell into one of these—which may perhaps be doubted—it was through too implicit a confidence in the powers of style. His open letter to the Rev. Dr. Hyde in vindication of Father Damien is perhaps his only literary mistake. It is a matchless piece of scorn and invective, not inferior in skill to anything he ever wrote. But that it was well done is no proof that it should have been done at all. "I remember Uzzah and am afraid," said the wise Erasmus, when he was urged to undertake the defence of Holy Church; "it is not every one who is permitted to support the Ark of the Covenant." And the only disquietude suggested by Stevenson's letter is a doubt whether he really has a claim to be Father Damien's defender, whether Father Damien had need of the assistance of a literary freelance. The Saint who was bitten in the hand by a serpent shook it off into the fire and stood unharmed. As it was in the Mediterranean, so it was also in the Pacific, and there is something officious in the intrusion of a spectator, something irrelevant in the plentiful pronouns of the first person singular to be found sprinkled over Stevenson's letter. The curse spoken in Eden, "Upon thy belly shalt thou go, and dust shalt thou eat all the days of thy life," surely covered by anticipation the case of the Rev. Dr. Hyde.

II. Romance.

The faculty of romance, the greatest of the gifts showered on Stevenson's cradle by the fairies, will suffer no course of development; the most that can be done with it is to preserve it on from childhood unblemished and undiminished. It is of a piece with Stevenson's romantic ability that his own childhood never ended; he could pass back into that airy world without an effort. In his stories, his imagination worked on the old lines, but it became conscious of its working. And the highest note of these stories is not drama, nor character, but romance. In one of his essays he defines the highest achievement of romance to be the embodiment "of character, thought, or emotion in some act or attitude that shall be remarkably striking to the mind's eye." His essay on Victor Hugo shows how keenly conscious he was that narrative romance can catch and embody emotions and effects that are for ever out of the reach of the drama proper, and of the essay or homily, just as they are out of the reach of sculpture and painting. Now, it is precisely in these effects that the chief excellence of romance resides; it was the discovery of a world of these effects, insusceptible of treatment by the drama, neglected entirely by the character-novel, which constituted the Romantic revival of the end of last century. "The artistic result of a romance," says Stevenson, "what is left upon the memory by any powerful and artistic novel, is something so complicated and refined that it is difficult to put a name upon it, and yet something as simple as nature. . . . The fact is, that art is working far ahead of language as well as of science, realising for us, by all manner of suggestions and exaggerations, effects for which as yet we have no direct name, for the reason that these effects do not enter very largely into the necessities of life. Hence alone is that suspicion of vagueness that often hangs about the purpose of a romance; it is clear enough to us in thought, but we are not used to consider anything clear until we are able to formulate it in words, and analytical language has not been sufficiently shaped to that end." He goes on to point out that there is an epical value about every great romance, an underlying idea, not presentable always in abstract or critical terms, in the stories of such masters of pure romance as Victor Hugo and Nathaniel Hawthorne.

The progress of romance in the present century has consisted chiefly in the discovery of new exercises of imagination and new subtle effects in story. Fielding, as Stevenson says, did not understand that the nature of a landscape or the spirit of the times could count for anything in a story; all his actions consist of a few simple personal elements. With Scott, vague influences that qualify a man's personality begin to make a large claim; "the individual characters begin to occupy a comparatively small proportion of that canvas on which armies manœuvre and great hills pile themselves upon each other's shoulders." And the achievements of the great masters since

Scott—Hugo, Dumas, Hawthorne, to name only those in Stevenson's direct line of ancestry—have added new realms to the domain of romance.

What are the indescribable effects that romance, casting far beyond problems of character and conduct, seeks to realise? What is the nature of the great informing, underlying idea that animates a truly great romance—'The Bride of Lammermoor,' 'Monte Christo,' 'Les Misérables,' 'The Scarlet Letter,' 'The Master of Ballantrae'? These questions can only be answered by deforming the impression given by each of these works to present it in the chop-logic language of philosophy. But an approach to an answer may be made by illustration.

In his 'American Notebooks' Nathaniel Hawthorne used to jot down subjects for stories as they struck him. His successive entries are like the souls of stories awaiting embodiment, which many of them never received; they bring us very near to the workings of the mind of a great master. Here are some of them:—

"A sketch to be given of a modern reformer, a type of the extreme doctrines on the subject of slaves, cold water, and the like. He goes about the streets haranguing most eloquently, and is on the point of making many converts, when his labours are suddenly interrupted by the appearance of the keeper of a madhouse whence he has escaped. Much may be made of this idea."

"The scene of a story or sketch to be laid within the light of a street lantern; the time, when the lamp is near going out; and the catastrophe to be simultaneous with the last flickering gleam."

"A person to be writing a tale and to find it shapes itself against his intentions; that the characters act otherwise than he thought, and a catastrophe comes which he strives in vain to avert. It might shadow forth his own fate—he having made himself one of the personages."

"Two persons to be expecting some occurrence and watching for the two principal actors in it, and to find that the occurrence is even then passing, and that they themselves are the two actors."

"A satire on ambition and fame from a statue of snow."

Hawthorne used this idea in one of his sketches.

"A moral philosopher to buy a slave, or otherwise get possession of a human being, and to use him for the sake of experiment by trying the operation of a certain vice on him."

M. Bourget, the French romancer, has made use of this idea in his novel called 'Le Disciple.' Only it is not a slave, but a young girl whom he pretends to love, that is the subject of the moral philosopher's experiment; and a noisy war has been waged round the book in France. Hawthorne would plainly have seized the romantic essence of the idea and would have avoided the boneyard of "problem morality."

"A story the principal personage of which shall seem always on the point of entering on the scene, but shall never appear."

This is the device that gives fascination to the figures of Richelieu in 'Marion Delorme' and of Captain Flint in 'Treasure Island.'

"The majesty of death to be exemplified in a beggar, who, after being seen humble and cringing in the streets of a city for many years, at length by some means or other gets admittance into a rich man's mansion, and there dies—assuming state, and striking awe into the breasts of those who had looked down upon him."

These are all excellent instances of the sort of idea that gives life to a romance—of acts or attitudes that stamp themselves upon the mind's eye. Some of them appeal chiefly to the mind's eye, others are of value chiefly as symbols. But, for the most part, the romantic kernel of a story is neither pure picture nor pure allegory, it can neither be painted nor moralised. It makes its most irresistible appeal neither to the eye that searches for form and colour, nor to the reason that seeks for abstract truth, but to the blood, to all that dim instinct of danger, mystery, and sympathy in things that is man's oldest inheritance—to the superstitions of the heart. Romance vindicates the supernatural against science, and rescues it from the palsied tutelage of morality.

Stevenson's work is a gallery of romantic effects that haunt the memory. Some of these are directly pictorial: the fight in the round-house on board the brig *Covenant*; the duel between the two brothers of Ballantrae in the island of light thrown up by the candles from that abyss of windless night; the flight of the Princess Seraphina through the dark mazes of the wood,—all these, although they carry with them subtleties beyond the painter's art, yet have something of picture in them. But others make entrance to the corridors of the mind by blind and secret ways, and there awaken the echoes of primeval fear. The cry of the parrot—"Pieces of eight"—the tapping of the stick of the blind pirate Pew as he draws near the inn parlour, and the similar effects of inexplicable terror wrought by the introduction of the blind catechist in 'Kidnapped,' and of the disguise of a blind leper in 'The Black Arrow,' are beyond the reach of any but the literary form of romantic art. The last appearance of Pew, in the play of 'Admiral Guinea,' written in collaboration with Mr. W. E. Henley, is perhaps the masterpiece of all the scenes of terror. The blind ruffian's scream of panic fear, when he puts his groping hand into the burning flame of the candle in the room where he believed that he was unseen, and so realises that his every movement is being silently watched, is indeed "the horrors come alive."

The animating principle or idea of Stevenson's longer stories is never to be found in their plot, which is generally built carelessly and disjointedly enough around the central romantic situation or conception. The main situation in 'The Wrecker' is a splendid product of romantic inspiration, but the structure of the story is incoherent and ineffective, so that some of the best passages in the book—the scenes in Paris, for instance—have no business there at all. The story in 'Kidnapped' and 'Catriona' wanders on in a single thread, like the pageant of a dream, and the reader feels and sympathises with the author's obvious difficulty in bringing it back to the scene of the trial and execution of James Stewart. 'The Master of Ballantrae' is stamped with a magnificent unity of conception, but the story illuminates that conception by a series of scattered episodes. That lurid embodiment of fascinating evil, part vampire, part Mephistopheles, whose grand manner and heroic abilities might have made him a great and good man but "for the malady of not wanting," is the light and meaning of the whole book. Innocent and benevolent lives are thrown in his way that he may mock or distort or shatter them. Stevenson never came nearer than in this character to the sublime of power.

But an informing principle of unity is more readily to be apprehended in the shorter stories, and it is a unity not so much of plot as of impression and atmosphere. His islands, whether situated in the Pacific or off the coast of Scotland, have each of them a climate of its own, and the character of the place seems to impose itself on the incidents that occur, dictating subordination or contrast. The events that happen within the limits of one of these magic isles could in every case be cut off from the rest of the story and framed as a separate work of art. The long starvation of David Balfour on the island of Earraid, the sharks of crime and monsters of blasphemy that break the peace of the shining tropical lagoons in 'Treasure Island' and 'The Ebb Tide,' the captivity on the Bass Rock in 'Catriona,' the supernatural terrors that hover and mutter over the island of 'The Merry Men,'—these imaginations are plainly generated by the scenery against which they are thrown; each is in some sort the genius of the place it inhabits.

In his search for the treasures of romance, Stevenson adventured freely enough into the realm of the supernatural. When he is handling the superstitions of the Scottish people, he allows his humorous enjoyment of their extravagance to peep out from behind the solemn dialect in which they are dressed. The brief tale of 'Thrawn Janet,' and Black Andy's story of Tod Lapraik in 'Catriona,' are grotesque imaginations of the school of 'Tam o' Shanter' rather than of the school of Shakespeare, who deals in no comedy ghosts. They are turnip-lanterns swayed by a laughing urchin, proud of the fears he can awaken. Even the 'Strange Case of Dr. Jekyll and Mr. Hyde' and the story of 'The Bottle Imp' are manufactured bogies, that work on the nerves and not on the heart, whatever may be said by those who insist on seeing allegory in what is only dream-fantasy. The supernatural must be rooted deeper than these in life and experience if it is to reach an imposing stature: the true ghost is the shadow of a man. And Stevenson shows a sense of this in two of his very finest stories, the exquisite idyll of 'Will o' the Mill' and the grim history of 'Markheim.' Each of these stories is the work of a poet, by no means of a goblin-fancier. The personification of Death is as old as poetry: it is wrought with moving gentleness in that last scene in the arbour of Will's Inn. The wafted scent of the heliotropes, which had never been planted in the garden since Marjory's death, the light in the room that had been hers, prelude the arrival at the gate of the stranger's carriage, with the black pine tops standing above it like plumes. And Will o' the Mill makes the acquaintance of his physician and friend, and goes at last upon his travels. In the other story, Markheim meets with his own double in the house of the dealer in curiosities, whom he has murdered. It is not such a double as Rossetti prayed for to the god of Sleep—

> "Ah! might I, by thy good grace,
> Groping in the windy stair,
> (Darkness and the breath of space
> Like loud waters everywhere,)
> Meeting mine own image there
> Face to face,
> Send it from that place to her!"

but a clear-eyed critic of the murderer, not unfriendly, who lays bare before him his motives and his history. At the close of that wonderful conversation, one of the most brilliant of its author's achievements, Markheim gives himself into the hands of the police. These two stories, when compared with the others, serve to show how Stevenson's imagination quickened and strengthened when it played full upon life. For his best romantic effects, like all great romance, are illuminative of life, and no mere idle games.

III. Morality.

His genius, like the genius of Nathaniel Hawthorne, was doubly rich, in the spirit of romance and in a wise and beautiful morality. But the irresponsible caprices of his narrative fancy prevented his tales from being the appropriate vehicles of his morality. He has left no work—unless the two short stories mentioned above be regarded as exceptions—in which romance and morality are welded into a single perfect whole, nothing that can be put beside 'The Scarlet Letter' or 'The Marble Faun' for deep insight and magic fancy joined in one. Hence his essays, containing as they do the gist of his reflective wisdom, are ranked by some critics above his stories.

A novel cannot, of course, be moral as an action is moral; there is no question in art of police regulations or conformity to established codes, but rather of insight both deep and wide. Polygamy and monogamy, suttee, thuggism, and cannibalism, are all acceptable to the romancer, whose business is with the heart of man in all times and places. He is not bound to display allegiance to particular moral laws of the kind that can be broken; he is bound to show his consciousness of that wider moral order which can no more be broken by crime than the law of gravitation can be broken by the fall of china—the morality without which life would be impossible; the relations, namely, of human beings to each other, the feelings, habits, and thoughts that are the web of society. For the appreciation of morality in this wider sense high gifts of imagination are necessary. Shakespeare could never have drawn Macbeth, and thereby made apparent the awfulness of murder, without some sympathy for the murderer—the sympathy of intelligence. These gifts of imagination and sympathy belong to Stevenson in a very high degree; in all his romances there are gleams from time to time of wise and subtle reflection upon life, from the eternal side of things, which shine the more luminously that they spring from the events and situations with no suspicion of homily. In 'The Black Arrow,' Dick Shelton begs from the Duke of Gloucester the life of the old shipmaster Arblaster, whose ship he had taken and accidentally wrecked earlier in the story. The Duke of Gloucester, who, in his own words, "loves not mercy nor mercy-mongers," yields the favour reluctantly. Then Dick turns to Arblaster.

"'Come,' said Dick, 'a life is a life, old shrew, and it is more than ships or liquor. Say you forgive me, for if your life is worth nothing to you, it hath cost me the beginnings of my fortune. Come, I have paid for it dearly; be not so churlish.'

"'An' I had my ship,' said Arblaster, 'I would 'a' been forth and safe on the high seas—I and my man Tom. But ye took my ship, gossip, and I'm a beggar; and for my man Tom, a knave fellow in russet shot him down. 'Murrain,' quoth he, and spake never again. 'Murrain' was the last of his words, and the poor spirit of him passed. 'A will never sail no more, will my Tom.'

"Dick was seized with unavailing penitence and pity; he sought to take the skipper's hand, but Arblaster avoided his touch.

"'Nay,' said he, 'let be. Y' have played the devil with me, and let that content you.'

"The words died in Richard's throat. He saw, through tears, the poor old man, bemused with liquor and sorrow, go shambling away, with bowed head, across the snow, and the unnoticed dog whimpering at his heels; and for the first time began to understand the desperate game that we play in life, and how a thing once done is not to be changed or remedied by any penitence."

A similar wisdom that goes to the heart of things is found on the lips of the spiritual visitant in 'Markheim.'

"'Murder is to me no special category,' replied the other. 'All sins are murder, even as all life is war. I behold your race, like starving mariners on a raft, plucking crusts out of the hands of famine, and feeding on each other's lives. I follow sins beyond the moment of their acting; I find in all that the last consequence is death; and to my eyes the pretty maid, who thwarts her mother with such taking graces on a question of a ball, drips no less visibly with human gore than such a murderer as yourself.'"

The wide outlook on humanity that expresses itself in passages like these is combined in Stevenson with a vivid interest in, and quick appreciation of, character. The variety of the characters that he has essayed to draw is enormous, and his successes, for the purposes of his stories, are many. Yet with all this, the number of lifelike portraits, true to a hair, that are to be found in his works, is very small indeed. In the golden glow of romance, character is always subject to be idealised; it is the effect of character seen at particular angles and in special lights, natural or artificial, that Stevenson paints: he does not attempt to analyse the complexity of its elements, but boldly projects into it certain principles, and works from those. It has often been said of Scott that he could not draw a lady who was young and beautiful; the glamour of chivalry blinded him, he lowered his eyes and described his emotions and aspirations. Something of the same disability afflicted Stevenson in the presence of a ruffian. He loved heroic vice only less than he loved heroic virtue, and was always ready to idealise his villains, to make of them men who, like the Master of Ballantrae, "lived for an idea." Even the low and lesser villainy of Israel Hands, in the great scene where he climbs the mast to murder the hero of 'Treasure Island,' breathes out its soul in a creed:—

"'For thirty years,' he said, 'I've sailed the seas, and seen good and bad, better and worse, provisions running out, knives going, and what not. Well, now, I tell you, I never seen good come o' goodness yet. Him as strikes first is my fancy; dead men don't bite; them's my views—Amen, so be it.'"

John Silver, that memorable pirate, with a face like a ham and an eye like a fragment of glass stuck into it, leads a career of whole-hearted crime that can only be described as sparkling. His unalloyed maleficence is adorned with a thousand graces of manner. Into the dark and fetid marsh that is an evil heart, where low forms of sentiency are hardly distinguishable from the all-pervading mud, Stevenson never peered, unless it were in the study of Huish in 'The Ebb Tide.'

Of his women, let women speak. They are traditionally accredited with an intuition of one another's hearts, although why, if woman was created for man, as the Scriptures assure us, the impression that she makes on him should not count for as much as the impression she makes on some other woman, is a question that cries for solution. Perhaps the answer is that disinterested curiosity, which is one means of approach to the knowledge of character, although only one, is a rare attitude for man to assume towards the other sex. Stevenson's curiosity was late in awaking; the heroine of 'The Black Arrow' is dressed in boy's clothes throughout the course of the story, and the novelist thus saved the trouble of describing the demeanour of a girl. Mrs. Henry, in 'The Master of Ballantrae,' is a charming veiled figure, drawn in the shadow; Miss Barbara Grant and Catriona in the continuation of 'Kidnapped' are real enough to have made many suitors for their respective hands among male readers of the book;—but that is nothing, reply the critics of the other party; a walking doll will find suitors. The question must stand over until some definite principles of criticism have been discovered to guide us among those perilous passes.

One character must never be passed over in an estimate of Stevenson's work. The hero of his longest work is not David Balfour, in whom the pawky Lowland lad, proud and precise, but "a very pretty gentleman," is transfigured at times by traits that he catches, as narrator of the story, from its author himself. But Alan Breck Stewart is a greater creation, and a fine instance of that wider morality that can seize by sympathy the soul of a wild Highland clansman. "Impetuous, insolent, unquenchable," a condoner of murder (for "them that havenae dipped their hands in any little difficulty should be very mindful of the case of them that have"), a confirmed gambler, as quarrelsome as a turkey-cock, and as vain and sensitive as a child, Alan Breck is one of the most lovable characters in all literature; and his penetration—a great part of which he learned, to take his own account of it, by driving cattle "through a throng lowland country with the black soldiers at his tail"—blossoms into the most delightful reflections upon men and things.

The highest ambitions of a novelist are not easily attainable. To combine incident, character, and romance in a uniform whole, to alternate telling dramatic situation with effects of poetry and suggestion, to breathe into the entire conception a profound wisdom, construct it with absolute unity, and express it in perfect style—this thing has never yet been done. A great part of Stevenson's subtle wisdom of life finds its readiest outlet in his essays. In these, whatever their occasion, he shows himself the clearest-eyed critic of human life, never the dupe of the phrases and pretences, the theories and conventions, that distort the vision of most writers and thinkers. He has an unerring instinct for realities, and brushes aside all else with rapid grace. In his lately published 'Amateur Emigrant' he describes one of his fellow-passengers to America:—

"In truth it was not whisky that had ruined him; he was ruined long before for all good human purposes but conversation. His eyes were sealed by a cheap school-book materialism. He could see nothing in the world but money and steam engines. He did not know what you meant by the word happiness. He had forgotten the simple emotions of childhood, and perhaps never encountered the delights of youth. He believed in production, that useful figment of economy, as if it had been real, like laughter; and production, without prejudice to liquor, was his god and guide."

This sense of the realities of the world—laughter, happiness, the simple emotions of childhood, and others—makes Stevenson an admirable critic of those social pretences that ape the native qualities of the heart. The criticism on organised philanthropy contained in the essay on 'Beggars' is not exhaustive, it is expressed paradoxically, but is it untrue?—

"We should wipe two words from our vocabulary. Gratitude and charity. In real life, help is given out of friendship, or it is not valued; it is received from the hand of friendship, or it is resented. We are all too proud to take a naked gift; we must seem to pay it, if in nothing else, then with the delights of our society. Here, then, is the pitiful fix of the rich man; here is that needle's eye in which he stuck already in the days of Christ, and still sticks to-day, firmer, if possible, than ever; that he has the money, and lacks the love which should make his money acceptable. Here and now, just as of old in Palestine, he has the rich to dinner, it is with the rich that he takes his pleasure: and when his turn comes to be charitable, he looks in vain for a recipient. His friends are not poor, they do not want; the poor are not his friends, they will not take. To whom is he to give? Where to find—note this phrase—the Deserving Poor? Charity is (what they call) centralised; offices are hired; societies founded, with secretaries paid or unpaid: the hunt of the Deserving Poor goes merrily forward. I think it will take a more than merely human secretary to disinter that character. What! a class that is to be in want from no fault of its own, and yet greedily eager to receive from strangers! and to be quite respectable, and at the same time quite devoid of self-respect; and play the most delicate part of friendship, and yet never be seen; and wear the form of man, and yet fly in the face of all the laws of human nature:—and all this, in the hope of getting a belly-god burgess through a needle's eye! Oh, let him stick, by all means; and let his polity tumble in the dust; and let his epitaph and all his literature (of which my own works begin to form no inconsiderable part) be abolished even from the history of man! For a fool of this monstrosity of dulness there can be no salvation; and the fool who looked for the elixir of life was an angel of reason to the fool who looks for the Deserving Poor."

