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> "Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Polit.* lib. i. cap. 1. Not.

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"Meditationis est perscrutari occulta; contemplationis est admirari perspicua . . . . . Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem."—*Hugo de S. Victore.*

—"Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condat,
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu."

J. B. Pinelli ad Mazonium.

CONTENTS OF VOL. X.

(FOURTH SERIES.)

NUMBER LXIII.—JULY 1855.

NUMBER LXIV.—AUGUST.

NUMBER LXV.—SEPTEMBER.

NUMBER LXVI.—OCTOBER.

NUMBER LXVII.—NOVEMBER.

NUMBER LXVIII.—DECEMBER.

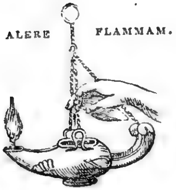
PLATES.

I. II. Illustrative of Mr. R. T. Forster's Paper on the Molecular Constitution of Crystals.

III. IV. V. Illustrative of Prof. Tyndall's Paper on the Nature of the Force by which Bodies are repelled from the Poles of a Magnet.

ERRATUM.

Page 50, line 6, *for* PETER WILLIAM BARLOW *read* WILLIAM HENRY BARLOW.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THE
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[FOURTH SERIES.]

JULY 1855.

I. *On the Induction Apparatus and its Effects.*

By J. C. Poggendorff*.

THE following investigation was suggested by an examination of an induction apparatus constructed by Ruhmkorff for the physical cabinet of the University of Berlin. There is no doubt that its construction and action are superior to those of any other instrument of the kind yet invented, but I feel convinced it is by no means so perfect as it may yet be made, nor have the phænomena which it manifests ever yet been sufficiently investigated in any published memoir. On this account the present investigation will be divided into two parts; the one will be devoted to the construction of the apparatus, the other to its effects.

1. *Construction of the Apparatus.*

In its present form the apparatus consists of no less than six parts; viz. 1, the coil of wire in which induction is developed, the induction coil; 2, the coil which conducts the inducing galvanic current, here called the primary coil; 3, the soft-iron core; 4, the current breaker; 5, the condenser; and 6, the voltaic battery or primary source of electricity; to which parts, lastly, some additional apparatus must be added. Each of these six elements acts, more or less, upon the remaining five; and the ultimate result, the induced current, depends upon a suitable distribution and arrangement of all.

Induction Coil.

On examining this coil, it appeared to me that hitherto it had not been constructed on proper principles. Usually the wire forms layers which extend, without interruption, from one end

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* From Poggendorff's *Annalen*, vol. xciv. p. 2.

of the coil to the other; the number of these layers is generally even, so that the beginning and end of the wire are at the same extremity of the coil. But it is clear, that in a coil, whose points are all exposed to about the same degree of electrical excitation, the tension must increase from the middle towards each extremity. Hence, when the wire is coiled in the manner above described, points whose electrical difference is very great are brought very near one another, and consequently the springing of sparks between two such points is much facilitated, as well as the external action of the coil weakened.

This imperfection appeared susceptible of partial remedy by dividing the length of the coil into several parts, and by covering each part with wire in the same manner as the whole coil was before covered, using, however, an odd instead of an even number of layers in order to carry the wire from one part to another, and at last to let the ends run out at opposite extremities of the coil.

Further, in order to obtain effects of great tension, it was evident that the wire must be well insulated. As far as I know, whenever a second insulator has been employed, to aid the covering of silk generally used and never to be dispensed with, a solution of shell-lac in alcohol has been chosen. But in the present case, where the wire, owing to its great length, cannot well be varnished other than lengthwise, such a method only imperfectly fulfils its purpose, because under such circumstances the varnish dries extremely slowly, and perhaps never perfectly; so that a certain degree of conductibility, even greater than that with silk alone, always exists.

Instead of the alcoholic solution of shell-lac, an easily fusible insulator (spermaceti, stearic acid, or a mixture of wax and oil*) appeared preferable. In applying the same, it requires to be heated above its melting-point in order to prevent its immediate solidification when in contact with the cold layers of wire, and to allow it to permeate and thoroughly saturate the silk covering.

Again, in order to obtain a good exterior insulation, the wire was coiled around a glass cylinder, at whose extremities were rims of gutta-percha in which the ends of the wire could be well fastened; besides this, the whole coil was provided with a thick covering of wax; and lastly, after this had been varnished, two strong rings of gutta-percha were affixed, by means of which the whole could rest on a wooden stand.

According to these principles, I have constructed three induction coils, each of which is $5\frac{3}{4}$ inches long†, has an internal dia-

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* Paraffine would be perhaps still better, as it may now be easily procured, easily melts, and is, according to Riess, a good insulator.

† Throughout the memoir the inches and feet are Parisian.

meter of $1\frac{5}{6}$ inch, and an external one of $2\frac{2}{3}$ inches. Two of these contain an extremely thin wire, 0·15 millim. in diameter, and the third a considerably thicker one, 0·25 millim. in diameter. In each of the two first the wire is about 10,000 feet long, and makes about 16,000 windings, in eight parts, each of which contains 33 layers. The wire of the third coil is about 2400 feet long, and each of its parts contains 19 layers. For certain purposes a fourth coil containing about 1200 feet of the thicker wire was constructed; its diameter was such as to permit of its being inserted in one of the primary coils to be afterwards described.

Although the greatest possible care was taken in the preparation of all these coils, and although they are, perhaps, more effective than others of equal dimensions constructed according to the usual method, still the desired object is not yet perfectly attained; on the contrary, the conviction forced itself upon me that the insulation was far from sufficient to protect the coil from the disturbing effects of internal sparks.

This was particularly perceptible in the second of the coils formed of fine wire. Immediately after its construction, the phænomena of sparks and tension which it exhibited were extraordinary, indeed wonderful, but after a quarter of an hour its action considerably decreased. This condition remained unchanged for months, and for my purposes was satisfactory enough, for by employing two pairs of Grove's battery I could obtain a spark at a distance of almost 12 millims. In all probability this condition would have been permanent had I not afterwards had occasion to use four pairs of Grove's battery. With this number I obtained sparks at a distance of 16 millims. and upwards, but only for a few minutes; afterwards the distance decreased considerably, and at length, when two pairs were again used, the distance, instead of being 12 millims. as before, was only 9·6 millims.

From this we may conclude, that in order to maintain an induction coil in good condition, it must not be strained too much,—at least when the current is broken so as to obtain sparks in air of ordinary density, because in such cases the electricity excited possesses great tension, and strives everywhere to break open a path between the windings of the wire. Of course such a deterioration of the coil need not be feared in experiments where the poles of the induction coil are connected by a pretty good conductor, for then the tension is either not so great, or is not at all present. In such cases, too, the wire need not be so perfectly insulated. For example, the phænomena of light in a partial vacuum are just as good when the wire is tolerably well insulated, as when the insulation is as perfect as possible;

nor does an increased length of wire add anything to the brilliancy of the phænomena as soon as the resistance of the partial vacuum is small in comparison to that of the induction wire. Without doubt the induction coils for obtaining sparks and other phænomena of tension would be more effective and durable if the thickness of the insulating medium were increased. But even if the length of wire might then be reduced to some extent, the coil would have to be large in size in order to produce the desired effects, the inducing coil would have to be increased to a corresponding extent, and a stronger voltaic battery would be requisite; in short, the apparatus would lose much of its present convenient dimensions for practical purposes.

I believe the above-mentioned imperfections might to a great extent be remedied,—1, by not using too thin a wire (for most purposes wire of $\frac{1}{4}$ millim. diameter would, perhaps, be best); 2, by giving the wire a very thick covering of silk; and 3, by using a liquid instead of a solid insulating medium, such as a fat or essential-oil, *e. g.* rectified oil of turpentine. By this means, at least, the gradual deterioration of the coil would be prevented, for the openings broken through the silk covering of the wire by the sparks would be always stopped up again by the liquid.

The number of parts on the coil might be greater than that I have chosen; the whole coil might be composed of loose and, of course, well insulated rings of wire. In one respect this would be advantageous; the rings when defective could be replaced by new ones, but the apparatus would be thereby rendered very complicated on account of the number of connecting clamps that would be required. Neither can we expect any particular advantage from different combinations of these rings of wire, for they are elements of unequal value in consequence of their different positions with respect to the inducing coil. If it be required to change the length and thickness of the wire in order to study their influence upon the phænomena, it will be found more convenient to prepare complete apparatus of smaller dimensions (say four similar to that of my third induction coil), and then to combine them as it may be found desirable.

Lastly, I am inclined to believe that the usual cylindrical form, which I have also adopted hitherto, is by no means the most advantageous for the induction coil. A solenoid or spindle shape would be better, because the inducing coil and the soft iron core are most effective in the middle. I have not yet had time, however, to try all these tedious changes in the apparatus.

Primary Coil.

I have generally used three inducing coils, all of which were exactly alike. In construction they differ little from the common form, except that they contain two insulated copper wires which can be used either singly or combined, side by side or one after the other.

The wire is 1 millim. thick, and each of its halves about 100 feet long. The tube upon which the wire is coiled in four layers is of mill-board in two instruments, and of glass in the third, and has an internal diameter of $1\frac{1}{3}$ inch.

In some cases I have also used a fourth primary coil with wire only 0·67 millim. in diameter, and nearly 400 feet long; this was coiled on a mill-board tube.

It may be here mentioned, that, under otherwise equal circumstances, the inducing action of a current, just as its magnetic action, is a product of its intensity and length; hence a short, thick wire and great intensity of current may sometimes be advantageously replaced by a long, thin wire, and small intensity of current*. The last is preferable when the action requires to be long sustained, because the voltaic battery is less attacked; on the other hand, however, the inner induction current, the so-called extra current, increases in intensity, and leads to disadvantages, of which more hereafter.

Soft Iron Core.

Usually this is formed of pretty thick wires, or we may almost say, small bars, held together by a common cap, but yet separated from each other by some insulating medium.

In some cases I have employed such a collection of bars, but oftener I used cores of much thinner wire; on the one hand, because this thinness must tend to increase the action, and on the other hand, because I was enabled to construct such cores for myself. The wire was only 0·25 millim. in diameter. It was heated to redness, cut into pieces of convenient length, bound in a bundle with silk thread, and lastly, surrounded with a paper covering.

I found it unnecessary to cover the wires with a varnish, partly because the stratum of protoxide which forms on its surface, when heated, is comparatively a bad conductor; partly because the irregularity of the spaces between the wires, none of which remain straight, is more than sufficient to destroy the pernicious continuity in the direction of the periphery of the core.

If care be taken not to diminish the interstices too much by unnecessary tightening, such a soft iron core will be as effective

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* The voltaic battery and the volume of the inducing wire of an induction coil being given, the maximum inductive effect is obtained when the resistance of the inducing wire is equal to the residual resistance in the battery; the action of the extra current, however, is here neglected. Vide Poggendorff's *Annalen*, vol. lv. p. 45.

as, or more so than, one of equal weight prepared in the most careful manner by means of strings and individually insulated bars. Two of the cores which I prepared are a little longer than the induction coil, 6 inches long and 13 ounces in weight. A third is three times as long, *i. e.* 18 inches. Each of them contains 4200 wires of the given dimensions.

I convinced myself, however, that this number may be diminished without injury to the action of the coil, in which case, of course, the primary coil will remain partly unoccupied. In fact, I found that a hollow core of the above-mentioned wires, which contained an empty cylindrical space three-quarters of an inch in diameter, and which weighed only half as much as the massive ones, was just as effective as the latter. It surprised me to observe that when a massive bar of soft iron was introduced into the hollow axis of this core, the action of the induction coil was not thereby diminished; whereas the same soft iron bar introduced alone, in absence of the hollow core, unmistakeably diminished the action which the primary coil itself exercised upon the induction coil. This fact is evidently analogous to one observed several years ago by Magnus, viz. that a massive bar of iron, when introduced into a slit tin tube, weakened but little the action of the latter*.

Practically the soft iron core is of great use, theoretically it causes much complication. If it were dispensed with on this account, it would be necessary to give the apparatus a different form in order not to diminish its action. It would be necessary to increase the diameter of the inducing coil, and to arrange the induction wire half inside it and half outside it, in order to make use of both sides of the inducing wire. The soft iron core has a strengthening influence, because, by a kind of reflexion, the inner side of the inducing coil is made to act externally.

Current-Breaker.

For breaking the current I employed as usual a Neef's, or rather Wagner's hammer, of which I possess two. One is, on the whole, constructed in the usual manner, *i. e.* a fixed pin descends, and against it, from below, beats a spring tongue with a small plate fixed to it. According to an improvement introduced by Riess, there is a second pin underneath the tongue, in order to make use of the downward vibrations for closing the circuit; so that the primary current can not only be doubly broken, but also reversed†. These effects can be accomplished in my instrument by means of a single galvanic battery in consequence of the two wires around the primary coil, the combina-

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* Poggendorff's *Annalen*, 1839, vol. xlviii. p. 105.

† Ibid. vol. xci. p. 290.

tions being so arranged that the current must pass alternately along the one and along the other wire, in the same or in opposite directions.

I have often employed the second pin, and a thick, angular-shaped wire, which could be screwed to it and made to press against the tongue in order to displace its fulcrum, to shorten the vibrating part, and thus quicken its motion. Too quick a motion of the tongue, however, combined with an amplitude so small that the vibrations are scarcely perceptible, and the noise similar to that of the humming of a bee, is prejudicial to the action of the induction wire.

This instrument also possesses a contrivance, by means of which, besides the first current, a second can be simultaneously, and at equal intervals, interrupted.

Another instrument was employed to break the current within a liquid. For this purpose the small electro-magnet was placed above the armature of the tongue. The tongue carries the pin or hammer; the anvil (a thick platinum wire) is placed in the middle of a small glass cylinder which rests on a brass pedestal. The platinum wire is screwed into this pedestal, and around it the glass cylinder is cemented with a layer of sulphur, which is well adapted to this purpose, because, when cold, it resists the greatest number of liquids*. In other respects this current breaker is constructed in the usual manner, and, as in the first instrument, the parts which strike against one another are of platinum, though for certain purposes I have used silver and other metals instead of platinum.

In most, if not all, induction apparatus the current-breaker forms an inseparable part of the whole; and in some, as in Ruhmkorff's, it is set in motion by the same soft iron core which increases the action of the inducing current. I preferred not only to produce this motion by a small separate electro-magnet, as in Halske's apparatus, but also to have the current-breaker constructed as a separate instrument, in order to connect it at will with other instruments, and, if need be, to use it under the air-pump.

Neef's hammer is, perhaps, the simplest contrivance for producing periodic interruptions of a galvanic current; but for certain purposes, of which I intend to speak another time, it has one imperfection,—it does not produce uniform interruptions, the time during which the circuit is broken is longer than that during which it is closed, and that because the tongue vibrates with its whole length downwards from the pin, and only with a part of its length upwards. In this respect Neef's hammer dif-

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* For this reason I have for many years used sulphur for cementing the platinum in the covers of my Grove's battery.

fers from the rotating current-breaker, where a straight electro-magnet oscillates horizontally on a point between the upright arms of a steel horseshoe magnet, and where the ends of the encircling wire dip into two semicircular mercury cups, connected with the poles of a galvanic battery, and separated by an insulating partition. Here the pauses are of much shorter duration than the moments of activity of the current, because the former occur only when the ends of the wire pass over the partition. The construction of this rotating current-breaker might, indeed, be so changed as to make the pauses equal in duration to the periods of activity; but to do so would be to render the instrument more expensive, and as this equality was not necessary in the present investigation, I have continued to use Neef's hammer.

Condenser.

At first my condenser was exactly similar to the one constructed by Ruhmkorff, probably according to Fizeau's own instructions; it consisted of a long piece of oiled silk covered on both sides with tinfoil, and folded to and fro in layers so as to occupy less space, and to allow only parts of one and the same sheet of tinfoil to touch each other. Each of these sheets of tinfoil was 8 feet long and 11 inches broad. Soon afterwards a second condenser, about one-fourth the size of the first, viz. 23 inches long and 11 inches broad, was used; the folded layers were separated from one another by mill-board in order to prevent contact between the posterior surfaces of the tinfoil, which, if present, would necessarily be prejudicial.

Afterwards I became acquainted with the simplification proposed by Halske, wherein the voluminous arrangement in Ruhmkorff's apparatus is replaced by a single covered sheet of mica not greater than the octavo form. I have had an opportunity of testing a condenser of this kind and of this magnitude prepared by Prof. Riess, and I thereby convinced myself, that, under the circumstances under which the test was applied, its effect was very nearly equal to that of the great oiled silk condenser.

This great superiority of the mica condenser, in comparison to its magnitude, is caused by one of the sides of the condensing surfaces being in free contact with the air, and by the two surfaces being nearer to one another than was the case with oiled silk, which, besides being somewhat thicker than the plate of mica, is so bent and folded as to prevent the close proximity of the sheets of tinfoil.

Mica is an excellent substance for such condensers, but it cannot readily be procured in plates of the requisite size. This caused me to search for substances capable of replacing it, amber varnish, or a solution of shell-lac in alcohol; thin wax-paper, such as may be procured from apothecaries, and covered with lac varnish. When rigidity, which these preparations possess only in a small degree, is not required, they are fully capable of replacing mica.

I have prepared many such paper condensers, some containing 54, others 30 square inches of covered surface. I have tested them singly and in pairs, connecting them in the latter case by a strip of tinfoil, and I have found that two together are not more effective than one, and that even one of the smaller kind was almost as effective as the great condenser of oiled silk. In order to discover how far this diminution of the condensers could be carried without injury to their actions, others were prepared containing on each side 16, 9, 4, and 1 square inches of covered surface; some were made of varnished wax-paper, others of gutta-percha, and others of oiled silk, upon which, owing to their small size, the sheets of tinfoil could be easily and effectually fastened by means of shell-lac varnish. To my astonishment, I found that all these small condensers, the smallest not excepted, were, with respect to the *length of the induction spark*, just as effective as the two great ones of oiled silk; at least the differences were very irregular and very trifling, so that, without difficulty, they could be ascribed to the changes which the parts of the contact-breaker which hammer against one another suffer, owing to the sparks which are produced between them. The sole advantage which the great condensers possessed over the small ones was, that the former produced less noisy sparks than the latter. With the former, too, the sparks followed one another more quickly when the distance between the poles approached the limit at which the sparks can be obtained singly, or even at all. On this account, therefore, the great condensers are not superfluous, nor are they so for other reasons. First, all the results mentioned were obtained with a current from one or from two pairs of Grove's battery; and the comparison of both cases appeared to indicate, that, by employing a more intense galvanic current (to which, however, I intentionally would not expose my induction coil), the relation between the actions of the different condensers would change. Secondly, in the induction apparatus, the action of the condensers is essentially influenced by the *extra current*, i. e. the induction current which is produced in the circuit of the inducing current itself, when it is broken.

All that has hitherto been said applies only to the case where this *inner* induction current is comparatively weak, consequently to the case where the primary current traverses a comparatively short and thick wire. If the former current is strengthened by conducting the latter through a long and thin wire, without at amongst which were post-paper, covered on both sides with the same time changing anything connected with the source of electricity or the soft iron core, the action of all small condensers considerably decreases, and that the more the smaller they are, until in the smallest no action whatever is observable; the great condensers, however, retain their force undiminished. I convinced myself of this by so combining the wires of the primary coil as to cause the current, instead of traversing two wires side by side, to traverse one after the other; therefore, instead of a wire of double section and single length, one of single section and double length was employed. The phænomenon was still more perceptible when the latter wire, which was now 1 millim. thick and 200 feet long, was replaced by a wire ⅔rds of a millim. thick and 400 feet long, without changing anything else about the apparatus. In both cases where the *primary current* was comparatively weak, and the inner *induction current* comparatively strong, the condenser of 1 square inch surface, which was before so effective, was now almost without effect.

Thirdly, the action of condensers depends essentially upon the *induction coils* on which their influence is to be exerted. All the results before mentioned were obtained with a very thin induction wire, 10,000 feet long; when the thicker wire, 2400 feet long, was employed, the small condensers again manifested a very small action, and sometimes none whatever; indeed, the two smallest condensers appeared not only not to strengthen the development of sparks between the poles of the induction coil, but actually to weaken the same. This phænomenon manifested itself equally when the galvanic current traversed a short and thick, or a long and thin wire. On the contrary, however, the two large condensers of oiled silk still retained, in both cases, their former energetic action; indeed it was clearly perceptible that the condenser 8 feet in length was superior to the one 2 feet long.

Hence the stronger the galvanic battery, the longer and more conducting the circuit of its current, and the thicker and more moderate the induction wire; in short, the more intense and energetic the inner induction or extra current is made, the greater the condenser must be in order to obtain an energetic development of sparks between the poles of the apparatus*. When the extra current is very intense, it passes through the condenser the more easily the smaller its surface; and from the sparks on the current-breaker, it is easy to see that the actual outer induction current has an influence upon the weakening of the extra

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* I may also add, that the degree of insulation of the induction wire appeared to exercise an influence upon the action of the condenser; this action appeared to increase the more imperfect the insulation became.

current, for under all circumstances these sparks are smaller with a long thin induction wire than with a short thick one.

What has here been said of sparks in free air, is partially true also for the phænomena of light in a partial vacuum. Under otherwise equal circumstances they are not so fully developed with small, as with large condensers.

Nevertheless it must be here remarked, that in general the strengthening action of condensers is less prominent the more intense the induction current itself, and the more perfect or conducting the vacuum. When the current in the induction wire was excited by two of Grove's elements, and the receiver exhausted to a few lines of mercury pressure, the great condenser itself scarcely exerted any influence upon the light produced; on the other hand, however, this influence immediately manifested itself when a little air was introduced, or when the long thin induction wire was replaced by the short thick one.

In all condensers, the wires which connected the same with the hammer were always at a short distance from, and on opposite sides of, the point of interruption. They can be connected with other and more distant points of the circuit, but by so doing their action is weaker.

Voltaic Battery.

On this subject I have only to remark, that, owing to the small dimensions of my apparatus, I never found it necessary to use more than two of Grove's elements, all the phænomena which I wished to study being sufficiently developed with that number.

Additional Apparatus.

Amongst these may be mentioned the small instrument, which I will call a discharger. It is requisite in order to observe many of the phænomena of the induction apparatus with convenience and without being exposed to electric shocks. On the whole, my discharger has somewhat the form of a spark micrometer, but its use is more general; the platinum wires, carried by glass rods and connected with the poles of the induction coil, are, in the horizontal position, capable of a horizontal, vertical, and rotatory motion; and, in the vertical position, they can be placed side by side, or one above another. These wires, on whose extremities thinner wires, knobs, or discs can be fastened, serve also, when in a vertical position, as electrodes in the chemical decompositions which can be effected by means of the induction current.

The second apparatus of this kind is known as the electric egg, whose construction needs no description.

I have employed two, of unequal dimensions. The axes of the greater are 10½ and 7 inches in length, and in it points, spheres, and discs can be introduced. The axes of the smaller one are 4½ and 3 inches in length. For most experiments the latter is sufficient, and besides this, it possesses the great advantage of being exhausted, to a few lines of pressure, by means of a few strokes of a good air-pump.

I have also employed an extra plate, such as is furnished with most air-pumps, but modified so as to suit the institution of electric experiments in a vacuum. A glass disc is cemented upon the brass disc; in both two holes are made, in which two thick copper pins are fastened so as to be air-tight, and yet not to touch the brass; these pins project above and below, are 6 inches distant from one another, and their extremities are provided with holes and clamp-screws, so as to receive the necessary connecting wires. By means of the glass disc, many experiments can be made which it would be impossible to make in the egg; for upon it several kinds of apparatus can be placed, and over all a bell-shaped receiver can be inverted in order to surround all by a vacuum. The arrangement is also more air-tight than the egg, as it contains no stuffing-box, which, however, should motion be required during the experiment, can be added to the glass receiver.

[To be continued.]

II. 1. *Description of a new Meteoric Iron from Chili, containing* Native Lead. *By* R. P. Greg, *Esq.*[*]

2. *Fall of a large mass of Meteoric Iron at Corrientes in South America*[†].

1. A SHORT time since I purchased a mass of meteoric iron weighing upwards of 17 lbs.; its shape was irregular and cup-like, considerably convex or hollowed out on one side, and the external surface more or less covered with small angular and conchoidal projections. It was found by Mr. Greenwood, Reporter of Mineral Properties, on the 26th of February, 1840, on the desert of Tarapaca, 80 miles N.E. of Talcahuaxo, and 46 miles from Hemalga; and was afterwards analysed, in 1853, by Mr. George Darlington, of the Museum of Practical Geology, with the following results:—

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[*] Communicated by the Author.

[†] From the Liverpool Literary and Philosophical Society's Journal.

	a.	*b.*
Iron	93·41	93·48
Nickel . . .	4·62	4·56
Cobalt . . .	0·36	0·37
Manganese . .	0·20	0·18
Phosphurets . .	1·21	1·26
Chromium . .	trace	trace
	99·80	99·85

In general composition it therefore closely resembles the majority of meteoric irons hitherto analysed. I am unable at present to say if it contains *Schreibersite.*

The specific gravity of a slice weighing 6 oz., containing, however, cavities, and other matter, I found to be about 6·5. For meteoric iron it is perhaps unusually soft; and though it shows no regular *crystalline* or Widmannstättian figures when a polished surface is treated with nitric acid, yet there is apparent a slightly welded or mottled texture, brighter in some parts than in others.

It was not, however, until the iron had been cut up into slices for the purpose of polishing and for exchange, that I discovered it had not a perfectly homogeneous structure, but was in many places more or less honeycombed with cavities, some of which actually contained what appeared to be pure *lead*! In some the lead was not larger than a pellet, and did not fill the entire cavity which contained it; in others the entire cavity was filled with lead, in size equal to a pea. Professor Shepard of America, who is so well acquainted with meteorites, along with Dr. Heddle and myself, saw some slices of this iron slit in the workshop of Mr. Young the lapidary, at Edinburgh, and we took lead out of the cavities immediately after they left the lathe, so that there could be no deception whatever.

To be quite certain, my friend Dr. Heddle examined some of it, and found it to be chemically pure lead; when the tarnished surface was not scraped off, small quantities of iron and alumina, and mere traces of phosphorus and magnesia, were found.

There are also two other substances in some of the cavities of this singular iron, which Dr. Heddle, when he has analysed, will separately report upon; the one a very hard, grayish-black, semi-metallic mineral; the other yellowish-brown, insoluble in acids, and with an earthy texture.

This is the first authentic instance of the existence of lead in meteoric bodies, and to find it so closely allied with, and buried, as it were, in metallic iron, is not only in itself singular, but difficult to account for. It is, however, probable that the lead was originally held in alloy along with the nickel and cobalt, and on intense heating or partial fusion of the iron mass, "sweated" out into vesicular cavities.

Should this be a correct view, it is a proof of the intense heat to which iron meteoric masses appear to have been subjected at the time of, if not previous to, their reaching the surface of the earth. Indeed meteoric *stones* seem to have been subjected to a much smaller degree of heat while falling, than *iron* masses, if we may judge by appearances, the only sign of fusion in *stones* being quite external, and merely marked by a thin, black, and shining crust.

Iron falls are extremely rare compared with what are called *stone* falls; so much so, indeed, that there are not more than three or four authentic accounts of the fall of *iron* masses, and these not large ones, bearing no comparison to the enormous masses weighing from 5 to 20 tons, which have been occasionally found on the plains of Mexico and South America. See Phil. Mag. for Dec. 1854.

2. Should any one be inclined to doubt the fact that such immense masses of iron have not, strictly speaking, a meteoric origin, I have added to this paper some particulars of the fall of one in South America in the year 1844, that first appeared in a philosophical journal some years ago, but which, from having a local* circulation, has not received the notice which it so eminently deserves.

It is important to notice the state of intense fusion exhibited by the entire mass at the time of falling; Mr. Symonds's description is most graphic. The account was given in a letter, read by Mr. Dickinson, from the observer of the phænomenon, a Mr. H. E. Symonds; and the following is an extract:—

"Having been deeply engaged in Argentine politics and wars in 1843 to 1844, I accompanied the Correntine army in its invasion of the province of Entre Rios. This army returned from that expedition in January 1844. Our rear, in which I marched, was so continually harassed by Entrerian skirmishers, that for ten days before we had gained the Corrientine frontier we had no time to sleep or change clothes; but soon after passing this, in Carritas Paso, on the river Mocorita, we placed a guard in the pass, and deeming ourselves secure, the whole division abandoned itself to the profoundest sleep.