An equal sense of the realities of life and death gives the force of a natural law to the pathos of 'Old Mortality,' that essay in which Stevenson pays passionate tribute to the memory of his early friend, who "had gone to ruin with a kingly *abandon*, like one who condescended; but once ruined, with the lights all out, he fought as for a kingdom." The whole description, down to the marvellous quotation from Bunyan that closes it, is one of the sovereign passages of modern literature; the pathos of it is pure and elemental, like the rush of a cleansing wind, or the onset of the legions commanded by

> "The mighty Mahmud, Allah-breathing Lord,
> That all the misbelieving and black Horde
> Of Fears and Sorrows that infest the Soul
> Scatters before him with his whirlwind Sword."

Lastly, to bring to an end this imperfect review of the works of a writer who has left none greater behind him, Stevenson excels at what is perhaps the most delicate of literary tasks and the utmost test, where it is successfully encountered, of nobility,—the practice, namely, of self-revelation and self-delineation. To talk much about oneself with detail, composure, and ease, with no shadow of hypocrisy and no whiff or taint of indecent familiarity, no puling and no posing,—the shores of the sea of literature are strewn with the wrecks and forlorn properties of those who have adventured on this dangerous attempt. But a criticism of Stevenson is happy in this, that from the writer it can pass with perfect trust and perfect fluency to the man. He shares with Goldsmith and Montaigne, his own favourite, the happy privilege of making lovers among his readers. "To be the most beloved of English writers—what a title that is for a man!" says Thackeray of Goldsmith. In such matters, a dispute for pre-eminence in the captivation of hearts would be unseemly; it is enough to say that Stevenson, too, has his lovers among those who have accompanied him on his 'Inland Voyage,' or through the fastnesses of the Cevennes in the wake of Modestine. He is loved by those who never saw his face; and one who has scaled that dizzy height of ambition may well be content, without the impertinent assurance that, when the Japanese have taken London and revised the contents of the British Museum, the yellow scribes whom they shall set to produce a new edition of the 'Biographie Universelle' will include in their entries the following item:—"*Stevenson, R. L. A prolific writer of stories among the aborigines. Flourished before the Coming of the Japanese. His works are lost.*

[W. R.]

WEEKLY EVENING MEETING,

Friday, May 24, 1895.

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

J. Viriamu Jones, Esq. M.A. F.R.S. *M.R.I.*

The Absolute Measurement of Electrical Resistance.

If we take a conductor, such as this piece of copper wire, and make it part of a conducting circuit in which an electric current is flowing, we find that the electromotive-force between its extremities is proportional to the strength of the current passing through it, so long as it remains in the same physical condition. If either the electromotive-force or the current strength varies, the other of these quantities varies in like proportion. Their ratio is constant, i. e. it has a value independent of them, and depending only on the dimensions of the conductor, the nature of the material of which it is made, the state of aggregation of its parts, and its temperature.

This ratio is called the electrical resistance of the conductor, and so defined it corresponds to a real physical quantity—it is a physical property of the conductor.

Probably no physical measurement can be made with greater accuracy than the comparison of electrical resistances. Such comparisons are daily made in many parts of the world, and it is clearly desirable, in order that the results obtained by one man may have meaning for others, that all should make their reckoning in terms of the same unit. Accordingly, the scientific world has given great attention to the definition of such a unit, and much international negotiation has taken place with a view of securing world-wide agreement on this point.

The most obvious method of procedure is to fix upon some convenient conductor as a standard, and to call its electrical resistance the unit of electrical resistance, other electrical resistances being then expressed as so many times or such and such a fraction of the resistance of this standard conductor.

The disadvantage of this method of defining the unit is that the resistance of such a standard may change from time to time through alterations of its physical condition. The most notable change is consequent on change of temperature; this may, however, be allowed for by defining the unit as the electrical resistance of the standard conductor when it is at a specified temperature. But the other changes of physical condition, changes in the state of aggregation of the parts of the conductor, effects of strain, alterations of molecular structure, &c., are more serious, because they are unknown, and we cannot in our definition provide against unknown possible changes.

We run the risk, therefore, of having a varying unit, and the prime requisite of a unit is constancy.

In England the Government has taken this risk, and committed itself—at least for the present—to this method of specifying the unit of electrical resistance.

The denomination of the standard of electrical resistance contained in the recent Order in Council regarding Standards for Electrical Measurements is as follows :—

"A standard of electrical resistance denominated one ohm, being the resistance between the copper terminals of the instrument marked 'Board of Trade Ohm Standard Verified, 1894' to the passage of an unvarying electrical current, when the coil of insulated wire forming part of the aforesaid instrument and connected to the aforesaid terminals is in all parts at a temperature of 15·4° C."

This is a plain adoption of an arbitrary standard. The resistance of a certain piece of wire wound into a coil is made the legal unit, and if this resistance varies our legal unit varies with it.

It is true that in constructing the standard, efforts have been made to ensure that its resistance shall be equal to 1000 million times the absolute unit of which I shall presently speak—that the standard was in construction based upon the ohm; but there is no provision in the Order in Council for revision if the standard varies, nor indeed has the Government any means at its disposal of directly measuring its standard in absolute measure.

I seem to be finding fault. That is not my intention. I am only advocating progress. The time is ripe for the adoption of the absolute unit, not merely nominally but really, for the frank acceptance of the absolute unit itself as the ultimate legal standard.

Now, what is an absolute unit? The following considerations affecting physical measurements generally will, I hope, serve to make this clear.

In order to specify the magnitude of any physical quantity, we bring it into relation with a standard magnitude of the same kind, called the unit, and say that it is so many times or such and such a fraction of this unit. Thus we speak of a length of 6 centimetres, a mass of 50 grammes, a time of 20 seconds, an electric current of 10 amperes, an electrical resistance of 30 ohms, and so on.

For the purpose of this specification any convenient magnitude of the physical quantity to be measured may be taken as the unit.

But science has to deal with many kinds of quantities, and when we consider the various units as constituting a system, our arbitrary choice is limited by two conditions of fundamental importance :—

1. The units must be chosen so as to simplify as far as may be the statement of the quantitative relations existing between various kinds of quantities.

Physical science, in so far as it is quantitative—may we not say in so far as it is perfected?—tells us of relations of interdependence between many different kinds of quantities. Thus, in geometry we have such relations between lengths, areas, and volumes; in kinematics, relations between these mathematical quantities and times, velocities, and accelerations; in dynamics, between these mathematical and kinematical quantities and mass, momentum, force, work, and energy; in electrical and magnetic science, relations between the foregoing quantities and electrical and magnetic magnitudes; and so on.

These relations of interdependence are expressed in equations, and it is of obvious advantage, both for ease of arithmetical calculation and clearness of thought, to rid these equations as far as possible of superfluous arithmetical constants. This may be done by a judicious selection of units, by making the units of the quantities that appear most complex in their relations of interdependence depend upon the units of the quantities that appear in their nature to be simplest. A unit of any quantity so defined with reference to the units of quantities apparently simpler is called a derived unit with reference to the latter units as fundamental.

Thus, the attraction of the earth on a mass of 1 lb.—the weight of 1 lb.—is an arbitrary unit of force. The force that, acting on a mass of 1 lb., increases its velocity by 1 ft. per second every second is a derived unit of force with reference to the units of length, time, and mass, as fundamental. It is usually called the absolute unit of force on the pound-foot-second system; and by the very nature of its definition it gets rid of an arbitrary constant in the relation between force, mass, and acceleration. If the arbitrary unit of force mentioned above—the gravitation unit—is used instead of the absolute unit, our dynamical equations are uselessly complicated by the introduction of "g," the acceleration of gravity at the particular point of the earth's surface at which we happen to be, and at the particular time when our measurements happen to be made.

2. The second condition of prime importance to a scientific system of units is that the units of all quantities should be invariable, unaffected by conditions of time and place, and independent of the properties of particular bodies, i. e. they should be *absolute*. The word "absolute" is in philosophy opposed to "conditioned." When it is applied to a unit in science the implication should be that the unit is the same at all times and in all places, and that it is unconditioned by the properties of any specified body or bodies, i. e. that its magnitude is brought into relation with and depends upon only the most permanent phenomena of the universe.

The modern system of absolute units goes far to fulfil the first of our two requirements; it only very partially fulfils the second.

When we speak of an absolute unit at present we mean a unit the magnitude of which depends on nothing else than the units of length, time, and mass, and the properties of the ether. The latter may be regarded as universal enough, but the units of length, mass, and time are arbitrary standards. The metre depends on the properties of a particular bar, the gramme on the properties of a particular piece of platinum, the mean solar second on particular bodies of the solar system.

The time will come when we shall be able to take another step forward, when we shall be in a position to make the unit of mass a derived unit with reference to the units of length and time as fundamental through that universal uniformity of nature—the law of gravitation; the units of length and time being based on some more permanent phenomena of the universe (the properties of the sodium molecule have, for instance, been suggested) than the length of a particular bar, and the motion of the earth on which we live.

But we cannot take this step forward yet. We do not know either the gravitation constant or the velocity of light with sufficient accuracy to enable us to define by means of them units precise enough to meet the needs of practical life. Yet the fact that if we knew these constants we could make an important advance in the realisation of a system of absolute measurement is an excellent reason why the masters of physical measurement should apply themselves to their precise determination.

The use of a derived unit of mass would have this among other advantages, it would rationalise the dimensions of the electric and magnetic units in length and time, and relegate the irrationality to the ether, to which I believe it properly belongs.

We must, however, for the present be content with a less complete reference to the most permanent phenomena of the universe, and by absolute measurement to-night I mean measurement in terms of a unit derived with reference to the unit of length, time, and mass as fundamental—i. e. a unit conditioned only by these units, and the properties of the ether.

There is such a unit of electrical resistance—a unit derived with reference to the units of length and time as fundamental—the magnitude of which is independent of the properties of any particular conductor, any particular coil of wire, and is conditioned only by the units of length and time, and the properties of the ether.

It may, indeed, at first sight seem astonishing that a quantity so different can be expressed in terms of a length and a time. Yet so it is. We can, in virtue of experiments made 63 years ago in this Institution by Michael Faraday, measure the resistance of a conductor with the help of no other standardising instruments than a tape or measuring machine and a clock. An electrical resistance is always proportional to a certain velocity, and if the magnetic permeability of the ether be taken to be unity, the number expressing the electrical resistance will be the same as the number expressing that velocity; the unit of electrical resistance may then be taken to be the resistance corresponding to unit velocity—i. e. on the C.G.S. system a velocity of 1 c. per second, and any electrical resistance may be conveniently expressed in terms of this unit as so many centimetres per second.

[You will observe that I do not say electrical resistance *is* a velocity. That would be to neglect the unknown dimensions of the magnetic permeability of the ether.]

If the unit of length, the unit of time, and the properties of the ether remain constant, this unit of electrical resistance remains constant. It is unconditioned by the properties of any material, by position in space, or point of time, and so far deserves the name of the absolute unit of electrical resistance.

How comes it that resistance can be so measured? The answer to this question is best found in a description of some one of the methods by which the measurement of an electrical resistance in terms of the absolute unit can be experimentally made.

And I proceed, therefore, to a description of the method which may fairly be described as the simplest, and which, in my opinion, having regard to the magnificent possibilities of mechanical engineering operations in this country, is undoubtedly capable of being made the most accurate of all the methods that have been proposed since the British Association Committee, more than thirty years ago, propounded the theoretical definition of the absolute unit.

The method is due to Lorenz; and Lord Rayleigh, at the conclusion of his masterly determinations of the value of the B.A. coils in absolute measure, expressed himself in regard to it as follows:—

"On the whole, I am of opinion that if it is desirable at the present time to construct apparatus on the most favourable scale so as to reach the highest attainable accuracy, the modification of Lorenz's method last described is the one that offers the best prospect of success."

The Paper from which I quote contained a comparison of the various methods of measuring resistance in absolute units, and an invitation to others to join in the work. It was the starting-point of my own researches in the matter, which have extended over some years, and I gladly take the opportunity of thanking Lord Rayleigh for this source of inspiration.

Faraday discovered that, if a conductor is made to move in a magnetic field so as to cut across the flux of magnetic induction, the conductor becomes the seat of electromotive-force, and that the electromotive-force so developed is proportional to the rate at which the induction flux is traversed.

If, for instance, we take a metal disc and make it rotate about a horizontal axis n times a second, any radius of the disc cuts through the earth's induction flux, unless the plane of the disc is in the magnetic meridian. There will, therefore, be electromotive-force between the centre and circumference of the disc. Further, since a radius traverses the whole area of the disc n times in a second, the rate at which the induction flux is being traversed by any radius is n I, where I is the total flux of induction through the disc area. But the electromotive-force is proportional to this rate. Therefore we have (with a proper choice of the unit of electromotive-force)

$$E_1 = n\,I,$$

where E_1 is the electromotive-force between the centre and circumference.

But we may, instead of using the earth's field, obtain a magnetic field by means of an electric current. The disc may be made to spin in the magnetic field due to a current in a coil of wire placed so as to be co-axial with it, and so that its middle plane coincides with the plane of the disc. In this case the magnetic field is symmetrical with regard to the common axis, and we have once more

$$E_1 = I\,n,$$

where E_1 is the electromotive-force between the centre and circumference consequent on the rotation of the disc at the rate of n turns per second in the induction flux due to the current in the coil, I being the total amount of that flux which passes through the disc circumference.

Now the current strength in a given circuit is by definition proportional to the intensity of the magnetic field produced by it at any point, and hence the magnetic induction through the disc due to the current in the coil is proportional to the strength of that current.

It follows that the magnetic induction through the disc is made up of two factors, viz. the current in the coil and the magnetic induction that would pass through the disc if unit current passed through the coil. The latter factor is called the coefficient of mutual induction of the coil and disc circumference, and its value depends only on their dimensions and relative positions, and the magnetic permeability of the medium in which they are placed. If the latter quantity is taken to be unity, the coefficient of mutual induction is expressible as a length, and it may be calculated from observations involving nothing else than measurement of the radius and breadth of the coil and the radius of the disc. Let this coefficient of mutual induction be denoted by M, and the current in the coil by γ_1; then

$$I = M\gamma_1;$$

and finally,

$$E_1 = n\,I = n\,M\gamma_1;$$

or

$$\frac{E_1}{\gamma_1} = n\,M.$$

We have therefore two ways of expressing the ratio of an electromotive-force to a current.

By definition (Ohm's law) this ratio is given to us as electrical resistance, or

$$\frac{E}{\gamma} = R.$$

By the experiment of the disc and coil it is given to us as the product of their coefficient of mutual induction and the rate of rotation of the disc (number of turns per second).

And if the ratio for the disc and coil is made the same as the ratio for the resistance (which it is possible in practice to arrange) we have

$$R = M n.$$

If the magnetic permeability of the medium be taken as unity, M is a length expressible as so many centimetres, and the number of turns per second is the reciprocal of the time of one revolution of the disc. Hence the resistance may be expressed as a velocity of M centimetres in the time of one revolution of the disc. This makes it clear that, with the assumption in regard to the permeability of the ether (or air) that we have made above, any electrical resistance is expressible as a velocity of so many centimetres per second.

We can in practice make the ratio for disc and coil the same as the ratio for the resistance by first sending the same current through coil and resistance and then varying the speed of the disc so as to make the electromotive-force between its centre and circumference equal to the electromotive-force between the resistance terminals.

How can we test the equality of the two electromotive-forces? If two equal electromotive-forces act in opposite directions in the same circuit the result is electrical equilibrium. We have, therefore, to proceed as follows:—Place the two electromotive-forces in the same circuit so as to act in opposite directions; then if they are the only electromotive-forces in the circuit, there will be an electric current round the circuit due to their difference if they are unequal, and no electric current at all if they are equal. The presence or absence of a current may be tested by the inclusion of a galvonometer in the circuit.

In practice it is not quite so simple as this, because it is impossible to make the two electromotive-forces in question the only electromotive-forces in the circuit. There will be others, and especially thermo-electric forces, always present. But this difficulty may be met thus:—Reverse the two electromotive-forces we are comparing; if the current in the circuit is not changed by this reversal of both, they must be equal—i. e. if the reading of the galvanometer be the same, whether the balancing electromotive-forces are in one direction or the other, these electromotive-forces are equal. The reversal of both our electromotive-forces may be readily effected in practice by simply reversing the direction of the current through the coil and resistance.

To measure our resistance in absolute measure we have then—

1. To make the coil (S S S, Fig. 1), and the resistance (X Y) parts of the same main circuit (B C F S S S P X Y Q C B), and to pass through this circuit an electric current. A commutator (C) is to be inserted in the circuit in order that the direction of the current may be changed at will.

2. To insert the electromotive-force between the centre and circumference of the disc (D D D) acting in one direction and the

electromotive-force between the extremities of the resistance acting in the opposite direction in a second circuit (X M O G Y X) containing a sufficiently sensitive galvanometer G. Brush contacts must be made at O and M.

3. To vary the rate of rotation of the disc until the reading of the galvanometer is the same whether the current in the main circuit through coil and resistance is being sent by the commutator in one direction or the other.

4. To measure this equilibrium rate of rotation.

5. To multiply this equilibrium rate of rotation (number of turns per second) by the coefficient of mutual induction of the coil and disc, which is calculated once for all from their measured dimensions. This product gives the resistance to be measured in absolute units.

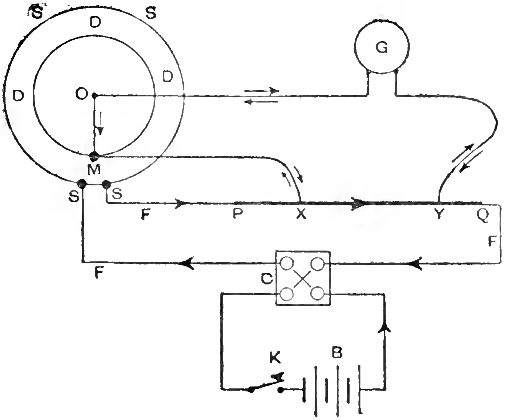

Fig. 1.

[The necessary arrangements were here experimentally shown by the lecturer.]

I trust that I have said sufficient to make clear to you the general theory of the measurement of a resistance in absolute measure by this method. It now remains to consider very briefly the practical side of the matter, the difficulties that arise, the way in which they may be met, and the accuracy attainable. The time at my disposal does not allow me to do this at all completely, and I must content myself with touching on a few points of special importance and interest.

The first great desideratum is that we should have the rate of rotation of the disc well under control, that it should run as uniformly as possible, and that its rate should be capable of sufficiently exact determination.

The disc, axle, and bearings in my apparatus at Cardiff are of phosphor bronze (Figs. 2 and 3). The disc is insulated from the

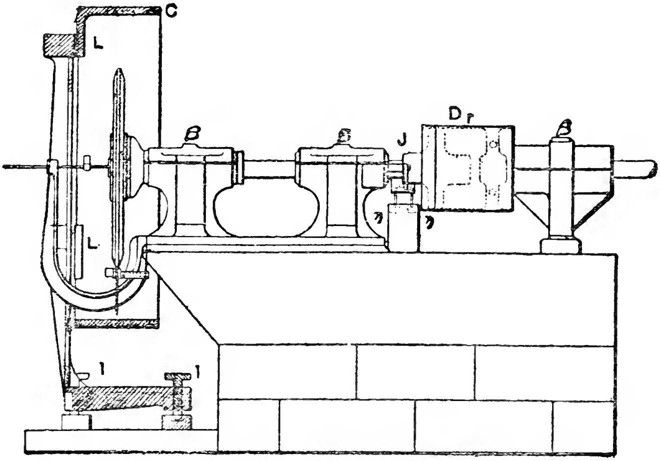

FIG. 2.—Disc and Coil. Elevation and part section on line A B.
Dr = Drum with rows of teeth on it.

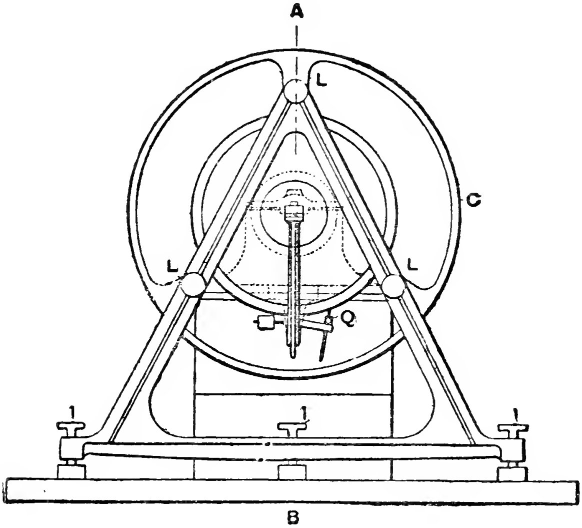

FIG. 3.—Disc and Coil. End View.

axle by well-paraffined ebonite (Fig. 4), otherwise there would be short-circuiting of part of its radius through the bearings and bed.

It was at first intended to drive the disc by rope gearing from the electromotor; but in the course of preliminary experiments, though the rope was 40 yards long, and joining of the ends took place over a length of at least 6 feet, there was a sudden variation in speed, producing a distinct movement of my galvanometer needle, and visible at the tuning fork, of which I shall presently speak, whenever the joint passed over the pulley. The rope gearing was therefore abandoned, and the motor coupled direct. The current driving the motor is supplied from secondary cells. It passes to the motor through resistance coils, and may be increased or diminished by throwing some of these coils into or out of the circuit. It may also be varied continuously through a small range by a slide resistance of platinoid wire after the larger adjustment has been made. A shunt worked by a lever provides means of taking out or putting in a small resistance suddenly, so as to allow the observer controlling the speed while an observation is being taken to counteract small variations of speed due to alteration in the lubrication of the bearings and the friction of the brushes on the commutator of the motor. With these

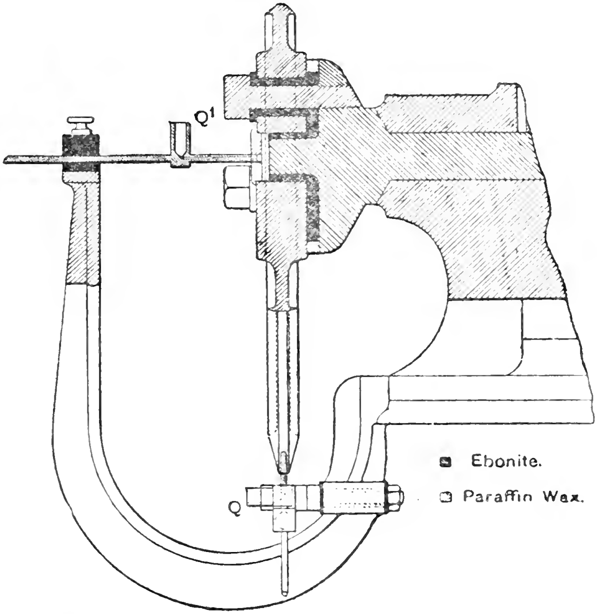

FIG. 4.—Details of Disc Insulation and Brushes. Part Elevation and part Section on lines *a b*, *b c*, Q Q^1 = brushes.

arrangements we can obtain any required rate of rotation from about 150 to 1500 revolutions per minute.

It is of the greatest importance, if the observations presently to be described are to be made with ease, that the rate of rotation should remain constant for the four or five minutes required, and no effort made to compass such uniformity is thrown away. The design, workmanship, and lubrication of all the bearings require closest attention, and the friction between the brushes and the motor commutator should be reduced to a minimum. In my Cardiff apparatus the axle near the motor bears a heavy fly-wheel.

During a run the observer controlling the rate of rotation requires some test of uniformity, so that he may, if there is a quickening or slowing down, diminish or increase respectively the current through the electromotor so as to recover the initial speed.

To accomplish this it is convenient to refer the rate of rotation by a stroboscopic method to a suitable tuning fork provided with riders and maintained in vibration electrically. Here is such a fork. At the end of each prong there is an aluminium plate with a slit in it parallel to the prong. When the fork is in equilibrium the slits are opposite one another, and an observer can see through both. When the fork is in vibration he can see through the slits only when they are opposite one another. This position occurs twice in every complete vibration. Hence, if the number of vibrations per second is, say, 64, he obtains 128 views per second.

Now let us suppose that he looks through the vibrating slits at a drum or disc coloured black, on which a number of white spaces, which I will call teeth, are painted at equal angular intervals. Here is such a disc attached to the rotating axle; on it there are three circular rows of radial teeth.

When the disc is in rotation, if the rate of rotation is such that in the interval between two views a tooth in any row exactly takes the place of the next, that row will appear, to an observer looking through the fork slits, stationary; if it does not quite do so, the row will appear to be moving backward; if it passes beyond, the row will appear to be moving forward.

These phenomena I can show you by illuminating the rotating disc by intermittent light. The light of this alternating arc varies in intensity at each alternation, passing from maximum to maximum through a minimum that is, however, very far from darkness. Our speed is such as to make the row of fourteen teeth appear almost stationary in this intermittent light. You can see the teeth, but they stand out somewhat faintly. The faintness is due to the fact that we have not complete darkness between the maxima of illumination.

We may do much better with such a tuning fork as I have described. I place its slits in front of this electric lantern, in which there is a continuous arc. The light falling on the disc has passed through the slits of the vibrating fork, and by varying the rate of revolution we obtain perfectly distinct stationary teeth. Let us suppose that the fork permits the light to pass through it 128 times per second, and that the row on the disc which looks stationary has fourteen teeth on it; then one tooth takes the place of the next in $\frac{1}{128}$ a second, but this corresponds to $\frac{1}{14}$ of a revolution, so that one revolution is performed in $\frac{14}{128}$ of a second, and the number of turns per second is $\frac{128}{14} = 9\frac{1}{7}$.

And generally

$$n = \frac{2\,\mathrm{P}}{\mathrm{Q}},$$

where P = the pitch of the fork,
n = the number of turns of the disc per second,
Q = the number of teeth in the stationary row.

We may then make the stationariness of a tooth, as seen by an observer through the slits, a test of the uniformity of the rate of rotation. If a tooth begins to move past a fixed reference wire placed immediately in front of the rotating disc or drum, the observer can at once bring it back by altering the current in the motor.*

If the tuning fork were itself a reliable time indicator to the degree of accuracy required, we might calculate from its pitch the speed corresponding to stationariness of a given row of teeth. But my experience is that the vibration period of a fork maintained in vibration electrically is not sufficiently constant. If stopped and set going again it may start with a period different from that of its last performance by several parts in 10,000. No previous determination of the pitch of the fork can therefore be relied on to give us the rate of rotation to a hundredth per cent., though once started the fork goes sufficiently uniformly to give us the means of control.

The period of a bowed fork does not vary in this fashion, but it is more troublesome to use for the purpose. The constant bowing required takes too much of the attention of the observer.

Accordingly, it is necessary to measure the rate of rotation during each run while the galvanometer observations are being made. This may be done telegraphically with great accuracy. The rotating disc is by means of an eccentric attached to its axle caused at each revolution to make and break an electric circuit passing through a Bain's electro-chemical telegraph instrument. The standard clock telegraphs seconds to the same instrument. The two records lie side by side on the same tape, and simple counting with careful fractional estimate gives with great precision the mean rate of rotation during the few minutes occupied in taking a set of galvonometer readings.

With this means of measuring the rate of rotation we may, establishing synchronism with a fork in the way I have indicated, find the pitch of the fork.

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* So far as I know this method of measuring a rate of rotation was first used by Lord Rayleigh. The method used by Macleod and Clarke for measuring the pitch of a tuning fork bears much likeness to it.

Table I. gives a set of measurements of the pitch of a bowed fork; I bring it before you as indicating the accuracy both of the synchronising and the time measurement.

TABLE I.

Date.	Duration of Observation.	Temperature. Centigrade.	Pitch of Fork. (Corrected to 15·5° C.)
1892	min.	deg.	
July 16	2	19·67	65·1823
July 17 (1)	4	19·17	65·1823
July 17 (2)	4	19·65	65·1827
July 17 (3)	5	20·05	65·1814
July 18 (1)	4	19·56	65·1809
July 18 (2)	4	19·92	65·1823
July 21	4	18·18	65·1812
July 22	4	17·96	65·1831
		Mean ..	65·1820

Extreme variation from mean is about 1 in 65,000.

When I first began working at this method I found it extremely difficult when the disc was in rotation to obtain a steady reading at the galvanometer. My galvanometer is a very sensitive Thomson reflecting galvanometer of about 1 ohm resistance, made by Elliott Bros. and fitted by my assistant, Mr. Harrison, with a long suspension for the support of the needle by a quartz fibre. When the disc was in uniform rotation the spot of light roamed continually over 50 or 100 divisions of the scale, and frequently there were sudden jerks of a most embarrassing kind. Readings with a galvanometer so sensitive were in fact impossible. (The scale divisions are 40 to the inch, and the scale is about 40 inches from the galvanometer.)

Now the electromotive-force in the circuit of the galvanometer when there is no current through the standard coil is due to three causes:—

1. The cutting of the earth's flux of induction by the radius of the rotating disc. The electromotive-force due to this cause is small in my apparatus, owing to the fact that the plane of my disc nearly coincides with the magnetic meridian. For a uniform rate of rotation it is constant.

2. The thermo-electric forces due to the general distribution of temperature in the galvanometer circuit. Changes in this distribution would be gradual, and would not account for the sudden irregular movements of the galvanometer needle.

3. The thermo-electric force at the contact of the external brush with the circumference of the disc. The sudden movements of the needle were obviously due to variations in this thermo-electric force.

I made many experiments with a view of diminishing these changes. I first tried an ordinary phosphor-bronze brush made of a number of layers of thin sheet and controlled by a spring, brush and disc circumference being well amalgamated. This was not at all successful. Trials were then made with amalgamated copper and amalgamated lead, and with the substitution of a dead-weight pressure for a spring. But no satisfactory result could be obtained.

It was noticed, however, that after amalgamation the readings were fairly steady for a short interval, and it seemed likely that if mercury could be continuously supplied to the surface of contact between the brush and disc the electromotive-force at their contact would be rendered much more constant. This led on to the idea of a brush consisting of a single wire, perforated by a channel through which a constant flow of mercury might be maintained from a cistern of adjustable height, and a brush of this description was made (Fig. 5). Trial showed that our difficulty was at an end. The variations almost disappeared, the sudden jumps quite ceased to trouble us, and the motion of the needle did not extend over more than one or two divisions of the scale for many minutes. A multiplication of such brushes round the circumference would make still further improvement.

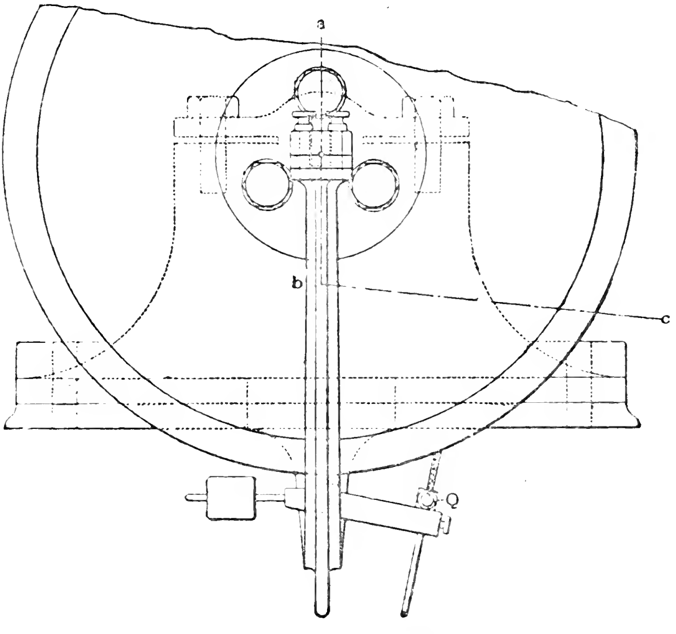

Fig. 5.—Part end View.

In measuring a resistance we have to determine what I have called the equilibrium rate of rotation corresponding to that resistance, i. e. the rate of rotation such that, on reversing the direction of the current through coil and resistance by the commutator, the reading of the galvanometer is unchanged.

Now, it is not very easy to hit this exactly without making a great many trials, and, fortunately, it is not necessary for us to do so. The current produced in the galvanometer circuit by the difference of the electromotive-forces at the disc and at the resistance is proportional to that difference, and, if the current is small, it is proportional to the scale deflection on reversal. We shall commit no appreciable error, therefore, if we are near the equilibrium rate, in assuming that the difference between the equilibrium rate and the actual rate is proportional to the scale deflection on reversal, or, what comes to the same thing, that the difference $R - Mn$ is proportional to the scale deflection on reversal.

If we take four or five rates of rotation near the equilibrium rate, and determine the deflections on reversal corresponding to each, we can by interpolation determine the rate of rotation that would correspond to no deflection, i. e. the equilibrium rate. If, say, we take five such rates, some on one side and some on the other of the equilibrium rate, we get five equations as follows:—

$$\begin{aligned} R - M n_1 &= C l_1, \\ R - M n_2 &= C l_2, \\ R - M n_3 &= C l_3, \\ R - M n_4 &= C l_4, \\ R - M n_5 &= C l_5, \end{aligned}$$

where l_1, l_2, l_3, l_4, l_5 are the deflections corresponding to rates of rotation n_1, n_2, n_3, n_4, n_5, and C is a constant.

Combining these by the method of least squares, we may readily obtain the most probable values of R and C.

If the greatest accuracy is not required, we may content ourselves with two observations, one on one side of the equilibrium rate, and the other on the other side.

The elementary observation in the determination of a resistance with any given apparatus is, then, the determination of scale deflection on current reversal corresponding to a rate of rotation simultaneously measured.

To obtain this scale deflection accurately we must obtain it as a mean of the values given for it by a number of successive reversals succeeding one another as quickly as possible, in order to eliminate slight variations in the position of the galvanometer needle due to the slight remaining variations in the thermo-electric force at the external brush contact, and the slight variations of speed that the observer at the tuning-fork has to correct. It is therefore best not to wait for the needle to come even approximately to rest after the disturbance due to the induction current on reversal; but to take the readings for the extreme positions in an oscillation, and having previously found the coefficient of damping, to calculate the position of rest from these two readings.

Here is a table of an average set of such observations taken during a three minutes' run. The readings with the commutator in one direction are denoted by E, and those with the commutator in the other direction are denoted by W. The deflections on reversal are given in the column marked W − E.

TABLE II.

E.		W.		E. (Calc.)	W. (Calc.)	W − E.
− 33·0	+ 19·5	+ 14·5	− 19·0	− 2·3	− 5·1	− 2·8
						− 3·4
− 28·0	+ 17·0	+ 16·5	− 18·5	− 1·7	− 4·0	− 2·3
						− 1·0
− 28·5	+ 15·0	+ 15·0	− 21·5	− 3·0	− 6·4	− 3·4
						− 2·8
− 28·5	+ 14·0	+ 17·0	+ 22·0	− 3·6	− 5·8	− 2·2
						− 2·8
− 27·0	+ 14·0	+ 17·0	− 22·5	− 3·0	− 6·1	− 3·1
						− 2·8
− 29·0	+ 15·0	+ 19·0	− 23·5	− 3·3	− 5·9	− 2·6
						− 3·5
− 27·0	+ 15·0	+ 18·5	− 22·5	− 2·4	− 5·5	− 3·1
						− 2·3
− 26·0	+ 13·0	+ 18·0	− 22·0	− 3·2	− 5·4	− 2·2
						− 2·4
− 28·5	+ 15·0	+ 18·5	− 22·5	− 3·0	− 5·5	− 2·5
						− 2·0
− 29·5	+ 15·0	+ 16·5	− 23·0	− 3·5	− 6·6	− 3·1
						− 3·5
− 26·5	+ 13·5	+ 22·5	− 24·5	− 3·1	− 5·0	− 1·9
						− 2·5
− 26·5	+ 14·5	+ 19·0	− 22·5	− 2·5	− 5·3	− 2·8
						− 1·8
− 29·5	+ 15·0	+ 16·5	− 21·0	− 3·5	− 5·4	− 1·9
						− 2·9
− 26·5	+ 14·5	+ 18·5	− 21·5	− 2·5	− 4·9	− 2·4
						− 2·1
− 28·5	+ 15·5	+ 17·0	+ 22·0	− 2·8	− 5·8	− 3·0
						− 2·6
− 22·5	+ 10·5	+ 22·5	− 25·0	− 3·2	− 5·3	− 2·1
						− 2·4
− 26·0	+ 13·5	+ 18·5	− 22·5	− 2·9	− 5·5	− 2·6
Coeff. of Damping = $\frac{1}{1\cdot41}$.				Mean		− 2·57

Summing up, then, we have during an observation one observer controlling the speed by the tuning-fork method I have described; another at the galvanometer continually reversing the commutator and calling out the galvanometer readings; at the same time the rate of rotation is being recorded on the tape of the Bain's telegraph instrument under the watchful care of a third; and a fourth person is required to write down the readings called out from the galvanometer. The run usually lasts one, two, or three minutes according to the degree of accuracy required.

The concordance of the results obtained from successive runs in measuring the same resistance is the best test of the success of the combination.

The following is a set of observations of a resistance of about $\frac{1}{2000}$ of an ohm made in July and August, 1893:—

Table III.

July 17th, morning	0·00050016
July 17th, afternoon	0·00050016
July 19th, morning	0·00050015
August 2nd, afternoon	0·00050020
August 3rd, morning	0·00050021
August 4th, morning	0·00050016
August 4th, afternoon	0·00050013
August 5th, morning	0·00050019
August 9th, morning	0·00050021
August 9th, afternoon	0·00050018
Mean	0·00050017

The maximum divergence from the mean is 0·00000004, or about one part in 12,000.

Here is another set, made last year in determining the value of the international ohm in absolute units.

The results are as follows, the figure in each case giving the value of the international ohm in true ohms:—

July 7th.—Standard coil carefully adjusted. Three-minute tapes.

	0·999703
	0·999761
	0·999807
Mean	0·999757

July 9th.—No readjustment of standard coil. One-minute tapes.

	0·999757
	0·999711
	0·999683
	0·999782
Mean	0·999733

July 10th, morning.—Standard coil readjusted. One-minute tapes.

0·999734
0·999818
0·999726

Mean .. .. 0·999759

July 10th, afternoon.—No readjustment of standard coil. Three-minute tapes.

0·999708
0·999742
0·999764

Mean .. .. .. 0·999738

July 11th, afternoon.—Standard coil readjusted. Three-minute tapes.

0·999693
0·999692
0·999679

Mean .. .. .. 0·999688

July 12, morning.—No readjustment of standard coil. Resistance coils reversed.

0·999713
0·999711
0·999692

Mean .. .. .. 0·999705

July 12th, afternoon.—Standard coil readjusted. Resistance coils removed from the mercury cups and replaced. Three-minute tapes.

0·999774
0·999787
0·999759

Mean .. .. .. 0·999773

July 13th.—Standard coil readjusted. Resistance coils removed from mercury cups and replaced. Three-minute tapes.

0·999847
0·999809
0·999782
0·999842 (morning of the 14th)

Mean .. .. .. 0·999820

July 14th, morning.—Standard coil readjusted. Resistance coils removed and replaced. Three-minute tapes.

0·999695
0·999692
0·999717

Mean .. .. .. 0·999701

July 14th, afternoon.—Standard coil readjusted. Resistance coils removed and replaced. Three-minute tapes.

	0·999853
	0·999866
	0·999875
Mean	0·999865

These results show that no single observation differs from the mean of a number by more than one part in ten thousand.

We have now to consider the other factor in the resistance—the coefficient of mutual induction of the coil and disc. This depends merely on the dimensions of the coil and disc if they are accurately centred, and as the adjustment for centre was made in between the successive sets given in the last table, we may take for granted that it can be made with the requisite accuracy.

The circumference of the disc is a sufficiently true circle, the disc having been ground true in place. Disc and axle were then removed and transferred to my Whitworth measuring machine. The measurement of the disc's diameter presented no difficulty. It was easily determined to the ten-thousandth of an inch. The mean radius of my coil cannot be determined with the same accuracy, but I believe it is known to the thousandth of an inch. The coil consists of a single layer of silk-covered wire wound in a screw thread cut in a brass frame. The silk covering of the wire introduces some uncertainty. It was measured bolted to its stand as in use. Measurements were taken along 18 diameters in the Whitworth machine with the following results:—

Diameter.	Measurement.	Diameter.	Measurement.
deg.		deg.	
0–180	21·0898	90–270	21·1038
10–190	21·0929	100–280	21·1056
20–200	21·0951	110–290	21·1041
30–210	21·0933	120–300	21·1014
40–220	21·0960	130–310	21·0979
50–230	21·0998	140–320	21·0945
60–240	21·1017	150–330	21·0924
70–250	21·1026	160–340	21·0900
80–260	21·1044	170–350	21·0910

Max., 21·1056
Min., 21·0898
0·0158

Mean, 21·09757
$t = 17°$ C.

These measures clearly show that the coil is elliptical in section, the difference between the major and minor axes being about 0·008 in., or about one part in 1300. The way in which this fact emerges from the measurements, is, however, some indication of the accuracy with which the individual measurements in the machine can be made, even in the case of a coil of insulated wire.

My coil, then, is faulty on account of this ellipticity. I have not yet calculated the possible error due to this cause; it cannot be very great. How much it is only calculation can settle; and the calculation is not without difficulty. But this, after all, is only a question of my apparatus, and does not affect the general question of the possible accuracy of the method.

A more solid metal frame might, no doubt, be turned true to the accuracy required, and if so turned might then be measured with the requisite accuracy in the Whitworth machine.

A still further improvement may be made by making the coil frame of insulating material, say paraffined marble, and winding on it naked wire instead of silk-covered wire. The radius might then be measured with certainty to something much better than 1 part in 10,000. The use of a coil with a single layer of wire instead of many layers greatly facilitates the determination of the mean radius. It, of course, necessitated the finding of a new formula for the coefficient of mutual induction.