"From this sleep we were all simultaneously awakened at about two o'clock in the morning; and as if actuated by electricity, each individual of our division (about 1400 men) sprung on his feet at the same moment. An aërolite was falling. The light that accompanied it was intense beyond description. It

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* See Proceedings of the Literary and Philosophical Society of Liverpool, No. 7. Session 1853.

fell in an oblique direction, probably at an angle of about 60° with the earth, and its course was from east to west.

"Its appearance was that of an oblongated sphere of fire, and its tract from the sky was marked by a fiery streak, gradually fading in proportion to the distance from the mass, but as intensely luminous as itself in its immediate vicinity. The noise that accompanied it, though unlike thunder, or anything else that I have heard, was unbroken, exceedingly loud and terrific. Its fall was accompanied by a most sensible movement of the atmosphere, which I thought at first repellent from the falling body, and afterwards it became something of a short whirlwind. At the same time I and my companions all agreed that we had experienced a violent electric shock; but probably this sensation may have been but the effect on our drowsy senses of the indescribably intense light and noise. The spot where it fell was about one hundred yards from the extreme right of our division, and perhaps four hundred from the place where I had been sleeping. Accompanied by our general (Dr. Joaquin Madauaga), I went within ten or twelve yards from it, which was as near as its heat allowed us to approach.

"The mass appeared to be considerably imbedded in the earth, which was so heated that it was quite bubbling around it. Its size above the earth was perhaps a cubic yard, and its shape was somewhat spherical; it was intensely ignited and radiantly light, and in this state it continued until early dawn, when the enemy, having brought his artillery to the pass, forced us to abandon it to continue our march. I may mention, that, at the time of its fall, the sky above us was beautifully clear, and the stars were perhaps more than usually bright; there had been sheet lightning the previous evening.

"I never afterwards had an opportunity of revisiting the Mocorita, for our permanent encampment was thirty-five leagues to the north of that pass, between which and our encampment the country was entirely depopulated by our long war; but as the spot where the aërolite fell was known to many of our subaltern officers, who were frequently sent to observe the frontier of Entre Rios, I have heard them describe it as a 'piedra de fiérro,' *i. e.* a stone of iron; and I once provided one of the most intelligent of them with a hammer in order that he might bring me a sample of it. On his return, he told me it was so excessively hard that the hammer bent, and was broken in unsuccessful attempts to break off a small piece of it."

III. *On the Relation of the Transmission of Radiant Heat through Crystals to the direction of Transmission. By* H Knoblauch, *Professor of Natural Philosophy in the University of Halle*.*

IN a memoir, a full translation of which is given in the last volume of the Scientific Memoirs, M. Knoblauch showed that radiant heat passed in different quantities in different directions through certain uniaxal crystals; and also that the quality of the heat, after having traversed the crystals in different directions, was different. In the present paper he extends his researches to biaxal crystals.

The transmission of radiant heat through a number of plates of biaxal crystals, which were all cut perpendicular to a line bisecting the acute angle enclosed by the optic axes (the "middle line"), was first examined. A beam of light was conducted by the mirror of a heliostat into a darkened room, and permitted to pass through a Nichol's prism before reaching the crystal.

When the principal section passing through the obtuse angle of the prism was vertical, the rays issuing from the prism were polarized in a horizontal plane; and when it was required to polarize them in a vertical plane, the prism was simply turned through an angle of 90 degrees. Having traversed the crystal, the rays fell upon a thermo-electric pile, and the deflection produced was a measure of the amount of heat transmitted.

Operating in this manner with *yellow heavy spar,* M. Knoblauch found that when the plane of polarization of the rays enclosed an angle of 90° with the plane of the optic axes, the heat passed in greater quantity in the direction of the middle line than when both planes were coincident, the ratio of the latter quantity to the former being 100 : 112.

The *quality* of the heat which had passed through the crystal was next examined, and proved to be the same, whatever the plane might be in which it was polarized. This result was obtained by introducing a diathermanous body in the path of the rays between the crystal and the pile: it was found that the quantity falling on the diathermanous body always bore the same ratio to the quantity transmitted by it, no matter how the rays might have been polarized.

The next crystal examined was a *reddish-yellow topaz,* and the result was different from that obtained in the case of heavy spar. In topaz the heat travels in greatest quantity in the direction of the middle line, where the plane of polarization coincides with the plane of the optic axes. The ratio of the respective quantities found by M. Knoblauch was as 100 : 96.

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* Abstracted from Poggendorff's *Annalen,* vol. xciii.

Qualitative differences were also observed in this case, dependent on the position of the plane of polarization. Glasses of different colours were used to test the quality of the heat. Calling the quantity falling upon the pile previous to the introduction of the glass 100, the ratio of this quantity to that which passed through the glass in the respective cases is given in the following table:—

	Planes coincident*.	Planes crossed.
Blue glass.........	100 : 44	100 : 34
Yellow glass......	100 : 58	100 : 60
Green glass	100 : 11	100 : 16
Red glass	100 : 49	100 : 45

We must conclude from this table that the heat whose plane of polarization coincides with that of the optic axes, possesses, after its passage through the topaz, a greater power of traversing the blue glass than the heat whose plane of polarization is perpendicular to that of the optic axes. The former, however, passes through the green glass in less quantity than the latter.

These differences were exhibited in a far more striking manner in the case of a plate of *yellow-green hornblende*. For the sake of brevity, we will call the quantity of heat which passes through the crystal when the planes of the optic axes and that of polarization are coincident, Q, and the quantity which passes when the planes are at right angles to each other, Q′. In the case of hornblende, the ratio of Q to Q′ was as 100 : 133.

Qualitative differences also exhibited themselves here, as is manifest from the following table:—

	Planes coincident.	Planes crossed.
Red glass	100 : 61	100 : 53
Green glass	100 : 4	100 : 17
Blue glass	100 : 29	100 : 41
Yellow glass	100 : 97	100 : 71

We here observe, that the rays which pass in greatest quantity through the red and yellow glass are the most intercepted by the green and blue. We also see that a characteristic difference exists between the heat issuing from hornblende and that issuing from topaz. For the heat whose plane of polarization coincides with that of the optic axes, possesses, in the case of topaz, a higher power, and in the case of hornblende a lower power, to radiate through the blue glass, than the heat whose plane of polarization crosses that of the optic axes.

In the case of a plate of *pistacite* the ratio of Q to Q′ was found to be as 100 : 146.

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* *i.e.* the plane of polarization and the plane of the optic axes.

The qualitative differences that were observed are exhibited in the following table:—

	Planes coincident.	Planes crossed.
Red glass	100 : 66	100 : 44
Green glass	100 : 12	100 : 16
Blue glass	100 : 47	100 : 36
Yellow glass	100 : 53	100 : 59

It is worthy of remark, that with the same position of the plane of polarization and of the optic axes, the heat, in the case of hornblende, passes worse through blue glass and better through yellow glass; and in the case of pistacite, better through blue glass and worse through yellow glass, than the rays for which the planes enclose an angle of 90°. In the case of red and green glass this antithesis is not observed.

M. Knoblauch next placed two plates of pistacite together, both cut at right angles to the middle line, and examined their effect upon the passage of heat in the direction of the said line, first, when the planes of the optic axes of both plates were coincident; and secondly, when they were at right angles to each other: the proportion of the quantity transmitted were as 100 : 130, the former representing the quantity transmitted when the planes were at right angles to each other, and the latter the quantity transmitted when they were parallel.

Qualitative differences were also exhibited, as shown by the following table:—

	Planes of the optic axes of the two plates.	
	Perpendicular.	Parallel.
Red glass	100 : 62	100 : 40
Green glass	100 : 10	100 : 24
Blue glass	100 : 42	100 : 27
Yellow glass	100 : 41	100 : 88

In these experiments the Nichol's prism was removed, and the light direct from the mirror of the heliostat was permitted to pass through both plates successively.

A plate of *brown mica* was next examined, in the same manner as the heavy spar, topaz and hornblende. In this case the ratio of Q to Q′ was as 100 : 109.

The power of the heat to pass through glasses of different colours is exhibited in the following table:—

	Planes coincident.	Planes crossed.
Red glass	100 : 52	100 : 43
Green glass	100 : 14	100 : 17
Blue glass	100 : 42	100 : 37
Yellow glass	100 : 61	100 : 67

In this case, the differences, both quantitative and qualitative, in both groups of rays are evident.

The ease with which plates of mica of any required thickness may be obtained, rendered this body peculiarly adapted to show the manner in which the differences exhibited above were affected by the thickness of the plates.

In the first place, a plate was chosen of about one-third the thickness of the plates made use of in the foregoing experiments. The proportion of Q to Q′, that is, of the quantity transmitted when the plane of polarization was parallel to that of the optic axes, to the quantity transmitted when both planes were at right angles to each other, was as 100 : 107.

When, however, instead of sending the heat through one plate of mica, it was sent through a number of plates whose total thickness amounted to about six times that of the single plate, under the same circumstances the proportion of Q to Q′ was as 100 : 119; a proof that the quantities of the two systems of rays polarized at right angles to each other differ the more from each other the greater the thickness of the crystal through which they have to pass.

To ascertain the influence of thickness upon the quality of the transmitted rays, M. Knoblauch proceeded as follows:—Having determined the rate of the quantities of heat transmitted direct from the plate of mica to the thermo-electric pile, a diathermanous body, a green glass, for example, was introduced between the mica and the pile, and the effects produced when the planes were coincident and at right angles to each other were compared. If the ratio observed, as before the introduction of the glass, showed itself here also, it would be a proof that equal proportions of both groups of rays were transmitted through the green glass. If the ratio were changed, it would show that the two systems of polarized heat had traversed the green glass in unequal quantities, and were therefore qualitatively different from each other after their passage through the mica.

It was found, in the case of a single plate of mica, that the proportion of the two quantities mentioned, before entering the glass, was as 100 : 107. After passing through the glass the proportion was precisely the same. After passing through about three times the thickness of mica, the proportion, before reaching the glass, was as 100 : 120; but after passing through the green glass, as 100 : 170. Doubling the last thickness, the proportion before entering the glass was as 100 : 126, but after leaving the glass it was as 100 : 284.

It thus appears that the difference of transmission of the two differently polarized groups of rays, through the same diathermanous substance, may be increased from zero to a very great amount, by simply augmenting the thickness of the crystal.

Similar experiments were made with red, blue and yellow glass. The results are contained in the following table:—

		Thin plate of mica.		Thick plate of mica.	
		Planes coincident.	Planes crossed.	Planes coincident.	Planes crossed.
Red glass	Before passage through ...	100	101	100	128
	After passage	100	99	100	117
Green glass	Before passage through ...	100	107	100	126
	After passage	100	107	100	284
Blue glass	Before passage through ...	100	111	100	125
	After passage	100	105	100	104
Yellow glass	Before passage through ...	100	112	100	124
	After passage	100	113	100	135

From this table we derive the general conclusion, that the qualitative difference of the rays which we have here compared become more evident as the thickness of the crystal through which they pass is increased.

In all the experiments hitherto recorded, the direction of the calorific rays was that of the line which bisected the acute angle enclosed by the two optic axes. M. Knoblauch next proceeded to compare the quantities of heat transmitted in different directions by the same crystal.

The Nichol's prism was removed, and the direct solar rays were permitted to fall upon a cube of *dichroite*; the rays first passed in the direction of the middle line just referred to, and afterwards in the direction of the line which bisected the *obtuse angle* enclosed by the optic axes, the "supplementary line." The quantity transmitted in the former case, compared to that transmitted in the latter, was as 100 : 36.

The crystal was next placed so that the supplementary line, and with it the plane of the optic axes, was vertical, and the quantity of heat transmitted along the middle line was compared with the quantity transmitted in a direction at right angles to the plane of the optic axes: these quantities were found to be in the ratio of 100 : 46.

In both these cases, therefore, the quantity transmitted along the middle line was greatest.

Comparing the direction of the supplementary line with the perpendicular to the optic axes, the quantities transmitted in these respective directions were found to be in the ratio of 100 : 139. Although the crystal here examined exhibited certain mechanical defects, M. Knoblauch considers it certain that the differences stated cannot be referred to this cause. In the case of dichroite, radiant heat passes in the direction of the middle line in the greatest quantity; in less quantity perpendicular to the plane of the optic axes, whereas the direction of minimum transmission is that of the supplementary line.

The qualities of the heat passing in these three directions were next examined. In the first compartment of the following table, the heat transmitted parallel to the middle line is compared with that transmitted parallel to the supplementary line; in the second compartment, the middle line and the perpendicular to the plane of the optic axes are compared; and in the third compartment, the supplementary line is compared with the perpendicular to the plane of the optic axes. In all cases the quantity falling *upon the glass* is set =100, and the ratio of this quantity to that which passes *through the glass* is stated.

	Parallel to the middle line.	Parallel to the supplementary line.
Yellow glass	100 : 71	100 : 25
Blue glass	100 : 18	100 : 60
Red glass	100 : 54	100 : 52
Green glass	100 : 31	100 : 14
	Parallel to the middle line.	Perpendicular to the plane of the optic axes.
Yellow glass	100 : 78	100 : 67
Blue glass	100 : 16	100 : 27
Red glass	100 : 51	100 : 54
Green glass	100 : 24	100 : 20
	Parallel to the supplementary line.	Perpendicular to the plane of the optic axes.
Yellow glass	100 : 34	100 : 71
Blue glass	100 : 60	100 : 20
Red glass	100 ; 51	100 : 54
Green glass	100 : 22	100 : 22

From these observations M. Knoblauch concludes, that the properties of calorific rays are different, that is, they possess different powers to pass through the same diathermanous body, according as they have traversed dichroite in the direction of the middle line, parallel to the supplementary line, or perpendicular to these two directions.

A cube of blue topaz was submitted to a similar examination:

here it was found that the calorific rays passed in greatest quantity parallel to the supplementary line, in less quantity perpendicular to the plane of the optic axes, and least of all in the direction of the middle line, a deportment antithetical to that of dichroite being thus exhibited. Small qualitative differences were also observed where the heat was transmitted through the various coloured glasses.

Remarkable quantitative and qualitative differences were shown in the case of a twin crystal of diopside, according as the calorific rays were transmitted through it in one or the other direction. From all the experiments made with biaxal crystals, M. Knoblauch considers the fact to be placed beyond doubt, that certain of these crystals transmit calorific rays with different facilities in different directions; and after this transmission exhibit different properties; further, that for polarized heat, differences are observed in one and the same direction according as the plane of polarization coincides with or is perpendicular to that of the optic axes.

In the memoir alluded to at the commencement of this abstract, M. Knoblauch showed that the transmission of heat through certain crystals possessing but one optic axis, was dependent on the direction of transmission. In addition to the observations there recorded, he has recently made others on amethyst and idocrase, and finds, in the case of amethyst, when polarized heat is used, that the calorific rays which pass perpendicular to the axis of the crystal are different, in quantity and quality, according as their plane of polarization occupies the one or the other position; but when the rays pass in the direction of the crystallographic axis, no such differences are observed.

The transmission of unpolarized heat was not examined in the case of amethyst, but in the case of idocrase the quantity of natural rays transmitted parallel to the axis was to the quantity transmitted perpendicular to the axis as 100 : 78; qualitative differences were also exhibited by the two groups of rays.

These differences were not observed in the rays which passed through the crystal in two directions at right angles to each other, and also at right angles to the optic axis.

When polarized heat was used, the quantities transmitted when the plane of polarization was respectively parallel and perpendicular to the axis, were found to be in the ratio of 100 : 79. Considerable qualitative differences were also observed in these rays on testing them by coloured glasses. When, however, the plane of polarization was preserved coincident with the optic axis, no matter how the direction of the ray through the crystal might be changed, neither qualitative nor quantitative differences were discernible. From all his observations on mineral crystals M. Knoblauch concludes, that with certain bodies of this class calorific rays exhibit both quantitative and qualitative differences dependent on the direction of the rays through the crystal. Only in directions which are perpendicular to the optic axis are the effects the same. If the heat is polarized, differences dependent on the position of the plane of polarization are observed in one and the same direction. Here also the radiations perpendicular to the axis are the same. Along the axis alone is the transmission of the heat, and its further deportment dependent on the plane of polarization. The differences spoken of are greater in the case of polarized than of natural rays, when the plane of polarization in the one case has the same direction as the axis, and in the other, encloses an angle of 90° with it; the differences disappear altogether when the plane of polarization is kept coincident with the crystallographic axis.

IV. *Notes on Mineralogy.*—No. II. *On the Chemical Composition of the Granites of the South-east of Ireland. By the* Rev. Samuel Haughton, *Professor of Geology in the University of Dublin*[*].

THE granites of the south-east of Ireland occur in the counties of Dublin, Carlow, Kilkenny, Wicklow, and Wexford, and may be divided physically into two distinct groups:—

1st. The chain of granite hills extending from Booterstown and Dalkey, co. Dublin, in a N.N.E., S.S.W. direction, to Poulmounty in the south of the county of Carlow, within five miles of New Ross. This granite chain has a length of 68 miles, and a breadth varying from 8 to 15 miles.

2nd. The series of granite hills occurring at intervals in the slate of the counties of Wicklow and Wexford, isolated from each other, and rising like islands through the slate. This group of granite hills lies between the main chain and the sea, and appears to be arranged in lines parallel more or less to the axis of the main chain. These granite hills are about twenty in number, and extend for a distance of 43 miles from Ballinaclash, county Wicklow, to Camarus Hill, county Wexford.

Hitherto, so far as I am aware, no *decisive* proof of difference of geological age has been discovered between these two groups of granites; they are both newer than the Silurian slates, which they penetrate and metamorphose.

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[*] Communicated by the Author.

Such being the state of geological knowledge on this subject, it occurred to me that it might be useful to direct attention to a distinction of a chemical character which appears to exist between these two groups of granites,—a distinction to which I have been led in the course of a series of analyses of Irish granites, in which I have been for some time engaged. The distinction to which I have alluded is the following. The granites of the main chain contain more potash than soda; and *vice versâ,* the granites to the east of the chain, which are isolated from it and from each other, contain more soda than potash; showing that the circumstances, unknown to us, under which the isolated granites were formed, were such as to yield to the molten mass a quantity of soda greater than that possessed by the granites of the principl chain.

In illustration of the foregoing generalization, I offer analyses of granites from the following localities:—

Granitic chain.

1. Dalkey, Co. Dublin.
2. Foxrock, Co. Dublin.
3. Three Rock Mountain, Co. Dublin.
4. Enniskerry, Co. Wicklow.
5. Ballyknocken, Co. Wicklow.
6. Killballyhugh, Co. Carlow.
7. Blackstairs, Co. Wexford.
8. Ballyleigh, Co. Wexford.

Isolated granites.

1. Cushbawn, Co. Wicklow.
2. Croghan Kinshela, Co. Wicklow.
3. Ballymotymore, Co. Wexford.
4. Ballynamuddagh, Co. Wexford.

It will be seen, on reference to the Ordnance Map, or any good map of Ireland, that the localities selected extend from the north to the south of both the granite series; and on reference to the geological maps of Wicklow and Wexford, it may be observed by those unacquainted with the geological structure of this part of Ireland, that the granites of the second group examined are taken from the distinct and distant isolated patches of granite.

In order to investigate the relative proportions of quartz, felspar and mica, of which these granites were composed, I used the following method, which appears to be as simple as any that has been proposed for such a purpose.

Let the per-centage of silica in the granite be divided by the atomic weight of silica, and let the quotient be denoted by a.

Let the per-centages of alumina and peroxide of iron be divided by the atomic weights of alumina and peroxide of iron respectively, and let the sum of the quotients so found be denoted by b.

Let the per-centages of lime, magnesia, potash, and soda be divided by the atomic weights of these elements, and the sum of the quotients called c.

Then, on the hypothesis that the granite is composed exclusively of quartz, felspar, and mica (margarodite), since

$$\begin{aligned}
&\text{Quartz} &&= SiO^3\\
&\text{Felspar} &&= RO,\ SiO^3 + R^2O^3,\ 3SiO^3\\
&\text{Margarodite} &&= RO,\ SiO^3 + 2(R^2O^3,\ SiO^3) + 2HO,
\end{aligned}$$

we find, if Q, F, M denote the number of atoms of quartz, felspar, and margarodite present in the granite, the following relations:—

$$\left.\begin{aligned}
a &= Q + 4F + 3M\\
b &= F + 2M\\
c &= F + M
\end{aligned}\right\} \quad . \; . \; . \; . \; . \; . \; . \quad (1)$$

In these equations, a, b, c are given by the analysis; and from these equations, Q, F, M may be found. Having determined Q, F, M, we can obtain the per-centages corresponding to them by multiplying Q, F, M by their respective atomic weights. The atomic weight of quartz is known, and is 46; but the atomic weights of felspar and mica vary with the relative proportions of the ingredients composing these minerals. Assuming the average of the analyses of micas from this granite range already given by me (Phil. Mag. vol. ix. p. 272), it is easy to infer from it an atomic weight of mica equal to 305. This atomic weight of mica has been used by me in the calculations made in this paper, and the per-centages of felspar found by difference.

The calculations just mentioned do not prove that the granites to which they are applied are composed of quartz, felspar and mica, as combinations of other minerals might equally well represent the analyses; but on the hypothesis that the granites are composed of these three minerals, they give their per-centages with a close degree of approximation: and further, if the equations cannot be satisfied with positive values of Q, F, M, it may be considered as proved that the granite under consideration cannot be simply a compound of quartz, felspar and margarodite.

It is to be observed, that if a granite be composed of four or more minerals, it is not possible to find by the process just described, the per-centages of each mineral, because in this case the number of unknown quantities is greater than that of the equations from which they are to be calculated.

The following Table contains the analyses of eight granites taken from north to south along the principal granite chain, and the notes appended contain a few interesting particulars respecting each granite.

Table I.—Analyses of Granites from principal Chain.

Localities.	Silica.	Alumina.	Peroxide of iron.	Lime.	Magnesia.	Potash.	Soda.	Loss by ignition.	Totals.
1. Dalkey	70·38	12·64	3·16	2·84	0·53	5·90	3·13	1·16	99·74
2. Foxrock.........	73·00	13·64	2·44	1·84	0·11	4·21	3·53	1·20	99·97
3. Three Rock ...	70·28	16·44	2·60	2·04	trace	5·79	2·82	......	99·97
4. Enniskerry ...	74·24	13·64	1·40	1·48	trace	3·95	2·72	1·20	98·63
5. Ballyknocken...	70·82	14·08	3·47	2·65	0·31	4·64	2·31	1·39	99·67
6. Killballyhugh...	73·24	15·45	1·60	0·99	trace	4·59	3·08	1·20	100·15
7. Blackstairs ...	73·20	15·48	1·72	0·96	trace	4·80	3·18	......	99·34
8. Ballyleigh	73·28	12·64	2·00	1·72	trace	4·70	2·97	1·04	98·35

No. 1. *Dalkey.*—Specific gravity, 2·647. A fine-grained granite containing black and transparent mica. This granite cannot be a ternary compound of quartz, felspar and margarodite. This granite was used in the construction of Kingstown Harbour.

No. 2. *Foxrock.*—Specific gravity, 2·638. A coarse granite, which strikes fire under the hammer; it forms a durable and strong building stone, and has been employed in the ring stones of Trinity College belfry, and in the construction of the O'Connell monument at Glasnevin.

No. 3. *Three Rock.*—Specific gravity, 2·652. This granite is rather coarse-grained; it was taken from Woodside Quarry, on the slope of the Three Rock Mountain, and like No. 2, has been used in the construction of the O'Connell monument.

No. 4. *Enniskerry.*—Specific gravity, 2·633. A rather coarse-grained granite, containing veins of black tourmaline.

No. 5. *Ballyknocken.*—Specific gravity, 2·636. This granite is the best building stone in the neighbourhood of Dublin, and has been extensively used in the public buildings of this city; it forms the principal part of the granite used in the belfry and museums of Trinity College. The quarries are situated beyond Blessington, in the county of Wicklow.

No. 6. *Killballyhugh.*—Specific gravity, 2·616. This is a fine-grained granite, and works freely; it has been employed in the construction of the Chapel of Ease in the town of Carlow.

No. 7. *Blackstairs.*—Specific gravity, 2·622. A mediumgrained granite from Kiltealy, on the Wexford slope of Blackstairs.

No. 8. *Ballyleigh.*—Specific gravity, 2·627. A fine-grained granite taken from near Poulmounty Bridge, at the south-west extremity of the granite chain.

Calculating the atomic quotients from Table I., we construct the following Table, containing the values of *a*, *b*, *c*, and of Q, F, M, calculated from equations (1).

Table II.—Atoms of Granitic Minerals.

	Atoms of silex $=a$.	Atoms of peroxides $=b$.	Atoms of protoxides $=c$.	Atoms of quartz.	Atoms of felspar.	Atoms of mica.
Dalkey	1·530	0·286	0·353			
Foxrock	1·587	0·296	0·274	0·513	0·252	0·022
Three Rock	1·528	0·352	0·287	0·445	0·222	0·065
Enniskerry	1·613	0·282	0·225	0·770	0·168	0·057
Ballyknocken	1·540	0·317	0·283	0·442	0·249	0·034
Killballyhugh	1·592	0·321	0·232	0·753	0·143	0·089
Blackstairs	1·587	0·322	0·239	0·714	0·156	0·083
Ballyleigh	1·593	0·271	0·257	0·579	0·243	0·014

Calculating, by the method already described, the per-centages of quartz, felspar and mica, contained in the different granites, we find the following:—

Table III.—Per-centages.

Locality.	Quartz.	Felspar.	Mica.
Dalkey			
Foxrock	23·60	69·66	6·71
Three Rock	20·47	59·68	19·82
Enniskerry	35·42	45·83	17·38
Ballyknocken	20·33	68·97	10·37
Killballyhugh	34·64	38·37	27·14
Blackstairs	32·84	41·19	25·31
Ballyleigh	26·63	67·45	4·27

From the foregoing calculations, it follows, that, with the exception of the Dalkey granite, the granites of the main chain examined might be represented by combinations of quartz, felspar and mica, in which the quartz is the most regular mineral, considered with reference to its per-centage.

The following Table contains analyses of granites belonging to the second group, and supposed to be newer than the others,

Table IV.—Isolated Granites.

Localities.	Silica.	Alumina.	Peroxide of iron.	Lime.	Magnesia.	Potash.	Soda.	Loss by ignition.	Totals.
1. Cushbawn	70·32	11·24	4·80	3·01	0·73	2·27	3·39	1·62	97·38
2. Croghan Kinshela..	80·24	13·24	0·72	0·89	trace	0·40	5·58	......	101·07
3. Ballymoty	66·60	13·26	7·32	3·36	1·22	2·31	3·60	2·34	100·01
4. Ballynamuddagh...	68·56	14·44	5·04	3·85	0·43	2·78	3·36	1·00	99·46

No. 1. *Cushbawn.*—Specific gravity, 2·671. A fine-grained granite, containing hornblende in addition to mica. Besides the constituents given in the table, the specimen examined by me contained 1·34 per cent. of carbonate of lime.

No. 2. *Croghan Kinshela.*—Specific gravity, 2·629. This granite is composed of quartz, felspar and chlorite; the specimen examined appeared to be composed exclusively of quartz and felspar. From the great quantity of soda, I infer that the felspar of Croghan Kinshela is probably albite. On the northern slope of this mountain are situated the old gold streams of Wicklow.

No. 3. *Ballymoty.*—Specific gravity, 2·659. A very fine-grained granite, but containing distinctly felspar, quartz and black mica.

No. 4. *Ballynamuddagh.*—Specific gravity, 2·670. A coarse-grained granite, with large plates of black mica.

Applying to the preceding granites the method of calculation already employed, we find, excluding the granite from Croghan Kinshela, which contains chlorite in place of mica,—

Table V.—Atoms of Granitic Minerals.

	Atoms of silex $=a$.	Atoms of peroxide $=b$.	Atoms of protoxide $=c$.	Atoms of quartz.	Atoms of felspar.	Atoms of mica.
Cushbawn	1·529	0·278	0·300			
Ballymoty	1·448	0·349	0·346	0·067	0·343	0·003
Ballynamuddagh...	1·490	0·344	0·325	0·209	0·306	0·019

From the foregoing Table, it appears that the granite of Cushbawn cannot be considered as a ternary compound of quartz, felspar and margarodite, and in fact it contains a sensible quantity of a mineral which is either hornblende or chlorite. Calculating the per-centages of quartz, felspar, and mica of the two granites, which may be represented as composed of these minerals, we find,—

Table VI.—Per-centages.