This I obtained by direct integration of the general integral for the case in point, viz. a circle and coaxial helix. The coefficient of mutual induction is given us as a sufficiently converging series involving elliptical or quasi-elliptical integrals.

We may, then, in the result conclude that, though I cannot guarantee my own coefficient of mutual induction as correct to 1 part in 10,000 till I have calculated the effect of the coil ellipticity, it lies well within the resources of our mechanical engineers to make a coil and disc free from uncertainty to this degree of accuracy. And if this be so I am warranted in stating that a resistance can be measured in absolute units to 1 part in 10,000.

Now, in the Order in Council, from which I quoted in the early part of my discourse, it is stated that in the use of the ohm standard the limit of accuracy attainable is one-hundredth part of 1 per cent.—i. e. one part in 10,000. Hence the Government gains nothing in precision to compensate for the risk it has taken in adopting as unit the resistance of a standard coil, which may vary from time to time in consequence of changes in the physical condition of the coil.

There is no valid ground left for adopting as ultimate unit any other unit than the absolute unit itself. We have not in our electrical standard legislation given full credit to the mechanical engineer for what he can do for us. He can make a machine that will measure resistance in absolute units with a precision as great as—I might even say greater than—that with which the Government is prepared to guarantee its comparisons. Such a machine ought to be at work in the Board of Trade laboratory, in order that there may be opportunity from time to time, at regular intervals, to measure the Government coil or coils in absolute units. It is necessary that this should be done if we are to be guarded against the perpetual inconvenience of unknown possible changes in the coil, the resistance of which is now the legal unit.

Such a step would have collateral advantages. It would enable the Board of Trade to certify standards of low electrical resistance. With such a machine standards of from a thousandth to a two-hundred-thousandth of an ohm may be measured to nearly the same percentage accuracy. All we have to do in dealing with the very low resistances is to pass a sufficiently large current through them, and shunt the standard coil of the machine. But to enter into details on this point would lead me too far. I must content myself with saying that I believe such a machine is much the best instrument for standardising low electrical resistances, and that accurate standards of low resistance would be of great service both in the laboratory and the workshop.

I have, in conclusion, only to express my obligation to my assistant, Mr. Samuel Harrison, for the great and constant help he has given me in the course of my investigations on this subject.

[J. V. J.]

WEEKLY EVENING MEETING,

Friday, May 31, 1895.

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

The Earl of Rosse, K.P. D.C.L. LL.D. F.R.S. *M.R.I.*

The Radiant Heat from the Moon during the Progress of an Eclipse.

Some twenty-two years ago (May 30, 1873) I delivered in this room a lecture, the title of which was 'On the Radiation of Heat from the Moon, the Law of its Absorption by our Atmosphere, and its Variation in amount with her Phases.'

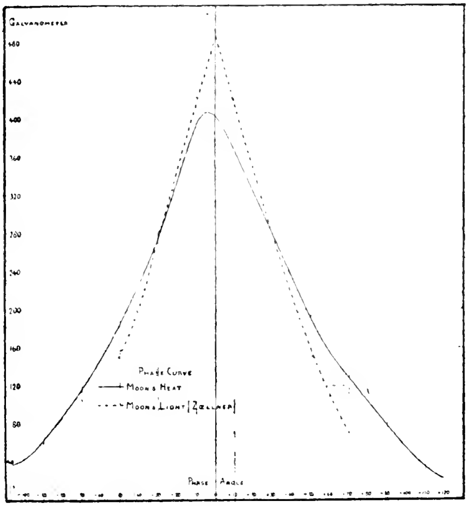

Fig. 1.

I then showed that I had been able, with the aid of a mirror of 3 feet aperture, not only to detect with certainty the heat radiated from the lunar surface, but also to estimate with tolerable accuracy the changes of its amount through the greater part of the lunar month, to ascertain roughly the average *quality* of her rays as regards refractive index, and to compare their amount at the time of Full Moon with those of the Sun.

I also showed that we had been able by means of a long series of determinations, continued on different nights, through considerable increases and decreases of altitude, to determine with considerable accuracy what may be called the extinction curve for heat with decreasing altitude; probably as completely as Seidel, at Munich, had deduced that for light from his photometrical observations on stars.

As the phase-curve descended on each side almost to zero on approaching New Moon, it was clear that little or none of the heat we were measuring came from the interior of the Moon. It was heat derived directly from the Sun.

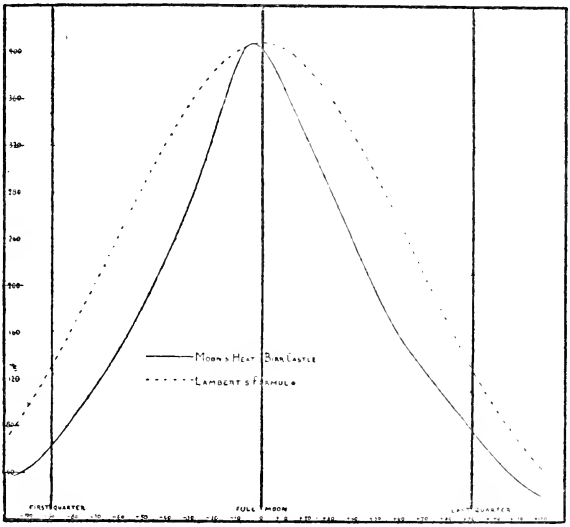

Fig. 2.

Some series of determinations were then made alternately with a sheet of glass interposed between the large speculum (of the telescope) and the apparatus and with the glass removed, and from the fact that from 8 to 17 per cent. of the heat (being greatest towards Full Moon) was transmitted by the glass, while some 90 per cent. of the sun's rays passed through the same sheet of glass, it was, we think, clearly established that the heat, which we had already concluded, as stated above, to be directly derived from the Sun was not reflected sun-heat, but heat absorbed and afterwards emitted by the lunar surface.

It might, perhaps, have been expected that, as is more largely the case on the Earth, the highest temperature of the lunar surface would occur appreciably later than the time of Full Moon,* but the phase-curve did not indicate this; on the contrary, whether from some accidental errors or from some real though unexpected cause, it showed a maximum at about 10 hours before Full Moon.

Slide 1 will now be thrown upon the screen, in which the observed phase-curve is compared with Zöllner's curve for light, the latter being much steeper towards the maximum near Full Moon. Zöllner's curve agrees more nearly with that calculated for a sphere covered with meridional corrugations whose sides are inclined 52° from the surface than with the smooth sphere assumed by Lambert and shown in Slide 2. In both cases the surfaces are assumed to be "matt," or free from polish.

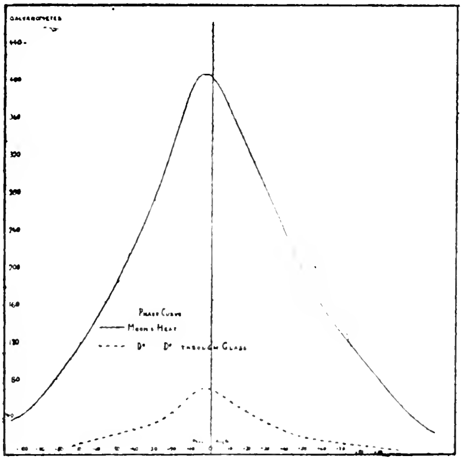

FIG. 3.

In Slide 3 the total heat-curve is compared with that of heat through glass. Slide 4 compares the heat through glass with Zöllner's light-curve, and it will be seen, as might be expected, that

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* If we compare these results with analogous cases on the earth, we find that a very much longer time is required before the air temperature, and with it more or less that of the earth's surface, arrives at the final temperature due to the sun's radiation at the time. The maxima and minima of temperature are generally three weeks later respectively than the summer and winter solstices, and the hottest time of the day is generally about two hours after noon. Again, the fall of temperature during an eclipse of the sun, though observations are very conflicting, would appear to be from 7° to 10° only. During the eclipse of July 1878, radiation thermometers gave a depression of 10°, 22½°, 32° and 42°, but this is far short of the depression of temperature of the lunar surface, 200°, if the surface has the same radiating power as lamp-black, but probably in reality considerably greater.

the former coincides far more nearly than does the total heat-curve with the latter.

Slide 5 gives the "Extinction-curve" for heat, compared with Seidel's for light. The curve gives the logarithm that must be added to that of the reading at any zenith distance to obtain the logarithm of the reading as it would have been had the Moon been in the zenith.* This curve gives a percentage to be added to the readings of

$1\frac{1}{4}$ at 29° zenith distance
7 „ 45° „
19 „ 60° „
28 „ 65° „
50 „ 79° „

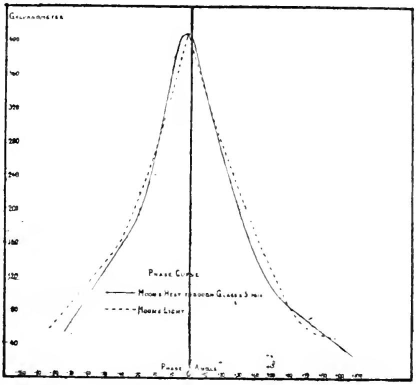

FIG. 4.

It then occurred to us that, though this real absorption of heat takes place so quickly that no appreciable time occurs between its reception and emission when observed through the comparatively slow changes of its incidence in the course of the lunar month, surely with the far more rapid changes of illumination during an eclipse—some three or four hundred times as rapid—the delay in the emission might be easily perceived.

Accordingly we have been on the watch for every lunar eclipse.

Such events are not very frequent, and of such as occur some are unsuitable for observation owing to the Moon's low altitude (or to her being below the horizon during the whole or part of their duration), while of the remainder most are more or less useless for our purpose owing to the uncertainties of our climate.

Before the date of my former lecture one eclipse had been utilised

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* See also Proc. Roy. Soc., 1869, No. 112, p. 436, and 1870, No. 123, p. 9; also Phil. Trans.

for this purpose, and I find that I am reported to have said in that lecture: "The eclipse was a very partial one, only about $\frac{1}{40}$ of the Moon's diameter being in shadow; but although this circumstance, coupled with the uncertain state of the sky, rendered the observation less satisfactory than it would otherwise have been, yet it was sufficient to show that the decline of light and heat as the penumbra came over the lunar surface, and their increase after the middle of the eclipse, were sensibly proportional." This eclipse occurred on November 14, 1872. On May 22, 1872, clouds had prevented observations during the whole progress of the eclipse.

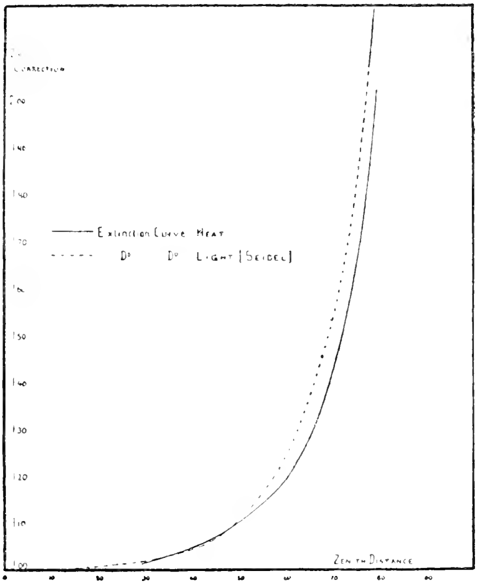

FIG. 5.

On August 23, 1877, some determinations of heat during the *total* eclipse were made. Clouds again interfered, and the observations were very unsatisfactory, but after considerable delay they were published in 'Copernicus,' vol. i. p. 22.

No further opportunity occurred until October 4, 1884,* when, during my absence in America, my astronomical assistant, Dr. Boeddicker, had the good fortune to get a far more satisfactory series of determinations than any previously obtained. The observations were carried on with as little interruption as possible from 20 hr. 27 min. sidereal time, or 11 minutes before the first contact with the shadow,

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* See also Trans. Roy. Dub. Soc., vol. iii., Series II., 1885, pp. 321–332.

to 1 hr. 55 min. sidereal time, or 45 minutes after the last contact with the penumbra. The values, however, up to 21 hr. 11 min. were obtained through a moderately dense covering of clouds, and being therefore of no value for our purpose, have been omitted from the slide (6). It will be seen that the heat (observed) varies pretty nearly as the light (calculated), a slight delay or lag being apparent in the heat; but the most remarkable fact that came out was that the heat rapidly increased after the termination of the total phase, not towards its usual value for Full Moon, but towards a value some 87 per cent. only of that amount, and though the observations were continued half an hour after the last contact with the penumbra, there appeared no disposition to a return to the standard Full Moon value. This value was deduced from observations on October 3, 5, 6, and November 2, 28, 30.

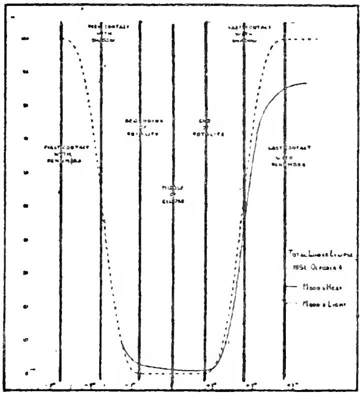

Fig. 6.

During the period of totality the observations were not reliable owing to the feebleness of the Moon's image, which made it uncertain whether or not it was properly upon the apparatus; but those made just before and just after totality indicate very little radiant heat.

The next opportunity—an exceptionally favourable one—occurred on January 28, 1888.* The sky was not obscured AT ALL during the *whole* progress of the eclipse, and the same apparent anomaly of the heat not returning to its standard value, even as long as 1 hr. 40 min. after the last contact with the penumbra was observed. But IN ADDITION it was found that the radiation, which we began to measure 1 hr. 5 min. before the first contact with the penumbra, seemed to begin to decrease almost immediately, so that when the time of "First contact with the penumbra" had arrived, the heat had been

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* See also Trans, Roy. Dub. Soc., vol. iv., Series II., 1895, pp. 481–512.

reduced to about 98 per cent. of its full value. At the time of last contact with the penumbra the heat was 80·6 only, and 1 hr. 40 min. later, 81 per cent. only of its full value. Thus the result arrived at in 1884 for the second period of the eclipse was FULLY confirmed.

Slide 7 exhibits the course of the changes of heat radiation during the progress of this eclipse.

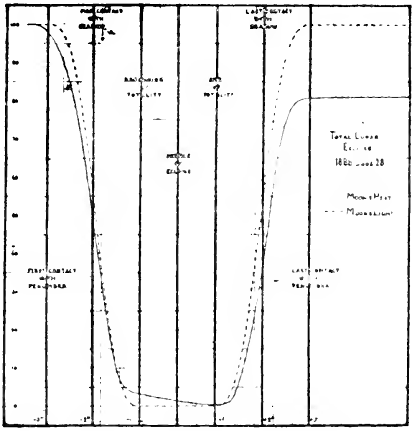


FIG. 7.

In 1884 the Moon rose eclipsed, and clouds interfered until 23 minutes before the total phase. This time good observations from over an hour before the first contact with the penumbra were possible. Thus the values after the eclipse could be compared with those *before* the eclipse on the *same* night.

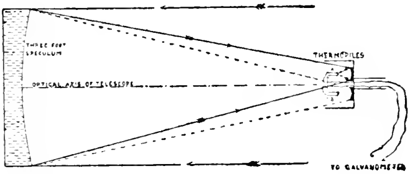


FIG. 8.

So far as could be perceived, the sky was cloudless and unchanged through the whole period of the observations. It will be observed that the heat declined to less than 1 per cent. during totality. Why did it not rise equally after the eclipse? Can it be that at the lower temperature of the eclipsed lunar surface the residual surface-heat was less capable of penetrating the Earth's atmosphere? Can it be that the difference—some 300° F.—between the lunar temperature when illuminated and when dark causes a sufficient difference in the power of the heat-rays to penetrate our atmosphere? I hardly think that this is a sufficient explanation.

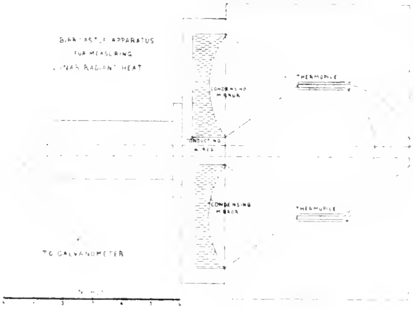


FIG. 9.

This is the last eclipse which we have been able to observe. We were ready for that of February last, but clouds interfered the whole time, otherwise the opportunity would have been valuable for the first period, and we might have carried our observations still further back before the eclipse. I should have much liked to give you some even imperfectly reduced results of that night's work this evening.

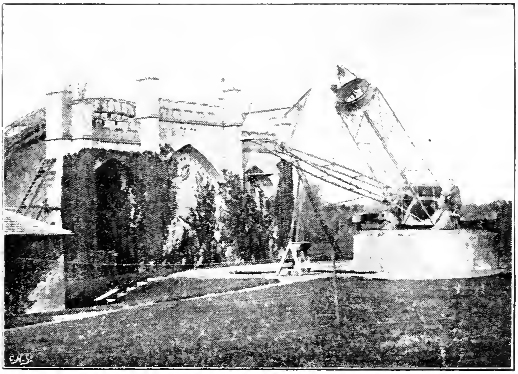


FIG. 10.

I will now describe briefly the apparatus used, preparatory to a few remarks on that of Professor Langley and of others, and on the results of their work.

Slide 8 represents in section the telescope with the apparatus attached. The concentration on a surface of one-third of an inch circular is equal to 5700 times, allowing for loss of light by reflection

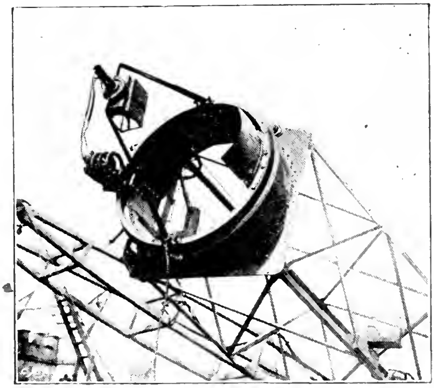


Fig. 11.

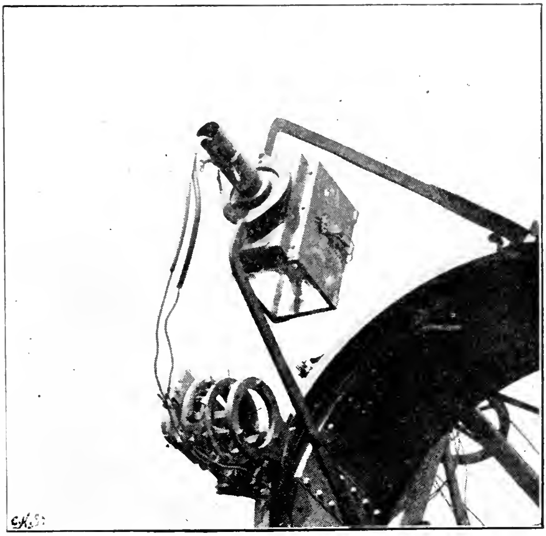


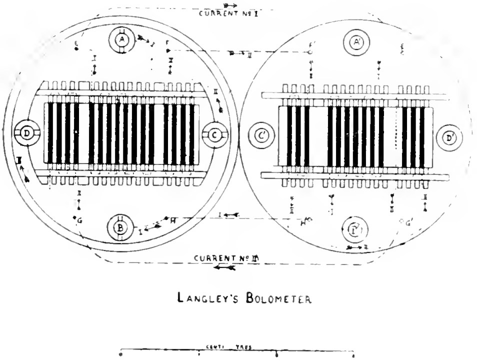
Fig. 12.

of the surfaces of the two mirrors. Slide 9 gives an enlarged section at the box with the thermopiles and the concave reflectors.

In Slides 10, 11, 12 we have views of the telescope and apparatus.

The ordinary thermopiles, made by Elliott Brothers, were at first used, but after a time thermocouples, consisting of bismuth and bismuth-tin alloy in very thin bars soldered to thin discs of copper to receive the heat, were substituted.*

Slide 13 represents Professor Langley's "bolometer." It is composed of two superimposed gratings of exceedingly thin metal (generally $\frac{1}{50}$ inch wide and $\frac{1}{6000}$ inch thick). The planes of the gratings are $\frac{1}{25}$ inch apart. The strips are connected up and down into two systems, which are respectively interposed in the two arms of a Wheatstone's bridge. A slight change of resistance in either system, through a change of temperature under radiant heat, is thus detected and measured by a galvanometer in the cross arm as usual. The strips are indicated by the strong black lines. The course of the current is indicated by the dotted lines. The two gratings are shown side by side, and may be imagined hinged and turned so that the corresponding plain and dotted letters come into electrical contact. There are two circuits: the first in the centre, on to which the radiation is alternately directed and then removed; the second, or screened circuit, on the two sides, which acts simply as a compensator for accidental disturbances.



LANGLEY'S BOLOMETER

FIG. 13.

A pair of single bolometers, made for me by a London artist, did not show more sensitiveness than my thermocouples, but they are not, I think, of the delicacy of the American instruments. It was evident that some experience and practice would be required for their successful use.

Langley has varied the proportions of his bolometers, and the descriptions of them are to be found in his several papers in the

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* See Proc. Roy. Soc. No. 112, 1870, p 553.

'American Journal of Science,' and in the 'Memoirs of the National Academy of Sciences.'