	Quartz.	Felspar.	Mica.
Ballymoty	3·08	96·02	0·91
Ballynamuddagh ...	9·62	84·05	5·79

Whatever doubt may attach to the preceding calculations, owing to the hypothesis on which they are of necessity founded, no such doubt or uncertainty can belong to the results of direct experiment contained in Tables I. and IV., which have a positive value, independent altogether of the inferences which may be deduced from them. From these tables I deduce the following as the average composition of the granites of the main chain and of the isolated granites:—

Table VII.—Average Composition of Granites.

	Main chain.	Isolated.
Silica	72·305	71·430
Alumina	14·251	13·045
Peroxide of iron	2·299	4·470
Lime	1·815	2·778
Magnesia	0·119	0·595
Potash	4·822	1·940
Soda	2·967	3·982
Loss by ignition	0·899	1·240
Totals.....................	99·477	99·480

On examining Tables I. and IV., several interesting differences present themselves in the composition of the two groups of granites, in addition to the important difference to which I have drawn attention with respect to their alkaline constituents; but I content myself at present with establishing this fundamental distinction between the two groups, and thus furnishing an additional proof of the service which may be rendered to geological science by the more exact and experimental sciences.

V. *On Liquid Diffusion.*

By Dr. ADOLPH FICK, *Demonstrator of Anatomy, Zürich**.

A FEW years ago Graham published an extensive investigation on the diffusion of salts in water, in which he more especially compared the diffusibility of different salts. It appears to me a matter of regret, however, that in such an exceedingly valuable and extensive investigation, the development of a fundamental law, for the operation of diffusion in a single element of space, was neglected, and I have therefore endeavoured to supply this omission.

It was quite natural to suppose, that this law for the diffusion of a salt in its solvent must be identical with that, according to which the diffusion of heat in a conducting body takes place; upon this law Fourier founded his celebrated theory of heat, and it is the same which Ohm applied with such extraordinary success, to the diffusion of electricity in a conductor. According to this law, the transfer of salt and water occurring in a unit of time, between two elements of space filled with differently concentrated solutions of the same salt, must be, *cæteris paribus*, directly proportional to the difference of concentration, and inversely proportional to the distance of the elements from one another.

In mathematical language this may be thus expressed:—In a volume of salt solution, let the concentration in each horizontal elementary stratum be constant and $=y$, a function of the height x of this stratum above any other stratum which may be assumed as the primary horizontal plane; the limitation being made, that the function y must diminish as x increases, that is, each higher stratum must be less concentrated, and therefore lighter, than all the subjacent ones, because it is only under this condition, that the diffusion will not be interfered with by gravity; then from the stratum between the horizontal planes at x and $x+dx$ (in which the concentration is y) there will pass, during an element of time dt, into the immediate superjacent stratum, bounded by the horizontal planes $x+dx$ and $x+2dx$ (in which the concentration $y+\frac{dy}{dx}dx$ prevails), a quantity of salt $=-\mathrm{Q}\,.\,k\,.\,\frac{dy}{dx}\,dt$, in which Q signifies the surface of the stratum, and k a constant dependent upon the nature of the substances. It is evident that a volume of water equal to that of the salt passes simultaneously out of the upper stratum into the lower.

Exactly according to the model of Fourier's mathematical

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* From Poggendorff's *Annalen*, vol. xciv. p. 59; abstracted and communicated by the Author.

development for a current of heat, we can obtain from this fundamental law for the diffusion-current, the differential equation

$$\frac{\delta y}{\delta t} = -k\left(\frac{\delta^2 y}{\delta x^2} + \frac{1}{Q}\frac{dQ}{dx}\cdot\frac{\delta y}{\delta x}\right), \quad . \quad . \quad . \quad . \quad (1)$$

when the section Q of the vessel in which the current takes place is a function of its height above the bottom. If the section be constant (*i. e.* the vessel cylindrical or prismatic), the differential equation becomes simplified to

$$\frac{\delta y}{\delta t} = -k\frac{\delta^2 y}{\delta x^2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Several methods for the experimental confirmation of this differential equation, and consequently of the fundamental law above advanced, presented themselves. In the first place, by integration of equation (2) the expression $y=f(x, t)$ could be obtained, and the calculated value of y compared with its observed value. I have, however, entirely omitted this method, because even in those cases in which the integral has a finite form, the numerical calculation of a sufficient number of values would have been extremely troublesome, whilst other unequivocal proofs were possible. For the same reason I also here omit to develope the particular integrals of equation (2) for special cases of diffusion-currents.

The experimental proofs just alluded to, consist in the investigation of cases in which the diffusion-current has become stationary, in which a so-called dynamic equilibrium has been produced, *i. e.* when the diffusion-current no longer alters the concentration in the spaces through which it passes, or in other words, in each moment expels from each space-unit as much salt as enters that unit in the same time. In this case the analytical condition is therefore $\frac{dy}{dt}=0$. Such cases can be always produced, if by any means the concentration in two strata be maintained constant. This is most easily attained by cementing the lower end of the vessel filled with solution, and in which the diffusion-current takes place, into a reservoir of salt, so that the section at the lower end is always maintained in a state of perfect saturation by immediate contact with solid salt; the whole being then sunk in a relatively infinitely large reservoir of pure water, the section at the upper end, which passes into pure water, always maintains a concentration $=0$. Now, for a cylindrical vessel, the condition $\frac{dy}{dt}=0$ becomes by virtue of equation (2),

$$0 = \frac{d^2 y}{dx^2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

The integral of this equation $y=ax+b$ contains the following proposition:—"If, in a cylindrical vessel, dynamic equilibrium shall be produced, the differences of concentration of any two pairs of strata must be proportional to the distances of the strata in the two pairs," or in other words, the decrease of concentration must diminish from below upwards as the ordinates of a straight line. Experiment fully confirms this proposition.

For the determination of the decrease of concentration in the cylindrical vessel conducting the diffusion-current, I sunk into the stratum to be estimated a glass bulb suspended from the beam of a balance, and calculated the specific gravity from the weight which required to be placed in the other scale-pan to balance the glass bulb. This method creates little confidence at first sight, nevertheless preliminary experiments showed it to be sufficiently accurate. The quotation of the numerical results of one experiment may suffice here.

Depth of Stratum below the Surface.	10 millims.	32·2	54·4	76·6	98·8	121·0	143·2	165·4	187·6	209·8	220·9.
Specific Gravity of Stratum.*	1·009	1·032	1·053	1·073	1·093	1·115	1·135	1·152	1·170	1·187	1·196.

That the degrees of concentration in the lower layers decrease a little more slowly than in the upper ones, is easily explained by the consideration, that the stationary condition had not been perfectly attained.

A second case of dynamic equilibrium was also observed, by replacing the cylindrical vessel in the above-described arrangement, by a funnel-shaped one with the apex downwards. As the section was now no longer constant, the condition for the dynamic equilibrium was deduced from the more general equation (1) in the form

$$0=\frac{\delta^2 y}{\delta x^2}+\frac{1}{Q}\cdot\frac{dQ}{dx}\cdot\frac{\delta y}{\delta x} \quad . \quad . \quad . \quad . \quad . \quad . \qquad (4)$$

For a perfect cone with circular base (the funnel-shaped vessel), we have $Q=\pi\,.\,\alpha^2 x^2$, if the origin be placed in the apex of the cone, and we call α the tangent of half the angle of aperture. By the substitution of this value, equation (4) becomes

$$0=\frac{d^2 y}{dx^2}+\frac{2}{x}\cdot\frac{dy}{dx},$$

the integral of which $y+c'=-\frac{c}{x}$. The two constants c and c' are to be so determined, that for a certain x (where the cone is cut off and rests upon the salt reservoir), y is equal to perfect

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* The excess of which over unity is proportional to the concentration.

saturation; and for a certain other value of x which corresponds to the base of the funnel, y becomes $=0$. In an experiment made with this apparatus, the following numerical values were obtained:—

Depth of Stratum below the Surface.

27·7 millims.	55·5	72·1	88·8	105·4	122·1	138·7	155·4.

Observed excess of Specific Gravity above unity (proportional to the concentration).

0·000 millim.	0·008	0·019	0·030	0·010	0·055	0·075	0·105.

Calculated excess of Specific Gravity above unity.

0·006 millim.	0·015	0·023	0·031	0·043	0·057	0·078	0·107.

The determination of the constants for different salts could now be proceeded with,—a determination which measures their diffusibility, and is expressed by k in equation (1). It may be defined as the quantity of salt which, during a unit of time, passes through the sectional-unit, out of one stratum into the next adjacent one, when the rapidity of the diminution of concentration $\left(\frac{dy}{dx}\right)$ is equal to unity. Let the sectional-unit be the section of our tubes, *i. e.* the surface of a circle of 1 millim. radius. The diminution of concentration $\left(\frac{dy}{dx}\right)$ will then be equal to unity, if, through a column of liquid assumed permanently constant, and the height of which is equal to the linear-unit 1 millim., it has for its result a difference of concentration of both terminal surfaces of such a character, that the one possesses a concentration corresponding to absolute saturation, the other a concentration $=0$. Let one day be a unit of time.

Three tubes of different lengths were now arranged exactly as the cylinder and funnel above described, and after the establishment of the stationary condition, the amount of salt was determined which diffused out of the terminal section of the tube in measured spaces of time, and which therefore also, in the same time, passed through any single section of the cylindrical tube. In this case the whole of the values required for the determination of k were known, and the rapidity of the diminution of concentration $\left(\frac{dy}{dx}\right)$ was especially determinable, inasmuch as it produced permanently, throughout the entire length of the tube, the difference between perfect saturation below and 0 above.

It is easy to perceive, that if our law be correct, the amounts of salt, passed through the three tubes in the same time, must be inversely proportional to the length of the tubes; and that if these amounts be divided by the time and multiplied by the length (expressed in millimetres), the same value, viz. the above-defined k, must be obtained for all three tubes. I here annex a short table of the best experiments with common salt, to which I have hitherto confined my attention:—

Temperature Centigrade during the operation.	k, calculated from the quantity which passed through the *longest* tube.	k, calculated from the quantity which passed through the *medium* tube.	k, calculated from the quantity which passed through the *shortest* tube.
Not determined.	11·71	12·36	11·08
15°·8 — 14°·8	9·67	9·70	9·30
15°·5 — 16°·0	......	9·57	......
16°·0 — 16°·5	......	9·94	......
17°·5 — 18°·5	10·79	......	......
18°·0 — 19°·0	10·71	11·08	10·50
20°	11·14	......	11·02
19°·0 — 22°·0	11·44	11·33	......
20°·0 — 21°·0	11·89	......	11·12

If we take into consideration the unavoidable sources of error, a nearer identity of numbers could scarcely be expected. The table shows, as might be already expected from Graham's experiments, that the value k increases with increase of temperature; probably, however, this dependence upon temperature is not a simple one. On the relations of k to other values expressing the essential properties of bodies, as, for instance, to atomic weight, nothing can be said until extensive series of experiments with different substances have been made. It is now of importance to make the views thus obtained furnish an insight into the diffusion of salt solutions through porous partitions. Brücke* first made an attempt to give a mechanical explanation to these phænomena. He went upon the assumption that the substance of the partition attracted the particles of water more strongly than the particles of salt. This assumption gained important support from the experiments of Ludwig† and Cloetta‡, who found that the solution of salt, imbibed by an animal membrane, was richer in water, and poorer in salt, than the solution in which the membrane was immersed. Brücke's theory, which was only indeed obscurely intimated, can be more clearly determined by the help of our fundamental law as follows.

Let us imagine a cylindrical pore of the radius ρ, in a membrane which is immersed in a saturated solution of a salt, and let us assume with Brücke, a stronger attraction between water and the molecules of the membrane, than between the latter and the salt molecules; then the density of the solution in each con-

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* Poggendorff's *Annalen*, vol. lviii. p. 77. † Ibid. vol. lxxviii. p. 307.

‡ Experiments on Diffusion through Membranes with two Salts. Zürich, 1851.

centric cylindrical film of radius r, of the porous partition will evidently be constant, and $=f(\rho-r)$. Respecting the nature of this function, it can only be said that its value for $r=\rho$, which is equal to 0, and that from this point it must increase (probably very rapidly) with the decrease of r; but if necessarily under all circumstances for $r=0$, it must be increased up to the density of the circumambient solution, is left undetermined, and appears to me, in fact, for very narrow pores very improbable. In a word, the density in the pore will increase from the wall towards the centre, and in the cylindrical film of the radius r there can certainly occur no higher concentration than $f(\rho-r)$ (but possibly a lower one). The radius ρ of the pores must be imagined so small, that a filtration of liquid, by virtue of their cohesion, together with the attraction of the membrane itself, is prevented, even under high pressure; and that also equalization of the differences of pressure when liquid is present on both sides of the porous membrane, if possible at all, is only so in a relatively long space of time; further, that currents of mixture, by virtue of the variations of specific gravity within the pore, cannot occur.

Let us now suppose that a membrane of this description separates a saturated solution of salt from pure water, and further, that the former is above, and the latter under this horizontal membrane. In any cylindrical pore of the said membrane, whose radius may be again $=\rho$, let us suppose a concentric elementary stratum, bounded by two cylindrical films infinitely near to each other, the radii of which are respectively r and $r+dr$. At the upper extremity, the highest possible concentration, $f(\rho-r)$, of the layer will immediately be produced. On the other hand, the lower extremity, by immediate contact with a relatively infinite quantity of pure water, will be constantly maintained at a concentration $=0$; and when a stationary condition is brought about, the degrees of concentration, within the assumed elementary stratum, must increase from 0 to $f(\rho-r)$, in proportion to the height above the lower boundary surface of the membrane. This arrangement would have the effect of producing a diffusion-current in the elementary stratum, which, according to our laws, would deliver a quantity of salt $2\pi\,.\,k\,.\,\frac{f(\rho-r)}{h}\,dr$ downwards, and an equal volume of water upwards, if we understand by k, as above, the diffusion-constant for the combination of salt and water in question, and by h the thickness of the membrane, consequently the length of the pore. No regard has been paid, however, to the retardation which the flowing molecules suffer from the attraction of the substance of the membrane along the sides of the pores; it is certain, however, that the tötal quantity of salt, passed through the entire pore, could not be greater than

$$2\pi\frac{k}{h}\int_0^\rho f(\rho-r)dr.$$

But the passage of the water to the other side requires a separate consideration. We have seen, namely, that at the upper extremity of the cylindrical stratum-unit, with the internal radius r, no higher concentration could take place than $f(\rho-r)$, which is certainly less than perfect saturation, and, in fact, becomes proportionally smaller the greater r is assumed to be. If, as we suppose, a relatively inexhaustible volume of saturated solution (obtained by the addition of crystals) be present on the upper side of the membrane, then, at the upper extremity of our elementary stratum, there must take place a sudden increase in the concentration, from $f(\rho-r)$ to perfect saturation. If we assume that this is the case for the first moment, there will now be, according to the general principles of diffusion, from the elementary stratum a relatively (in comparison with the amount which a continual transition of density requires) infinite quantity of water required, and an equally infinite amount of salt forced in. The latter will be inevitably hindered by the nature of the membrane, and the excess of salt forced against the pore must in some way glide off laterally; on the other hand, more water, than the arrangement of the densities in our elementary stratum requires, can easily, to a certain extent, be drawn through towards the denser solution, so that in the pore, the particles of water move upwards with a greater velocity than the particles of salt move downwards. The excess of water now spreads out on all sides, into the saturated solution (as the mouths of the pores must lie at a certain distance from each other), partly by diffusion, partly by mixing streams proceeding from difference of specific gravity; until a stationary condition has been in such a way produced, that a conical space increasing upwards is supported upon the upper annular section of the elementary stratum, in which space the concentration $f(\rho-r)$ increases to perfect saturation, and which determines a diffusion-current of such a strength, that thereby exactly as much water is passed upwards, as in the same time can diffuse itself into the reservoir of saturated solution from the upper end of the space, without changing the concentration. Then the above-mentioned space would evidently be immediately lengthened upwards, (and thereby the intensity of the diffusion-current be diminished) so soon as more water passed through, and therefore the concentration at the upper end of the space continues to vary; and if, on the contrary, less water passed upwards, sudden transition of concentration must immediately occur in certain places, which sudden changes determine, as it were, a diffusion-current of infinite strength, and therefore again immediately draw the requisite quantity of water. Now this quantity of water, which, *cæteris paribus,* during the unit of time, can diffuse itself into the saturated solution without sensibly altering the concentration at that point, depends upon the easy mobility of the particles of the solution. The space in which the compensation takes place must therefore also be, *cæteris paribus,* so much shorter, and consequently the stream of water towards the saturated solution stronger, the more easily mobile the particles of the solution are. I was unable to verify this theoretical conclusion by direct experiment. I expected that the excess of water passing through the membrane, above the quantity of salt passing in the contrary direction, must be found much smaller, when the mobility of the upper solution was impeded by the addition of chalk magma. No such diminution of this excess, however, could be remarked in the experiment.

A second conclusion from the hypothesis just developed can be thus expressed: the excess of the diffusing water, above the salt passing in the opposite direction, is smaller, the wider the pores of the separating partition. Or if, according to Folly's* method, we define, as the endosmotic equivalent, the quotient obtained by dividing the amount of water diffused by the quantity of salt contemporaneously passed, the endosmotic equivalent is smaller the larger the pores of the partition. This conclusion is confirmed by experiment. Two membranes, which differ only in the diameter of their pores, cannot, it is true, be obtained; nevertheless we are safely warranted in assuming, that a transparent film of collodion possesses pores so very much narrower, than an animal membrane formed of interlacing fibres, that all other differences between the two tissues influencing diffusion, disappear in this difference of the size of pores; in fact, an animal membrane was found to possess a very much smaller endosmotic equivalent than a collodion film; for whilst, by the use of pig's bladder, only from 4 to 6 times as much water as common salt passed through, there diffused through a collodion film, during the time that an imponderable trace of chloride of sodium passed, a considerable quantity of water, probably many thousand times greater than that of salt.

Let us now suppose, that, instead of pure water, a solution of a certain concentration c of the same salt which is present on the upper side of the membrane in concentrated solution, be placed on the under side of that membrane, then the following considerations present themselves. All the elementary strata, from the wall of the pore, to an imaginary cylindrical film of the radius r, that $f(\rho-r)=c$, can only be filled from the top to the bottom

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* Poggendorff's *Annalen,* vol. lxxviii. p. 361.

with solution of the there *highest* possible concentration, and cannot therefore give origin to a usual double diffusion-current. Water will, however, pass over from the lighter to the denser solution, because a force of suction comes into play on each side of the membrane, proportional to the difference of concentration, consequently a stronger force at the upper side corresponding to the saturated solution. On the other hand, all those strata lying nearer to the axis, behave exactly in the above manner, with the single difference, that in them the concentration, instead of increasing from 0, now increases from below upwards from c to the respective maxima, and consequently an absolutely weaker diffusion-current is produced. It therefore follows in this case, that in places where, in the first instance, salt passed to the one side and water to the other, only a single diffusion of water takes place, consequently the proportion between water and salt (the endosmotic equivalent) must be *greater* here than there. If, for instance, $c > (\rho - r)$ (which is possible in the case of very fine pores), *no salt whatever* could pass through; the endosmotic equivalent would be $= \infty$, or the current one-sided.

This conclusion is also confirmed by experiment. Even the above-cited experiments of Ludwig and Cloetta intimate this to be the case. I have myself obtained still more decided results. A membrane which gave an equivalent between 5 and 6, when it separated a saturated solution from pure water, yielded an equivalent $=11{\cdot}05$, in another case even $=17{\cdot}05$, when the pure water was replaced by a solution of common salt of 22 per cent. Lastly, if we analyse the case in which, instead of a saturated solution, a dilute one be placed above, and again pure water below. Let us again express the concentration of the upper solution by c. Evidently in an axial cylinder whose radius r is so great, as exactly to make $f(\rho - r) = c$, the concentration from the lower to the upper end, will now regularly increase from 0 to c, and a usual diffusion-current take place, which will pass a volume of salt downwards, exactly equal to the volume of water propelled upwards. Only in those layers situated nearer to the wall, can the concentration at the upper end never rise to c, and there will therefore pass through these layers alone, according to the previous considerations, more water than salt. The radius r, of the above-mentioned axial cylinder, is evidently as much greater as c is smaller, therefore the endosmotic equivalent must also decrease very rapidly with c; and we might expect that, for moderately small values of c, the endosmotic equivalent would be found smaller than unity (because a volume of salt weighs more than an equal volume of water). Ludwig's experiments with common salt are perfectly in accordance with this conclusion; in some cases in his experiments, the equivalent sunk from 4 to 1 when the concentration of the upper solution was decreased from saturation to 2 per cent. My own experiments in the same direction with common salt gave a purely negative result, as the equivalent was not perceptibly decreased.

The comparison of the experiments adduced above with the hypothesis developed on the foundation of the diffusion law, shows, though not absolutely, that the truth of this hypothesis may be determined; and it is in fact highly probable that, with or without modification, such an hypothesis may serve as the foundation of a subsequent theory of these very dark phænomena.

VI. *Analysis of Lunnite from Cornwall.*

By Dr. Heddle*.

THIS mineral was sent me for analysis by Mr. Greg, under the hope that it might prove to be a new *phosphate of copper*. It is doubtful from which of the Cornish mines it was procured, and the specimen in Mr. Greg's collection is, as far as is known, the only one which was found.

The specimen consists of aggregated congeries of minute spheres, the structure of which is radiated. The specific gravity is 4·25; little dependence can, however, be placed upon this determination, as it was found impossible to free the specimen from air, and minute crystals of *quartz* were imbedded in the mass.

The powdered mineral absorbs ·28 per cent. of moisture.

Analysis of 34 grs.:—

Oxide of copper . .	68·13
Phosphoric acid . .	22·73
Water	8·51
Silica	·48
	99·85

The silica was mechanically mixed.

This substance occurs in few localities. By this analysis a new one is given, and the mineral is now for the first time noticed as a British species.

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* Communicated by the Author.

VII. *On the Spontaneous Decomposition of certain Sulphomethylates.* *By* Arthur H. Church, *Esq.*[*]

IN the latter part of June 1853, I prepared a quantity of sulphomethylic acid, and neutralizing the mixture of sulphuric and sulphomethylic acids with finely-powdered native carbonate of baryta of unusually excellent appearance, expected to obtain sulphomethylate of baryta not far from pure; but the carbonate of baryta was contaminated with a considerable amount of carbonate of lime, consequently the product was a mixture of sulphomethylate of baryta with the corresponding lime-salt: that this was the case I did not discover, however, until the present year. The mixed salts, crystallized, and dried between bibulous paper, were set aside and forgotten until April last. The weight of the salts was not less than 380 grms. The general formula of the sulphomethylates, dried as just mentioned, is $C^2H^3M\,2SO^4+2aq$. The loosely-stoppered bottle containing the salts had been exposed in diffused daylight for a period of twenty-two months before it was disturbed. At the end of this time complete decomposition had taken place, the bottle no longer contained a trace of the original salts. At first three-fourths, the bottle was now little more than half full; the upper portion of its contents was a yellowish liquid of disagreeable odour and acid reaction; the lower was a white, apparently amorphous solid, but on this last-mentioned substance there rested a crystalline crust, and from this numerous distinct crystals sprang, their bases remote from the light, and their summits inclined towards it.

And first of these crystals, which were perfectly transparent and colourless. Their hardness exceeded 1°·5, but was less than 2°. The specific gravity of different specimens varied between 2·21 and 2·325. One of the largest crystals was 61 millims. in length and 2·5 millims. in breadth. The crystals belong to the oblique prismatic system, and exhibit many interesting modifications; their behaviour in polarized light was the same as that of selenite. In some aspects their lustre is pearly, in others vitreous. A qualitative examination indicated the presence of calcium, sulphuric acid, and water; while the quantitative estimation of these constituents in the crystals dried over sulphuric acid gave me the following numbers:—

·5015 grm. furnished ·2902 grm. of $Ca\,CO^3$.
·3031 grm. furnished ·41 grm. of $Ba\,SO^4$.
·2448 grm. lost on ignition ·0514 grm. of HO.

These results correspond with the following per-centage composition:—

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[*] Communicated by the Author.

	Experiment.	$Ca\,SO^4 + 2aq.$
Ca	23·150	23·256
SO^4	55·724	55·814
HO	21·000	20·930
	99·874	100·000

The crystals were not altered by an exposure of two hours to a temperature of 100° C.; heated a few degrees above this point they lose water, becoming milk-white.

The white amorphous powder filling the lower part of the bottle was sulphate of baryta, but occurring here and there in it were botryoidal masses of granular sulphate of lime. An analysis of this sulphate gave numbers approaching somewhat closely to the formula $2Ca\,SO^4 + aq$; a sulphate of lime having this formula has been observed occurring as a grayish crystalline deposit from a boiler working under a pressure of two atmospheres*.

The crystalline crust from which rose the prismatic crystals already described, was also sulphate of lime, of the formula $Ca\,SO^4 + 2aq$.

The yellow fluid previously mentioned was thoroughly examined. Its reaction was strongly acid: on the addition of pure precipitated carbonate of baryta, an escape of carbonic acid took place and a heavy powder was precipitated, this was separated by filtration and proved to be sulphate of baryta. The filtrate, now perfectly neutral, was introduced into a retort and distilled; water having a slightly aromatic odour collected in the receiver; the residue in the retort solidified on cooling to a mass of colourless crystals. From this substance, recrystallized by means of alcohol, dissolved in the smallest possible quantity of water, and separated again by spontaneous evaporation of the water, the following analytical result was obtained, the crystals having been dried between folds of bibulous paper:—·415 grm. of salt gave ·2445 grm. of sulphate of baryta, corresponding to 58·91 per cent. of $Ba\,SO^4$; theory, as $C^2\,H^3\,Ba\,2SO^4 + 2aq$, requires 59·02. A second analysis, for which a portion of the salt dried at 100° C. was employed, gave me the following numbers:—·5605 grm. of salt furnished ·3637 grm. of sulphate of baryta, corresponding to 64·88 per cent. of $BaSO^4$; theory, as $C^2H^3Ba2SO^4$, requires 64·9.

The saturated solution of this salt is not altered by prolonged ebullition, and the crystals may be heated above 100° C. without becoming acid or otherwise decomposing.

The final products of the spontaneous decomposition occurring

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* Johnston, Phil. Mag. vol. xiii. p. 325.

in the mixture of the sulphomethylates of lime and baryta*, are thus well-crystallized sulphate of lime, sulphate of baryta, water, traces of an oily odorous body, with free sulphuric and sulphomethylic acids, the baryta salt of this last acid differing in many respects from the ordinary sulphomethylate of baryta although isomeric with it; I term it provisionally β-sulphomethylate of baryta.

This baryta salt evidently corresponds in the methyle series with the stable sulphæthylate (parathionate) of Gerhardt†, obtained from ordinary sulphæthylate of baryta by repeated ebullition and neutralization with carbonate of baryta of its concentrated aqueous solution, and which more recently has been identified by Berthelot‡ with the sulphæthylate of baryta obtained by saturating with carbonate of baryta the mixture of 4 vols. sulphuric acid and 1 vol. alcohol, which has been employed in making olefiant gas, or the solution of that gas in oil of vitriol. My opinion is also confirmed by the following experiments:—if to a strong solution of ordinary sulphæthylate or sulphomethylate of baryta a solution of protonitrate of mercury be added, a gray or slaty precipitate occurs—with the parathionate of Gerhardt, with the corresponding methyle compound which may be obtained by the same method§, and with the β-sulphomethylate of baryta the precipitate is white. The two last-mentioned salts are in fact in every respect identical.

If the analytical results did not forbid such a view, it might have been imagined that the modified or stable baryta salt was really not the sulphomethylate, but the hyposulphomethylate of baryta, for an aqueous solution of this latter salt, or indeed of any of its analogues and homologues, is quite unalterable. Sulphomethylic acid bears the same relation to hyposulphomethylic acid as sulphophenic acid does to sulphobenzolic acid, and it may not be uninteresting to present in a tabular form some relations between these two series. In the first column will be found the formulæ of a series of baryta salts, the aqueous solutions of which are not alterable by ebullition—a series where the proportion of sulphur to oxygen in the dry salts is as 2 to 6;

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* For an account of the spontaneous decomposition of certain sulphovinates, see Marchand in the *Journal für Praktische Chemie*, xliv. 122.

† *Compt. Rend. des travaux de Chimie*, 1845, p. 176.