In 1883 Langley commenced his experiments on the relative percentage of the Sun's heat and the Moon's heat transmitted by glass, obtaining not widely differing numbers for each with the interposed glass near to the face of the bolometer; but subsequently, with the glass removed to a greater distance, his results approximated to mine, 75 per cent. being the value found for sunlight, and 12 to 14 per cent. for moonlight with the same sample of glass.

Langley, as far as he was able, observed the eclipse of October 4, 1884, and he says the eclipsed Moon rose behind clouds, and there was still haze as the penumbra passed off. "The inference from the observations, so far as any could be drawn, was that about the same amount of heat was received as was to be expected had there been no previous eclipse."

He remarks of the Birr Castle observations during this eclipse (before the *complete* account had been published): "They appear to bear but one interpretation, that all the heat from the Moon disappears immediately that it passes into the Earth's shadow, and there is no evidence of any being retained for any sensible time, more than if it were reflected." This we have seen appears to have been very nearly, but perhaps not quite the case.

Langley, in 'Memoirs of the National Academy of Sciences,' vol. iii. infers: "1st. That there is a certain presumption that the Earth's atmosphere is diathermanous to heat of lower wave length than has been heretofore supposed, and *of lower wave length* than appears to reach us from the Sun.

"2nd. If we may make any inference from comparisons with the Leslie cube, the sunlit surface of the Moon is not far from the freezing temperature, but not so far below it as we might expect to find that of an airless planet.

"3rd. So far as our limited observations allow of an inference, there is a "not very materially *greater* coefficient" [query less] "of transmission for lunar than for solar."

The second of these inferences he makes from the distribution of heat in the lunar spectrum, a very difficult thing to measure satisfactorily. This he admits, and says that the work has been one of great labour, but justified by its having been given to a question of abstract interest, but one "to which the whole subjects of terrestrial radiation and the conditions of organic life upon our planet are intimately related."

Remarks of a similar character may be made with regard to the results of the eclipse observations. What limits the extent of our atmosphere? Is it boundless? Does it always follow Boyle's Law, or where does it depart from it? These may be questions of interest to physicists. The observations before the commencement of the Eclipse of 1888 seem, therefore, so far as they go, to be of extreme importance. It is certainly unsatisfactory, and perhaps it may be premature to put them forward when still unconfirmed. My excuse must be that I have waited in vain for more than seven years for an opportunity to test their accuracy and reliability.

Commenting on our results on heat through glass, Langley says that "The low transmissibility by glass is quite confirmatory of the results of Lord Rosse, though not necessarily of his inferences from them," and he goes on to say that it may be partly accounted for by the supposition that the rays which reach us have suffered selective reflection at the surface of the Moon, and in support of this he quotes Sir John Herschel. "From the fact," he says, "that the lunar light is not white like the Sun's, but *yellowish* (Sir J. Herschel compares the Moon's surface to that of sandstone rock), it was antecedently probable that such was the case." Langley, however, seems to have mistaken the meaning of Herschel's remark. He says,* "Nor let it be thought surprising that a solid substance thus illuminated should appear to shine and again illuminate the Earth. It is no more than a *white* cloud does, standing off upon the clear blue sky. By day the Moon can hardly be distinguished in brightness from such a cloud, and in the dusk of the evening appears with a dazzling splendour, not inferior to the seeming brightness of the Moon at night." And in a note he remarks, "The actual illumination of the lunar surface is not much superior to that of weathered sandstone rock in full sunshine. I have frequently compared the Moon setting behind the *gray* perpendicular façade of the Table Mountain, illuminated by the Sun just risen in the opposite quarter of the horizon, when it has scarcely been distinguishable *in brightness* [the italics in both cases are mine] from the rock in contact with it." His remarks, therefore, appear to refer to brightness rather than to colour.

Langley, however, with the aid of prisms and lenses of rock salt, obtained an absorption heat spectrum for the lunar rays, but such an investigation is far beyond what we were able to attempt. I am not quite clear at present, whether any bands of absorption were detected in the heat spectrum of the Moon which do not equally exist in that of the Sun.

I still think that our inference is pretty nearly the correct one, that the radiant heat from the Moon may be treated as consisting of two portions—one, of the reflected rays of various refrangibilities in about the same proportions as they exist in the direct radiation from the Sun, the other of rays absorbed and subsequently emitted by the lunar surface, and containing the rays of various refrangibilities in the proportions due to a body at the surface temperature of the Moon. Langley more recently inclined much to this opinion, and in a letter to me, dated October 6, 1885, he says, "Since then" (that is the date of my visit to Alleghany in 1884) "I have obtained so much additional information by the study of the lunar heat spectrum, that I have concluded to make a second memoir. While these observations

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* Herschel's 'Outlines,' p. 272, edition 1858.

seem to me to indicate a low temperature of the lunar soil, they have made me a convert to the opinion that your measures on the transmissibility of glass indicate a great relative emission from the soil, and I feel that the effect can only in part be accounted for by selective reflection." It is much to be regretted that at the death of Mr. Thaw, a citizen of Pittsburg, the funds which he had furnished to Professor Langley for these special researches ceased. Professor Langley was soon after appointed secretary of the Smithsonian Institution, and most unfortunately his further undivided attention to these investigations was lost to science.

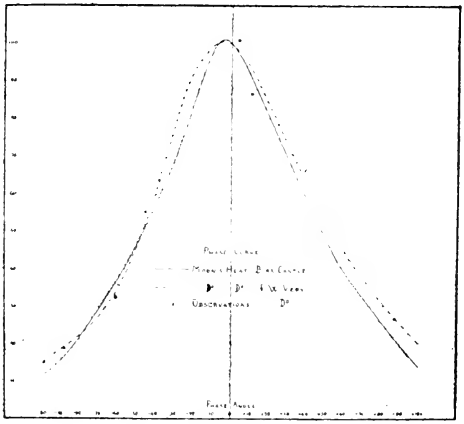

Fig. 14.

I should here mention the 'Prize Essay' of Mr. Very, of Alleghany, with whom Langley left his radiation apparatus. In response to the request for a lunar-heat phase-curve, sent out by the Utrecht Society of Arts and Sciences,* Mr. Very constructed such a curve from only eight values, obtained on eight nights. The curve agrees nearly with ours, but he had our curve, based on some thirty or more nights' work to guide him. Slide 14 is a reproduction of his diagram. He, however, did also what was new, but outside the request of the Society, and it is for this reason that I allude to his work. He made determinations of the relative radiation from various parts of the visible lunar surface on each of those eight nights, and Slide 15 is a reproduction of his diagrams of the distribution of heat. From summation of these values he obtained his total heat-values for each night.

Mr. C. V. Boys, F.R.S., has made a new departure in apparatus, taking as his model the galvanometer of D'Arsonval. In this, instead

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* Martinus Vishoff, The Hague, 1895.

of a fixed coil carrying the current to be measured, causing the motion of a magnetic needle delicately suspended within it, there is a fixed horseshoe magnet, between whose poles is suspended a coil which the current enters from, say above, and leaves below, through two fine wires, which hold it in position. To this instrument there is the drawback that the wires from the coil must be small, otherwise they would impede its movements. Hence it is unfitted for the measurements of currents of low tension like those coming from the thermocouple.

Mercury contacts, owing to capillary forces and an incipient coating of oxide, give endless trouble. Mr. Boys has modified the instrument by rigidly connecting the thermocouple with the coil, or the single circuit which replaces the coil. Thus a far more sensitive arrangement than mine (some 6000 times he states) has become possible; but it has the disadvantage that it cannot be fitted to the ordinary telescope, as it must never change its position, and hence a siderostat—a not very simple thing to provide when we work with large apertures—is essential. I have a radiomicrometer, but I have not as yet found an opportunity for the construction of the required siderostat.*

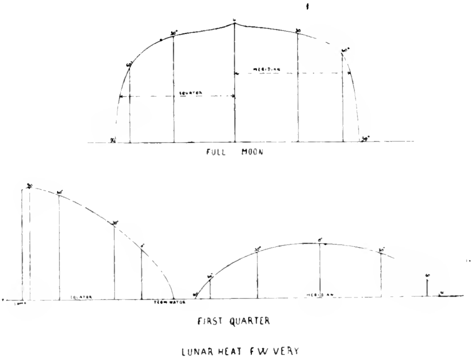

Fig. 15.

Slide 16 gives † Mr. Boys' curve for the deviation as the Moon's image slowly passes by diurnal motion over the instrument, thus showing approximately the distribution of the radiation from the lunar disc. Owing to the very small mass of metal to be heated, and the absence of swing, or the "dead beat" of the needle, this method of observation has become possible. With our arrangement the comparative slowness of the needle in coming to rest has entirely prevented this method of observation.

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* See Phil. Trans. vol. clxxx. 1890, p. 159.

† See Proc. Roy. Soc. vol. xlvii. 1890, p. 480.

As to future extension of these observations. Notwithstanding all discouragements of weather, we shall still watch for eclipses and even "near approaches" at Full-Moon; but we much desire a more sensitive apparatus, and we must turn our thoughts to the radiomicrometer with suitable siderostat, to the bolometer, or to what I should much prefer, a more sensitive reflecting galvanometer to be used with the thermocouple. I believe that skilful and delicate hands can improve much upon the present instrument.

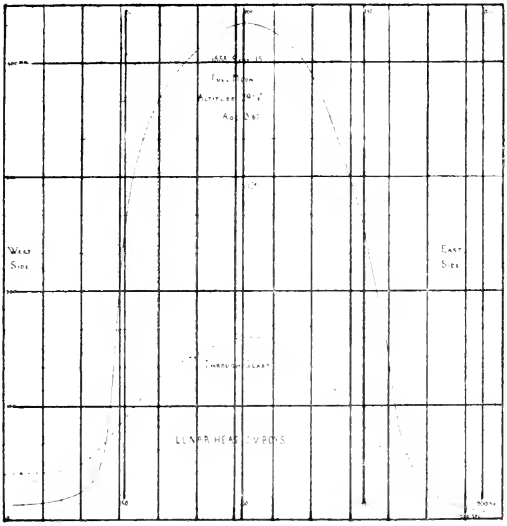

FIG. 16.

It may be asked—can heat experiments be extended to other Heavenly Bodies? The step is a long one. The relative proportions of light coming to us have been estimated as below—

$$\frac{\text{Full Moon}}{\text{Venus}} = 1325 ; \quad \frac{\text{Full Moon}}{\text{Jupiter}} = 6426$$

and

$$\frac{\text{Full Moon}}{\text{Sirius,}} = 12,703,$$

or, if we assume the Moon's heat-radiation as compared with her light to be eight times as great as that of the Sun, or a star, the proportion between the heat-radiations of the Moon and of Sirius is 100,000. Much greater sensitiveness is therefore needed. It will be quite possible, I believe, even with the present thermopile and galvanometer to measure the radiation from the various parts of the

Moon's primary image. The concentration of heat will be reduced some sixty times, but it will be at the same time possible to screen off extraneous disturbances of the needle better.

I think that the comparison of the heat through glass with the total heat is of sufficient importance for repetition with greater care, and that of the Sun's heat with the Moon's heat should be gone over again, as also that of the Sun's light with the Moon's light, when we find so large disagreements between the various figures hitherto published. We have—

Lambert	400,000	to 1
Bouguer	300,000	,,
Wollaston	800,000	,,
Bond	480,000	,,
Zöllner	618,000	,,
W. H. Pickering	350,000	,,
Sir William Thomson	70,000	,, *

Again, one would like to see Langley's experiments on the solar and lunar heat spectrum repeated.

We want, however, more workers. If preparations for heat observations during eclipses were made at several stations, widely apart, and as much as possible in less cloudy climates, over the surface of the earth, we should in a comparatively short time clear up doubtful points to an extent for which a lifetime of our work would be inadequate, and if this lecture should conduce to this end I shall feel, if for no other reason, that my trouble will have been fully rewarded.

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* This last value was probably much affected by the smoky atmosphere of Glasgow.

WEEKLY EVENING MEETING,

Friday, June 7, 1895.

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

Prof. Alfred Cornu, D.C.L. F.R.S. Officier de la Légion d'Honneur, Vice-Président de l'Académie des Sciences, Paris, *Hon. M.R.I.* etc.

Phénomènes Physiques des Hautes Régions de l'Atmosphère.

La cause première et décisive de presque tous les phénomènes physiques ayant pour siège l'atmosphère terrestre est la chaleur solaire. L'atmosphère peut donc être considérée comme une immense machine thermique dont le foyer est le soleil ; la chaudière est figurée par le sol, ou les nuages échauffés par ses rayons, et le condenseur par le rayonnement vers l'espace interplanétaire.

Les moyens dont disposent les physiciens et les météorologistes pour étudier les diverses régions de l'atmosphère sont très limités: ils sont obligés de se contenter le plus souvent d'observations très indirectes et de procéder par induction. En effet, les phénomènes les plus interessants se passent dans les hautes régions, c'est à dire à des hauteurs presque inaccessibles. Le but de cette lecture est de vous montrer par quelques expériences que les physiciens météorologistes commencent à s'approcher beaucoup de l'explication véritable des phénomènes naturels. Vous verrez, en effet, que, dans certains cas, on arrive non seulement à obtenir une image exacte de ces phénomènes, mais souvent à en produire une véritable synthèse par l'emploi de procédés tout à fait analogues à ceux qui fonctionnent réellement dans la nature.

Je commencerai par énumérer les moyens en usage parmi les météorologistes pour étudier les différentes régions de l'atmosphère.

La méthode la plus directe repose sur l'emploi de l'aérostat: l'aérostat ou ballon permet effectivement de porter les instruments de mesure au sein même des couches atmosphériques qu'on veut étudier. Malheureusement le moyen est difficile, coûteux, et même dangereux; il n'est donc employé que d'une manière exceptionnelle. Les ascensions aérostatiques les plus fructueuses ont été celles de Gay-Lussac (1804), de Glaisher (1862) et récemment du Dr. Berson, de Stassfurt (1894), qui s'est élevé à plus de 9000 mètres.

Les faits les plus importants observés en ballon étaient fort inattendus; en voici le résumé.

1°. Il existe très fréquemment des nuages formés de *cristaux de glace:* ils constituent les cirrus qui flottent à des hauteurs très grandes;

2°. La *direction* des *vents change* à diverses hauteurs;

3°. La température ne diminue pas toujours régulièrement avec l'altitude: on rencontre souvent des *couches froides* et des *couches chaudes alternativement.*

La seconde méthode directe pour étudier l'atmosphère est la création des observatoires de montagne, autant que possible sur des pics isolés. Dans ces observatoires on vérifie journellement la réalité de ces *inversions* si imprévues des *vents* et *de la température* à diverses altitudes.

Quant aux nuages de glace, ils sont trop élevés pour être atteints directement par les observatoires de montagne.

Il sera probablement intéressant pour vous de connaître les principaux observatoires de montagne créés en France.

Projection des photographies des observatoires suivants:

Pic du Midi	(altitude	2800	mètres)	dans les Pyrénées.
Mont Ventaux	,,	1900	,,	en Provence.
Puy-de-Dôme	,,	1900	,,	en Auvergne.
Tour Eiffel	,,	330	,,	à Paris.

Ce dernier observatoire, grâce à la légèreté de sa construction tout à jour, peut être considéré presque comme un ballon captif, permanent et fixe, à 300 mètres au-dessus du sol.

Halos.—Nous avons dit que les observatoires de montagne n'atteignent pas la région des nuages glacés (6000 à 10,000 mètres d'altitude): on serait donc condamné à ne jamais les observer qu'en ballon. Heureusement ces cristaux de glace se révèlent par un phénomène optique qu'on aperçoit même du niveau du sol, le *Halo.* C'est un cercle brillant de 22° environ de rayon, qui entoure le soleil ou la lune; il présente une teinte rougeâtre à l'intérieur et légèrement bleuâtre à l'extérieur. On l'explique, ainsi que beaucoup d'apparences du même genre, par la réfraction de la lumière de l'astre à travers les aiguilles glacées: en effet, les cristaux de glace sont des prismes hexagonaux dont les faces sont de deux en deux inclinées de 60°. Ces cristaux, disséminés dans l'air et orientés dans toutes les directions, réfractent la lumière, mais les rayons réfractés ne peuvent dépasser l'oblique de 22° que leur impose le *minimum de déviation* découvert par Sir Isaac Newton: la limite des rayons réfractés est donc un cône de 22° autour de la ligne qui va de l'œil à l'astre.

Expérience imitant le halo.—On fait naître des cristaux dans un milieu transparent constitué par un mélange de liquides appropriés; on reproduit ainsi exactement le mélange des couches chaudes et humides de l'atmosphère avec les couches froides qui font naître les cristaux de glace.

À cet effet on place dans une cuve de verre une solution aqueuse saturée d'alun de potasse, et à travers cette cuve on fait passer un faisceau lumineux projetant l'image d'une ouverture circulaire figurant le soleil sur un ciel obscur. Puis on ajoute un quart du volume total d'alcool rectifié; l'alun, insoluble dans l'eau alcoolisée, le précipite en cristaux très petits qui flottent au sein du liquide. L'image du soleil se trouble d'abord comme dans un brouillard, mais bientôt un cercle brillant et légèrement irisé se dessine et figure très exactement l'apparence du halo: l'expérience est brillante et instructive.

Ce phénomène est bien connu des gens de la campagne: c'est un signe certain de pluie lorsqu'il apparaît pendant une journée chaude, même lorsqu'aucun autre indice ne fait prévoir de perturbation météorologique.

Alternance et inversion des températures.—Dans les observatoires voisins situés à des altitudes très différentes, comme celui du Puy-de-Dôme et celui de Clermont, on constate très souvent l'existence de courants chauds dans les régions supérieures. C'est à des inversions successives de même nature que Mr. Amsler de Schaffouse attribue ce beau phénomène connu en Suisse sous le nom d'*Alpenglühen* et qui consiste dans une nouvelle illumination des sommets neigeux des Alpes plusieurs minutes après que le coucher du soleil les a rendus obscurs.

Projection d'une photographie des sommets de l'Oberland bernois, la Jungfrau, le Mönch, l'Eiger; la vue étant prise de St. Beatenberg, près du lac de Thoune. Imitation pittoresque du phénomène par un verre coloré et des diaphragmes convenables.

L'explication de Mr. Amsler est fondée sur le changement du sens de la courbure de la trajectoire des rayons lumineux suivant que l'air du fond des vallées est plus chaud ou plus froid que celui des régions élevées.

Avant le coucher du soleil, le sol échauffé par la chaleur solaire, imprime à la trajectoire une courbure analogue à celle du *mirage* S A M B,

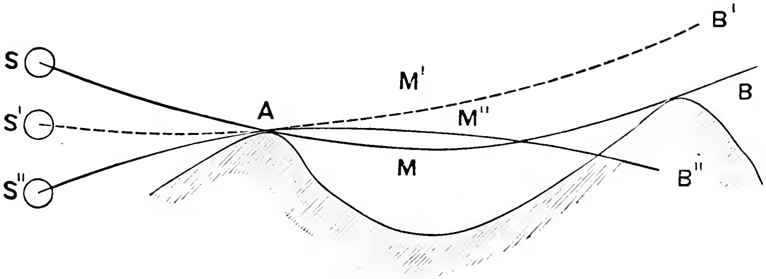

Fig. 1.

c'est à dire, convexe vers la terre; le soleil en s'abaissant en S′ fait que l'ombre du sommet A se projette sur le sommet B qui devrait désormais rester dans l'ombre puisque le soleil continue à s'abaisser et que le dernier rayon est S′ A M′ B′. Mais si dans l'intervalle l'air de la vallée se refroidit suffisamment, la trajectoire prend une courbure inverse S″ A M″ B″ et le sommet B se trouve de nouveau illuminé.

Réalisation expérimentale de l'inversion des courbures des trajectoires lumineuses.—Avec un peu de précaution on arrive à superposer dans une cuve transparente de 20 cm. d'épaisseur trois couches de liquide dont la composition et indiquée ci-contre. Un miroir mobile LL amène un faisceau de lumière par l'ouverture S d'un diaphragme. Ce faisceau envoyé sous diverses inclinaisons se réfléchit soit sur la couche inférieure de chlorure de zinc (dense, mais moins refringente), soit sur la couche de glycérine diluée (plus légère et aussi moins refringente que la couche intermédiaire).

Un peu de fluorescéine illumine la trajectoire des faisceaux et rend bien visibles leurs courbures; on arrive ainsi à représenter l'*Alpenglühen* avec quelques dispositifs accessoires.

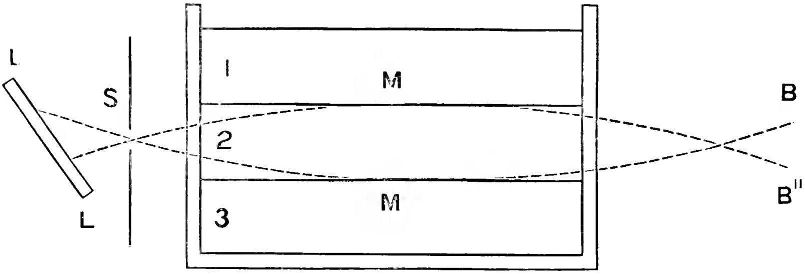

FIG. 2.