‡ *Annales de Chimie et de Physique*, 1855, p. 392.

§ In 1853, at the suggestion and under the direction of Dr. Hofmann, I prepared this salt and submitted it to a complete analysis. It is identical in composition with ordinary sulphomethylate of baryta $C^2 H^3 Ba 2SO^4 + 2aq$. Although it is only recently that the metamorphoses of the sulphæthylates have been accurately studied, it would appear to have long been known that aqueous solutions of the sulphomethylates undergo by ebullition similar changes, and yield products exactly analogous to those of the æthyle salts.

in the second column another series is given of salts decomposable by ebullition of their aqueous solutions, with separation of sulphate of baryta, and formation (in most cases) of new acids, and in which the proportion of sulphur to oxygen is as 2 to 8: the observations were made in all cases with baryta salts; where an asterisk is prefixed, the experiment was either made for the first time or repeated by myself.

I.	II.
*$C^2 H^3 Ba\,2SO^3$. (Hyposulphomethylate of baryta.)	*$C^2 H^3 Ba\,2SO^4$. (Sulphomethylate of baryta.)
*$C^{12} H^5 Ba\,2SO^3$. (Sulphobenzolate of baryta.)	‡*$C^4 H^5 Ba\,2SO^4$. (Sulphæthylate of baryta.)
*$C^{14} H^7 Ba\,2SO^3$. (Sulphotoluolate of baryta.)	§$C^6 H^7 Ba\,2SO^4$. (Sulphopropylate of baryta.)
*$C^{16} H^9 Ba\,2SO^3$. (Sulphoxylolate of baryta.)	*$C^{10} H^{11} Ba\,2SO^4$. (Sulphamylate of baryta.)
†$C^{18} H^{11} Ba\,2SO^3$. (Sulphocumolate of baryta.)	*$C^{12} H^5 Ba\,2SO^4$. (Sulphophenate of baryta.)
†*$C^{20} H^{13} Ba\,2SO^3$. (Sulphocymolate of baryta.)	*$C^{14} H^7 Ba\,2SO^4$. (Sulphotoluenylate of baryta.)
*$C^{20} H^7 Ba\,2SO^3$. (Sulphonaphthalate of baryta.)	

The experiment might no doubt have been tried with many other salts of similar constitution with analogous results.

In column II., by the ebullition of solutions of the salts 1 and 2, modified stable salts may be obtained; a similar action probably takes place with salts 3 and 4, for the solution of ordinary sulphamylate of baryta gives a blue-gray precipitate with protonitrate of mercury, while the salt obtained by boiling, &c. the solution of sulphamylate of baryta gives a white precipitate. The metamorphosis of sulphamylate of baryta takes place readily on heating in an oil-bath the solution of this salt enclosed in a sealed tube; with sulphophenate of baryta under similar conditions, regeneration of phenylic alcohol takes place:

$$C^{12} H^5 Ba\,2SO^4 + 2HO = C^{12} H^6 O^2 + Ba\,SO^4 + HSO^4.$$

The relations of æthyle, &c. to phenyle are very remarkable. The consideration of these analogies is likely to lead to many interesting results; for instance, phenate of ammonia, when heated in a closed tube, is well known to be resolved into aniline and water, and I have found that an exactly analogous reaction

† Gerhardt and Cahours, Delalande. ‡ Gerhardt. § Berthelot.

takes place with æthylate of ammonia; the following equations represent these changes:—

$$C^{12}H^5NH^4O^2 = C^{12}H^7N + 2HO$$
$$C^4H^5NH^4O^2 = C^4H^7N + 2HO.$$

Æthylate of ammonia may be prepared by the action of sulphate of ammonia upon potassium-alcohol. Also we might hope to obtain sulphobenzolic acid, $C^{12}H^6 2SO^3$, by the action of nitric acid on sulphocyanide of phenyle, just as hyposulphæthylic acid is obtained from the corresponding æthyle term. A more extended study too of sulphophenic acid and its derivatives would be of great interest. The action of sulphæthylate of silver upon iodide of methyle, and of sulphomethylate of silver upon iodide of æthyle, gives rise to products now under investigation, products which no doubt have their representatives in the phenyle series. A digestion of sulphæthylate of silver with iodide of æthyle appears to yield an oily substance, probably sulphæthylate of æthyle (sulphate of æthyle), to the presence of which in sulphæthylates undergoing decomposition, their peculiar odour is perhaps to be attributed, and from which by further change parathionic acid may be derived.

June 1855.

VIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. ix. p. 544.]

March 22, 1855.—The Lord Wrottesley, President, in the Chair.

THE following communications were read:—

"On the Anatomy of *Nautilus umbilicatus*, compared with that of *Nautilus Pompilius*." By John Denis Macdonald, Esq., R.N.

During a visit of H.M.S.V. 'Torch' to the Isle of Pines in July 1854, a recent specimen of *Nautilus umbilicatus* was picked up on the outer reef off Observatory Island. It was alive when brought on board, but was too much exhausted to exhibit active movements. Part of the hood appeared to have been eaten away behind by some predaceous enemy, but in other respects the animal was perfect.

The body when retracted lay more deeply in the shell than that of *N. Pompilius*, so that no part was visible in a lateral view, and on account of the great depth of the chamber of occupation the orifice of the siphuncle in the last septum could not be seen when the soft parts were removed. As to this difference, however, the author observes that it may depend on the time elapsed since the formation of the last partition.

Apart from the shells, the author finds a close resemblance between the corresponding parts of the two species.

The specimen of *N. umbilicatus* examined proved to be a female;

a fact which may serve to modify the views of those who, adopting the speculations of D'Orbigny on the sexes of the Ammonites as indicated by the characters of their shells, apply them also to the several kinds of *Nautili* known.

The body of *N. umbilicatus* is larger and more elongated than that of *N. Pompilius* as it occurs in the South Seas, although the specimens of the latter species brought from the Chinese Seas much exceed both in size. In the *N. umbilicatus*, the longitudinal lamellæ on the median lobe of the external labial processes are divided by a wide groove into two lateral sets, and the corresponding lamellæ between the internal labial processes are about seventeen in number and of considerable thickness. In *N. Pompilius*, the latter lamellæ are much thinner and more numerous, and the lateral sets of the former are united together in the median line, commencing anteriorly with an azygos transverse lamina. In both kinds, however, the corresponding tentacula may be distinctly traced out, with only such minor differences as might be expected to occur in different specimens of either separately; the digital, labial and ocular groups agreeing sufficiently both as to number and character in the two cases, considering the liability of these parts to slight modifications, from arrest of development or redundance, in the same species.

Referring to former observations of his own on the eye of *N. Pompilius*, the author observes that they closely apply to *N. umbilicatus*, which affords confirmation of his opinion that the pigmentary coating is subjacent to the retina. He finds no vestige of a lens, and in place of vitreous humour, a mere viscous matter protecting the retina from the sea-water.

The organ of hearing, which had escaped detection in the specimen of *N. Pompilius* dissected by Professor Owen, altered as it doubtless had been by long immersion in spirit, was discovered in the example of *N. umbilicatus* examined by the author. It consists of two spheroidal acoustic capsules placed, one on each side, at the union of the supra- and subœsophageal ganglia, and measuring about one-twelfth of an inch in diameter. Each capsule rests internally against the nervous mass, and is received on its outer side into a little depression in the cephalic cartilage. It is enveloped in a kind of fibrous tissue and filled with a cretaceous pulp consisting of minute, elliptical, otoconial particles, presenting under a high power a bright point near each end, varying much in size, and sometimes combined into stellate, cruciform or other figures. Cilia were not observed within the capsules.

The inside of the mouth is furnished with three groups of papillæ, one of which occupies the median line between the orifice of the tongue-sac and commencement of the œsophagus. These lingual papillæ, as well as the rest, are clothed with long and slender columnar epithelium-particles.

The author agrees with Mayer in regarding the well-known follicular appendages of the afferent branchial vessels of the Cephalopoda, as performing the function of kidneys, but admits that they may also serve, by altering their capacity, to regulate the amount of blood passing through the branchiæ under changes of pressure to which the animal may be subjected at different depths. These follicles are subcylindrical in form, and somewhat dilated at the free extremity, to which is appended a folded and funnel-shaped process of membrane which expands rather suddenly and presents a jagged border. They open by an oval or slit-like orifice into the afferent branchial vessels, on each of which, as Professor Owen has observed, they are disposed in three clusters. The outer membrane is smooth and glossy, homogeneous in structure, and sprinkled over with minute, rounded, transparent bodies, resembling the nuclei of cells. Beneath this layer, flat bundles of fibres, apparently muscular, are traceable here and there, principally disposed in a longitudinal direction, and sometimes branched. The lining membrane consists of a loose epithelial pavement, similar in many respects to that of the uriniferous tubules of the higher animals, the cells containing, besides the nuclei, numerous minute oil-globules, or a substance much resembling concrete fatty matter. This membrane is thrown up into very numerous papillæ and corrugations, so as greatly to increase the extent of surface. The papillæ are more numerous towards the attached end, and a circlet of longitudinal folds, with transverse zigzag corrugations, radiate from the bottom of the follicle, in which a number of small pits or fenestrations are sometimes visible. The funnel-shaped membranous process above noticed is continuous with the lining membrane. The cavity of each follicle, therefore, communicates with the exterior through the centre of this process, and the aperture is thus guarded by a kind of circular valve permitting the escape of secreted matters, but effectually preventing the entrance of fluid from without.

Some considerations are next offered in support of the view adopted as to the functions of these vascular appendages.

Lastly, on the question whether the peculiarities of structure recognized respectively in *N. Pompilius* and *N. umbilicatus* are sufficient to establish a difference of species, or are attributable merely to variety, the author observes, that any tendency in a being to revert to an original type, when such has been determined, betrays variety; but this tendency is never manifested in the *Nautili* under consideration by the occasional occurrence of specimens presenting characters which place them intermediately between *N. Pompilius* and *N. umbilicatus*. Having visited the Fijii Islands since he formerly wrote on *N. Pompilius*, he finds that the umbilicated *Nautili* are not known to the natives, although *N. Pompilius* is very plentiful; but at Fatuna or Wallis's Island, where both are found, the people recognize the difference between them depending on the presence or absence of umbilical pits. On this the author remarks, that although particular localities, with all attending circumstances, may favour the production of varieties, yet the permanence of the distinctive characters of these *Nautili* without symptom of amalgamation, and the discovery of a female specimen of *N. umbilicatus*, are strong arguments in support of the view that they are distinct species, though very closely allied.

"On a Class of Differential Equations, including those which occur in Dynamical Problems."—Part. II. By W. F. Donkin, M.A., F.R.S., F.R.A.S. &c.

This is the second and concluding part of a paper of which the first part was printed in the Philosophical Transactions for 1854. In the fourth section (the first of this part) some of the most important results of the former part are recapitulated.

In the fifth section the theory of the Variation of Elements is considered under that aspect which belongs to it in connexion with the general methods of this paper; and the facility of its application is shown in two instances: (1) the expressions for the variations of the elliptic elements of a disturbed planet's orbit are deduced from the results of Art. 30 (Part I.), on undisturbed elliptic motion; (2) the problem of determining the motion of a free simple pendulum (omitting the effect of the earth's rotation) is treated by considering the orbit of the projection of the bob upon a horizontal plane as a disturbed ellipse. The differential equations which define the variations of the elements of the ellipse are given in a rigorous form, and integrated approximately so as to give the motion of the apsides of the mean ellipse in any case where the pendulum never deviates much from the vertical, and the motion is not very nearly circular. The result agrees with the conclusions of the Astronomer Royal (Proceedings of the Royal Astronomical Society, vol. xi. p. 160).

In the fifth section the transformation of the differential equations by the substitution of new variables is considered, and particularly that kind of transformation, called by the author a *normal transformation,* which leads to a new system of equations, not merely possessing the same general form as the old, but distinguished also by other common properties. A definition is given of those transformations which may be properly called, from analogy, *transformations of coordinates,* and it is shown that all transformations of coordinates are *normal.* General formulæ are given for transforming the equations of any dynamical problem from *fixed* or *moving* systems of axes of coordinates; and an illustration is drawn from the case of the motion of a planet referred to axes in the varying plane of its own orbit.

In the seventh and last section the principles of transformation developed in the preceding section are applied in a more general manner to the differential equations of the planetary theory; and it is shown that when the motions of a planetary system are referred to a system of rectangular axes having their origin in the sun, and otherwise moving in any arbitrary manner, the variations of the elements will still be determined by the same formulæ as if the axes were fixed, provided there be added to the disturbing function R, for each planet, the expression

$$\sqrt{\mu a(\iota-e^2)}\,.\,(\omega_0 \sin \nu \sin \iota-\omega_1 \cos \nu \sin \iota+\omega_2 \cos \iota),$$

in which ι is the inclination of the orbit to the (moving) plane of xy, the longitude of the node reckoned from the axis of x, and ω_0, ω_1, ω_2; the angular velocities of the moving system of axes about the three axes themselves. In this expression $\omega_0, \omega_1, \omega_2$ may be any arbitrary functions either of the time or of the elements; but in any case these functions are to be exempted from differentiation with respect to the elements in taking the partial differential coefficients of the disturbing function.

This result is illustrated by referring the motion of a system consisting of *two* planets to axes so chosen that the plane of xy shall always coincide with the *principal plane* of the system, and the axis of x, from which all longitudes are reckoned, shall always coincide with the line of nodes; there are thus obtained twelve rigorous simultaneous differential equations, of which nine form a system apart, containing only the major axes, excentricities, epochs, longitudes of perihelia, and mutual inclination of the two orbits, and afford an example of the so-called "elimination of the nodes;" whilst the remaining three (which contain also these nine elements, but not their differential coefficients) determine the motion of the principal plane and of the line of nodes, relatively to fixed space. The mutual inclination of the two orbits being supposed known, their several inclinations to the principal plane are given by simple relations; and the positions of the planets in their orbits (their longitudes reckoned from the line of nodes) being supposed known, their motions relatively to fixed space would thus be completely determined.

Extract of a Letter, dated January 6, 1855, from J. Mitchell, Esq., Quartermaster of Artillery, Bangalore, "On the Influence of Local Altitude on the Burning of the Fuses of Shells."

"In the early part of the year 1848, at the annual practice of the Artillery in this garrison, it was observed that the fuses burned too long a time. The regular burning of fuses being a matter of much importance, the circumstance was duly reported to Artillery Head Quarters, and a portion of each kind was directed to be sent to St. Thomas's Mount (eight miles from Madras and on the same level) for examination, where they were found to burn correctly, and as at that time no one suspected the real cause of the discrepance, it was concluded there had been some error in the length of our pendulum, and there for the time the matter rested.

"But as I was satisfied, for many reasons, that it was not owing to an error in the pendulum, I determined to keep the subject in mind, and for the practice in the following year (1849) I caused to be made an adjustable pendulum which beat seconds very correctly for several minutes. This pendulum was daily compared with two or more seconds' watches to make all safe, and, as our longest time of flight did not exceed twenty seconds, no error could possibly arise from this source. The result was that the fuses again burnt too long at Bangalore, and were again found to burn correctly at St. Thomas's Mount.

"This was a mystery to all; but after the matter had cost me much thought, it occurred to me that the cause was to be sought in the difference of altitude between St. Thomas's Mount and Bangalore, nearly 3000 feet; and as a means of putting this to the test, I suggested that some fuses should be burnt at the Mount, or at Madras, under a receiver, exhausted until the barometer stood at the Bangalore mean height, or about 27 inches. This, however, it was not found convenient to do; but, as an equally satisfactory way of testing the accuracy of my conclusions, a small number of fuses were prepared and burnt at St. Thomas's Mount, at Bangalore, and at two different altitudes on the Neilgherry Hills, as will be seen by the annexed copy of an official memorandum; and although this experiment was too limited to enable us to compile a scale of the probable times a certain length of fuse composition would burn at given altitudes, it amply proves the fact that combustion is retarded at considerable elevations.

"Memorandum of an experiment to ascertain whether the atmosphere influences the burning of fuses:—

"Eighteen 8-inch fuses, made of the same description of wood (Congo), were filled with composition made for the purpose. The same man drove the whole on the morning of the 2nd August 1849, using the same mallet and drifts. Six of the fuses were burnt at the Mount, six at Bangalore, and six on the Neilgherry Hills; all in the presence of artillery officers; the result is shown below":—

Time.	Station.	Length of fuse.	Time of burning.	Barometer.	Thermometer.	Height above the level of the sea.
		in.	secs.	in.	°	
August 2, 1849. ¼ past 12 o'clock.	St. Thomas's Mount.	3 3 3 3 3 3	14·15 14·30 14·30 14·30 14·15 14·30	29·78	89	Artillery Depôt yard.
August 17, 1849. ½ past 5 P.M.	Bangalore.	3 3 3 3 3 3	16·00 0·0* 15·75 15·15 15·75 16·25	26·89	82	3000 ft.
August 31, 1849. ½ past 7 A.M.	Kotagherry, Neilgherries.	3 3 3 3	17·00 17·00 0·0* 17·30	24·025	Att. 62·7 Det. 61·8	6500 ft.
Sept. 8, 1849. 20 minutes past 8 A.M.	Cotacamund, Neilgherries.	3 3	18·00 18·25	23·030	54·2	7300 ft.

The writer attributes the result to the rarity of the atmospheric air, and of its constituent oxygen at the higher stations.

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* The fuses thus marked accidentally ignited at both ends.

March 29, 1855.—Thomas Bell, Esq., V.P., in the Chair.

The following communications were read:—

"On the existence of an element of Strength in beams subjected to Transverse Strain, arising from the Lateral Action of the fibres or particles on each other, and named by the author the 'Resistance to Flexure.'" By Peter William Barlow, Esq., F.R.S.

The author commences by observing, that under the existing theory of beams, which recognizes only two elements of resistance, namely tension and compression, the strength of a beam of cast iron cannot be reconciled with the results of experiments on the direct tensile strength, if the neutral axis is in the centre of the beam.

He then proceeds to describe experiments made on two solid beams of cast iron to determine the position of the neutral axis. The beams employed were 7 feet long, 6 inches deep and 2 inches thick, on each of which small vertical ribs were cast, 12 inches apart; nine small holes were drilled opposite to each other in each rib, for the purpose of inserting the pins of a delicate measuring instrument. The distances of the holes of the centre division of both beams were measured under various strains, and the results show that the extensions and compressions proceed in an arithmetical ratio from the centre to the upper and lower sides of the beam; and that at any given distance on either side of the centre, the amount of extension is equal to the amount of compression.

The position of the neutral axis being thus conclusively ascertained to be in the centre, it is shown that, not only the ultimate strength, but also the amount of extension and compression with a given strain, indicates the existence of another element of resistance, in addition to the resistances to extension and compression.

The author then points out, that in applying the law of "*ut tensio sic vis*" to contiguous fibres, under different degrees of extension and compression, the effect of the lateral adhesion has been omitted, and each fibre has been supposed to be capable of taking up the same degree of extension or compression as if it acted separately, and independently of the adjoining fibres.

It is then shown that this supposed independent action of the fibres is inconsistent with other practical results, and evidence is exhibited of a powerful lateral action when unequal strains are exerted.

From these and other considerations, the author is led to think that the effect of the lateral action, tending to modify the effect of the unequal and opposite strains in a beam, constitutes, in effect, a "resistance to flexure" acting in addition to the resistances of tension and compression.

In order to ascertain whether the apparent difference in the amount of tensile strength when excited by direct and transverse strains is due to *flexure*, the author caused open beams or girders to be made, each of which was formed by two bars of metal; the upper and lower bars of the same beam were in every case of the same form and dimensions; but the depth of metal and the distance to which the bars were separated vertically, was varied in the several forms of girder experimented upon. By these means the bar forming the lower side of each girder was torn asunder under different degrees of flexure. The different forms of girder experimented upon were of equal length, and were compared with solid beams and with bars of the same metal broken by direct tensile strain.

From the mean of *four* experiments on each form of girder, the value of the total resistance at the outer fibre is ascertained, and exhibits the following results:—

1. In girders having the same depth of metal, namely about 2 inches, but the total depth of the girder, and consequently the deflections different—

Form of beam or girder.	Depth of girder.	Deflection.	Total resistance at the outer fibre.
			lbs.
No. 1. Solid beam	2·02	·670	41709
No. 2. Open girder	2·51	·510	35386
No. 3. Open girder	3·00	·401	31977
No. 4. Open girder	4·00	·301	28032

2. In girders having the same total depth (namely 4 inches), and consequently nearly the same deflection, but differing in the depth of metal—

Form of beam or girder.	Depth of metal.	Total resistance at the outer fibre.
		lbs.
No. 5. Open girder	3·01	37408
No. 4. Open girder	1·97	28032
No. 7. Open girder	1·56	27908
No. 6. Open girder	1·48	25271
The tensile strength of the metal employed was found to be		18750

From these experiments, the particulars of which are fully detailed, the following facts are elicited:—

1. That in all cases the total resistance at the outer fibre, at the time of rupture, is greater than the tensile strength.

2. That in girders having the same depth of metal, it increases when the deflection increases; and

3. That in girders having the same total depth, and the same deflection, the resistance is greater when the depth of metal in the beam is greater.

And it follows that there is an element of strength depending on the depth of metal in connexion with the deflection; or in other words, dependent on the *degree* of *flexure* to which the metal forming the beam is subjected.

The author next proceeds to examine the law under which this resistance varies; and considering the total resistance in the solid beam to be composed of two resistances, one being constant and due to the tensile strength, and the other variable and depending on the depth of the metal in connexion with its deflection, the experiments indicate that the resistance to flexure varies, throughout all the girders, directly as the amount of deflection into the depth of the metal.

The paper concludes by pointing out the important amount of this resistance, the operation of which has been hitherto unknown, and which in cast iron exceeds the tensile strength of the metal, and shows that comparisons of the strength of different forms of section, based on the existing theory, which assumes the resistance at the outer fibre to be constant and equal to the tensile strength of the metal, must be entirely fallacious.

The paper is accompanied by full details of all the experiments, and the measurements for determining the position of the neutral axis.

"On the Metallic and some other Oxides, in relation to Catalytic Phænomena." By the Rev. J. Eyre Ashby.

I purpose to detail some experiments on the metallic (and a few other) oxides, made with a view to ascertain their powers to produce and maintain, catalytically, the combustion of various gases and vapours; and to annex such considerations as appear to be suggested by the facts. By catalysis I understand the operation of one body upon another, under favourable circumstances, whereby the second body is resolved into new chemical combinations, while the first (whatever may happen during the process) remains finally unchanged. This must be taken as not including explosion by percussion, in which the change takes place owing to the external application of dynamic force.

The apparatus for experimenting comprehends a variety of shallow capsules; wire-gauze, of iron, copper, and brass, of different degrees of fineness, cut into discs a little larger than the vessels on which they are to be superimposed; a spirit-lamp with large wick; a pair of pliers, and a few rings of wire to support the gauze, if necessary, while heating it in the spirit flame. The method of procedure is simple: the watch-glass, or capsule, is *nearly* filled with the liquid whose vapour is to be tried; on a wire-gauze disc is spread the oxide whose powers as a catalyser are to be tested, and this being warmed (more or less) over the lamp, is set down upon the upper rim of the capsule. Sometimes it is necessary to heat a layer of the oxide in the middle of a small combustion-tube, and pass over it the gas, or mixture of gases.

I tried the following substances with pyroxylic spirit (hydrated oxide of methyle) and alcohol separately.

1. CoO appeared to possess the power in some degree, but perhaps the specimen was in too dense agglomeration, which is not essentially reduced by trituration.

2. Co_2O_3 maintained the catalytic combustion very well.

3. AgO, reduced to metallic silver, which shows a strong tendency on gauze, and acts perfectly in the combustion-tube.

4. U_2O_3, HO became, at red heat, anhydrous mixture of U O and U_3O_4, showing strong tendency. A very pure specimen catalysed the vapour as it changed from yellow to green, after which it died away. Will not act below 570° (F.).

5. SnO; strong tendency.

6. SnO_2; slight tendency.

7. WO_3 apparently produces the effect if placed while glowing, over alcohol, but gradually dies away, as if very slowly cooling.

8. Pb_3O_4 changed to PbO, and showed a strong tendency, but quickly faded and grew cold.

9. CdO, placed while very warm over pyroxylic spirit, burst into glow and catalysed, but always died off after the lapse of from half a minute to two or three minutes, and then became incapable.

10. CaO (on the gauze), no effect.

11. SiO_2 exhibited a tendency.

12. Stourbridge clay; no effect.

13. Al_2O_3 appeared to have no effect in maintaining catalytic combustion on the gauze, but when made red-hot and quenched in absolute alcohol, it changed from pure white to a black substance and oxidized a portion of the alcohol. That this is not owing to carbon in the alcohol is evident, because the same change occurs when it is quenched in strong liquid ammonia. I suspect that it is a new oxide of aluminum.

14. Ni_2O_3, formed by heating carbonate of nickel nearly to redness, failed; prepared from the common nitrate, it acted for a short time; reduced as an intensely black velvety substance from the purest nitrate, then warmed but not made red-hot, it glowed and catalysed with alcohol or ether. With pyroxylic spirit, it was left at the end of the operation of a greenish drab, which I suspect to be a mixture of Ni_2O_3 with NiO, although it may be Ni_2O_3 changed only in appearance, for when treated with nitric acid no nickel is dissolved.

15. MnO_2 is changed at red heat into Mn_2O_3, which, with alcohol, ether, and pyroxylic spirit, continues the slow combustion very steadily. A specimen of very pure Mn_2O_3 acted extremely well, as did also a portion of "euchrome" (a hydrated sesquioxide of manganese (impure) dug from the estate of Lord Audley), after being heated in the air to drive away the carbonaceous matter with which it was mingled. Mn_2O_3 will, if sufficient care be taken, catalyse the moist gas arising from a strong solution of ammonia.

16. Fe_2O_3, when in the state of a light puffy powder, catalyses the vapour of ether, alcohol, and pyroxylic spirit, only requiring to be heated on the gauze before it is laid over the capsule. It is cheap, easily employed, and of invariable action. I have kept up the combustion for several hours on a surface of 120 square inches.

By means of a catalytic lamp in which the liquid employed is continually supplied from a reservoir and maintained at a constant level in the capsule, I have used 7 or 8 square inches continuously during thirty-six hours. This lamp I have occasionally used for laboratory purposes, where a gentle and equable heat was required for several hours.

Pursuing my experiments with the oxides of the metals, heated on wire gauze, I tried as many as I could procure or make, and by a tolerably wide induction I found that the *sesquioxides* have the strongest tendency to produce and maintain the catalytic glow, and do produce it in every case in which they are not decomposed by the amount of heat required to begin the operation.

When hydrated $Fe_2 O_3$ is heated and placed over alcohol, its colour is deepened towards black, but not uniformly, and when cold the original colour returns. But if it be made red-hot and quenched in boiling alcohol out of contact with air, it is converted into hydrated $Fe_3 O_4$, and remains permanently a deep black magnetic powder, soluble in acids. A strong solution of ammonia may be substituted for the alcohol with the same effect, but in this case some of the sesquioxide will remain unaltered and mixed with the black oxide. The alcohol or ammonia is correspondingly changed by oxidation derived from the oxygen which has been released from combination with the iron. If the hydrated $Fe_3 O_4$ be heated in contact with air, it immediately (even when it has been kept for many months) becomes $Fe_2 O_3$ by oxidation from the atmosphere, but if heated to redness *in vacuo*, it cools unchanged. [Can the black powder of alumina be $Al_3 O_4$, formed in a similar way?] The process of catalysation by $Fe_2 O_3$ is thus evident; the heated sesquioxide loses a portion of oxygen to the alcohol and becomes $Fe_3 O_4$, which is instantly reconverted into $Fe_2 O_3$ by receiving oxygen from the air, and this alternation is constantly going on in every portion of the glowing mass. It is not a mere *action de présence*, but alternate reduction and oxidation of the sesquioxide, producing a continuous oxidation of the alcohol.

This suggests a consideration apparently adverse to the atomic hypothesis of Dr. Dalton. How can a single compound molecule $Fe_2 O_3$ be changed by deoxidation into another compound molecule $Fe_3 O_4$, when, according to theory, there are in it but *two* combining proportions of iron, whereas the resultant contains *three*? and how (by deoxidation) can the resultant molecule contain *four* combining proportions of oxygen when the primary contained only *three*? We can indeed represent to the imagination that *three* molecules of the sesquioxide, acting as if they were one triple molecule, lose *one* combining proportion of oxygen, and are converted into *two* molecules of the black oxide; and conversely, that *two* molecules of the black oxide, acting as if they were *one* double molecule, combine with *one* atom of oxygen, and are converted into *three* atoms of sesquioxide. The only way to account for this, in accordance with the popular atomic theory, seems to be, to assume that the notation for these oxides is incorrect, and that

for $Fe_2 O_3$ we should write $Fe_6 O_9$,
and for $Fe_3 O_4$ we should write $Fe_6 O_8$.

If the current notation be retained, and any law be admitted, in virtue of which three molecules of sesquioxide may suffer reduction as if they were only one molecule, and divide into two molecules of the magnetic oxide, we might conceive a peculiar structure in the Fe_3O_4, with a tendency to separate again into FeO, Fe_2O_3; that it is really in combination as Fe_3O_4, but ready to yield to slight causes and become FeO, Fe_2O_3. This would explain Mr. Mercer's experiment (quoted by Brande, I. 716, edition 1848) of the chemical union of a mechanical mixture of protoxide and peroxide of iron.

Perhaps the sesquioxides occupy a middle place in the scale of effects. Take the case of iron; we have

$Fe+O$, pyrophorus,—violent oxidation,
$Fe_2O_3+NH_3+O$, alternate reduction and oxidation,
FeO_3+NH_3 (in water), reduction.

To show the last, add ammonia to a solution of FeO_3, KO, and Fe_2O_3 will be precipitated.

A mixture of ten parts by weight of powdered chlorate of potassa with one part of Fe_2O_3 disengages oxygen with extreme facility and great œconomy of heat as compared with the oxides of copper and manganese; and it is the more convenient because *n* grains of the mixture will represent almost exactly *n* cubic inches of disengaged oxygen.

A state of mechanical division is not absolutely necessary for the catalysation of some inflammable vapours by Fe_2O_3; an old nail, entirely transmuted into rust, will perform the operation; and when we consider that in many cases of fermentation, decay and putrefaction, this oxide may be present, divided or aggregated, while heat is evolved, and inflammable gases and vapours are set free, we may hereafter be able to trace some instances of "spontaneous combustion" to the catalytic action of the sesquioxides of iron.

"Ocular Spectres and Structures as Mutual Exponents." By James Jago, A.B. Cantab., M.B. Oxon.

The present communication is a revised and modified version of a paper bearing the same title, which was read on the 18th of January and 1st of February, and which was, by permission, withdrawn. The chief modification applies to the author's views respecting the structure of the vitreous body as deduced from entoptical phænomena. He is now of opinion that the arborescent system of which he infers the existence in that part of the eye (Phil. Mag. vol. ix. p. 305) does not consist of *tubes* filled with globules or cells, as he at first supposed, but of cell-constituted filaments. In a note, dated March 27, 1855, he gives the following enunciation of his present views as to the structure of the vitreous body:—

"In the vitreous humour are innumerable transparent globules, beads or cells, of less specific gravity than the fluid, extremely minute, and of uniform size, which are arranged, *without exception*, in rows to form the threads of a lax, elongated, irregular web, springing from the general surface of the capsule by, commonly, exquisitely small meshes, and extending into the interior by others of increasing size, so that the innermost part of the web—which lies nearer the circumference of the vitreous body than its centre—consists of comparatively large ones.

"Whenever the eye rotates, this filamentous peripheral system will in its relative (counter) rotation, *gradually, though soon,* come to the end of its tether, and in this interval and when there, act as a check upon the relatively rotating fluid; perpetually reiterating obstruction, above all in the immediate vicinity of the capsule and (by the disposition of the threads to float vertically) most effectually in the most important or horizontal direction. The middle of the vitreous body being free of impediment, relative rotation of the fluid expends itself there, whilst a practical concurrence in the ocular rotation ensues near the capsule. And thus in the incessant movements of the eye, head and body, the wall that confines the fluid can suffer no severe concussions from eddies in the latter. In other words, we have herein a provision that the crystalline lens may not be shaken, the circulation in the retinal vessels may not be deranged, and sensations of light may not be ever assailing us from impulses of the vitreous fluid, perhaps that the retina may not itself suffer direct injury therefrom."

"An Account of some Experiments made with the Submarine Cable of the Mediterranean Electric Telegraph." By Charles Wheatstone, F.R.S.

The following results were obtained between May 24 and June 8 in last year, with the telegraphic cable manufactured by Messrs. Kuper and Co. of East Greenwich, for the purpose of being laid across the Mediterranean sea, from Spezia on the coast of Italy to the island of Corsica. The manufacturers, in conjunction with Mr. Thomson the engineer of the undertaking, kindly afforded me every facility in carrying on the experiments. The short time that elapsed between the opportunity presenting itself and the shipping of the cable for its destination, prevented me from determining with sufficient accuracy some points of importance, respecting which I was only able to make preliminary experiments, but the following, which I was able to effect with the means at hand, may possess sufficient interest to be made public. They present perhaps nothing theoretically new, but I am not aware that experimental verifications of some of these points have been made before. I assume that the reader is acquainted with the experiments of Dr. Faraday described in the Philosophical Magazine, Ser. 4, vol. vii. p. 197.

The cable was 110 miles in length, and contained six copper wires, one-sixteenth of an inch in diameter, each separately insulated in a covering of gutta percha one-tenth of an inch in thickness. The whole was surrounded by twelve thick iron wires twisted spirally around it, forming a complete metallic envelope one-third of an inch in thickness. A section of the cable presented the six wires arranged in a circle of half an inch diameter, and one-fifth of an inch from the internal surface of the iron envelope.

The cable was coiled in a dry well in the yard, and one of its ends was brought into the manufactory. The wires were numbered 1, 2, 3, 4, 5, 6, and the ends in the well were indicated by an accent;

the ends 1′2, 2′3, 3′4, 4′5, 5′6 were connected by supplementary wires, so that the electric current might be passed in the same direction through all the six wires joined to a single length, or through any lesser number of them, the connexions being made at pleasure in the experimenting room.

The rheomotor employed was an insulated voltaic battery consisting of twelve troughs, each of twelve elements, which had been several weeks in action.

First Series.

The following experiments show that the iron envelope of the compound conductor gives rise to the same phænomena of induction which occur when the insulated wire is immersed in water, as in Dr. Faraday's experiments.

Exp. 1. One end of the entire length, 660 miles, was brought in connexion with one of the poles of the battery, the other end remaining insulated. The wire became charged with negative electricity when its end touched the zinc pole, and with positive electricity when it communicated with the copper pole. A current, indicated by a galvanometer placed near the battery, existed as long as the charge was going on, and ceased when it arrived at its maximum. [The feeble current attributed to imperfect insulation, which continues as long as the contact with the battery remains, is here left out of consideration.] When the wire was charged, and the discharge effected by a wire communicating with the earth, the current produced was in the same direction, whether the discharge was made near the battery or at the opposite end; *i. e.* the current in both cases proceeded from the wire to the earth in the same direction.

Exp. 2. On bringing one end of the wire in contact with one of the poles of the battery, the other pole having no communication with the earth, the wire remained uncharged. A very slight and scarcely perceptible tremor was observed in the galvanometer needle interposed between the battery and the wire.

Exp. 3. To each of the poles of the battery was attached a wire 220 miles in length, and similar galvanometers were interposed between the two wires (the remote extremities of which remained insulated) and the battery. So long as one wire alone was connected with the battery no charge was communicated to it, but on connecting the other wire with the opposite pole both wires were instantaneously charged, as the strong deflection of both needles rendered evident. On bringing the free end of one of the wires in communication with the earth it alone was discharged, the other wire remaining fully charged.

Second Series.

Exp. 4. One pole of the battery was connected with the earth, and the other with 660 miles of wire, which had an earth communication at its opposite end; three galvanometers were interposed in the course of the conductor; the first near the battery, the second in the middle of the wire, *i. e.* 330 miles from each extremity, and the third at the remote end near the communication with the earth. When the connexion of the battery with the wire was completed, the galvanometers were successively acted upon in the order of their distances from the battery, as in the experiments recorded by Dr. Faraday. When the earth connexion at the remote extremity of the wire, on the contrary, was completed, the disturbance of equilibrium commenced at this end, and the galvanometers successively acted in the reverse order, *i. e.* the galvanometer which was the most distant from the battery was the first impelled into motion. In the latter case, before the completion of the circuit the needles of the galvanometers had assumed constant deflections to a limited extent, owing to a feeble current arising from the uniform dispersion of the static electricity along the wire.

Exp. 5. The two extremities of the 660 miles of wire were brought into connexion with the opposite poles of the battery. When one of the ends previously disconnected from the battery was united therewith, the galvanometers at the extremities of the wire, and consequently which were at equal distances from the poles of the battery, were immediately and simultaneously acted upon, while that which was in the middle of the wire was subsequently caused to move. When the wire disconnected in the middle instead of near one of the poles of the battery was again united, the middle galvanometer, which was the most remote from the battery, was the first acted upon, and those near the poles subsequently.

The comparison of the two above-mentioned experiments show that the earth must not be regarded simply as a conductor, which many suppose to be the case. Since in the first experiment there were not many yards' distance between the two earth terminations, did the extent of ground between them act only as a conductor, the two galvanometers at the extremities of the wire should have acted simultaneously, as in the second experiment, and as would have been the case had a short wire united the two extremities which proceeded to the earth.

Third Series.

Exp. 6. One pole of the battery was connected with the earth, and the opposite pole with one extremity of the 660 miles of wire, the other end remaining insulated; a delicate galvanometer was interposed near the battery. Notwithstanding there was no circuit formed, the needle showed a constant deflection of 33½°; the feeble current thus rendered evident is not so much to be attributed to imperfect insulation, as to the uniform and continual dispersion of the static electricity with which the wire is charged throughout its entire length, in the same manner as would take place in any other charged body placed in an insulating medium. The strength of the current thus occasioned appears to be nearly, if not exactly, proportional to the length of the wire added, as the following table will show: the first column indicates the number of miles of wire subjoined beyond the galvanometer, and the second the corresponding deflections of the needle:—

miles.	°
0	0
110	6½
220	12
330	18
440	23½
550	28
660	31

Exp. 7. One end of the 660 miles of wire was now allowed to remain constantly in contact with one of the poles of the battery; but the galvanometer was successively shifted to different distances from the battery. The strength of the current was now shown to be inversely as the distance of the galvanometer from the battery, becoming null at its extremity, as shown in the following table. The first column shows the distance from the battery at which the galvanometer was placed, and the second column the corresponding deflection of the needle.

miles.	°
Near the battery	33½
110	31
220	25
330	15
440	12
550	5
660	0

The deflections of the needle of the galvanometer employed in these experiments were, when they did not surpass 36°, very nearly comparable with the force of the current. This I ascertained in the following way. I took six cells of the small constant battery described in my paper "On new Instruments and Processes for determining the Constants of a Voltaic Circuit," printed in the Philosophical Transactions for 1843, and placed in the circuit formed of the 660 miles of wire, the earth, and the galvanometer, successively 1, 2, 3, 4, 5 and 6 cells. Leaving out of consideration the resistances in the cells themselves and in the earth, which were very inconsiderable in comparison with that in the long wire, the force of the current should be approximately proportionate to the number of the elements; and since the deflections of the needle nearly indicated this proportionality, as the following table will show, it may be assumed that the force of the current, when the deflection of the needle did not surpass 36°, nearly corresponded with the angular deviation.

cell.	°
1	6
2	14
3	19
4	28
5	32
6	36

From the preceding experiments (6. and 7.) it seems to result, that whatever length of wire is connected with the battery, if a galvanometer is placed at the farther extremity of the wire and a constant length added to the other termination of the galvanometer, its indication remains always nearly the same. Thus the galvanometer indicated $6\frac{1}{2}°$ when it was placed close to the battery and 110 miles of wire were subjoined beyond it; and 5° when 550 miles were interposed between the battery and galvanometer, the same length, 110 miles, being subjoined. In like manner, when 220 miles were added beyond the galvanometer placed near the battery, the indication was 12°; precisely the same as when 440 miles were interposed and 220 added. So also when 330 miles were added, the deviation of the galvanometer was 18°; and 15° when 330 miles were interposed and 330 added. I have no doubt that the correspondence would have been closer had it not been for the fluctuations of the battery.

It would appear from this, that whatever be the length of wire attached to the insulated pole of a battery, it becomes charged to the same degree of tension throughout its entire extent; so that another insulated wire brought into connexion with its free extremity exhibits precisely the same phænomena, in kind and measure, as when it is brought into immediate connexion with a pole of the battery. Some important practical consequences flow from this conclusion, which I will not develope at present, as I have not yet had an opportunity of submitting them to the test of experiment.

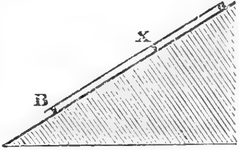
April 19, 1855.—The Lord Wrottesley, President, in the Chair.

The following communication was read:—

"On the Descent of Glaciers." By the Rev. Henry Moseley, M.A., F.R.S.

If we conceive two bodies of the same form and dimensions (cubes for instance), and of the same material, to be placed upon a uniform horizontal plane, and connected by a substance which alternately extends and contracts itself, as does a metallic rod when subjected to variations of temperature, it is evident that by the extension of the intervening rod each will be made to recede from the other by the same distance, and, by its contraction, to approach it by the same distance. But if they be placed on an inclined plane (one being lower than the other), then when by the increased temperature of the rod its tendency to extend becomes sufficient to push the lower of the two bodies downwards, it will not have become sufficient to push the higher upwards. The effect of its extension will therefore be to cause the lower of the two bodies to descend whilst the higher remains at rest. The converse of this will result from contraction; for when the contractile force becomes sufficient to pull the upper body down the plane it will not have become sufficient to pull the lower up it. Thus, in the contraction of the substance which intervenes between the two bodies, the lower will remain at rest whilst the upper descends. As often, then, as the expansion and contraction is repeated the two bodies will descend the plane until, step by step, they reach the bottom.

Fig. 1.



Suppose the uniform bar AB placed on an inclined plane, and subject to extension from increase of temperature, a portion XB will descend, and the rest XA will ascend; the point X where they separate being determined by the condition that the force requisite to push XA up the plane is equal to that required to push XB down it.

Let AX$=x$, AB$=$L, weight of each linear unit $=u$, $i=$ inclination of plane, $\phi=$ limiting angle of resistance,

$$\therefore \quad ux = \text{weight of AX;}$$

$$u(\mathrm{L}-x) = \text{weight of BX.}$$

Now, the force acting parallel to an inclined plane which is necessary to push a weight W up it, is represented by

$$\mathrm{W}\frac{\sin(\phi+i)}{\cos\phi};$$

and that necessary to push it down the plane by

$$\mathrm{W}\frac{\sin(\phi-i)}{\cos\phi};$$

$$\therefore \quad ux\frac{\sin(\phi+i)}{\cos\phi} = u(\mathrm{L}-ux)\frac{\sin(\phi-i)}{\cos\phi}$$

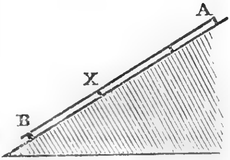
$$\therefore \quad x\{\sin(\phi+i)+\sin(\phi-i)\} = \mathrm{L}\sin(\phi-i)$$

$$\therefore \quad 2x\sin\phi\cos i = \mathrm{L}\sin(\phi-i)$$

$$\therefore \quad x = \frac{1}{2}\mathrm{L}\frac{\sin(\phi-i)}{\sin\phi\cos i}$$

$$\therefore \quad x = \frac{1}{2}\mathrm{L}\left\{1-\frac{\tan i}{\tan\phi}\right\}.$$

Fig. 2.



When contraction takes place the converse of the above will be true. The separating point X will be such, that the force requisite to pull XB up the plane is equal to that required to pull AX down it. BX is obviously in this case equal to AX in the other.

Let λ be the elongation per linear unit under any variation of temperature; then the distance which the point B (see fig. 1) will be made to descend by this elongation

$$=\lambda\,.\,\overline{\mathrm{BX}}$$

$$=\lambda(\mathrm{L}-x)$$

$$=\frac{1}{2}\lambda\mathrm{L}\left(1+\frac{\tan i}{\tan\phi}\right).$$

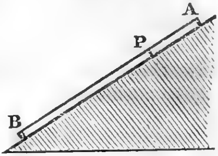
If we conceive the bar now to return to its former temperature, contracting by the same amount (λ) per linear unit; then the point B (fig. 2) will by this contraction be made to ascend through the space

$$\begin{aligned} \mathrm{BX}.\lambda &= x\lambda \\ &= \frac{1}{2}\mathrm{L}\lambda\left\{1-\frac{\tan i}{\tan\phi}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1) \end{aligned}$$

Total descent l of B by elongation and contraction is therefore determined by the equation

$$l=\mathrm{L}\lambda\frac{\tan i}{\tan\phi} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Fig. 3.



To determine the pressure upon a nail, driven through the rod at any point P fastening it to the plane.

It is evident that in the act of extension the part BP of the rod will descend the plane and the part AP ascend; and conversely in the act of contraction; and that in the former case the nail B will sustain a pressure upwards equal to that necessary to cause BP to descend, and a pressure downwards equal to that necessary to cause PA to ascend; so that, assuming the pressure to be downwards, and adopting the same notation as before, except that AP is represented by ρ, AB by a, and the pressure upon the nail (assumed to be downwards) by P, we have in the case of extension

$$\mathrm{P}=u\rho\frac{\sin(\phi+i)}{\cos\phi}-u(a-\rho)\frac{\sin(\phi-i)}{\cos\phi},$$

and in the case of contraction

$$\mathrm{P}=u(a-\rho)\frac{\sin(\phi+i)}{\cos\phi}-u\rho\frac{\sin(\phi-i)}{\cos\phi}.$$

Reducing, these formulæ become respectively

$$\mathrm{P}=\frac{u}{\cos\phi}\left\{2\rho\sin\phi\cos i-a\sin(\phi-i)\right\} \quad . \quad . \quad . \quad (3)$$

$$\mathrm{P}=\frac{u}{\cos\phi}\left\{a\sin(\phi+i)-2\rho\sin\phi\cos i\right\} \quad . \quad . \quad . \quad (4)$$

My attention was first drawn to the influence of variations in temperature to cause the descent of a lamina of metal resting on an inclined plane, by observing, in the autumn of 1853, that a portion of the lead which covers the south side of the choir of the Bristol Cathedral, which had been renewed in the year 1851, but had not been properly fastened to the ridge beam, had descended bodily 18 inches into the gutter; so that if plates of lead had not been inserted at the top, a strip of the roof of that length would have been left exposed to the weather. The sheet of lead which had so descended measured, from the ridge to the gutter, 19 feet 4 inches and along the ridge 60 feet. The descent had been continually going on, from the time the lead had been laid down. An attempt made to stop it by driving nails through it into the rafters had failed. The force by which the lead had been made to descend, whatever it was, had been found sufficient to draw the nails*. As the pitch of the roof was only $16\frac{1}{2}°$, it was sufficiently evident that the weight of the lead alone could not have caused it to descend. Sheet lead, whose surface is in the state of that used in roofing, will stand firmly upon a surface of planed deal when inclined at an angle of 30°†, if no other force than its weight tends to cause it to descend. The considerations which I have stated in the preceding articles led me to the conclusion that the daily variations in the temperature of the lead, exposed as it was to the action of the sun by its southern aspect, could not but cause it to descend considerably, and the only question which remained on my mind was, whether this descent could be so great as was observed. To determine this I took the following data:—

Mean daily variation of temperature at Bristol in the month of August, assumed to be the same as at Leith (Kaemtz, Meteorology, by Walker, p. 18), 8°·21 Cent.

Linear expansion of lead through 100° Cent. ·0028436.

Length of sheets of lead forming the roof from the ridge to the gutter 232 inches.

Inclination of roof 16° 32′.

Limiting angle of resistance between sheet lead and deal 30°.

Whence the mean daily descent of the lead, in inches, in the month of August, is determined by equation (2) to be

$$l=232\times\frac{8°{\cdot}21}{100}\times{\cdot}0028436\times\frac{\tan 16°\ 32'}{\tan 30°}$$

$$l={\cdot}027848 \text{ inches.}$$

The average daily descent gives for the whole month of August a descent of ·863288. If the average daily variation of temperature of the month of August had continued throughout the year, the lead would have descended 10·19148 inches every year. And in the two years from 1851 to 1853 it would have descended 20·38296 inches. But the daily variations of atmospheric temperature are less in the other months of the year than in the month of August. For this reason, therefore, the calculation is in excess. For the following reasons it is in defect:—1st, the daily variations in the temperature of the lead cannot but have been greater than those of the surrounding atmosphere. It must have been heated above the surrounding

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* The evil was remedied by placing a beam across the rafters near the ridge, and doubling the sheets round it, and fixing their ends with spike nails.

† This may easily be verified. I give it as the result of a rough experiment of my own. I am not acquainted with any experiments on the friction of lead made with sufficient care to be received as authority in this matter. The friction of copper on oak has, however, been determined by General Morin to be 0·62, and its limiting angle of resistance 31° 48′; so that if the roof of Bristol Cathedral had been inclined at 31° instead of 16°, and had been covered with sheets of copper resting on oak boards, instead of sheets of lead resting on deal, the sheeting would not have slipped by its weight only.

atmosphere by radiation from the sun in the day-time, and cooled below it by radiation into space at night. 2ndly. One variation of temperature only has been assumed to take place every twenty-four hours, viz. that from the extreme heat of the day to the extreme cold of the night; whereas such variations are notoriously of constant occurrence during the twenty-four hours. Each cannot but have caused a corresponding descent of the lead, and their aggregate result cannot but have been greater than if the temperature had passed uniformly (without oscillations backwards and forwards) from one extreme to the other.

These considerations show, I think, that the causes I have assigned are sufficient to account for the fact observed. They suggest, moreover, the possibility that results of importance in meteorology may be obtained from observing with accuracy the descent of a metallic rod thus placed upon an inclined plane. That descent would be a measure of the aggregate of the changes of temperature to which the metal was subjected during the time of observation. As every such change of temperature is associated with a corresponding development of mechanical action under the form of work*, it would be a measure of the aggregate of such changes and of the work so developed during that period; and relations might be found between measurements so taken in different equal periods of times, successive years for instance, tending to the development of new meteorological laws.

The following are the results of recent experiments† on the expansion of ice:—

Linear expansion of ice for an interval of 100° of the Centigrade thermometer.

0·00524,	Schumacher.
0·00513,	Johrt.
0·00518,	Moritz.

Ice, therefore, has nearly twice the expansibility of lead, so that a sheet of ice would, under similar circumstances, have descended a plane similarly inclined, twice the distance that the sheet of lead referred to in the preceding article descended. Glaciers are, on an increased scale, sheets of ice placed upon the slopes of mountains, and subjected to atmospheric variations of temperature throughout their masses by variations in the quantity and the temperature of the water, which flowing from the surface everywhere percolates them. That they must from this cause descend into the valleys is therefore certain. That portion of the Mer de Glace of Chamouni which extends from Montanvert to very near the origin of the Glacier de Léchaud, has been accurately observed by Professor James Forbes‡. Its length is 22,600 feet, and its inclination varies from 4° 19′ 22″ to 5° 5′ 53″. The Glacier du Géant, from the Tacul to the Col du

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* Mr. Joule has shown (Phil. Trans. 1850, Part I.) that the quantity of heat capable of raising a pound of water by 1° Fahr. requires for its evolution 772 units of work.

† Vide Archiv f. Wissenchaftl. Kunde v. Russland, Bd. vii. S. 333.

‡ Travels through the Alps of Savoy. Edinburgh, 1843.

Géant, Professor Forbes estimates (but not from his own observations, or with the same certainty) to be 24,700 feet in length, and to have a mean inclination of 8° 46′ 40″.

According to the observations of De Saussure, the mean daily range of Reaumur's thermometer in the month of July, at the Col du Géant, is 4°·257*, and at Chamouni 10°·092. The resistance opposed by the rugged channel of a glacier to its descent cannot but be different at different points and in respect to different glaciers. The following passage from Professor Forbes's work contains the most authentic information I am able to find on this subject. Speaking of the Glacier of la Brenva, he says, "The ice removed, a layer of fine mud covered the rock, not composed however alone of the clayey limestone mud, but of sharp sand derived from the granitic moraines of the glacier, and brought down with it from the opposite side of the valley. Upon examining the face of the ice removed from contact with the rock, we found it *set* all over with sharp angular fragments, from the size of grains of sand to that of a cherry, or larger, of the same species of rock, and which were so firmly fixed in the ice as to demonstrate the impossibility of such a surface being forcibly urged forwards without sawing and tearing any comparatively soft body which might be below it. Accordingly, it was not difficult to discover in the limestone the very grooves and scratches which were in the act of being made at the time by the pressure of the ice and its contained fragments of stone." (Alps of the Savoy, pp. 203–4.) It is not difficult from this description to account for the fact that small glaciers are sometimes seen to lie on a slope of 30° (p. 35). The most probable supposition would indeed fix the limiting angle of resistance between the rock and the under surface of the ice, set all over, as it is described to be, with particles of sand and small fragments of stone, at about 30°, that being nearly the slope at which calcareous stones will rest on one another. If we take then 30° to be the limiting angle of resistance between the under surface of the Mer de Glace and the rock on which it rests, and if we assume the same mean daily variation of temperature (4·257 Reaumur, or 5·321 Centigrade) to obtain throughout the length of the Glacier du Géant, which De Saussure observed in July at the Col du Géant; if, further, we take the linear expansion of ice at 100° Centigrade to be that (·00524) which was determined by the experiments of Schumacher; and, lastly, if we assume the Glacier du Géant to descend as it would if its descent were unopposed by its confluence with the Glacier de Léchaud, we shall obtain, by substitution in equation (2) for the mean daily descent of the Glacier du Géant at the Tacul, the formula—

$$l=24700\times 5{\cdot}321\times\frac{{\cdot}00524}{100}\times\frac{\tan 8^\circ 46'}{\tan 30^\circ}$$

$$l=1{\cdot}8395 \text{ feet.}$$

The actual descent of the glacier in the centre was 1·5 foot. If the Glacier de Léchaud descended at a mean slope of 5°, singly in a

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* Quoted by Professor Forbes, p. 231.

sheet of uniform breadth to Montanvert without receiving the tributary glacier of the Talèfre, or uniting with the Glacier du Géant, its diurnal descent would be given by the same formula, and would be found to be ·95487 foot. Reasoning similarly with reference to the Glacier du Géant, supposing it to have continued its course singly from the Col du Géant to Montanvert without confluence with the Glacier de Léchaud, its length being 40,420 feet, and its mean inclination $6^\circ 53'$, its mean diurnal motion l at Montanvert would, by formula (2) have been 2·3564 feet*. The actual mean daily motion of the united glaciers, between the 1st and the 28th July, was Montanvert (Forbes's 'Alps of the Savoy,' p. 140),—

Near the side of the glacier	1·441 foot.
Between the side and the centre ..	1·750 foot.
Near the centre	2·141 feet.

The motion of the Glacier de Léchaut was therefore accelerated by their confluence, and that of the Glacier du Géant retarded. The former is dragged down by the latter.

I have had the less hesitation in offering this solution of the mechanical problem of the motion of glaciers, as those hitherto proposed are confessedly imperfect. That of De Saussure, which attributes the descent of the glacier simply to its weight, is contradicted by the fact that isolated fragments of the glacier stand firmly on the slope on which the whole nevertheless descends; it being obvious that if the parts would remain at rest separately on the bed of the glacier, they would also remain at rest when united.

That of Professor J. Forbes, which supposes a viscous or semifluid structure of the glacier, is not consistent with the fact that no viscosity is to be traced in its parts when separated. They appear as solid fragments, and they cannot acquire in their union properties in this respect which individually they have not.

Lastly, the theory of Charpentier, which attributes the descent of the glacier to the daily congelation of the water which percolates it, and the expansion of its mass consequent thereon, whilst it assigns a cause which, so far as it operates, cannot, as I have shown, but cause the glacier to descend, appears to assign one inadequate to the result; for the congelation of the water which percolates the glacier does not, according to the observations of Professor Forbes †, take place at all in summer more than a few inches from the surface. Nevertheless it is in the summer that the daily motion of the glacier is the greatest.

The following remarkable experiment of Mr. Hopkins of Cambridge‡, which is considered by him to be confirmatory of the sliding theory of De Saussure as opposed to De Charpentier's dilatation theory, receives a ready explanation on the principles which I have laid down in this note. It is indeed a necessary result of them.