1, glycérine $\frac{1}{3}$, eau $\frac{2}{3}$; 2, glycérine $\frac{2}{3}$, eau $\frac{1}{3}$; 3, chlorure de zinc anhydre $\frac{1}{3}$, eau $\frac{2}{3}$.

Scintillation des étoiles.—Ce phénomène est aussi une preuve des alternances de température et de mouvement des couches d'air dans les hautes régions. L'analyse spectrale montre que la scintillation est produite par une disparition suivant une marche quasi régulière (en accord avec la variation de distance zénithale de l'étoile) des couleurs successives du spectre.

Imitation du phénomène.—On l'obtient par une expérience très brillante en projetant avec une lentille L l'image d'une ouverture lumineuse O sur une petite boule argentée B de 3 à 4 cm. de diamètre, posée sur un velours noir. On à ainsi l'aspect d'une étoile fixe, d'un éclat remarquable.

Mais l'ouverture lumineuse O est percée dans un carton sur lequel se projette l'image spectrale d'une fente F dispersée par un prisme à vision directe P. À vrai dire, le carton CO n'est pas au foyer du spectre, lequel foyer se forme plus loin dans le plan de la lentille L. Il en résulte que l'image irisée de la fente sur le carton offre en son milieu une région blanche: c'est là qu'on place l'ouverture O. Aussi la lumière projetée sur la boule B est-elle parfaitement incolore. Mais le faisceau, au sortir de l'ouverture, s'épanouit en spectre sur la lentille de projection L qui la recompose en B comme dans une célèbre expérience Newtonienne.

Alors en déplaçant un grillage à larges mailles devant la lentille L on enlève certaines radiations et l'étoile B paraît colorée.

Une demi-lentille divergente D, de même foyer que L, annule son effet et le spectre de l'étoile avec les bandes artificielles créées par le grillage, apparaissent sur un écran blanc à côté de la boule. C'est l'imitation de l'analyse spectrale de la scintillation des étoiles.

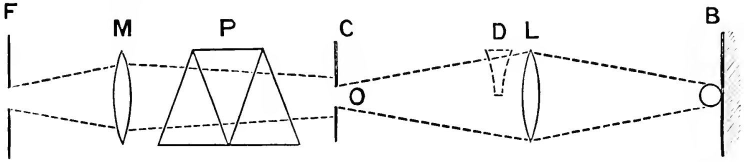

Fig. 3.

On voit par ces quelques exemples que l'étude des *phénomènes optiques* de l'atmosphère, aidée par l'*analyse* et la *synthèse* physiques, peut et doit apprendre beaucoup sur les phénomènes calorifiques des régions inaccessibles.

Phénomènes dynamiques de l'atmosphère.—Les phénomènes étudiés jusqu'ici sont dûs à des états d'équilibre presque complets dans les couches atmosphériques: on pourrait les appeler *statiques.* Mais l'action calorifique du soleil, combinée à l'action refroidissante du rayonnement dans l'espace, peut produire des phénomènes de mouvement présentant tous les degrés d'intensité depuis les plus faibles jusqu'aux plus violents : nous les appellerons phénomènes *dynamiques.*

Ils se manifestent sous des formes très diverses:

1°. Sous forme d'*énergie mécanique :* vents, tourbillons, cyclones, trombes, etc. ;

2°. Sous forme d'*énergie calorifique* qui se traduit par la formation des nuages, de la pluie, de la grêle, correspondant à des changements d'état de *l'eau*, l'élément continuellement variable de l'atmosphère.

3°. Sous forme d'*énergie électrique :* éclairs, tonnerre, etc.

En fait, c'est la transformation de l'énergie solaire en énergie mécanique qui est le phénomène fondamental ; il entraîne tous les autres. C'est la seule transformation que, pour abréger, je considérerai ici.

Le phénomène mécanique le plus simple qui se produit dans l'atmosphère est le *vent.* Le vent a pour origine une différence de pression entre deux points plus ou moins éloignés : on sait depuis Pascal que la pression de l'air est mesurée par le baromètre. On pourrait donc penser, d'après cette propriété, que la direction du vent est toujours déterminée par les indications de cet instrument ; c'est à dire que le vent doit aller du point où la pression barométrique est la plus forte au point où la pression barométrique est la plus faible.

Eh bien ! il n'en est presque jamais ainsi : la direction réelle du vent est toujours oblique sur cette direction théorique ! Ce fait est connu seulement depuis très peu d'années ; ce sont les cartes météorologiques générales—imaginées il y a trente ans par Le Verrier —si répandues aujourd'hui, qui ont mis ce fait hors de doute.

La direction du vent semble *tourner autour* du point de la carte où se trouve la pression *minimum* en *sens inverse* des aiguilles d'une montre; ou bien dans le *sens direct* autour du point de pression *maximum.* Tel est le sens du phénomène dans l'*hémisphère boréal :* c'est l'inverse dans l'*hémisphère austral.* En un mot, le mouvement le plus ordinaire de l'atmosphère est un mouvement *gyratoire*, ce qu'on nomme un *tourbillon.*

Il y a longtemps que le mouvement tourbillonnaire de l'air a été aperçu: nous le voyons se produire bien souvent autour de nous; la poussière, les feuilles mortes sont soulevées par le vent en tourbillons semblables aux *remous* de l'eau dans les rivières. Les marins connaissent les *cyclones*, les *trombes*, dont ils redoutent les dangereux effets. Sur le continent américain on observe aussi des ouragans terribles nommés *tornados.* Ces mouvements gyratoires paraissaient ne convenir qu'à ces grandes perturbations orageuses; mais à mesure que l'on poursuit dans le détail l'étude de l'atmosphère on reconnaît que ce genre d'ébranlement se rencontre dans toutes les manifestations de l'air déplacé. On en conclut que le mouvement tourbillonnaire est l'état en quelque sorte *normal* de l'air agité; on ne peut guère exercer d'efforts sur une masse gazeuse sans y développer des rotations plus ou moins rapides qui tendent à se conserver en *régime permanent.*

Preuves expérimentales.—Toutes les fois qu'on produit un jet de gaz rapide, il se forme un ou plusieurs tourbillons à côté du jet. Le tourbillon prend la forme d'un anneau si la colonne projetée est bien cylindrique; témoins les couronnes de fumée qu'on observe après l'explosion des canons, des fusils, etc.

Répétition de l'expérience bien connu des belles couronnes de fumées produites en frappant le fond en toile d'une caisse remplie de vapeurs de chlorhydrate d'ammoniaque et offrant une ouverture circulaire à la paroi opposée: on les rend visibles en les lançant dans l'alignement d'un faisceau de lumière électrique.

Origine multiple des mouvements gyratoires de l'atmosphère.—Presque toutes les causes générales qui agissent sur le mouvement de l'atmosphère sont des influences gyratoires: une fois le mouvement *amorcé*, il continue de lui-même et va parfois en s'exagérant. En premier lieu on doit citer le mouvement de rotation de la terre qui apporte toujours une petite composante de rotation pour un déplacement d'une masse gazeuse en *latitude* ou en *altitude.* En second lieu et comme cause décisive, la chaleur solaire, qui échauffe l'air près du sol ou près des nuages: comme la force ascensionnelle du gaz échauffé ne peut pas être uniforme sur toute la surface exposée au rayonnement du soleil (tant à cause de la nature du sol que de son relief), il y a rupture d'équilibre en certains points et des colonnes gazeuses tendent à s'élever. On se trouve ainsi dans le cas des jets cités plus haut et par conséquent dans les circonstances favorables à des gyrations autour d'axes horizontaux. Une fois la gyration établie, les causes qui l'ont déterminées, l'entretiennent et l'augmentent.

L'existence de tourbillons à axe horizontal dans les orages à grêles (en particulier dans celui du 20 mai 1893, à Pittsburg), a été observée par un météorologiste américain, Mr. Frank W. Very, et lui a fourni une très ingénieuse explication de la formation de la grêle. En effet, un tel tourbillon, s'il a des dimensions suffisantes, transporte l'air chaud et humide de la surface du sol dans les régions élevées et froides. La vapeur se condense, se congèle et les cristaux de glace sont entraînés dans le mouvement gyratoire : ils montent et descendent alternativement en suivant les spirales du tourbillon et s'accroissent à chaque passage dans les régions inférieures chargées d'humidité. Cette explication rend compte de toutes les particularités qu'on observe dans la chute des grêlons ; structure zônée, température très basse ; bruit spécial avant la chute ; manifestations électriques qui les accompagnent, car le tourbillon de grêle est une véritable machine électrique à influence, une sorte de *replenisher*.

Reproduction artificielle des phénomènes gyratoires naturels.—Les phénomènes produits par la rotation rapide de l'air sont tout à fait imprévus par la singularité des forces mises en jeu. Les lois ordinaires de la mécanique auxquelles l'expérience journalière nous a accoutumés, paraissent entièrement différentes de celles auxquelles semblent obéir les mouvements tourbillonnaires ; et cela ne doit point nous étonner. Nous avons réduit la mécanique à ses éléments les plus simples ; le point matériel, la force constante, le mouvement rectiligne : grâce a ces simplifications nous avons bien saisi le mouvement de projectiles sphériques, celui d'un pendule, la rotation d'un volant, etc. Mais, dès que le corps solide devient complexe comme forme, lorsque le mouvement qu'il peut prendre comporte à la fois une translation et une rotation, notre imagination se le représente mal ; si, à cette complication de forme, se joint la résistance du milieu ambiant, alors nous n'avons plus aucune idée de l'effet résultant probable ; témoin le *boumerang*. Quant aux mouvements des fluides, ils sont pour nous si difficiles à prévoir que nous sommes toujours surpris lorsque nous manœuvrons un vase plein d'eau ; dès que la masse de liquide est un peu considérable les mouvements tumultueux que nous y faisons naître sans le vouloir nous amènent toujours à faire quelque maladresse.

On conçoit alors dans quelle impossibilité nous nous trouvons pour prévoir les mouvements de l'atmosphère dont la masse est immense, car chaque mètre cube pèse 1300 gr. ; si l'énergie dépensée pour mettre en mouvement de pareilles masses est considérable, inversement la stabilité du régime est énorme, puisqu'il faut attendre la dissipation de cette énergie par les résistances passives, presque toujours réduites aux frottements sur la surface terrestre.

Nous ne chercherons donc pas à analyser les forces mises en jeu dans les mouvements gyratoires de l'air ; je me bornerai à répéter devant vous quelques-unes des belles expériences de M. Ch. Weyher, lequel a bien voulu venir lui-même me prêter son concours et disposer les appareils placés en ce moment sous vos yeux.

Voici une sphère composée de dix palettes circulaires, mise en rotation rapide autour de l'axe A B (Fig. 4): l'air entraîné dans la rotation produit un mouvement tourbillonnaire général, symétrique par rapport au plan de l'équateur. De tous les côtés l'air est aspiré par la sphère tournante ainsi qu'on en peut juger par les fumées ou les fragments de papier qu'on en approche. Cet air est expulsé sur la circonférence équatoriale et seulement dans le plan presque mathématique de cette circonférence; voyez, en effet, ces couronnes de papier qui se maintiennent concentriquement à l'équateur suivant une disposition qui rappelle l'anneau de Saturne: la tension du papier et son frémissement montrent bien que c'est la répulsion par le souffle équatorial qui les maintient.

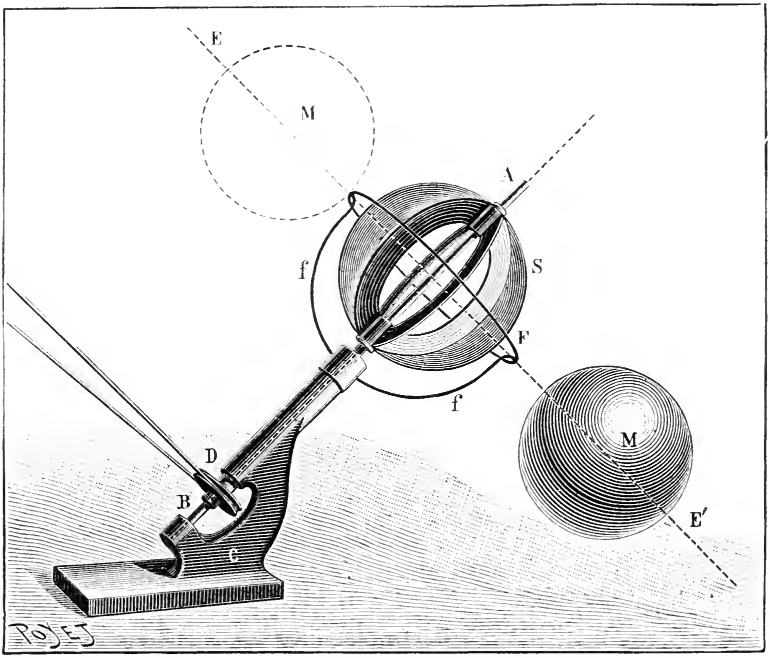

Fig. 4.

On pourrait croire dès lors que cette sphère tournante ne peut produire que des répulsions équatoriales; mais la complexité des filets tourbillonnaires déjoue les prévisions les plus évidentes. Approchons un léger ballon à une petite distance de la sphère, il est vivement attiré et se mit à tourner avec rapidité autour de la sphère motrice dans le plan équatorial: un second, un troisième lancés de la même manière le suivent avec des vitesses diverses et représentent des satellites ; la figuration planétaire est donc complète.

Ce paradoxe d'une répulsion transformée en attraction par le changement de forme du corps présenté, se résout aisément en considérant la résultante des actions aspirantes et répulsives sur la surface du mobile. Sur la plus grande étendue angulaire autour de la sphère tournante c'est l'attraction tourbillonnaire qui domine. On le prouve aisément en plaçant au-dessous de cette sphère un bassin plein d'eau chaude ; si l'air de la salle est bien calme on voit peu à peu la vapeur s'agréger en un filet tourbillonnant depuis la surface de l'eau jusqu'à la sphère tournante. C'est l'image d'une trombe marine. L'importance de ce phénomène a conduit Mr. Weyher à le reproduire sous une forme plus frappante et en mettant en jeu une quantité d'énergie mécanique beaucoup plus considérable, rappelant mieux celle qui constitue ce phénomène naturel.

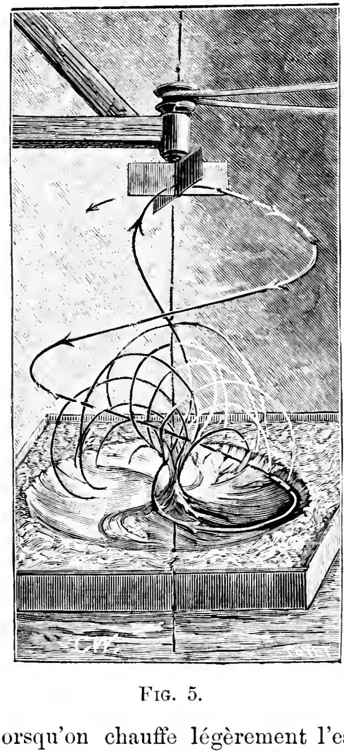

Fig. 5.

L'excitation du mouvement gyratoire (qui, dans la nature, a sa source aux régions supérieures de l'atmosphère) est produite par un moulinet placé à 3 mètres au-dessus d'un réservoir d'eau de 4 mètres de diamètre (Fig. 5). Lorsqu'on met en rotation le moulinet (400 à 500 tours par minute), le tourbillon aérien gagne peu à peu la surface de l'eau qu'on voit s'agiter en formant des spirales *centripètes* et en produisant un cône liquide de plusieurs centimètres de hauteur : au-dessus de ce cône s'épanouit une gerbe de gouttelettes qui retombent en tourbillonnant. Cette attraction à distance est encore plus frappante lorsqu'on chauffe légèrement l'eau ; la vapeur forme alors un tube *creux* dont on distingue la partie vide par sa teinte sombre et sa régularité géométrique ; il s'élance de la surface de l'eau vers le moulinet en soulevant les objets légers, comme des brins de paille, flottant sur le liquide.

Telle est l'expérience faite en plein air dans la grande usine de la Société Weyher et Richmond en 1887. Avec l'appareil réduit placé sous vos yeux (Fig. 6) nous pouvons la répéter dans des conditions aussi concluantes. Le moulinet à palettes est placé au haut de cette caisse de 2 mètres de hauteur fermée d'un côté par une glace : l'eau, légèrement chauffée et contenant un peu de savon, est placée dans un bassin au fond de la caisse. Je mets le moulinet en marche, vous voyez aussitôt l'agitation de l'eau se produire, les bulles de savon se précipiter autour du pied d'une colonne de vapeur : bientôt la colonne prend la forme décrite ci-dessus et présente exactement l'aspect des trombes naturelles ; au bas, le *buisson*, c'est à dire cette gerbe de bulles et de gouttelettes ; en haut, la forme évasée du tube creux de vapeur. Un léger ballon placé à la surface de l'eau est d'abord amené au centre et rendu captif à son pied ; en accélérant la rotation (ce qui augmente la puissance du tourbillon) le ballon est enlevé par la trombe dont il suit le fuseau quelquefois sur toute sa hauteur.

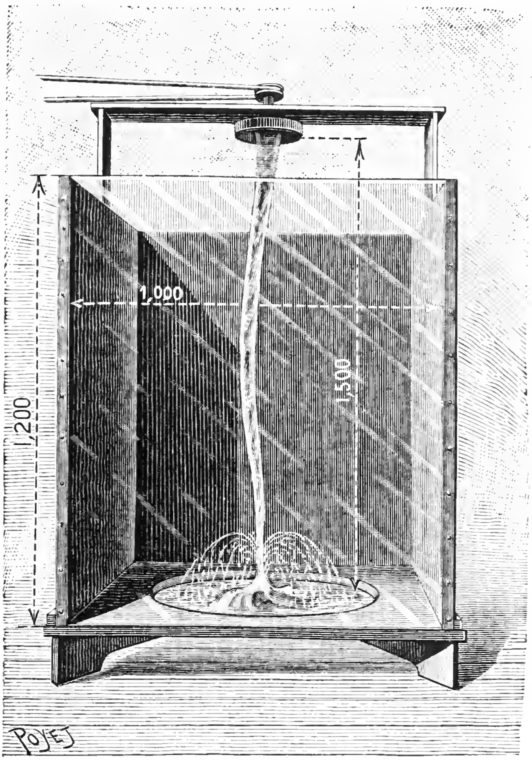

Fig. 6.

Le mouvement hélicoïdal de ce ballon léger, de même que l'aspect du fuseau nébuleux, montrent bien la constitution de la trombe ; on reconnaît des enroulements superposés de veines hélicoïdales, allant les unes en *montant*, les autres en *descendant*: c'est un va et vient perpétuel entre le moulinet supérieur et la surface de l'eau. Comme toutes les veines tournent dans un même sens, si celles qui montent décrivent des *hélices à droite*, celles qui descendent décrivent des *hélices à gauche.* C'est faute d'avoir reconnu ce double mouvement d'ascension et de descente que s'éternise le malentendu entre les partisans des trombes ascendantes et ceux qui soutiennent qu'elles sont seulement descendantes.

Le mouvement ascensionnel des ballons légers entraînés par la trombe montre bien les vitesses ascendantes; il est plus difficile de mettre en évidence la région descendante, invoquée dans certaines théories comme existant exclusivement, parce qu'elle occupe dans l'expérience réduite un espace très petit; elle est confinée dans l'intérieur même de la gaine nébuleuse, qui par sa couleur sombre en dessine le vide central : je vais pourtant vous la montrer à l'aide d'un artifice très simple. Portons au haut de la trombe un corps émettant de la fumée ; nous voyons aussitôt cette fumée aspirée, gagner l'intérieur de la gaine, s'enrouler en un cône effilé et descendre vers la surface de l'eau. C'est exactement ce qu'on voit dans la nature lorsque, dans une trombe marine, les nuages descendent sous forme d'un fuseau qui vient se greffer au centre du buisson formé par l'eau à la surface de la mer bouillonnante. Ce fuseau, c'est la partie pour ainsi dire inoffensive de la trombe; la partie terrible est invisible, elle est formée par la gaine d'air qui tourbillonne autour de ce fuseau. Dans l'expérience mise sous vos yeux c'est l'inverse ; la gaine tourbillonnaire est bien visible grâce à la vapeur qu'on lui fournit, le fuseau intérieur reste sombre ; c'est par l'introduction de la fumée qu'on en vérifie l'existence et la forme.

Il resterait à vous montrer qu'on peut avec un dispositif analogue, reproduire un cyclone avec toutes ses particularités; variations de pression au passage du météore, minimum barométrique, calme central, brusque saute de vent, œil de la tempête, etc., c'est ce que réalise aussi M. Weyher. Mais le temps nous manque; les expériences que vous venez de voir suffiront, je l'espère, à vous montrer combien ces synthèses expérimentales sont complètes et comment elles reproduisent dans les moindres détails les phénomènes naturels.

Je conclurai en vous faisant simplement remarquer combien la météorologie gagne en ampleur et en certitude à devenir une science expérimentale.

[A. C.]

GENERAL MONTHLY MEETING,

Monday, June 10, 1895.

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

Benjamin Bennett, Esq.
Mrs. Henry Burton Buckley,
William Watson Cheyne, Esq. M.B. F.R.S. F.R.C.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 at Low Temperatures:—

The Right Hon. Lord Playfair £10

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—Local Particulars of the Total Eclipse of the Sun, 1898, Jan. 21, 22. (Nautical Almanac Circular, No. 16.) 8vo. 1895.
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Brewers' Journal for May, 1895. 4to.
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Electrical Engineer for May, 1895. fol.
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Electric Plant for May, 1895. 8vo.
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Engineering for May, 1895. fol.
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Industries and Iron for May, 1895. fol.
Iron and Coal Trades Review for May, 1895. 8vo.
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GENERAL MONTHLY MEETING,

Monday, July 1, 1895.

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

Sidney Crompton, Esq.
Walter Daniel Cronin, Esq.
Lady Evans,
Miss S. Rose-Innes,
Augustus Frederick Montagu Spalding, 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 Lords of the Admiralty—Report of Astronomer Royal to Board of Visitors, June 1, 1895. 8vo.

The Secretary of State for India—Annual Progress Report of Archæological Survey Circle. 8vo. 1894.

The Governor-General of India—Geological Survey of India: Records, Vol. XXVIII. Part 2. 8vo. 1895.

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  Chemical News for June, 1895. 4to.
  Chemist and Druggist for June, 1895. 8vo.
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  Electrical Engineering for June, 1895. 8vo.
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  Monist for June, 1895. 8vo.
  Nature for June, 1895. 4to.
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Pharmaceutical Society of Great Britain—Journal for June, 1895. 8vo.

Philadelphia Geographical Club—Bulletin, Vol. I. No. 5. 8vo. 1895.

Physical Society of London—Proceedings, Vol. XIII. Part 7. 8vo. 1895.

Rochechouart, Société des Amis des Sciences et Arts—Bulletin, Tome IV. Nos. 4–6. 8vo. 1894–5.

Rome, Ministry of Public Works—Giornale del Genio Civile, 1895, Fasc. 3°, 4°. And Designi. fol.

Royal Society of London—Proceedings, No. 346. 8vo. 1894.

Selborne Society—Nature Notes for June, 1895. 8vo.

Society of Arts—Journal for June, 1895. 8vo.

Stoney, G. Johnstone, Esq. D.Sc. F.R.S. M.R.I. (the Author)—The Compulsory Retirement of Professors who are Civil Servants. 8vo. 1895.

St. Pétersbourg, Académie Impériale des Sciences—Bulletin, V^{e} Série, Tome II. No. 4. 8vo. 1895.

Toulouse: Société Archéologique du Midi de la France—
Mémoires, Tome XV. Livraison 1er. 4to. 1894.

United Service Institution, Royal—Journal, No. 208. 8vo. 1895.

United States Patent Office—Official Gazette, Vol. LXXI. Nos. 6, 7. 8vo. 1895.

U.S. Department of Agriculture—Experiment Station Record, Vols. I.–VI.
Insect Life, Vols. I.–VI.
North American Fauna, Nos. 2–8.
Division of Chemistry, Bulletins.
Office of Experiment Station, Bulletins.
Division of Botany, Bulletins.
Division of Vegetable Pathology, Bulletins.
Division of Statistics, Bulletins.
Division of Economic Ornithology, Bulletins.

Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1895: Heft 5. 4to. 1895.

Vienna, Geological Institute, Imperial—Verhandlungen, 1895, Nos. 4–7. 8vo.

Zoological Society of London—Transactions, Vol. XIII. Part 10. 4to. 1895.
Proceedings, 1895, Part 1. 8vo. 1895.

GENERAL MONTHLY MEETING,

Monday, November 4, 1895.

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

H.R.H. Prince Louis Philippe, Duc d'Orleans,
Walter Allcroft, Esq.
James Beale, Esq.
Sir John Evans, K.C.B. D.C.L. LL.D. Treas. R.S.
The Hon. Adrian Verney Verney-Cave,

were elected Members of the Royal Institution.

The Managers reported that the late John Bell Sedgwick, Esq. M.R.I. had bequeathed 300*l.* to the Royal Institution in aid of the Fund for the Promotion of Experimental Research at Low Temperatures.

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

Sir Frederick Abel, Bart.	£50

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—Kafiristan and its People. By G. S. Robertson. fol. 1895.

The Lords of the Admiralty—Report of Her Majesty's Astronomer at the Cape of Good Hope for the Year 1895. 4to. 1895.
  Greenwich Spectroscopic and Photographic Results for 1892. 4to. 1894.
  Catalogue of 1713 Stars for the Equinox 1885.0, made at Cape Observatory during 1879 85. 4to. 1894.
  Results of Meridian Observations of Stars made at Cape Observatory from 1885–87. 4to. 1894.
  Greenwich Observations for 1892. 4to. 1894.

Archæological Survey of India—The Moghul Architecture of Fathpur-Sikri, Part 1. By E. W. Smith. 4to. 1894.
  Great Trigonometrical Survey of India, Vol. XXXIV. 4to. 1894.

The British Museum, Natural History—Catalogue of Fishes, Second Edition, Vol. I. 8vo. 1895.

Descriptive Catalogue of the Spiders of Burma. 8vo. 1895.

The Meteorological Office—Report of the First Meeting of the International Meteorological Committee of the Conference at Munich at Upsala in 1894. 8vo. 1895.

Accademia dei Lincei, Reale, Roma—Atti, Serie Quinta: Rendiconti. Classe di Scienze Morali, etc. Vol. IV. Fasc. 4–8. 8vo. 1895.

Classe di Scienze Fisiche, etc. 1^{o} Semestre, Vol. IV. Fasc. 12. 8vo. 1895. 2 Semestre, Vol. IV. Fasc. 1–7.

Agricultural Society of England, Royal—Journal, Vol. VI. Parts 2, 3. 8vo. 1895.

American Association for the Advancement of Science—Proceedings, Brooklyn Meeting, 1894. 8vo. 1895.

American Geographical Society—Bulletin, Vol. XXVII. Nos. 2, 3. 8vo. 1895.

American Philosophical Society—Proceedings, Vol. XXXIV. No. 147. 8vo. 1895.

Amsterdam Royal Academy of Sciences—Verhandelingen, 1 Sectie: Deel 1, Nos. 1–8; Deel 2, Nos. 1–8; Deel 3. Nos. 1–4.

2nd Sectie: Deel 1, Nos. 1–10; Deel 2; Deel 3, Nos. 1–14; Deel 4, Nos. 1–6; 8vo. 1892–95.

Zittingsverslagen, 1892–93; 1893–94; 1894–95. 8vo.

Jahrboek. 1894. 8vo.

Asiatic Society of Great Britain, Royal—Journal for July–October, 1895. 8vo.

Asiatic Society, Royal (Bombay Branch)—Journal, Vol. XIX. No. 51. 8vo. 1895.

Astronomical Society, Royal—Monthly Notices, Vol. LV. No. 8. 8vo. 1895.

Bankers, Institute of—Journal, Vol. XVI. Part 7. 8vo. 1895.

Belgium, Académie Royale de—Bulletins, 3^{e} Série, Tomes XXVI.–XXIX. 8vo. 1893–95.

Mémoires, Tomes XLVII. L. (Part 1), LI. LII. 8vo. 1892–95.

Mémoires, Tomes L. (Part 2), LI. LII. 4to. 1893–94.

Mémoires Couronnés, Tome LIII. 4to. 1894.

Annuaires, 1894 and 1895. 8vo.

Berlin, Royal Prussian Academy of Sciences—Sitzungsberichte, 1895, Nos. 26–38. 8vo.

British Architects, Royal Institute of—Journal, 1894–95, Nos 17, 18.

British Astronomical Association—Journal, Vol. V. Nos. 9, 10. 8vo. 1895.

Memoirs, Vol. III. Part 5. 8vo. 1895.

Cambridge University, Library—Annual Report of the Library Syndicate, 1895. 8vo.

Camera Club—Journal for July–Oct. 1895. 8vo.

Canada, Geological Survey of—Annual Report, New Series, Vol. VI. 1892–93. 8vo. 1895.

Palæozoic Fossils. By J. F. Whiteaves. Vol. III. Part 2. 8vo. 1895.

Chemical Industry, Society of—Journal, Vol. XIV. Nos. 6–9. 8vo. 1895.

Chemical Society—Journal for July–Oct. 1895. 8vo.

Proceedings, No. 154. 8vo. 1895.

Chili, Société Scientifique du—Actes, Tome IV. Livr. 5. 8vo. 1895.

City of London College—Calendar, 1895–96. 8vo.

Civil Engineers, Institution of—Minutes of Proceedings, Vols. CXXI. CXXII. 8vo. 1895.

Catalogue of the Library, 3 vols. 8vo. 1895.

List of Members, &c. 1895. 8vo.

Subject Index to Minutes of Proceedings, Vols. LIX.–CXVIII. 8vo. 1895.

Colonial Institute, Royal—Proceedings, Vol. XXVI. 1894–95. 8vo. 1895.

Congrès International des Américanistes—Programa: Reunion en Mexico, 1895. 8vo.

Cornwall, Polytechnic Society, Royal—Sixty-second Annual Report, 1894. 8vo.
Cracovie, l'Académie des Sciences—Bulletin, 1895, Nos. 6, 7. 8vo.
Cust, Robert Needham, Esq. LL.D.—Classified Catalogue of Books connected with Languages. 8vo. 1895.
Editors—American Journal of Science for July–Oct. 1895. 8vo.
Analyst for July-Oct. 1895. 8vo.
Astrophysical Journal for July-Oct. 1895. 8vo.
Athenæum for July–Oct. 1895. 4to.
Author for July–Oct. 1895.
Ateneo Veneto for 1894. 8vo.
Brewers' Journal for July-Oct. 1895. 4to.
Chemical News for July–Oct. 1895. 4to.
Chemist and Druggist for July-Oct. 1895. 8vo.
Electrical Engineer for July–Oct. 1895. fol.
Electrical Engineering for July–Oct. 1895.
Electrical Review for July-Oct. 1895. 8vo.
Electric Plant for July–Oct. 1895. 8vo.
Engineer for July–Oct. 1895. fol.
Engineering for July–Oct. 1895. fol.
Horological Journal for July–Oct. 1895. 8vo.
Industries and Iron for July-Oct. 1895. fol.
Law Journal for July-Oct. 1895. 8vo.
Machinery Market for July–Oct. 1895. 8vo.
Monist for July 1895. 8vo.
Nature for July–Oct. 1895. 4to.
Nuovo Cimento for July–Oct. 1895. 8vo.
Open Court for July–Oct. 1895. 4to.
Optician for July–Oct. 1895. 8vo.
Photographic Work for July–Oct. 1895. 8vo.
Physical Review, Vol. II. No. 7; Vol. III. Nos. 1, 2. 1895.
Scots Magazine for July-Oct. 1895. 8vo.
Technical World for July–Oct. 1895. 8vo.
Transport for July–Oct, 1895. fol.
Tropical Agriculturist for July-Oct. 1895. 8vo.
Work for July–Oct. 1895. 8vo.
Zoophilist for July–Oct. 1895. 4to.
Electrical Engineers, Institution of—Journal, Vol. XXIV. No. 118. 8vo. 1895.
Firenze, Reale Accademia dei Georgofili—Atti, Vol. XVIII. Disp. 2_a. 8vo. 1895.
Florence, Biblioteca Nazionale Centrale—Bollettino, Nos. 228–36. 8vo. 1895.
Franklin Institute—Journal, Nos. 836–8. 8vo. 1895.
Geographical Society, Royal—Geographical Journal for July–Oct. 1895. 8vo.
Geological Society—Quarterly Journal, Nos. 203, 204. 8vo. 1895.
Harlem, Société Hollandaise des Sciences—Archives Néerlandaises, Tome XXIX. Livr. 2-3. 8vo. 1895.
Harvard Unversity, U.S.A.—Annals of the Astronomical Observatory of Harvard College: Vol. XI. Parts 1, 2; Vol. XII.; Vol. XIII. Parts 1, 2; Vol. XIV. Parts 1, 2; Vol. XV. Parts 1, 2; Vols. XVI. XVII. XVIII. XIX. Parts 1, 2; Vol. XX. Parts 1, 2; Vol. XXI. Parts 1, 2; Vols. XXII. XXIII. XXIV. XXV XXVI. Part 1; Vols. XXVII. XXIX. XXX. Parts 1-3; Vol. XXXI. Parts 1, 2; Vol. XXXII. Part 2; Vol. XXXIV. Part 1; Vols. XXXV. XL. Parts 1-3; Vol. XLI. Nos. 1, 2. 4to. 1879–94.
Hopkins University—University Studies: Thirteenth Series, Nos. 6–10. 8vo. 1895.
American Chemical Journal, Vol. XVII. No. 8. 8vo. 1895.
University Circulars, Nos. 2, 18, 26, 33, 85, 116, 120. 4to. 1880–95.
Horticultural Society, Royal—Journal, Vol. XIX. Part 1. 8vo. 1895.
Hoyle, William S. Esq. (*The Compiler*)—A Catalogue of the Books and Pamphlets in the Library of the Manchester Museum, Owens College. 8vo. 1895.

Illinois State Laboratory of Natural History—Reports, Bulletins, &c. 8vo. 1884 *et seq.*

Imperial Institute—Supplement to the Year Book. 8vo. 1895.

Iron and Steel Institute—Journal, Vol. XLVII. 1895, No. 1. 8vo.

Jervis, M. C. Chev. G.—New Cycloidal Projection devised by T. B. Jervis. fol. 1895.

Junior Engineering Society—Record of Transactions, Vol. IV. 8vo. 1895.

Keeler, James E. Esq. (The Author)—A Spectroscopic Proof of the Meteoric Constitution of Saturn's Rings. 8vo. 1895.

Conditions affecting the Form of Lines in the Spectrum of Saturn. 8vo. 1895.

Linnean Society—Transactions, Vol. IV. Part 2; Vol. V. Part 1; Vol. VI. Part 3. 4to. 1894–95.

Journal, No. 161. 8vo. 1895.

London County Council Technical Education Board—London Technical Education Gazette, July–Oct. 8vo. 1895.

Manchester Geological Society—Transactions, Vol. XXIII. Parts 8, 9. 8vo. 1895.

Manchester Literary and Philosophical Society—Memoirs and Proceedings, Vol. IX. No. 6. 8vo. 1894–5.

Massachusetts Institute of Technology—Technological Quarterly and Proceedings of the Society of Arts, Vol. VIII. No. 1. 8vo. 1895.

Mechanical Engineers, Institution of—Proceedings, 1895. Nos. 1, 2. 8vo.

Medical and Chirurgical Society, Royal—Medico-Chirurgical Transactions, Vol. LXXVIII. 8vo. 1895.

Meteorological Society, Royal—Quarterly Journal, No. 95. 8vo. 1895.

Meteorological Record, No. 56. 8vo. 1895.

Mexican Minister, The—Latest Correspondence between Mexico and Guatemala. 8vo. 1895.

Microscopical Society, Royal—Journal, 1895, Part 5. 8vo.

Munich, Bavarian Academy of Sciences, Royal—Sitzungsberichte, 1895: Heft 2. 8vo. 1895.

Musical Association—Proceedings, 1894–95. 8vo. 1895.

New Jersey, Geological Survey of—Report on Water Supply, Water Power, The Flow of Streams, and Attendant Phenomena. By C. C. Vermeule. 8vo. 1894.

Annual Report of the State Geologist for 1893. 8vo. 1894.

New South Wales Government—Statistical Register for 1893 and previous years. 8vo. 1894.

Annual Report. fol. 1895.

North of England Institute of Mining and Mechanical Engineers—Transactions, Vol. XLIV. Part 4. 8vo. 1895.

Report of Proceedings of the Flameless Explosives Committee, Part 1, Section 2. 8vo. 1895.

Nottingham, Borough of—Annual Report of University College, &c. 1894–95. 8vo.

Numismatic Society—Chronicle and Journal, 1895, Part 2. 8vo.

Odontological Society of Great Britain—Transactions, Vol. XXVII. No. 8. 8vo. 1895.

Onnes, Professor H. Kamerlingh—Communication from the Laboratory of Physics at the University of Leiden, Nos. 2, 4, 5, 7–15, 17, 19. 8vo. 1891–95.

Petrie, W. M. F. Esq. D.C.L. (the Author)—Egyptian Decorative Art: a Course of Lectures delivered at the Royal Institution. 8vo. 1895.

Pharmaceutical Society of Great Britain—Journal for July–Oct. 1895. 8vo.

Philadelphia, Academy of Natural Sciences—Proceedings, 1895, Part 1. 8vo.

Photographic Society, Royal—Journal and Transactions, Vol. XIX. Nos. 10, 11. 8vo. 1895; The Photographic Journal, Sept.–Oct. 8vo. 1895.

Physical Society of London—Proceedings, Vol. XIII. Parts 8–11.

Prince, C. Leeson, Esq. M.R.C.S. F.R.A.S. &c. (the Author)—A Literal Translation of the Astronomy and Meteorology of Aratus, with some Bibliographical Remarks. 8vo. 1895. [*Privately printed.*]

Raffard, N. J. Esq. (the Author)—Considérations sur les Phénomènes du Frottement dans les Machines. 8vo. 1895.

Rio de Janeiro, Observatorio—Annuario, 1895. 8vo.

Royal College of Surgeons of England—Calendar. 8vo. 1895.

Royal Engineers' Institute, Chatham—Professional Papers of the Corps of Royal Engineers. Vol. XX. 1894. 8vo. 1894.

Royal Society of Canada—Transactions for 1894. 4to. 1895.

Royal Society of Edinburgh—Proceedings, Vol. XX. (pp. 385-480). 8vo. 1895.
Transactions, Vol. XXXVII. Part 4. 4to. 1895.

Royal Society of London—Philosophical Transactions, Vol. CLXXXVI.: B, Part I. 4to. 1895.
Proceedings, Nos. 347-51. 8vo. 1895.

Sanitary Institute—Journal, Vol. XVI. Parts 2, 3. 8vo. 1895.

Saxon Society of Sciences, Royal—
Mathematisch-Physische Classe—
Berichte, 1895, Nos. 2-4. 8vo. 1895.
Philologisch-Historische Classe—
Berichte, 1895, Nos. 1, 2. 8vo.

Selborne Society—Nature Notes for July-Oct. 1895. 8vo.

Siemens, Alexander, Esq. M.R.I. &c.—Works of Werner von Siemens, Vol. II. 8vo. 1895.

Smithsonian Institution—
Bureau of Ethnology—
Chinook Texts. 8vo. 1894.
James and Potomac Valleys. 8vo. 1894.
The Siouan Tribes of the East. 8vo. 1884.

Society of Architects—Journal for July-Oct. 1895. 8vo.

Society of Arts—Journal for July-Oct. 1895. 8vo.

Stanley, W. F. Esq. (the Author)—Notes on the Nebular Theory in Relation to Stellar, Solar, Planetary, Cometary, and Geological Phenomena. 8vo. 1895.

Statistical Society, Royal—Journal, Vol. LVIII. Parts 2, 3. 8vo. 1895.

Stockholm, Royal Swedish Academy of Sciences—Öfversigt, Vol. LI. 8vo. 1895.
Handlingar, Band XXVI. 4to. 1894-95.

Tacchini, Prof. P. Hon. Mem. R.I. (the Author)—Memorie della Società degli Spettroscopisti Italiani, Vol. XXIV. Disp. 6. 4to. 1895.

Tallack, William, Esq. (the Author)—Penological and Preventive Principles, 2nd Edition. 8vo. 1895.

Teyler Museum, Haarlem—Archives, Série II. Vol. IV. Part 4. 8vo. 1895.

Toronto, University of—The University of Toronto Quarterly, Vol. I. Nos. 1, 2. 8vo. 1895.

United Service Institution, Royal—Journal, No. 209-12. 8vo. 1895.

United States Department of Agriculture—Monthly Weather Review for February 1895. 4to.

United States Department of the Interior—Report on Indians Taxed and not Taxed in the United States at the Eleventh Census, 1890. 4to. 1894.
Report on Statistics of Churches in the United States at the Eleventh Census, 1890. 4to. 1894.
Report on Population of the United States at the Eleventh Census, 1890, Part 1. 4to. 1895.

United States Geological Survey—Geological Atlas of the United States, Folios 1-6, 8-12. fol. 1894.
Bulletins, Nos. 118-122. 8vo. 1894.
Monographs, Nos. XXIII. XXIV. 4to. 1894.
Fourteenth Annual Report. Parts 1, 2. 4to. 1892-93.

United States Patent Office—Official Gazette, Vol. LXXI. Nos. 1-13. 8vo. 1895.

Universal Publishing Co.—The Evils of Society and their Remedies. By Lady Cook. 8vo. 1895.
University of London—Accessions to the Library, 1886 to June 1895. 8vo. 1895.
Upsal, Royal Society of Sciences—Nova Acta, Ser. III. Vol. XV. Fasc. 2. 4to. 1895.
Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1895: Heft 6-8. 4to. 1895.
Vienna, Geological Institute, Royal—Jahrbuch, Band XLIV. Heft 2-4. 8vo. 1894.
Vienna—Imperial Geological Institute—Verhandlungen, 1895. Nos. 8, 9. 8vo.
Wagner Free Institute of Science—Transactions, Vol. III. Part 3. 8vo. 1895.
Yale University Observatory—Report for 1894-95. 8vo.
Zoological Society of London—Proceedings, 1895, Part 2. 8vo. 1895.