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* On the 1st of July, the centre of the actual motion of the Mer de Glace at Montanvert was 2·25 feet.

† Travels in the Alps.

‡ I have quoted the above account of it from Professor Forbes's book, p. 419.

"Mr. Hopkins placed a mass of rough ice, confined by a square frame or bottomless box, upon a roughly chiselled flag stone, which he then inclined at a small angle, and found that a slow but uniform motion was produced, when even it was placed at an inconsiderable slope." This motion, which Mr. Hopkins attributed to the dissolution of the ice in contact with the stone, would, I apprehend, have taken place if the mass had been of lead instead of ice; and it would have been but about half as fast, because the linear expansion of lead is only about half that of ice.

IX. *Intelligence and Miscellaneous Articles.*

ON THE ANÆSTHETIC PRINCIPLE OF THE *LYCOPERDON PROTEUS* AND CERTAIN OTHER FUNGI. BY THORNTON HERAPATH, ESQ.

THE smoke of the puff-ball, it is well known, has been long employed in some parts of the country, by apiarists, for stupefying bees. In a paper "On the Anæsthetic Properties of the *Lycoperdon proteus*, or common Puff-ball," which was read before the Medical Society of London in 1853, Mr. B. W. Richardson called particular attention to this fact, and stated that the fumes of the burning fungus produced the most perfect anæsthesia, not only in insects, but also in dogs, cats, rabbits, and probably in all the larger animals, and might consequently be applied as a substitute for the vapour of chloroform and æther in producing insensibility to pain in surgical practice. With the assistance of Dr. Willis, he said, he had removed a large tumour from the abdomen of a dog that had been placed under the influence of the narcotic, without any sign of pain being exhibited by the animal during the operation. From this gentleman's experiments it appeared, that when a moderate quantity of the fumes was inhaled slowly, the narcotism came on and passed off slowly, the animal exhibiting all the symptoms of intoxication, with convulsions and sometimes vomiting; but that when they were administered in larger quantity, life was invariably destroyed. The consideration of these and other facts induced Mr. Richardson to conclude, that the peculiar effects that were produced by the inhalation of the smoke of the puff-ball were caused by a volatile narcotic principle contained in the fungus, which was liberated by the action of heat, but was not absorbable by water, alcohol, or a strong alkaline solution. What the exact nature of this principle was, however, he confessed himself to be unable to determine. About eight or nine months ago I carefully repeated Mr. Richardson's experiments, and after making several futile attempts, at last, I believe, succeeded in isolating the narcotic constituent of the smoke.

The first step I considered it necessary to take in the investigation, was to determine in what part of the fungus the anæsthetic ingredient was contained; that is to say, whether in the sporules, the cellular tissue, or the matters soluble in water. I accordingly digested two or three ounces of the fungus, previously torn up into small pieces, in moderately warm water, and by means of pressure and washing, separated the sporules and soluble constituents from the cellular matter. Then, by allowing the water that had been used in this operation to remain undisturbed for several hours, the sporules were collected in the form of a dark brown-coloured, muddy deposit. This was well washed once or twice with water, and dried in an oven, as was also the cellular matter, and the watery solution was evaporated to dryness. On testing these three substances, it was found that only two of them, namely the sporules and the cellular tissue, were capable of producing anæsthesia; the aqueous extract evolved a thick irritating vapour, but this did not occasion insensibility on inhalation.

It was clear, therefore, that the narcotic principle should be looked for in the two former. Accordingly, small portions of each of them were digested for several hours in boiling alcohol, æther, bisulphide of carbon, wood-spirit, chloroform, diluted sulphuric acid, and fusel oil, but in every instance the residuary matter, when pressed and dried, was found to retain its original narcotic quality. Fresh quantities were then soaked for a considerable period in hot alkaline lye, and in a hot solution of moderately strong nitric acid, until nothing further was dissolved out by either of the reagents; the insoluble portion was well washed with water, and again dried in an oven. On this being tested as before, anæsthesia was found to be no longer produced.

In the next series of experiments I operated in a different way. I introduced the fumes of the burning fungus into bottles containing small quantities of liquor potassæ, dilute hydrochloric acid, alcohol, fusel oil, and diluted sulphuric acid. The bottles were then well shaken for several minutes, and the properties of the purified fumes were tested by introducing flies, bees, or wasps, secured by cement to the ends of long splinters of wood, into the bottles, and observing the effects. In every case, however, insensibility was still produced, thus showing that the narcotic quality of the fumes was not caused by any body soluble in these solutions. There being no substance with which I am acquainted, except carbonic oxide, nitrous oxide, and perhaps some compounds of cyanogen which possess all these properties; and having, moreover, in the mean time read a paper, by M. Adrien Chenol, "On Pure Oxide of Carbon, considered as a Poison*," it immediately occurred to me that it was the former of these substances that was the cause of the narcotism. I therefore specially examined the fumes for carbonic oxide, by agitating them with an acid solution of chloride of copper, and also by absorbing the carbonic acid, ammonia and oxygen, by means of lime-water, diluted muriatic acid, and a solution of the protosulphate of iron saturated with nitric oxide gas, when indications of the presence of carbonic oxide were readily obtained; the fumes, after agitation with the solution of chloride of copper, no longer induced narcotism; whilst those, on the contrary, which had been treated with the other solvents, were more

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* *Comptes Rendus*, No. 16, April 17, 1854.

than ordinarily powerful, and rendered an insect insensible much more quickly than before; they also burnt with a blue flame, and possessed all the well-known characters of the oxide of carbon. The correctness of this conclusion was, moreover, confirmed by experimenting with carbonic oxide prepared by acting on oxalic acid with oil of vitriol, and passing the gas evolved through caustic soda-ley. Even when largely diluted with air, it still continued to produce insensibility in insects, and acted in every way like the purified fumes of the *Lycoperdon**.

It is not difficult to understand how carbonic oxide is formed by the ignition of the fungus, as this gas is invariably produced in larger or smaller quantity when certain organic substances are decomposed by heat, though some yield it in greater proportion than others; and consequently, as might have been anticipated, I find that the fumes of several other fungi act in the same manner towards animals as those of the *Lycoperdon proteus*. The principal of those to which I allude are the common *Lycoperdon* of the druggist, *L. giganteum*, and the mushroom, *Agaricus campestris*.

Old Park, Bristol,
May 25, 1855.

ON A STRONGLY FLUORESCENT FLUID. BY RUDOLPH BÖTTGER.

Dr. Böttger has informed Prof. Poggendorff, that a solution of platino-cyanide of potassium possesses the property of fluorescence in a still higher degree than sulphate of quinine. The solution of the metallic salt fluoresces with a yellowish light, resembling one of the colours exhibited by its dichroitic crystals.—Poggendorff's *Annalen*, vol. xcv. p. 176.

ON THE QUANTITATIVE DETERMINATION OF WATER.

BY DR. H. VOHL.

Water is almost always determined by the loss which a body undergoes when exposed to a high temperature. In those cases, however, in which the constituents of the body under investigation are liable to change when heated by taking up oxygen, the determination of water is attended with many difficulties, and unsatisfactory results are in consequence often obtained. The method usually employed consists in heating the body in an atmosphere free of oxygen, and passing the gas charged with watery vapour through a carefully weighed chloride of calcium tube. The loss of the substance should then be exactly equal to the increase in the weight of the chloride of calcium tube.

Another method which I have frequently employed with very satisfactory results, consists in mixing the readily oxidizable sub-

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* See also 'A Treatise on Poisons,' by Professor Christison, 4th edition, p. 827, for an account of the peculiar effects produced by the inhalation of the oxide of carbon.

stances with an excess of bichromate of potash, pouring water over the mixture in a platinum or porcelain crucible, and then drying it, first in the water-bath and afterwards in the air-bath, at 392° Fahr. In many cases the mixture may be heated almost to redness over the flame of a lamp.

The intermixture of bichromate of potash is a source of oxygen, which readily furnishes that substance to the oxidizable body, without, however, in any way altering the weight. Consequently if the weight of the substance employed, and of the bichromate of potash added to it be known, the loss, if no volatile body be present, gives only the quantity of the water contained in it, and volatilized from the substance under investigation. It is a matter of course that this substance should contain neither organic compounds nor ammoniacal salts. The addition of bichromate of potash is not always advisable, but in many cases the neutral salt must be employed, as in many salts the acid will have a decomposing action upon the chromic acid, giving rise to an evolution of oxygen or chlorine, and thus a higher amount of water will be obtained. This will occur, for instance, with all protosalts of iron containing sulphuric acid or chlorine; with these the neutral chromate of potash must always be employed, when, if the heat be not raised to redness, no decomposition of the chromic acid will take place. The chromate employed must be perfectly dry and free from all organic matter.

To obtain it in this state, the commercial salt is to be purified by crystallization, and the crystals obtained fused at a gentle heat (below redness) in a platinum or porcelain crucible. On cooling, the mass solidifies at first in a crystalline form, and afterwards falls into a coarse powder, which may be employed without further preparation, and should be weighed while still warm. The neutral chromate of potash is dried in a covered crucible and weighed whilst warm.—Liebig's *Annalen*, May 1855, vol. xciv. p. 216.

ON THE DETERMINATION OF LIME. BY H. VOHL.

Lime is usually determined either as carbonate or sulphate. A method which furnishes very exact results consists in precipitating the lime in the form of oxalate, washing the precipitate by decantation, and then treating it in the apparatus of Will and Fresenius with bichromate of potash and sulphuric acid. If a few particles float upon the surface during the decantations, they may be got rid of by the addition of a few drops of æther. The sulphuric acid sets free the oxalic acid of the lime-salt, which is then oxidized by the chromic acid, forming carbonic acid, which is evolved. From this loss the amount of lime is calculated in accordance with the following formula:—

$$3(\mathrm{CaO}, \mathrm{C}^2\mathrm{O}^3) + \mathrm{KO}, 2(\mathrm{CrO}^3) + 7(\mathrm{SO}^3)$$
$$= 3(\mathrm{CaO}, \mathrm{SO}^3) + \mathrm{KO}, \mathrm{SO}^3 + \mathrm{Cr}^2\mathrm{O}^3 + 3(\mathrm{SO}^3) + 6(\mathrm{CO}^2),$$

or for 84·396 of lime, 132·000 of carbonic acid will be evolved.

As the washing by decantation requires some practice, and is not always practicable, the oxalate of lime may be filtered and washed; then dissolved on the filter with very dilute boiling muriatic acid. The solution obtained, which may be reduced by evaporation if necessary, is then put into the apparatus for the evolution of the carbonic acid, and after it has been neutralized with ammonia, the chromate of potash is added. In order to prevent any evolution of chlorine, a small quantity of oxide of mercury must be added before the sulphuric acid is added to the mixture.—Liebig's *Annalen*, May 1855, vol. xciv. p. 217.

METEOROLOGICAL OBSERVATIONS FOR MAY 1855.

Chiswick.—May 1. Densely clouded: dry haze: clear. 2. Clear: sharp frost at night. 3. Fine: densely overcast. 4. Cloudy and cold: frosty. 5. Excessively dry air, with bright sun. 6. Fine. 7. Fine: overcast: boisterous at night. 8. Cloudy: clear, with sharp frost at night. 9. Fine: rain at night. 10. Cloudy. 11. Cloudy and fine: thunder-storm in afternoon: clear. 12. Cloudy and cold: very clear: frosty. 13. Overcast: boisterous, with heavy rain at night. 14. Cloudy. 15. Cloudy: rain. 16. Cloudy: clear and frosty at night. 17—19. Very fine. 20. Foggy: very fine. 21. Overcast. 22. Cloudy. 23—25. Fine. 26. Very fine: hot and dry. 27. Slight haze: cloudy: rain. 28. Rain. 29. Cloudy: showery. 30. Cold showers. 31. Rain.

Mean temperature of the month	48°·88
Mean temperature of May 1854	50 ·07
Mean temperature of May for the last twenty-nine years ...	53 ·72
Average amount of rain in May	1·85 inch.

Boston.—May 1. Cloudy. 2. Fine. 3. Cloudy: rain and snow P.M. 4—6. Cloudy. 7. Cloudy: rain A.M. and P.M. 8. Cloudy: rain, hail and snow P.M. 9. Cloudy. 10. Cloudy: rain A.M. and P.M. 11. Cloudy: rain P.M. 12. Fine. 13. Cloudy: rain P.M. 14. Cloudy. 15. Cloudy: rain A.M. 16, 17. Cloudy: rain P.M. 18—20. Cloudy. 21. Cloudy: rain A.M. and P.M. 22. Cloudy. 23—28. Fine. 29—31. Cloudy: rain A.M. and P.M.

Sandwick Manse, Orkney.—May 1. Cloudy A.M.: clear, fine P.M. 2. Drizzle A.M.: showers P.M. 3. Hail-showers A.M. and P.M. 4. Cloudy A.M.: clear P.M. 5. Cloudy A.M.: rain P.M. 6. Sleet-showers A.M. and P.M. 7, 8. Sleet-showers A.M.: cloudy P.M. 9. Hail, frost A.M.: clear P.M. 10. Bright A.M.: cloudy P.M. 11. Clear A.M. and P.M. 12, 13. Bright A.M.: cloudy P.M. 14. Cloudy A.M.: clear P.M. 15. Showers A.M.: cloudy P.M. 16. Cloudy A.M.: showers P.M. 17. Bright A.M.: drops P.M. 18. Bright A.M.: cloudy P.M. 19. Showers A.M.: cloudy P.M. 20. Bright A.M.: clear, fine P.M. 21. Cloudy A.M. and P.M. 22. Small rain A.M. and P.M. 23. Small rain A.M.: clear, fine P.M. 24, 25. Hazy A.M. and P.M. 26. Drops A.M.: cloudy P.M. 27. Hazy A.M. and P.M. 28. Cloudy A.M. and P.M. 29. Bright A.M.: clear P.M. 30. Cloudy A.M. and P.M. 31. Clear A.M. and P.M.

This month has been remarkably cold, the mean temperature being lower than that of any May during the twenty-eight years of my observations,—4°·18 below the average, and the first half of it was only about the average temperature of March.

Mean temperature of May for twenty-eight previous years .	47°·99
Mean temperature of this month	43 ·81
Mean temperature of May 1854	48 ·39
Average quantity of rain in May for fourteen previous years	1·68 inch.

Meteorological Observations made by Mr. Thompson *at the Garden of the Horticultural Society at* CHISWICK, *near London*; *by* Mr. Veall, *at* BOSTON; *and by the* Rev. C. Clouston, *at Sandwick Manse*, ORKNEY.

Days of Month.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
1855. May.	Max.	Min.		9¼ a.m.	8½ p.m.	Max.	Min.		9¼ a.m.	8½ p.m.						
1.	30·063	30·019	29·73	30·37	30·36	52	32	44	47	41	e.	nw.	calm	....	....	....
○ 2.	30·047	29·857	29·79	30·04	29·80	62	20	45	45½	37	ne.	ne.	nw.	....	....	·04
3.	29·702	29·638	29·37	29·99	29·86	64	35	46	33	33	n.	n.	nnw.	....	....	·10
4.	29·865	29·704	29·47	29·86	29·80	50	24	41·5	36½	37	ne.	nne.	sw.	....	·08	·09
5.	29·930	29·914	29·66	29·43	29·19	57	30	44	44½	40	ne.	nne.	sw.	....	....	....
6.	29·951	29·938	29·50	29·30	29·49	63	41	52	41	41	w.	wsw.	w.	....	....	·31
7.	29·884	29·719	29·52	29·54	29·47	60	33	50	44	40½	sw.	ssw.	nw.	·06	·08	·21
8.	29·830	29·772	29·33	29·63	29·69	56	21	46	42	39	nw.	nw.	wsw.	·04	·09	·19
☾ 9.	30·004	29·616	29·60	29·71	29·73	62	43	46·5	38½	35	sw.	ssw.	ne.	·09	·20	·09
10.	29·508	29·409	29·05	29·57	29·56	63	44	49	42½	39	sw.	nw.	e.	·04	....	....
11.	29·624	29·366	29·00	29·66	29·80	59	34	51·5	42½	38	sw.	n.	nnw.	·36	·24	....
12.	29·831	29·780	29·43	29·82	29·82	53	28	43·5	44½	39½	n.	n.	n.	....	·03	....
13.	29·618	29·406	29·27	29·77	29·84	50	38	51	44	39½	se.	s.	n.	·49	....	....
14.	29·678	29·615	29·38	29·83	29·77	57	35	47	44½	39	ne.	n.	n.	....	·02	·05
15.	29·572	29·551	29·26	29·75	29·77	48	41	48	45	44½	ne.	n.	n.	·05	....	·08
● 16.	29·832	29·696	29·34	29·74	29·72	50	30	48	47	43½	n.	n.	nw.	....	·05	....
17.	29·990	29·901	29·48	29·75	29·84	61	30	51	45	41½	nw.	nw.	nw.	·01	·02	·04
18.	30·109	30·078	29·70	29·94	29·92	64	39	49	49	43½	sw.	wsw.	e.	....	·15	....
19.	30·025	29·860	29·60	29·85	29·92	69	48	59·5	50	49	s.	sw.	sse.	....	....	·02
20.	29·883	29·800	29·47	30·04	30·14	68	35	61	54½	47	nw.	ne.	e.	....	....	....
21.	29·862	29·830	29·47	30·10	29·99	55	46	47	46	45	w.	n.	nne.	....	....	....
22.	29·840	29·779	29·45	29·83	29·70	64	31	50	47	47	n.	nw.	ne.	·01	·16	....
23.	29·819	29·780	29·40	29·62	29·67	60	40	50	49	47	sw.	ssw.	e.	....	....	·16
☽ 24.	29·766	29·759	29·36	29·72	29·81	71	44	58	46½	46	s.	sw.	ese.	....	....	....
25.	29·802	29·789	29·40	29·92	30·00	78	42	61	52½	49½	se.	se.	esc.	....	....	....
26.	29·876	29·759	29·46	30·08	30·13	82	58	63	52	45½	e.	e.	e.	....	....	....
27.	29·749	29·723	29·40	30·14	30·12	75	52	57·5	47	45	se.	e.	ne.	·10	....	....
28.	29·856	29·789	29·42	30·10	30·14	56	35	51	45½	44½	ne.	ne.	ne.	·09	....	....
29.	29·893	29·872	29·50	30·22	30·28	59	37	47	49½	44	n.	nnw.	nne.	·02	....	....
30.	29·949	29·879	29·62	30·27	30·25	50	40	42·5	46	45	n.	ne.	ne.	·14	·14	....
○ 31.	29·683	29·450	29·43	30·25	30·25	54	47	45·5	47	45½	ne.	ne.	e.	·46	....	....
Mean.	29·840	29·708	29·44	29·865	29·865	60·38	37·39	49·9	45·43	42·20				1·94	1·26	1·38

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X. *On the Formation of Indigo-blue.*—Part I.

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INDIGO, one of the most important and extensively used dyes, owes its value entirely to a peculiar colouring matter contained in it, to which the name of *indigo-blue* or *indigotine* is applied by chemists. This substance has been repeatedly subjected to investigation, and several distinguished chemists have bestowed their attention and labour upon it. Its properties, composition, and products of decomposition, have been so carefully examined, that it may safely be asserted that there are few organic substances whose nature is more accurately known than that of indigo-blue. If, however, we inquire into the state of our knowledge regarding the origin and mode of formation of this body, it will be found that our information on this part of the subject is extremely defective. Indigo-blue may be obtained from a variety of plants, which, though belonging to the most different genera and orders, are rather limited in number. It has sometimes been observed to form in the milk of cows, especially such as have been fed exclusively on saintfoin[†]. Latterly it has been discovered by Hassall[‡] and others[§] in human urine, where its occurrence is attributed to a morbid state of the system. It is therefore a substance which is formed sparingly indeed, but in widely distant parts of the organic world. The properties of indigo-blue, which are so peculiar as almost to separate it from all

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[*] From the Memoirs of the Literary and Philosophical Society of Manchester, vol. xii.; read April 3, 1855.

[†] *Annales de Chimie et de Physique*, vol. iii. p. 269.

[‡] Philosophical Transactions for 1854.

[§] *Ann. der Chem. und Pharm.* vol. xc. p. 120.

other organic bodies, and to constitute it one *sui generis*, naturally suggest the inquiry, in what form it is contained in the plants and animals from which it is derived. If it exists ready formed in the indigo-bearing plants, how is it that though, when in a free state, insoluble in water, acids, alkalies, alcohol, and most simple menstrua, it should so easily be extracted from those plants by a mere infusion with cold water? If it does not pre-exist in the plant, in what state of combination is it contained therein, and what is the nature of the process by which it is eliminated? The usual method of preparing indigo from the *Indigoferæ* consists in steeping the plant, especially the leaves, in water, drawing off the infusion, allowing it to undergo fermentation, and then precipitating by means of agitation with air and the addition of lime-water. Now it may be asked, is this process of fermentation, which is often very tedious and difficult to manage, essential to the formation of indigo-blue, or is it merely an accidental phænomenon attending its preparation? If it is essential, at what stage of the process is the formation of the colouring matter to be considered as completed; and is it necessary, as some persons assert, to continue it until actual putrefaction has commenced, or not? These are points, which, though perhaps of little consequence to the dyer and consumer of indigo, are of great interest in a chemical point of view, and are of the greatest importance to the manufacturer of indigo. To the latter it must surely be extremely desirable to know the exact nature of the process on which his manufacture depends, and to ascertain whether this process yields into his hands the whole quantity of the product which the material employed is capable of yielding, and also whether the manner of conducting it is in perfect accordance with theoretical requirements. If, however, we consult the authors who have written on this subject, and the chemists who have endeavoured to elucidate it, we shall obtain very unsatisfactory replies to our inquiries. The views entertained on these different points are either mere surmises, or they are conclusions founded on a limited number of frequently imperfect experiments. The chief cause of our ignorance on these questions is, probably, that the process of manufacturing indigo is one carried on, not in the more highly civilized regions, but in remote parts of the world; and we are consequently obliged to rely for our knowledge concerning it chiefly on the accounts of travellers, who are usually possessed of merely general information, or of the manufacturers themselves, who are far from competent to give an opinion on a complex organo-chemical process.

Fourcroy, according to Robiquet, considered the formation of indigo-blue to be a result of the process of fermentation employed in its preparation.

Roxburgh, who is the earliest authority I have had an opportunity of consulting on this subject, states that his predecessor, De Cossigny, whose work on the manufacture of indigo, published in the Mauritius, is very rare, was of opinion that volatile alkali was the agent by which the colouring matter was extracted from the plant and held in solution until volatilized by the agitation process. Roxburgh, who, like De Cossigny, was one of the few possessing special chemical information who have examined the process of manufacturing indigo from the *Indigoferæ* on the spot, concluded, from his experiments, "that the indigo plants contain only the base of the colour, which is naturally green; that much carbonic acid is disengaged during its extrication from the leaves; that the carbonic acid is the agent whereby it is probably extracted and kept dissolved; that ammonia is not formed during the process; that the use of alkalies is to destroy the attraction between the base and the carbonic acid; and that the vegetable base being thereby set at liberty, combines with some colouring principle from the atmosphere, forming therewith a coloured insoluble fecula, which falls to the bottom and constitutes indigo*." Roxburgh first directed attention to the fact, that it is possible to obtain indigo by merely treating the plant with hot water, and then agitating the infusion with air, from which it follows that fermentation is not an absolutely essential condition of the formation of indigo-blue.

Chevreul†, who was the first chemist of any eminence who examined the indigo-bearing plants and their constituents, inferred from his analyses of the *Isatis tinctoria* and *Indigofera anil*, that these plants contain indigo in the white or reduced state, in the same state in which it exists in the indigo vat; that in this state it is held in solution by the vegetable juices; and that when this solution is removed from the plant, it is converted by the action of the atmospheric oxygen into indigo-blue. The authority of so distinguished an investigator as Chevreul has had great weight with chemists, and most persons have adopted his view without question, though it is founded chiefly on the fact of the colouring matter being deposited from a watery extract of the plant, and of the only form in which it is known to be soluble in water being that of reduced indigo.

According to Michelotti‡, the extraction of indigo consists simply in dissolving a compound of malic acid and indigo, which is afterwards decomposed by the precipitants employed.

A few years after the appearance of Chevreul's memoirs, Giobert, Professor of Chemistry at Turin, published a work on

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* Transactions of the Society of Arts, vol. xxviii.

† *Ann. de Chim.* sér. 1. vol. lxvi. p. 5; vol. lxviii. p. 284.

‡ *Journal de Physique,* Avril 1812.

Woad*, which enunciates ideas on this subject far more nearly approaching the truth than those either of his predecessors or successors on the same field of investigation. The chief conclusions at which he arrived, partly by experiment and partly by reasoning, are contained in the following propositions:—1. Indigo-blue does not pre-exist in the plant, but is formed by the operations by means of which we believe it to be extracted. 2. There exists in a small number of plants a peculiar principle, different from all the known proximate constituents of plants, and which has a tendency to be converted into indigo; this principle may be called *indigogene*. 3. This principle differs from indigo in containing an excess of carbon, of which it loses a portion in passing into the state of indigo-blue, by means of a small quantity of oxygen which it takes up. 4. The loss of this portion of carbon is caused by the latter undergoing combustion, and being converted into carbonic acid. 5. It differs in its properties from common indigo in being colourless, in being soluble in water, and by its greater combustibility, which causes it to undergo spontaneous combustion at the ordinary temperature of the atmosphere. 6. Its combustibility is enhanced by heat and by combination with alkalies, especially lime; it is diminished by the action of all acids, even carbonic acid.

About the year 1839, the *Polygonum tinctorium*, an indigo-bearing plant indigenous to China, became the subject of a series of investigations by several French chemists, chiefly in order to ascertain whether this plant, if grown in France, could be advantageously employed for the preparation of a dye to substitute foreign indigo, so as to obviate the necessity of paying such large sums to foreign nations for this article, a necessity which seems at all times to have been a subject for extreme regret in France. Baudrimont and Pelletier, after an examination of this plant, concurred in the opinion of Chevreul, that the indigo is contained in it as reduced indigo; and the latter adduced in support of this view an experiment, which consisted in treating fresh leaves of the *Polygonum* with æther, taking care to exclude the air, until the green colour had changed to white, when, on exposure to the atmosphere, they speedily became blue. Robiquet†, Colin‡, Turpin§, and Joly||, on the other hand, expressed

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* *Traité sur le Pastel et l'Extraction de son Indigo.* Paris, 1813.

† *Journal de Pharmacie,* vol. xxv. p. 62.

‡ *Mémoire sur la Renouée des Teinturiers.* Paris, 1839.

§ *Etudes microscopiques sur le gisement de la matière bleue dans les feuilles du* Polygonum tinctorium, *Mémoire lu à l'Académie des Sciences le* 12 *Novembre,* 1838.

|| *Observations générales sur les plantes qui peuvent fournir des couleurs bleues à la teinture.* Montpellier, 1839.

a very decided conviction that indigo-blue pre-exists in the *Polygonum tinctorium,* but not in a free state; that it is combined with some organic substance or substances, which render it soluble in water, æther and alcohol; and that it requires the operation of potent agencies in order to destroy this combination and set the indigo at liberty. Osmin Hervey*, in a memoir on the *Polygonum tinctorium,* which in some parts is rather obscure, inferred from his experiments:—1. That indigotine exists in the leaves of this plant in a state of combination with a resin. 2. That this natural compound of indigo and resin contains both white indigo and blue indigo, and of the latter a larger proportion the older the leaves are. 3. That by the influence of certain organic substances, the indigo-blue is again reduced to the colourless state, if the solution be effected by means of water, without any destruction of the natural compound taking place. Girardin and Preisser† again returned to Chevreul's view, that the colouring matter is contained in the leaves of this plant in the form of reduced indigo. Since the publication of these treatises, no new ideas have, as far as I know, been promulgated by chemists in reference to this subject‡.

It will be seen that the opinions of the chemists which I have just shortly reviewed are of three kinds, and may be stated as follows:—1. Indigo-blue exists ready formed in the plants from which it is derived. 2. It is contained in these plants in the form of reduced indigo. 3. It does not pre-exist in the vegetable, but is formed subsequently to the extraction of the latter by means of a process of fermentation, a process which manifests itself by the evolution of gases of various kinds. To each of these views very strong objections may be raised. If the colouring matter is formed at once in the plant, it is difficult to conceive by what means it comes to be dissolved by water, for no combination of indigo-blue with any organic substance can be produced which is soluble in water. If to this it be objected, that a compound of this nature is produced by the plant, and cannot after decomposition be reproduced, then it is at once admitted that indigo-

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* *Journal de Pharmacie,* vol. xxvi. p. 290.