GENERAL MONTHLY MEETING,

Monday, December 2, 1895.

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

Charles Hugh Berners, Esq. J.P.
John Mitchell Bruce, M.D. F.R.C.P.
Francis Chambers, Esq.
Alexander Macomb Chance, Esq. J.P.
Alfred E. Fletcher, Esq. F.I.C.
Francis Fox, Esq. J.P. M.Inst.C.E.
Mrs. S. H. Phillips,
Horace Seymour, Esq.
Kenneth Trevor Stewart, M.D.
George Herbert Strutt, Esq.
Frederick Tendron, Esq. F.G.S.
William H. Warner, Esq.
Matthew Webb, Esq. J.P.

were elected Members of the Royal Institution.

The following Lecture Arrangements were announced:—

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 SOUND, HEARING AND SPEECH (experimentally illustrated). On Dec. 28 (*Saturday*), Dec. 31, 1895; Jan. 2, 4, 7, 9, 1896.

PROFESSOR CHARLES STEWART, M.R.C.S. F.L.S. Fullerian Professor of Physiology, R.I. Eleven Lectures on THE EXTERNAL COVERING OF PLANTS AND ANIMALS: ITS STRUCTURE AND FUNCTIONS. On *Tuesdays*, Jan. 14, 21, 28, Feb. 4, 11, 18, 25, March 3, 10, 17, 24.

PHILIP H. WICKSTEED, Esq. M.A. Four Lectures on DANTE. On *Thursdays*, Jan. 16, 23, 30, Feb. 6.

PROFESSOR H. MARSHALL WARD, D.Sc. F.R.S. F.L.S. Professor of Botany in the University of Cambridge. Three Lectures on SOME ASPECTS OF MODERN BOTANY. On *Thursdays*, Feb. 13, 20, 27.

THE REV. WILLIAM BARRY, D.D. Four Lectures on "MASTERS OF MODERN THOUGHT"—VOLTAIRE, ROUSSEAU, GOETHE, AND SPINOZA. On *Thursdays*, March, 5, 12, 19, 26.

DR. A. DONALDSON SMITH, F.R.G.S. TO THE NORTH OF LAKE RUDOLF AND AMONG THE GALLAS. And

WALTER R. LAWRENCE, Esq. I.C.S. C.I.E. On THE VALLEY OF KASHMIR. Two Lectures on *Saturdays*, Jan. 18, 25.

Professor C. Hubert H. Parry, Mus. Doc. M.A. Professor of Musical History and Composition at the Royal College of Music. Three Lectures on Realism and Idealism in Musical Art (with Musical Illustrations). On *Saturdays*, Feb. 1, 8, 15.

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 Light. On *Saturdays*, Feb. 22, 29, March 7, 14, 21, 28.

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—Classe di Scienze Fisiche, Matematiche e Naturali. Atti, Serie Quinta: Rendiconti. 2° Semestre, Vol. IV. Fasc. 8. 8vo. 1895.
Astronomical Society, Royal—Monthly Notices, Vol. LV. No. 9. 8vo. 1895.
Bandsept, A. Esq. (the Author)—Brûleurs Auto-Melangeurs Atomiseurs. fol. 1895.
Bankers, Institute of—Journal, Vol. XVI. Part 8. 8vo. 1895.
Barker, Captain D. Wilson, F.R.C.S. &c. (the Author)—Clouds and Weather. 8vo. 1895.
The Development of Instrumental Nautical Astronomy and soundings in shallow water. 8vo. 1891.
The Compass. 8vo. 1892.
Notes on Handling Ships. 8vo. 1895.
Cloud Nomenclature. 8vo. 1890.
On the Study of Natural History as a Recreation for Sailors. 8vo. 1894.
British Architects, Royal Institute of—Journal, 3rd Series, Vol. III. No. 2. 4to. 1895.
British Astronomical Association—Journal, Vol. VI. No. 1. 8vo. 1895.
Cambridge Philosophical Society—Proceedings, Vol. VIII. No. 5. 8vo. 1895.
Camera Club—Journal for Nov. 1895. 8vo.
Canada, Geological Survey of—Maps. fol. 1895.
Chemical Industry, Society of—Journal, Vol. XIV. No. 10. 8vo. 1895.
Chemical Society—Journal for Nov. 1895. 8vo.
Proceedings, No. 155. 8vo. 1894–5.
Clinical Society of London—Transactions, Vol. XXVIII. 8vo. 1895.
Devonshire Association—Report and Transactions, Vol. XXVII. 8vo. 1895.
Editors—American Journal of Science for Nov. 1895. 8vo.
Analyst for Nov. 1895. 8vo.
Anthony's Photographic Bulletin for Nov. 1895. 8vo.
Astro-Physical Journal for Nov. 1895. 8vo.
Athenæum for Nov. 1895. 4to.
Brewers' Journal for Nov. 1895. 8vo.
Chemical News for Nov. 1895. 4to.
Chemist and Druggist for Nov. 1895. 8vo.
Electrical Engineer for Nov. 1895. fol.
Electrical Engineering for Nov. 1895. 8vo.
Electrical Review for Nov. 1895. 8vo.
Electric Plant for Nov. 1895. 4to.
Electricity for Nov. 1895. 8vo.
Engineer for Nov. 1895. fol.
Engineering for Nov. 1895. fol.
Engineering Review for Nov. 1895. 8vo.
Homœopathic Review for Nov. 1895. 8vo.
Horological Journal for Nov. 1895. 8vo.
Industries and Iron for Nov. 1895. fol.

Editors—continued.
Invention for Nov. 1895.
Ironmongery for Nov. 1895. 4to.
Law Journal for Nov. 1895. 8vo.
Lightning for Nov. 1895. 8vo.
London Technical Education Gazette for Nov. 1895. 8vo.
Machinery Market for Nov. 1895. 8vo.
Nature for Nov. 1895. 4to.
Nuovo Cimento for Nov. 1895. 8vo.
Photographic News for Nov. 1895. 8vo.
Photographic Work for Nov. 1895. 8vo.
Science Siftings for Nov. 1895.
Scots Magazine for Nov. 1895. 8vo.
Technical World for Nov. 1895. 8vo.
Transport for Nov. 1895. fol.
Tropical Agriculturist for Nov. 1895.
Work for Nov. 1895. 8vo.
Zoophilist for Nov. 1895. 4to.

Florence, Biblioteca Nazionale Centrale—Bolletino, No. 237. 8vo. 1895.

Franklin Institute—Journal, No. 839. 8vo. 1895.

Geographical Society, Royal—Geographical Journal for Nov. 1895. 8vo.

Historical Society, Royal—Transactions, New Series, Vol. IX. 8vo. 1895.

Horticultural Society, Royal—Journal, Vol. XIX. Part 2. 8vo. 1895.

Imperial Institute—Imperial Institute Journal for Nov. 1895.

Johns Hopkins University—American Chemical Journal, Vol. XVII. No. 9. 8vo. 1895.
University Circulars, No. 121. 8vo. 1895.

Kuhl, W. H. Esq. (the Publisher)—Æronautische Bibliographie, 1670–1895. 8vo.

Linnean Society—Journal, Nos. 212–213. 8vo. 1895.
List of Members, &c. 1895–6. 8vo.

Manchester Free Libraries Committee—Forty-third Annual Report, 1894–5. 8vo.

Manchester Steam Users' Association—M.S.U.A. Memorandum, 1895: Economic Tests of Boilers and Engines, &c. 8vo. 1895.

Massachusetts Institute of Technology—Technology Quarterly and Proceedings of the Society of Arts, Vol. VIII. No. 2. 8vo. 1895.

Numismatic Society—Chronicle and Journal, 1895, Part 3. 8vo.

Odontological Society of Great Britain—Transactions, Vol. XXVIII. No. 1. 8vo. 1895.

Pascoe, C. E. Esq. (the Editor)—London of To-Day's Calendar for Nov. 1895. 8vo.

Perthshire Society of Natural Science—The Natural History of the Banks of the Tay. 8vo. 1895.

Pharmaceutical Society of Great Britain—Journal for Nov. 1895. 8vo.

Rome, Ministry of Public Works—Giornale del Genio Civile, 1895, Fasc. 7°. And Designi. fol.

Rotterdam, Société Batave de Philosophie Expérimental de—Programme, 1895. 8vo.

Royal Society of London—Proceedings, No. 352. 8vo. 1894.
Philosophical Transactions, Vol. CLXXXVI. A. Part 1. 4to. 1895.

Saxon Society of Sciences, Royal—Mathematisch-Physische Classe, Berichte, 1895, No. 3. 8vo.

Selborne Society—Nature Notes for Nov. 1895. 8vo.

Society of Apothecaries of London—Calendar, 1895. 8vo.

Society of Arts—Journal for Nov. 1895. 8vo.

St. Bartholomew's Hospital—Statistical Tables for 1894. 8vo. 1895.

St. Pétersbourg, Académie Impériale des Sciences—Bulletin, V^{e} Série, Tome II. No. 5. 8vo. 1895.

Tacchini, Professor P. Hon. Mem. R.I. (the Author)—Memorie della Società degli Spettroscopisti Italiani, Vol. XXIV. Disp. 8. 8vo. 1895.

United Service Institution, Royal—Journal, No. 213. 8vo. 1895.

United States Department of Agriculture—Monthly Weather Review for April–May, 1895. 8vo.

Climate and Health, No. 2. 8vo. 1895.

Division of Ornithology and Mammalogy, Bulletin, No. 6. 8vo. 1895.

Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1895: Heft 9. 4to. 1895.

Vincent, Benjamin, Esq. Hon. Librarian R.I. (*the Editor*)—Haydn's Dictionary of Dates, Twenty-first Edition. 8vo. 1895.

WEEKLY EVENING MEETING,

Friday, January 18, 1895.

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

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

Phosphorescence and Photographic Action at the Temperature of Boiling Liquid Air.

[Abstract.]

Continued investigation of the properties of matter at extremely low temperatures has resulted in a considerable addition to our knowledge on this subject, more especially in regard to phosphorescence and photographic action. Phosphorescence and fluorescence are terms applied to similar phenomena which apparently differ only in degree, the first being practically an instantaneous effect, while the other lasts for a relatively long period after the withdrawal of the light stimulus. In all cases the luminous effects called phosphorescence and fluorescence belong to a less refrangible part of the spectrum than the exciting rays. Professor Stokes has shown that the singular surface appearance observed in fluorescent liquids is due to a change of refrangibility of the light absorbed, and again given off by their upper layers. Phosphorescence may be regarded as a kind of fluorescence which lasts a long time after the excitation has ceased, and may be briefly defined as the phenomena observed when certain substances give out light through the transformation of absorbed vibrations of shorter period. This must not be confused with the luminosity due to the slow oxidation of phosphorus; nor with the "phosphorescent" appearance accompanying the slow combustion of decaying animal and vegetable matter; nor with the more or less voluntary display of light by fireflies, glow-worms and small marine animals. The researches of Becquerel showed that the intensity of phosphorescence depended directly on the product of the intensity of the stimulating light, and a factor of absorption, and inversely, as some coefficient representing molecular friction or damping. When phosphorescing sulphides of calcium are heated they increase in their light emission, whereas if cooled to − 80° they cease altogether to be luminous, and if maintained at this low temperature for hours, keep a latent store of light energy that may be again evolved on allowing the sulphide to rise to the ordinary temperature.

But while the temperature of − 80° is sufficient to stop all sensible emission from previously-excited sulphide, it does not prevent an unexcited sulphide from "absorbing" light energy that can be evolved at higher temperature. By means of liquid air we can now cool substances to temperatures ranging from – 180° to – 200°. Under such conditions all known organic compounds are solids, and this condition of matter is specially favourable to phosphorescent phenomena. Moreover, the list of truly phosphorescent bodies has been greatly extended, and knowledge of their peculiarities in this direction increased.

The effect of temperature on phosphorescence is easy to observe by taking two portions of the same substance placed in similar very thin test tubes, cooling one of the specimens in liquid air, and then quickly exposing both samples side by side to the same light stimulation. The form of apparatus used is shown in Fig. 1. A is a powerful electric lamp in a lantern, the latter carrying a fitting whereby the light is screened from the eye of the observer. E is a double vacuum vessel containing liquid air or oxygen. The substance to be examined is plunged below the surface of the liquid air, as at F. When it is thoroughly cooled, it is withdrawn from the liquid and exposed for a few seconds to the full light of the arc in the horizontal part of the tube. It is then quickly withdrawn and examined for phosphorescence in the darkened room. The fitting B is another modification of the apparatus for experimental purposes, and consists of a slit and suitable lens and prism, whereby the spectrum can be thrown on to a table, as at D C, and cooled bodies examined in various parts of the spectrum. If, during the light excitation caused by burning magnesium or a flash of the electric light, the eyes are carefully covered, then the comparative phosphorescence, if any, of the cooled and uncooled substance can be observed. In this mode of working the action of the very short wave-lengths of light are stopped by the opacity of glass, but the solid condition of all substances at the low temperature enables

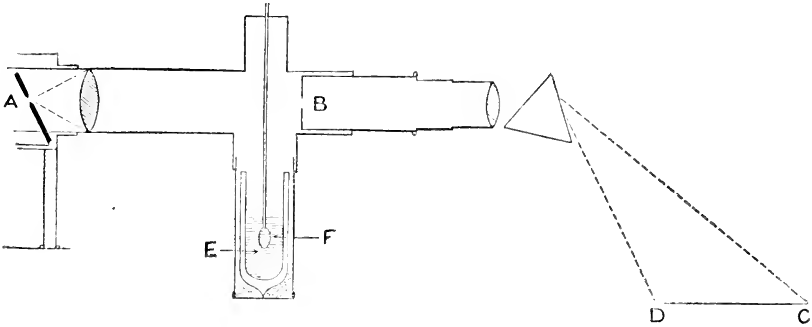

Fig. 1.

the use of glass to be abandoned when necessary. As a general rule it may be stated that the great majority of substances exhibiting feeble phosphorescence at ordinary temperature, become markedly more active at these very low temperatures. Thus gelatin, celluloid, paraffine, ivory, horn, and india-rubber become distinctly luminous, with a bluish or greenish phosphorescence, after cooling to −180° and being stimulated by the electric light. Hydroquinone was more luminous than the isomeric resorcinol or pyrocatechol, and in the same way pyrogallol was faint compared with phloroglucol. All alkaloids forming fluorescent solutions become phosphorescent at low temperatures. The hydrocarbons, alcohols, acids and ethers of the fatty series are all more or less active, and glycerin, sulphuric and nitric acids are all very bright, so also are concentrated hydrochloric acid and strong ammonia solution. Coloured salts generally show little activity, but a large number of colourless salts are very luminous. Water when pure is only feebly phosphorescent, but remarkably so when impure. Acetic acid and acetamide appeared fairly equal in luminosity; hippuric acid was very fine, as were most substances containing a ketone group. Lithium platinocyanide changed from white to red on cooling, and was excelled in phosphorescing power by yellow ammonium platinocyanide, which was exceedingly bright.

Definite organic substances possessing exceptional powers of phosphorescence when stimulated at − 180° C., are acetophenone, benzophenone, asparagin, hippuric acid, phthalic anhydride, urea, creatine, urethane, succinimide, triphenyl methane, diphenyl, salicylic acid, glycogen, aldehyde-ammonia, &c. It will require long and laborious experiments, however, to measure the relative brightness of the phosphorescence of bodies belonging to definite series.

Remarkable results were obtained with an egg-shell and a feather respectively. The egg shone brilliantly as a globe of blue light, and the feather was equally brilliant, its outline showing clearly in the darkened room. Other organic substances giving good results were cotton-wool, paper, leather, linen, tortoiseshell, and sponge, all phosphorescing brightly, as did also a white flower, a cultivated species of *Dianthus*. Coloured glasses and papers as a rule exhibit no phosphorescence, and when the alcohols are coloured by the addition of a trace of iodine, the luminous effect is destroyed. Milk was shown to be highly phosphorescent and much brighter than water. The white of egg has greater phosphorescing power than the yolk, white substances generally being superior in this respect to coloured ones. On cooling a layer of white of egg on the outside of a test tube to −190°, and then exposing it to a flash of the electric arc, the brilliancy of the phosphorescent light is very striking. The chloro-, bromo-, iodo-, sulpho-, and nitro-compounds, as a rule, show nothing, or are but faintly luminous. Amongst basic bodies nicotine is more luminous than quinoline or pyridine. Metals also phosphoresce, but in this case the action is due to some organic film deposited from the air, because it disappears on ignition. If the metal is subsequently touched, the phosphorescence re-appears.

So far as the examination has been carried, the two most remarkable classes of substance for phosphorescence are the platinocyanides amongst inorganic compounds, and the ketonic compounds, like acetophenone and ethyl phenyl ketone, and others of the same type, amongst organic. When ammonium platinocyanide is cooled with liquid air and maintained at this temperature by being immersed in the liquid while stimulated by exposure to a beam of the electric arc, it continues to glow in the dark with a feeble emission as long as the temperature is kept about −180°. On pouring off, however, the liquid air from the crystals so that the temperature may rise, then the interior of the test tube glows like a lamp from the sudden increase of light emissivity as the temperature rises. It seems clear from this experiment that similar initial light intensities being used for stimulating, the substance at this low temperature must have acquired increased power of absorption, and it may be that at the same time the factor of molecular friction or damping may have diminished. That the absorptive power of substances for light is greatly changed at low temperatures is proved by the change of colour in substances like oxide, iodide, and sulphide of mercury, chromic acid, &c., when cooled. Many quantitative photometric measurements must be made before the actual changes taking place in the conditions governing the phenomena can be definitely stated.

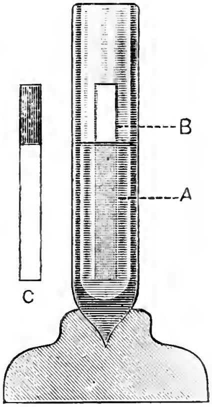

FIG. 2.

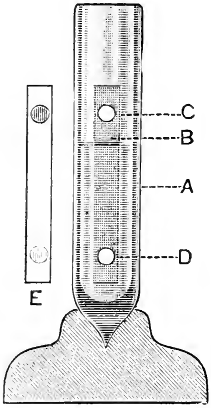

FIG. 3.

Along with these experiments on phosphorescence, a number of photographs have been taken at − 180°, using various sensitive plates and films, and these have been compared with similar photographs taken at the same time under similar conditions at the ordinary temperature. The first plan (Fig. 2) was simply to immerse a strip of sensitive bromide paper A B, in one of the vacuum vessels containing liquid oxygen, and when the part immersed had been thoroughly cooled down, exposing it to the light of a piece of burning magnesium. The paper was then developed, when a result something resembling C was obtained. The part which had been cooled by the liquid oxygen, as at A, was untouched by the light, whereas the portion of paper above the liquid at B developed up quite black. Further modifications were made in Fig. 3, where the strip of sensitive film E was enclosed in a cover of sheet lead B, having two small discs cut away as at C and D. The strip was then cooled in liquid oxygen A, and then exposed to a flash of burning magnesium. After development, the strip appeared something like E, when again the action of the light was considerably diminished on the part of the film which had been cooled. In Fig. 4 a form of apparatus was adopted whereby the exposures were made without the disadvantage of the light passing through the glass sides of the vacuum vessels. B and E were vacuum vessels enclosed in a blackened box; into B a quantity of liquid oxygen was poured. The sensitive plate or film was then lowered so as just to touch the surface of the liquid as at C. D was a comparison plate exposed at the same time and at the same distance from the source of light A, only the comparison plate

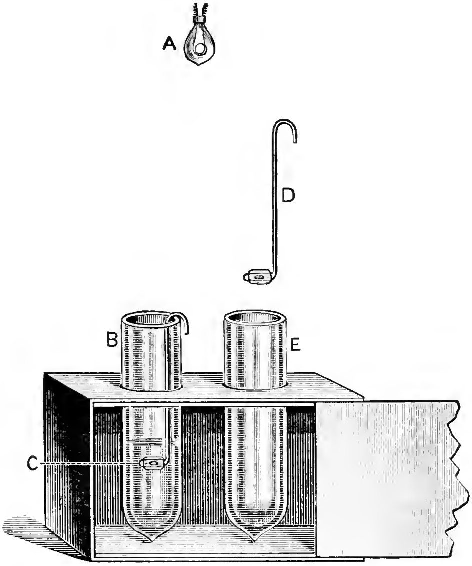

FIG. 4.

was taken at ordinary temperature. By immersing the photographic plate to different depths in the liquid oxygen or air, the comparative opacities of the liquids could be observed. After exposure, the two plates were removed from the supports and developed together in the same solution, when similar results were obtained as in the previous experiments. The photographs have been examined by Captain Abney, who reports that the photographic action has been reduced by 80 per cent. at the temperature of − 180°. If the photographic action is brought about by a chemical change, then it appears to be the only one that can be traced under such conditions, as substances having the most powerful affinities have no action on each other, and all voltaic combinations cease to give a current at such low temperatures. It is certain that the Eastman film, cooled to −200° by the evaporation of air *in vacuo* is still fairly sensitive to photographic action. Much further work, however, will be required to reach a definite conclusion as to what is taking place when substances sensitive to photographic action are subjected to such conditions of temperature.

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