† Ibid. p. 344.

‡ I cannot refrain from expressing on the present occasion my regret, which is probably shared by many others, at the want of a general chemical bibliography, comprising references to all the known works, treatises, papers, &c. on chemical subjects written since the commencement of the modern æra in chemistry. In searching for the authorities referred to in this paper, I have felt this want very sensibly. It is with some difficulty that the mere names of all the works and memoirs relating to any special branch of the science, particularly such as have fallen into oblivion, are discovered. The only attempt to supply this deficiency, and that only in regard to one department of chemistry, is Wolff's *Quellenliteratur der Organischen Chemie.* Halle, 1845.

blue is not contained as such within the vegetable. That it cannot exist as reduced indigo is evident, since the latter requires the presence of some alkali for its solution in water, and the juice of most, if not all, indigo-bearing plants is acid. It is difficult, moreover, to conceive how deoxidized indigo, a body having so great an affinity for oxygen, can exist in the interior of plants which we know are constantly evolving that element. That the colouring matter is formed by the process of fermentation to which the extract of the plant is subjected, as it is the oldest, so it is the most probable view. Nevertheless, the fact that indigo may be procured from plants by mere infusion with hot water and precipitation with lime-water, without any of the usual signs of fermentation being manifested, appears to militate against this view. On one point all authorities seem to agree, viz. that the contact with oxygen is a necessary condition of the formation, or at least precipitation of the indigo from the watery extract*.

Such being the state of our knowledge on this rather obscure department of chemical science, I resolved, though without anticipating any very decided success, to endeavour to throw a little more light on it. I was induced to do so chiefly by the following consideration. The principal vegetable colouring matters have now been discovered to be not direct products of the vital energy of plants, but products of decomposition of substances contained in the vegetable, which are themselves mostly colourless. The formation of these colouring matters takes place equally well out of the plant as within it. Indeed, it is probable that it never happens within the plant until decay has commenced, or at least until the vital energy has begun to decline. The processes of decomposition by which colouring matters are formed from other substances are of two kinds. The first consists in the absorption of oxygen and the elimination of hydrogen in the form of water; it is a process of decay (*Verwesung*, Liebig), and requires the presence, not only of oxygen, but of some alkali or other base. The second process is one which consists in the splitting up of the original compound into two or more simpler bodies, of which one or more are colouring matters; it is a process of fermentation, and may in general be effected as well by the action of strong acids as by that of ferments. The first process gives rise to colouring matters of a very fugitive nature, such as the colouring matters of logwood and archil. Indeed, in this case the colouring matter, if this name be applied merely to substances

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* Gehlen is the only chemist who, as far as I am aware, has asserted that the agitation with air in the manufacture of indigo may have for its object, not so much the oxidation as the aggregation or separation of the particles of indigo from the solution. *Vide* Schweigger's Journal, vol. vi. 1812.

endowed with a striking and positive colour, is only one of a long chain of bodies succeeding one another, and is generally not the last product of decomposition. The other process, of which the formation of alizarine is an example, yields colouring matters of a fixed and stable character, which are not further changed by a continuance of the process to which they owe their formation. Now if indigo-blue be a body which is formed from some colourless substance existing in the plant, we should infer *à priori* that the process by which it is formed is one of fermentation or putrefaction, not requiring the intervention of oxygen or of alkalies; a conclusion, however, so much at variance with the generally received ideas on the subject of the formation of indigo-blue, as to require the aid of very decisive experiments for its establishment.

I shall now proceed to give an account of the experiments which I have undertaken with a view to elucidate this subject.

The only plant cultivated in this country which is known to yield indigo in any quantity is woad, *Isatis tinctoria*; and as it was necessary to examine the indigo-bearing plant in a fresh state and in considerable quantities, I had recourse to this one for the purpose. Having procured 10 lbs. of good French woad seed, I sowed it at the commencement of the spring of last year on about half an acre of land. It was sown in drills about two feet apart, each drill being previously well supplied with farmyard manure. In a short time the young plants appeared, and grew vigorously during the summer months. Some of the plants bore flowers, and ripened their seeds in the course of the autumn.

At no time during the whole progress of the growth were there any visible indications of the presence of blue colouring matter on the leaves or stems. Some of the ripe seeds only were tinged with a dark purple colour, forming a thin coating on the exterior. The greater proportion of the plants, however, bore leaves which did not exhibit the glaucous appearance nor the fleshy consistency, which, according to authors, are characteristic of the cultivated variety of woad. They were, on the contrary, of a bright grass-green, and possessed but little succulence—characters which belong rather to the wild variety.

As soon as I could collect a small quantity of leaves, I commenced my experiments. Having taken some leaves, I chopped them fine and then extracted them with boiling water. The filtered liquid was light brown and transparent, it had a bitter taste and an acid reaction. It deposited no indigo-blue, however long it might be left exposed to the atmosphere, and hence it might have been inferred that it contained no indigo-blue. Nevertheless, a very simple experiment sufficed to show that it was capable of yielding an appreciable quantity of that colouring matter. On adding to it sulphuric or muriatic acid and boiling, it became of a darker colour, and deposited a quantity of dark brown, almost black flocks. Now these flocks contained indigo-blue; for if after collecting them on a filter and washing out the acid, they were treated with boiling alcohol, they communicated to the latter a bright blue colour; and on being treated with a boiling alkaline solution of protoxide of tin, they gave a yellow solution, which on exposure to the air became covered with a thin blue film. A small quantity of finely-chopped woad leaves having been pounded in a mortar with water until converted into a uniform green pulp, yielded on being strained through calico a dark green opake liquor. On heating this liquor to near the boiling-point, the vegetable albumen contained in it coagulated, carrying down with it the green colouring matter. On now filtering through paper, a green coagulum was left on the filter, while a clear light yellow liquid ran through. On adding acetate of lead to the liquid, a yellow precipitate fell; and on again filtering, the liquid ran through almost colourless. The lead precipitate being decomposed with dilute sulphuric acid, the filtered acid liquid was boiled, when it gave a few black flocks, which, however, contained no indigo-blue*. If, however, sulphuric or muriatic acid in excess were added to the liquid filtered from the lead precipitate, the liquid soon became green, and began to deposit indigo-blue even before it could be filtered from the sulphate or chloride of lead, and after filtration and boiling it yielded flocks containing an abundance of the colouring matter. If acetate of lead was added to an extract of woad leaves made either with hot or cold water, if the precipitate thereby produced was separated by filtration and ammonia was added to the filtrate, a pale yellow precipitate fell. This precipitate having been decomposed with sulphuric acid, the filtered acid liquid gave on boiling flocks containing indigo-blue, while the flocks obtained in a similar manner by boiling with acid the liquid filtered from this precipitate gave no indigo-blue. If this lead precipitate was treated with a cold concentrated solution of carbonate of soda, a yellow solution was obtained, which on being tried, as before, with sulphuric acid, afforded no indigo-blue; whilst the precipitate, on being treated with acid, gave indigo-blue as before. But if the lead precipitate was suspended in water, and a current of carbonic acid gas was passed for some time through the liquid, it was completely decomposed: its colour changed from yellow to white, and it now consisted almost entirely of carbonate of lead, whilst the liquid had acquired a yellowish colour, and on being boiled with acid, deposited a quantity of blue flocks, which con-

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* If the acetate of lead is in the least degree contaminated with basic acetate, it will be found to precipitate some of the indigo-producing body.

sisted of indigo-blue in a state of great purity, as they dissolved in boiling alcohol with a beautiful blue colour, the alcohol depositing on cooling crystalline scales, which were blue by transmitted, and copper-coloured by reflected light. If, instead of adding acid to any of the solutions yielding indigo-blue, caustic soda was first added in excess and the solution was left for a few moments, and then boiled with an excess of acid, it merely became brown without depositing any indigo-blue. Having taken some finely-chopped woad leaves, I pounded them in a mortar with cold alcohol. On filtering, I obtained a clear green solution, leaving on evaporation at a gentle heat a green syrup, from which, on the addition of water, a quantity of chlorophyll and fatty matter separated in drops. The watery solution, which after filtration had only a yellowish tinge, on being boiled with the addition of sulphuric acid, deposited a quantity of purple flocks, which were treated, after filtration and washing with water, with successive portions of boiling alcohol. The first portions of the alcohol with which they were treated acquired a beautiful purple colour, and the last portions a pure blue, each portion depositing, on standing, some flocks of a fine blue colour. The green mass insoluble in water contained no indigo-blue. Having carefully dried a few woad leaves, I reduced them to powder and then treated them in a bottle with cold æther. I obtained a dark green solution, which after being filtered and evaporated, spontaneously left a green syrupy residue, from which water extracted, as in the preceding case, a substance which, by the action of boiling sulphuric acid, yielded an abundance of very pure indigo-blue.

By these and similar simple and easily-performed experiments, I was enabled to infer, with positive certainty, that the *Isatis tinctoria* contains a substance easily soluble in hot and cold water, alcohol and æther, which, by the action of strong mineral acids, yields indigo-blue; that the formation of the colouring matter from it can be effected without the intervention of oxygen or of alkalies; and that the latter, indeed, if allowed to act on it before the application of acid, entirely prevent the formation of colouring matter, and it now only remained to separate it from the other constituents of the plant and ascertain its properties and composition. But, though I arrived at the conclusion just stated without any great difficulty, I found that the isolation and preparation, in a state of purity, of the substance whose existence had been indicated by these experiments, constituted a problem of no easy solution. I soon discovered that this body is extremely liable to decomposition; so much so, as completely to justify the assertion of an author, who, in speaking of the difficulties of the manufacture of indigo, says that "nothing is more fugitive, and more liable to be acted on by destructive agencies, than the colouring principle of the *Indigoferæ**." The continued action of water, even at a moderate, but especially at a high temperature, as well as that of alkalies, I found to induce a complete change in the body which I was endeavouring to isolate. The fact of its being completely precipitated from the watery extract of woad by means of acetate of lead and ammonia, and of the lead compound being readily decomposed by means of a current of carbonic acid gas, seemed at first to lead to an easy method of preparation. But on extracting a large quantity of the plant with cold water, adding sugar of lead to precipitate the albumen and green colouring matter, and then adding ammonia to the filtered liquid, I found that the precipitate produced by ammonia, though the alkali was not used in excess, contained the substance in an altered state. This change, the nature of which I shall treat of presently, is readily indicated by boiling some solution of the substance in water with sulphuric or muriatic acid, when it will be found that black flocks are deposited, which, when treated with boiling alcohol, impart to the latter no trace of blue, but a pure brown colour, a great part of the flocks remaining undissolved by the alcohol in the shape of a black powder. This change is readily effected in the watery solution, either by the addition of alkali, or by the temperature of the solution rising to any great extent above that of the atmosphere. When dissolved in alcohol or æther, on the other hand, the substance exhibits a much greater resistance to change than when dissolved in water. I therefore soon abandoned all idea of extracting the plant with the latter menstruum, and had recourse to alcohol and æther. I shall refrain from giving an account of the numerous experiments I made, which led to no successful issue, but shall briefly describe the three methods of preparation, which were attended with more or less advantageous results.

All three methods consist in extracting with alcohol or æther. But, before commencing, it is necessary that the plant should be thoroughly dried. The leaves alone being gathered, which should only be done when not moistened with rain or dew, are spread out in a thin layer in some warm, dry place, as, for instance, near a stove or in a drying-room. All yellow or decayed leaves should be rejected. When they are perfectly dry they are carefully examined. All those leaves which during the drying have become brown—a result which takes place when the heat has been too great,—are cast aside, and only those are retained which have a pale green colour. These must now be reduced to a coarse powder, an operation easily effected if the leaves be taken while still warm, as they are then quite brittle; whereas if

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* Perrottet, *Art de l'Indigotier*, p. 110.

allowed to remain exposed to the atmosphere they attract moisture again, and become flaccid and difficult to pulverize. The powder must be kept warm until it is used.

I. The leaves thus reduced to powder, being put in a displacement apparatus, such as is used for the preparation of tannic acid, are extracted with cold alcohol. The percolating liquid is dark green. The extraction is continued until the liquid runs through of a light green colour. The alcoholic extract is now distilled in a retort, until a great part of the alcohol has passed over; it is then evaporated in a basin at a moderate temperature. During evaporation, a large quantity of chlorophyll and fatty matter separates as a green glutinous mass, which is to be separated by filtration. The filtered liquid, which is brown, is to be further evaporated until it leaves a thick brown syrup. This syrup, being poured into a flask, is redissolved in warm alcohol, which it does with some difficulty, on account of the change which the substance has undergone in consequence of the heat employed in distilling the alcohol. To the warm solution there is now added several times its volume of æther. The æther renders the solution milky, and precipitates a large quantity of matter, which collects at the bottom of the flask into a dark brown syrup. After this syrup has completely settled, the æther, which is also dark brown, is distilled. When the greatest part has distilled over, water is added to the syrupy residue. A quantity of brown fatty matter is precipitated, which is filtered. The filtered liquid is light yellow, and contains the substance almost in a state of purity. Should a further purification, however, be necessary, this is best effected by agitating the liquid in the cold with hydrated oxide of copper. A green solution is obtained, which is filtered, and a current of sulphuretted hydrogen gas being passed through it, it is again filtered from the precipitated sulphuret of copper, and evaporated either spontaneously or *in vacuo* over sulphuric acid. This method is attended with considerable loss, as the whole of the brown syrup insoluble in æther, the quantity of which is not trifling, is formed at the expense of the indigo-producing body.

II. The second method consists in simply extracting the pounded woad leaves with æther in a displacement apparatus, distilling the greatest part of the æther, evaporating the remaining green liquid at a moderate temperature, adding a little cold water to the syrupy residue, separating the insoluble chlorophyll and other matters by filtration and evaporating the yellow liquid as before, either spontaneously in the air or *in vacuo*. If purification should be necessary, it is effected, as in the preceding case, by means of oxide of copper. I endeavoured to modify this method by agitating the ætherial extract with cold water, but without any advantage resulting, as the substance was not entirely removed from the æther by means of water, a considerable quantity still remaining dissolved in the æther.

III. The pounded woad leaves are extracted in a displacement apparatus, as before, with cold alcohol. To the green alcoholic extract there is added an alcoholic solution of acetate of lead, which produces a pale green precipitate, the precipitation being then completed by the addition of a little ammonia. The precipitate, which is bulky, is placed on a filter and washed with cold alcohol, until the percolating liquid instead of being dark green is only light green, and the excess of acetate of lead and ammonia have been removed. It is then suspended in water, and a current of carbonic acid gas is passed through the liquid. The precipitate gradually becomes paler in colour and at last almost white, and loses considerably in bulk, while the liquid acquires a yellow colour. The latter being filtered, sulphuretted hydrogen is passed through it to precipitate a little oxide of lead contained in it, and being again filtered is evaporated as before, either in the air or *in vacuo* over sulphuric acid.

Of these three methods I prefer the last, as being more expeditious than the first and less costly than the second. One precaution must not be forgotten, that of never in the last instance attempting to evaporate the solution at a higher temperature than the ordinary one. If the attempt be made, the substance will undergo an entire change, as I have repeatedly discovered to my cost. This change consists in the substance taking up the elements of water. It takes place as well *in vacuo* as in the air. I have no reason to believe that the oxygen of the atmosphere has any influence in producing decomposition, at least at the usual temperature; and though I have generally in the last instance evaporated *in vacuo,* I think the evaporation may just as well be conducted in the air. Notwithstanding all precautions, however, it is difficult to avoid some portion of the substance becoming changed during evaporation.

The body, the preparation of which I have just described, I propose to call *Indican**. By evaporation of its watery solution, it is obtained in the form of a yellow, transparent, glutinous residue, which can only be rendered dry by spreading it out in

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* As the termination *an* has not yet been applied by chemists to designate any peculiar class of bodies, I propose to restrict it to the names of such substances of a complex constitution, like rubian, as are direct products of the vital energy of plants or animals, and which by their decomposition give rise to one or more series of organic compounds of a simpler constitution. In applying it, it will of course be necessary carefully to ascertain that the substance to be named is really a proximate constituent of some organism, and not itself the result of any process of decomposition either within or out of the organism—that it is an educt and not a product.

thin layers, and leaving it for some time *in vacuo* over sulphuric acid. On attempting to dry it in the water-bath, it immediately undergoes a complete alteration. Its taste is slightly bitter and nauseous. Its solutions have always an acid reaction, but whether this reaction is peculiar to it in an absolutely pure state, I am unable to say. When heated in a tube it swells up, and gives fumes which condense to a brown oily sublimate, in which, after some time, a white crystalline substance is formed. When boiled with caustic alkali it evolves ammonia. Its compounds have a yellow colour. With caustic alkalies, baryta, and lime-water, the watery solution turns of a bright yellow. The alcoholic solution gives with sugar of lead a bright sulphur-yellow precipitate, which is increased by the addition of ammonia. The watery solution gives no precipitate with acetate of lead until ammonia is also added. Its most remarkable and interesting property is that of yielding indigo-blue when treated with strong acids. If sulphuric or muriatic acid be added to its watery solution, no change whatever is perceptible for some time; but on heating to near the boiling-point, the solution immediately becomes sky-blue. On boiling for a short time, the solution becomes opalescent. On continuing to boil, it acquires a purple colour, and then, provided the solution is tolerably concentrated, a copious deposit consisting of dark purplish-blue flocks is formed. The liquid filtered from these flocks retains a yellow colour, and contains a peculiar species of sugar, to which I shall return presently. The flocks themselves do not consist of indigo-blue only. After being collected on a filter and washed with water, they appear of a dark purple colour, the filter also acquiring during washing a purple tinge. If they be now treated with alcohol, a part dissolves even in the cold, but to a greater extent on heating, the alcohol acquiring a beautiful purple colour. If the flocks remaining undissolved be treated after filtration with an additional quantity of boiling alcohol, the latter acquires a more bluish tinge. Each succeeding portion of alcohol with which the flocks are boiled acquires more and more of a blue colour, until at last the colour is a pure indigo-blue. There remains in general a large quantity of indigo-blue undissolved, and the alcoholic liquids, on standing, deposit the colouring matter contained in them in the shape of bright blue flocks. The purple alcoholic solution leaves, on evaporation, a reddish-brown residue, which bears the greatest resemblance to, if it is not identical with, the indigo-red of Berzelius. Like the latter substance, it is quite insoluble in caustic alkalies and gives, when heated in a tube, purple fumes and a small quantity of a white crystalline sublimate. I propose to call this substance *Indirubine.* I have found that it is invariably formed along with indigo-blue whenever indican is decomposed by acids. Nevertheless, the quantity of indigo-blue produced is always relatively larger when the indican is pure than when the latter has begun to change. Of the two colouring matters, the indigo-blue is always the first to be formed. If sulphuric or muriatic acid be added to a solution of indican in the cold, and the mixture be allowed to stand in the cold for some time, a slight precipitate is gradually deposited which consists almost entirely of indigo-blue. It is only after boiling for some time that the formation of indirubine commences, when the colour of the liquid changes from blue to purple. The constant occurrence of a red colouring matter both in indigo and in the indigo-bearing plants along with indigo-blue—a fact which has been repeatedly observed,—has led chemists to suspect that there must be some necessary connexion between the two. From the experiments just described, it follows that, in the case of woad at least, they are both products of decomposition of one substance. If nitric acid be added to a watery solution of indican, a slight deposit of indigo-blue is formed, which of course disappears immediately on heating the liquid.

There is another very remarkable property of indican which I have to describe, a property, the knowledge of which will probably throw great light on the process of manufacturing indigo. If indican in the form of syrup, as obtained by evaporation of the watery solution, be heated for some time in the water-bath, or if its watery solution be boiled, or even moderately heated, it undergoes a complete metamorphosis. If the solution be now evaporated, it leaves a yellow syrupy residue, not to be distinguished in appearance from indican itself. It will be found, however, to have become insoluble in æther, and not easily soluble in alcohol. If æther be added to its solution in alcohol, the solution becomes milky, and deposits oily drops which collect at the bottom of the vessel to a yellow or brown syrup, the unchanged indican, if there be any present, remaining dissolved in the æther. In its other outward properties it has not undergone any marked change. In the next stage of the process the indican acquires a brown colour, and becomes quite insoluble in cold alcohol; but it still dissolves, though with difficulty, in boiling alcohol. By continuing the process the substance acquires a dark brown colour, and its watery solution now gives a copious precipitate with acetate of lead. As soon as the indican has entered even on the first stage of this process of change, it ceases to give the least trace of indigo-blue with acids. A short period occurs at the commencement of the process, during which the watery solution, when boiled with sulphuric acid, deposits purple flocks consisting of indirubine only. Afterwards, however, it yields other products, which are always the same at every subsequent stage of the process. If the watery solution be then boiled with the addition of sulphuric or muriatic acid, it becomes in the first instance of a darker colour, and after considerable boiling, deposits slowly a quantity of dark brown, almost black flocks. The liquid filtered from these flocks contains sugar, just as in the case of indican itself. The flocks themselves generally consist of two bodies. If they be collected on a filter, washed with water, and then treated with boiling alcohol, a part dissolves with a brown colour, and after filtration and evaporation, is left as a dark brown, shining, resinous substance. This substance melts in boiling water into brown coherent masses. It is completely dissolved and decomposed by boiling nitric acid. It dissolves in ammonia with a brown colour, and the solution gives brown precipitates with the chlorides of barium and calcium. It is completely precipitated from its alcoholic solution by sugar of lead, the precipitate being brown. I propose to call this body *Indiretine.* That portion of the dark brown flocks which is insoluble in boiling alcohol, dissolves in caustic alkalies with a dark brown colour, and is precipitated by acids in black flocks. As it bears some resemblance in its outward properties to humus, I shall call it *Indihumine.* Its similarity to the indigo-brown of Berzelius is so great, as almost to lead me to suspect that it is the same body. Whether this is the case or not can only be ascertained by analysis. Sometimes the brown substance formed by the action of acids on modified indican is entirely soluble in boiling alcohol, and contains no indihumine, but under what conditions this takes place I am unable to say. The change which indican undergoes during this process consists merely in its absorbing the elements of water. It proceeds *in vacuo* as well as in the air, provided the temperature be raised to a certain degree, which proves that oxygen plays no part in the process. It is apparently effected instantly when indican comes into contact with alkalies in its watery solution, though the alcoholic solution may be made alkaline, with ammonia at least, without any alteration taking place. It is certainly a most remarkable circumstance, that by merely taking up the elements of water, indican should be converted into a substance, which, when exposed to the action of acids, yields no longer indigo-blue and its allied red colouring matter, but bodies of an entirely distinct nature which have none of the properties of colouring matters; and it goes far to prove that indigo-blue does not pre-exist in indican even as a copula, but is merely contained in it potentially. That the view I have taken of this metamorphosis is the correct one, is proved by a singular observation which I once accidentally made. Having on one occasion obtained a dilute solution of indican, I tried a small quantity of it by boiling with sulphuric acid, and ascertained that it gave indigo-blue and indirubine; but on heating the whole quantity to the boiling-point with acid, I obtained instead of these two bodies, a substance dissolving in alcohol with a brown colour. It was evident that the indican, in consequence probably of the solution being very dilute, had taken up the elements of water before the acid could act on it, and that the latter then gave rise to the products of decomposition peculiar to the hydrate. In general, however, the nature of the flocks which are deposited on boiling a watery solution of indican with sulphuric or muriatic acid, affords a very good test of the purity of the indican. A solution is boiled in a test-tube partly filled, and the flocks which are formed collected on a filter, washed with water, and then treated with successive portions of boiling alcohol until no more will dissolve. If to the first portions of alcohol a purple colour be imparted, and a fine purplish-blue to the succeeding ones, then the indican may be considered pure. If the colour of the alcohol is brown, and if black flocks are left undissolved, the substance has undergone a complete change.

The sugar which is formed when acids act either on indican or its hydrates, is obtained in a state of purity in the following manner. If sulphuric acid be employed, which is preferable to muriatic, the acid liquid is filtered from the flocks consisting of indigo-blue and other products of decomposition, and the acid is removed by means of an excess of acetate of lead. If to the liquid filtered from the sulphate of lead an excess of ammonia is added, the sugar is precipitated in combination with oxide of lead. The precipitate, which is usually yellow and bulky, is, after washing, decomposed with sulphuretted hydrogen, and from the liquid filtered from the sulphuret of lead the sugar is again precipitated with acetate of lead and ammonia. The second precipitate, which is usually almost white, is again decomposed with sulphuretted hydrogen, and the filtered liquid is evaporated over sulphuric acid, when it leaves a colourless or only slightly yellow syrup, which has the following properties. It has a faintly sweet taste. When heated it swells up, emitting the usual smell of burning sugar, and then burns leaving much charcoal. With concentrated sulphuric acid it strikes a dark red colour, which on heating becomes black. Boiling nitric acid decomposes it with an evolution of nitrous fumes. When its watery solution is boiled with caustic soda, it becomes yellow and deposits a few brown flocks. With sulphate of copper and caustic soda it gives a blue solution, which on boiling becomes yellow, and then deposits suboxide of copper. If nitrate of silver be added to its watery solution while boiling, a little metallic silver is precipitated; and when ammonia is added, a further reduction takes place, accompanied by the formation of a metallic mirror. On adding chloride of gold to the watery solution and boiling, a quantity of metallic gold is deposited in bright scales and spangles; and on adding caustic alkali to the filtered solution, an additional quantity of gold is precipitated as a purple powder. The watery solution gives no precipitates either with neutral or basic acetate of lead,—only on adding ammonia does any precipitation take place. It is soluble in alcohol, but not in æther. In its outward properties, therefore, this sugar does not differ in any marked degree from other kinds of sugar obtained by the decomposition of complex organic bodies, such as that derived from rubian. In its composition, however, it differs essentially from other species of sugar, as I shall presently show.

I have hitherto been unable, I regret to say, to ascertain the exact composition of indican by direct experiment. On account of its deliquescent nature, and its so readily undergoing change when heated, it was impossible to subject it to analysis in a free state, and I was therefore obliged to have recourse to the lead compound. But when this compound is precipitated from a watery solution by means of acetate of lead and ammonia, it no longer contains unchanged indican, but one of the bodies formed by the combination of the latter with water. It is necessary, however, to use water in some stage of the preparation; for if the lead compound be precipitated from an alcoholic extract of woad with acetate of lead and ammonia, if the precipitate be decomposed, after washing with alcohol, by suspending it in alcohol and passing a stream of carbonic acid through the liquid, and the substance be again precipitated from the filtered liquid by means of acetate of lead and ammonia, the lead compound thus formed will be found to contain, besides indican, a quantity of fatty matter, from which the indican can only be separated by means of water, and its analysis leads, as I have ascertained, to no satisfactory results. It is therefore necessary to evaporate a watery solution of indican spontaneously, to dissolve the residue in alcohol, and precipitate with acetate of lead and ammonia, taking care to leave a slight excess of indican in solution. The following analysis was made with a specimen of the lead compound prepared in this manner, the indican itself having been obtained by the third method described above.

I. 1·4340 grm. of the compound, dried *in vacuo* and burnt with oxide of copper and chlorate of potash, gave 0·9600 grm. carbonic acid and 0·2860 water.

1·2170 grm. gave 0·1700 grm. chloride of platinum and ammonium.

0·3790 grm. gave 0·3080 grm. sulphate of lead.

These numbers lead to the following composition:—

	Eqs.		Calculated.	Found.
Carbon	52	312	18·67	18·25
Hydrogen . .	35	35	2·09	2·21
Nitrogen . . .	1	14	0·83	0·87
Oxygen	38	304	18·23	18·88
Oxide of lead .	9	1005·3	60·18	59·79
		1670·3	100·00	100·00

Notwithstanding the care, however, which I took in the preparation of this specimen, I found that it did not contain unchanged indican, as a little of it when tested with sulphuric acid gave no indigo-blue. It is, nevertheless, the purest specimen of the lead compound which I have analysed; that is to say, the substance combined with the oxide of lead contained the least amount of hydrogen and oxygen.

The next analysis which I shall give, places in a striking light the effect which alkalies exert on indican. I took some of the same solution of indican which I had employed for the preceding analysis, and which I found to give, when a little of it was boiled with acid, very pure indigo-blue; but instead of evaporating it, I added a large quantity of alcohol to it, and then precipitated with acetate of lead and ammonia. The precipitate no longer contained unchanged indican, and the substance combined with the oxide of lead differed in composition from that of the preceding analysis by containing the elements of two equivalents more of water. The third analysis was performed with a lead compound made in the same way as that of the first analysis, but from a specimen of indican prepared by the first method. The composition I found to be exactly the same as that of the compound of the second analysis.

II. 1·0960 grm. dried *in vacuo* and burnt with oxide of copper and chlorate of potash, gave 0·7060 grm. carbonic acid and 0·2020 water.

1·5600 grm. gave 0·1880 grm. chloride of platinum and ammonium.

0·8930 grm. gave 0·7520 grm. sulphate of lead.

III. 1·1000 grm. gave 0·7135 grm. carbonic acid and 0·2050 water.

1·8350 grm. gave 0·2260 grm. chloride of platinum and ammonium.

0·5710 grm. gave 0·4770 grm. sulphate of lead.

From these numbers I deduced the following composition:—

	Eqs.		Calculated.	II.	III.
Carbon . .	52	312	17·33	17·56	17·69
Hydrogen .	37	37	2·05	2·04	2·07
Nitrogen .	1	14	0·77	0·75	0·77
Oxygen . .	40	320	17·80	17·69	18·01
Oxide of lead	10	1117	62·05	61·96	61·46
		1800	100·00	100·00	100·00

After deducting the oxide of lead, the amount of which is unusually large for a compound of definite constitution, the organic substance combined with it has the following composition:—

	Eqs.		Calculated.	II.	III.
Carbon . .	52	312	45·68	46·16	45·90
Hydrogen .	37	37	5·41	5·36	5·37
Nitrogen .	1	14	2·04	1·97	1·99
Oxygen . .	40	320	46·87	46·51	46·74
		683	100·00	100·00	100·00

An analysis which I made of a lead compound, prepared directly from a watery extract of woad by precipitating with acetate of lead, filtering, and then adding ammonia to the filtered liquid, gave a composition agreeing pretty well with the formula $C^{52}H^{43}NO^{46}+13PbO$.

It appears, therefore, that the organic substances contained in these lead compounds differ from one another merely by the elements of water; and it may hence be inferred, with a great degree of probability, that indican itself in a state of purity differs in composition from them merely by containing the elements of several equivalents of water less. Assuming its composition to be represented by the formula $C^{52}H^{33}NO^{36}$, it must be shown how this formula explains the formation of indigo-blue. Before this can be done, however, it is necessary to know the composition of the sugar which is always formed simultaneously with the indigo-blue. The lead compound of the sugar, prepared as above described, and dried *in vacuo*, was analysed with the following results:—

I. 1·0580 grm., burnt with chromate of lead, gave 0·4550 grm. carbonic acid and 0·1670 water.

0·7620 grm. gave 0·7490 grm. sulphate of lead.

II. 1·0960 grm., after being dried *in vacuo* for some time longer, gave 0·4740 grm. carbonic acid and 0·1640 water.

0·6420 grm. gave 0·6370 grm. sulphate of lead.

These numbers lead to the following composition:—

	Eqs.		Calculated.	I.	II.
Carbon . .	12	72	11·69	11·72	11·79
Hydrogen . .	9	9	1·46	1·75	1·66
Oxygen . .	11	88	14·30	14·21	13·55
Oxide of lead	4	446·8	72·55	72·32	73·00
		615·8	100·00	100·00	100·00

If the formula of the lead compound is $C^{12}H^{9}O^{11}+4PbO$, it is probable that the sugar in an uncombined state has the formula $C^{12}H^{10}O^{12}$. It differs, therefore, from other kinds of sugar by containing less hydrogen than is necessary to form water with the oxygen. Its formula is, however, perfectly in accordance with the one which I have adopted for indican, viz. $C^{52}H^{83}NO^{36}$. If we suppose the latter to take up two equivalents of water, it will then simply split up into 1 equivalent of indigo-blue and 3 equivalents of sugar, as will be seen by the following equation:—

$$\left.\begin{array}{ll}\text{1 eq. indican} & C^{52}H^{33}NO^{36} \\ \text{2 eqs. water} & \quad\;\, H^{2} \quad O^{2} \\ \hline & C^{52}H^{35}NO^{38}\end{array}\right\} = \left\{\begin{array}{ll}C^{16}H^{5}NO^{2} & \text{1 eq. indigo-blue.} \\ C^{36}H^{30}\;\;O^{36} & \text{3 eqs. sugar.} \\ \hline C^{52}H^{35}NO^{38} & \end{array}\right.$$

I assume that the composition of the blue colouring matter derived from indican is the same as that of indigo-blue, since an examination of the properties of the former leaves little doubt concerning their identity. I regret not having as yet been able to ascertain by analysis whether this is the case or not, as I have devoted the whole of the material at my disposal to an investigation of the properties and composition of indican itself.

I also assume that indican, in undergoing decomposition with acids, splits up immediately into 1 equivalent of indigo-blue and 3 equivalents of sugar. It is, however, possible that these three equivalents of sugar may not be eliminated all at once; and from one analysis which I made, I should conclude that they separated successively. Having extracted some dried woad leaves with cold æther, I poured the ætherial extract into a large bottle and agitated it with about half its volume of cold water. The æther was poured off, and the watery liquid was employed again for agitation with several successive portions of ætherial extract of woad. It acquired at last a dark yellow colour. The æther contained in it was removed by spontaneous evaporation, and it was then evaporated under a bell over sulphuric acid. At first it yielded indigo-blue when boiled with sulphuric acid; but the evaporation having been conducted in too warm a place, the indican contained in it became changed, and it ceased to give blue flocks with acids. After the evaporation was completed there was left a brown syrupy residue, which was redissolved in alcohol.

Acetate of lead produced in the alcoholic solution a cream-coloured precipitate, which was separated by filtration; and on adding a small quantity of ammonia to the filtered liquid, a cream-coloured precipitate again fell, which was collected on a filter, washed with alcohol, and dried *in vacuo*. On analysing it I obtained the following results:—

0·3705 grm., burnt with oxide of copper and chlorate of potash, gave 0·2670 grm. carbonic acid and 0·0730 water.

0·3800 grm. gave 0·0680 grm. chloride of platinum and ammonium.

0·2680 grm. gave 0·2160 grm. sulphate of lead.

In 100 parts it contained, therefore,—

Carbon	19·65
Hydrogen	2·18
Nitrogen	1·12
Oxygen	17·75
Oxide of lead	59·30
	100·00

After deducting the oxide of lead, the amount of which stands in no simple relation to that of the other constituents, the organic substance combined with the oxide of lead will be found to have a composition expressed by the formula $C^{40}H^{26}NO^{27}$, as will be seen by the following calculation:—

	Eqs.		Calculated.	Found.
Carbon	40	240	48·38	48·28
Hydrogen	26	26	5·24	5·35
Nitrogen	1	14	2·82	2·75
Oxygen	27	216	43·56	43·62
		496	100·00	100·00

This body is therefore formed from indican by the latter taking up 3 equivalents of water and then losing 1 equivalent of sugar, as will be evident from the following equation:—

$$\left.\begin{array}{ll}\text{1 eq. indican} & C^{52}H^{33}NO^{36}\\ \text{3 eqs. water} & \underline{\quad H^{3}\quad O^{3}}\\ & C^{52}H^{36}NO^{39}\end{array}\right\} = \left\{\begin{array}{ll} C^{40}H^{26}NO^{27} & \\ \underline{C^{12}H^{10}\quad O^{12}} & \text{1 eq. sugar.}\\ C^{52}H^{36}NO^{39} & \end{array}\right.$$

It is perfectly conceivable that an additional equivalent of sugar may separate from the body $C^{40}H^{26}NO^{27}$ before the decomposition is finally completed; that is to say, that there exists another intermediate body containing 28 equivalents of carbon.

Of the red colouring matter, which I have called indirubine, I have not yet obtained a sufficient quantity for analysis.

I have several times submitted indihumine, obtained on different occasions, to analysis, but without being able to arrive at any positive conclusion regarding its composition. The difficulty of doing so arises from the circumstance of this substance forming compounds with alkalies and other bases which are not completely decomposed by acids. The analyses which I have made agree best with the formula $C^{16}H^{8}NO^{5}$. If this be the correct formula, it differs in composition from indigo-blue by the elements of three equivalents of water, just as the body or bodies from which it is formed differ from indican by containing the elements of several equivalents more water.

The analyses of indiretine led to more definite results. The substance was prepared from indican, which had undergone the alteration of which I have several times spoken, by treating it with boiling sulphuric acid, collecting the dark brown deposit which was formed on a filter, washing out the acid, treating with boiling alcohol, filtering from the indihumine which remained undissolved, and evaporating the alcoholic solution to dryness.

I. 0·4420 grm., dried in the water-bath and burnt with oxide of copper and chlorate of potash, gave 0·9930 grm. carbonic acid and 0·2100 water.

0·5470 grm. gave 0·3415 grm. chloride of platinum and ammonium.

II. 0·5370 grm. of another preparation gave 1·2130 grm. carbonic acid and 0·2675 water.

0·8780 grm. gave 0·4320 grm. chloride of platinum and ammonium.

III. 0·2760 grm. of a third preparation gave 0·6190 grm. carbonic acid and 0·1420 water.

These numbers correspond with the following composition:—

		Eqs.	Calculated.	I.	II.	III.
Carbon .	36	216	61·02	61·27	61·60	61·16
Hydrogen	20	20	5·64	5·27	5·53	5·71
Nitrogen	1	14	3·95	3·92	3·09	
Oxygen .	13	104	29·39	29·54	29·78	
		354	100·00	100·00	100·00	

Assuming $C^{36}H^{20}NO^{13}$ to be the correct formula for indiretine, then the formation of this substance from indican, or rather from its hydrates, can only be explained by supposing that carbonic acid is evolved during the process. If we take, for instance, the substance represented by the formula $C^{52}H^{37}NO^{40}$, which gives indiretine when treated with acids, we may suppose it to split up into 1 equivalent of indiretine, 1 equivalent of sugar, 4 equivalents of carbonic acid, and 7 equivalents of water, as will be seen by the following equation:—

$$C^{52}H^{37}NO^{40} = \left\{ \begin{array}{llll} C^{36} & H^{20} & NO^{13} & \text{1 eq. indiretine.} \\ C^{12} & H^{10} & O^{12} & \text{1 eq. sugar.} \\ C^{4} & & O^{8} & \text{4 eqs. carbonic acid.} \\ & H^{7} & O^{7} & \text{7 eqs. water.} \\ \hline C^{52} & H^{37} & NO^{40} & \end{array} \right.$$

I have certainly not observed the disengagement of carbonic acid during the formation of indiretine, but the evolution of gas might easily elude observation during the long-continued boiling which is necessary for the production of this substance.

The want of material has for the present interrupted the further prosecution of my experiments. As soon as I shall have obtained an additional quantity of the fresh plant, it is my intention to continue them. The action of acids on indican requires still further examination, and the effects produced by ferments and other agents remain to be investigated. I also propose to examine other indigo-bearing plants, in order to ascertain whether they contain indican or not.

The results of the present investigation may be summed up in the following propositions:—

1. The *Isatis tinctoria* does not contain indigo-blue ready formed, either in the blue or colourless state.

2. The formation of the blue colouring matter in watery extracts of the plant is neither caused nor promoted by the action of oxygen or of alkalies.

3. Indigo-blue cannot be said to exist in any state of combination in the juices of the plant: it is merely contained in them potentially.

XI. *On the alleged Fluorescence of a solution of Platino-cyanide of Potassium. By* Professor G. G. STOKES, *F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I was surprised, some time since, by reading in Poggendorff's *Annalen* the statement of Dr. Böttger, which has since been republished in the Philosophical Magazine, that a solution of platino-cyanide of potassium is highly fluorescent. My own observations had led me to the conclusion, that "it is only in the solid state that the platino-cyanides are sensitive,—their solutions look like mere water*." In consequence of Dr. Böttger's statement, I have just examined again a solution of platino-cyanide of potassium, with the advantage of bright sunshine and a complete quartz train; and so far from its being eminently fluorescent, I find it to be not fluorescent at all. It is useless to conjecture the cause of the discrepancy, which will probably be cleared up in time; my only object in writing is to prevent the spread of an erroneous statement.

I am, Gentlemen,
Your faithful Servant,
G. G. STOKES.

Pembroke College, Cambridge,
July 5, 1855.

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* Phil. Trans. for 1853, p. 396.

XII. *On the power of Icebergs to make rectilinear, uniformly-directed Grooves across a Submarine Undulatory Surface.* By C. Darwin, *Esq., Vice-Pres. R.S., F.G.S.**

HAVING been induced to believe, with many geologists, that certain continuously scored and polished surfaces of rock were due to icebergs, and not to glaciers, I have nevertheless always felt much difficulty in understanding how long, rectilinear scratches, running in one given direction across an *undulatory* surface, could have been thus formed. Others have felt this same difficulty, and it has been advanced as an insuperable difficulty by the opponents of iceberg action. The following considerations, though possessing little or no novelty, have in my own case removed the difficulty. But first, to give one instance of such scratches, I may quote a passage from Agassiz†, who, in describing the state of the surface near Lake Superior, says, "nothing is more striking in this respect than the valleys or depressions of the soil running E. and W., where we see the scratches crossing such undulations at right angles, descending along the southern gentle slope of a hill, traversing the flat bottom below, and rising up the next hill south in unbroken continuity." He proceeds to state that the scratches run up even steep northern slopes, though the southern faces of the hills are generally rugged. A glacier driven straight forwards over its unequal bed would perfectly account for these facts; but not so, at first appearance, floating ice, whether that of coast-ice or of icebergs. For such masses being borne along on the level ocean, would, when driven on shore or against a submarine hill, be deflected, as it might be thought, from their course, and mark the rocks horizontally or nearly so,—some allowance being made for the rise and fall of the tide. And although during either the submergence or emergence of the land, the whole surface of a mountain might become thus marked, yet the successive scores at each level would all be nearly horizontal. No doubt short inclined grooves might be formed by masses of ice being driven by gales up the beach; but as sea-shores run in every possible direction, it is obvious that such grooves could follow no uniform course, nor could they be of any considerable length; hence grooves thus made would not be comparable with those now under discussion.

The plasticity of glaciers, as shown by the manner in which they immediately expand after passing through gorges, and in which they mould themselves to every sinuosity and prominence in their beds, is now, thanks to the labours of a few eminent men, familiarly known to every geologist. It is asserted by

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* Communicated by the Author.

† Lake Superior, its Physical Character, &c., by L. Agassiz, p. 406.

some authors that glacier ice is most plastic when most charged with water, and the lower part of an iceberg must be water-logged. Again, a glacier, for instance of 1000 feet in thickness, must press on its bed with the whole immense weight of the superincumbent ice; but in an iceberg 1000 feet thick, as the whole floats, there will of course be no pressure on a surface exactly level with its bottom, and if driven over a prominence standing up at the bottom of the sea some 50 or 100 feet above the basal line of the berg, only the weight of as much ice as is forced up above the natural level of the floating mass, will press on the prominence. It may therefore, I think, be concluded that an iceberg could be driven over great inequalities of surface easier than could a glacier. That the weight of a comparatively thin sheet of ice is sufficient to groove rocks, we may infer from the case described by Sir C. Lyell of the scores made by the packed *shore-ice* on the coast of the United States. That icebergs do not break up when grounded, as *à priori* might have seemed probable, is obvious from the simple fact of their having been often observed in this condition in open turbulent seas. Let anyone who has witnessed the crash of even so small an object as a ship, when run into by another having only a barely perceptible movement, reflect on the terrific momentum of an iceberg, some mile or two square, and from 1000 to 2000 feet in thickness, when, borne onwards by a current of only half a mile per hour, it runs on a submarine bank: may we not feel almost certain, that, moulding itself like a glacier (of which it originally was a portion), but owing to its water-logged state and little downward pressure moulding itself more perfectly than a glacier, it would slide straight onwards over considerable inequalities, scratching and grooving the undulatory surface in long, straight lines? In short, if in our mind's eye we look at an iceberg, not as a rigid body (as has hitherto been always my case) which would be deflected or broken up when driven against any submarine obstacle, but as a huge semi-viscid, or at least flexible mass floating on the water, I believe much of the difficulty will be removed which some have experienced in understanding how rectilinear grooves could be formed continuously running, as if regardless of the outline of the surface, up and down moderately steep inequalities, now existing as hills on the land. It should be borne in mind that the course of deeply-floating icebergs is determined by the currents of the sea, and not, as remarked by Scoresby, by the shifting winds; and as the currents of the sea are well known to be definite in their course, so will be the grooves formed by current-borne icebergs. It is indeed difficult to imagine any difference between the effect on the underlying surface, of a glacier propelled by its gravity, and that of a mountainous island of ice driven onwards by an oceanic current, except that the iceberg would perhaps have the power, from the causes above specified, of even more closely moulding itself, and, as it were, of *flowing* straight over submarine obstacles, than has a glacier on the dry land.

One other point is perhaps worth considering. I have elsewhere* endeavoured to show that the action of coast-ice and of icebergs must be considerably different in transporting boulders; the worn stones on the beach being imbedded in coast-ice, and fragments of rock which had originally fallen on the parent-glacier being carried by icebergs as on rafts. But when we reflect that icebergs are driven onwards year after year in certain definite directions by the currents of the sea,—that they float so deeply as to have been seen aground at the depth of 1500 feet,—that when stranded they must (as I conceive) mould themselves to the inequalities of the bottom and slide some distance over it,—it can hardly be doubted that they also must, like glaciers on the land, push in certain determinate directions moraines before them. Although a fragment of rock or an irregularly formed moraine may by any one iceberg be propelled for only a very short distance, yet in the course of years the transportal can hardly fail to become far extended, the boulders being rolled over large inequalities of surface, and even up heights by the action of successively smaller bergs: an abyss, however, deeper than the deepest-floating iceberg would, of course, absolutely stop this rolling or pushing action. Finally, in the case of every mass of erratic boulders, we have now to determine, and I believe hereafter it will be so determined, whether they were transported by glaciers or by floating ice, and in this latter case whether imbedded in coast-ice, strewed on the surface of icebergs, or pushed onwards as a subaqueous moraine.

XIII. *On Electric Conduction.*

By Professor Faraday, *D.C.L., F.R.S.*†

SINCE the time when the law of definite electrolytic action was first laid down (Exp. Res. 783–966), it has become a question whether those bodies which form the class of electrolytes conduct only whilst they are undergoing their proper change under the action of the electric current; or whether they can conduct also as metals, dry wood, spermaceti, &c., do in different

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* Transactions of the Geological Society, vol. vi. (2nd series) 1841. p. 430.

† From the Proceedings of the Royal Institution of Great Britain, for Friday, May 25, 1855.

degrees, *i. e.* without the accompaniment of any chemical change within them. The first kind of conduction is distinguished as the *electrolytic*, the transference of the electric force appearing to be essentially associated with the chemical changes which occur; the second kind may be called *conduction proper*, and there the act of conduction leaves the body ultimately as it found it. Electrolytic conduction is closely associated with the liquid state, and with the compound nature and chemical proportions of the bodies in which it occurs, and it is considered as varying in degree (*i. e.* in facility) with the affinities of the constituents belonging to these bodies; there are, however, other circumstances which evidently, and indeed very strongly, affect the readiness of transfer, such as temperature, the presence of extraneous matters, &c. Conduction proper differs as to facility by degrees so far apart, that that quantity of electricity which could pass through a hundred miles of one substance, as copper, in an inappreciably small portion of time, would require ages to be transmitted through the like length of another substance, as shell-lac; and yet the copper with its similars offers resistance to conduction, and the lac and its congeners conduct.

The progress and necessities of science have rendered it important within the last three or four years, and especially at the present moment, that the question "whether an electrolyte has any degree of conduction proper" should be closely considered, and the experiments which are fitted to probe the question have been carried to a very high degree of refinement. Buff*, by employing the electric machine and Wollaston terminals, *i. e.* platinum wires sealed into glass tubes, and having the ends only exposed, has decomposed water by a quantity of electricity so small that it required four hours to collect gas enough to fill a little cylinder only one-tenth of an inch in diameter, and one-fifth of an inch in length; yet the decomposition was electrolytic and polar, and therefore the conduction was electrolytic also. When one pole only was in the water and the other in the air over it, still the decomposition, and therefore the conduction, was electrolytic; for one element appeared at the pole in the water, and the other in the air or gas over the water at the corresponding pole. Buff concludes that electrolytes have no conduction proper. Many other philosophers have supported, with more or less conviction, the same view, and believe that electrolytic conduction extends to, and includes cases which formerly were supposed to depend upon conduction proper. Soret advances certain experimental results†, but reserves his opinion from being absolute. Von Breda and Logeman adopt the more

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* MS. letter.

† *Annales de Chimie*, xlii. 257.

general view unreservedly*. De la Rive, I think, admits that a very little may perhaps pass by conduction proper, but that electrolytic conduction is the function of electrolytes†. Matteucci has at one time admitted a little conduction proper, but at present, I believe, denies that any degree exists. On the other hand, Despretz‡, Leon Foucault§, Masson‖, and myself, have always admitted the possibility that electrolytes possess a certain amount of conduction proper—small indeed, but not so small as to prevent its being evident in certain forms of experiments; and beautiful and close as the electrolytic proofs have been carried, they are not by us considered as sufficient to show that the function of conduction proper is altogether absent from electrolytes.

[Some account was then given of the experiments and arguments on both sides; and of the striking electrolytic fact, that if a current of electricity, however small, is sent through a circuit containing a couple of platina plates in dilute sulphuric acid, the plates are found thereby electrically polarized.]

The inquiry as regards electrolytes takes on three forms. They may possess a degree of conduction proper at all times; or they may be absolutely destitute of conduction proper; or they may possess conduction proper up to a certain condition, governed either by requisite intensity for electrolyzation, or by other circumstances, but which, when that condition is acquired, changes into electrolytic conduction; and these three forms may be further varied by considerations dependent upon the physical state of the electrolyte, as whether it be solid or liquid, hot or cold, and whether it be pure or contain other substances mingled with it.

From the time when the question was raised by myself, twenty years ago, to the present day, I have found it necessary to suspend my conclusions; for close as the facts have in certain cases been urged by those who believe they have always obtained decomposition results when an electrolyte has performed the part of a conductor, and freely as I could have admitted the facts and the conclusions if there had been no opposing considerations, still, because there are such considerations, I am obliged to reserve my judgement. In the first place, all bodies not electrolytic, even up to gases (Becquerel), are admitted to possess conduction proper; *à priori*, therefore, we have reason to expect that electrolytes will possess it also. If from amongst different

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* Phil. Mag. viii. 465. † *Bibl. de Genève*, xxvi. 134, 144; xxvii. 177.

‡ *Comptes Rendus*, xxxviii. 897.

§ *Comptes Rendus*, xxxvii. 580; or *Bibl. de Genève*, xxiv. 263; xxv. 180; xxvi. 126.

‖ Prize Essay, Haarlem Trans. xi. 78.

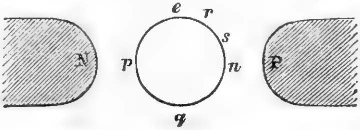
bodies we retain for consideration the class of electrolytes only, then, though the amount of electricity of a given intensity which these can transmit electrolytically when they are *fluid* is often almost infinitely greater than that which they can convey onwards by conduction proper when they are *solid,* still the conduction in the latter cases is very evident. A piece of perfectly dry solid nitre, and of many other electrolytes, discharges a gold-leaf electrometer very freely, and I believe by the power of conduction proper; and that being the case, I do not see that the assumption of the very highest condition of electrolytic conduction when the nitre is rendered fluid is any argument for the absolute disappearance of the conduction proper which belonged to the body in the solid state, though it may override the latter for the time, and make it insensible. These considerations are, however, such as arise rather from the absence of the final and strict proof on the opposite side, than from anything very positive in their own character; but it has occurred to me that the phænomena of static electricity will furnish us with many reasons of a positive nature in favour of the possession by liquid electrolytes of the power of conduction proper. Some of these I will endeavour briefly to state, illustrating the subject by a reference to water, which in its pure state has but a low degree of electrolytic conduction.

The ordinary phænomena of static charge and induction are well known. If an excited glass rod or other body be held near a light gilt sphere, suspended from the hand by a metal thread, the inductive action disturbs the disposition of the electricity in the sphere, and the latter is strongly attracted; if in place of the sphere a soap bubble be employed, the same results occur. If a dish filled with pure distilled water be connected with the earth by a piece of moist bibulous paper, and a ball of excited shell-lac be suspended 2 or 3 inches above the middle of the water, and if a plate of dry insulating gutta-percha, about 8 inches long and 2 inches wide, have its end interposed between the water and the shell-lac, it may then be withdrawn and examined, and will be found without charge, even though it may have touched the shell-lac; but if the end once touch the water under the lac (and it may be dipped in) so as to bring away a film of it, charged with the electricity the water has acquired by the induction, it will be found to possess, as might be expected, a state contrary to that of the inductric shell-lac.

In order to exclude any conducting body but water from what may be considered as a reference experiment, two calico globular bags with close seams were prepared, and being wetted thoroughly with distilled water, were then filled with air by means of a fine blowpipe point; they were then attached to two suspending bands of gutta-percha, by which they were well insulated, and being about 3 inches diameter, they formed, when placed in contact, a double system 6 inches in length. A metallic ball, about 4 inches in diameter, was connected with the electric machine to form an inductric body, an uninsulated brass plate was placed about 9 inches off to form an inducteous body; between these the associated water balls could be placed so as to take part in the induction, and when the electric charge was so low that the moist atmosphere caused no transmission of electricity, the balls could be introduced into position and brought away without having received any permanent charge. Under these circumstances, if the associated balls were brought into the place of induction, were then separated, withdrawn and examined, they were found, the one charged positively and the other negatively, by electricity derived from themselves, and without conductive or convective communication with any other substance than their own water.

It is well known indeed that by the use of water we may replace metal in all electro-static arrangements, and so form Leyden jars, condensers, and other induction apparatus, which are perfect in principle though with imperfect action. The principles are the same whether water or metals be used for conductors, and the function of conduction is essential to all the results; therefore conduction cannot be denied to the fluid water, which in all such cases is acting as the only conductor. In nature, indeed, the phænomena of induction, rising up to their most intense degree in the thunder-storm, are almost, if not altogether, dependent upon the water which in the earth, or the clouds, or the rain, is then acting by its conducting power; and if this conducting power be of the nature of *conduction proper*, it is probable that that function is as large and as important as any exercise of the electrolytic conduction of water in other natural phænomena.

But it may be said that all these cases, when accompanied by conduction, involve a corresponding and proportionate electrolytic effect, and are therefore cases of electrolytic conduction; and it is the following out of such a thought that makes me think the results prove a conduction proper to exist in the water. For suppose a water bubble to be placed midway between a positive and a negative surface, as in the figure, then the parts at



and about *p* will become charged positive, and those at and about *n* negative, solely by the disturbance of the electric force originally in the bubble, *i. e.* without any direct transmission of the electric force from N or P; the parts at *e* or *q* will have no electric charge, and from those parts to *p* and *n* the charge will rise gradually to a maximum. The electricity which appears at *p*, *n*, and elsewhere, will have been *conducted* to these parts from other parts of the bubble; and if the bubble be replaced by two hemispheres of metal, slightly separated at the equatorial parts *eq*, the electricity (before *conducted* in the continuous bubble) will then be seen to pass as a bright spark. Now the particles at any part of the water bubble may be considered under two points of view, either as having had a current passed through them, or as having received a charge; in either view the idea of conduction proper supplies sufficient and satisfactory reasons for the results; but the idea of electrolytic conduction seems to me at present beset with difficulties. For consider the particles about the equator *eq*,—they acquire no final charge, and they *have conducted*, as the action of the two half spheres above referred to show; and they are not in a state of mutual tension, as is fully proved by very simple experiments with the half hemispheres. Therefore oxygen must have passed from *e* towards *n*, hydrogen from *e* towards *p*, *i. e.* towards and to the parts to which the electricity has been conducted, for without such transmission of the anions and cations there would be no transmission of the electricity, and so no electrolytic conduction. But then the questions arise,—Where do these elements appear? is the water at *n* oxygenated, and that about *p* hydrogenated? and may the elements be at last dispersed into the air at these two points, as in the case of decompositions against air poles? (Exp. Res. 455, 461, &c.) In regard to such questions other considerations occur respecting the particles about *p* and *n*, and the condition of charge they have acquired. These have received the electricity which has passed as a current through the equatorial parts, but they have had no current or no proportional current through themselves—the conduction has extended *to* them but not *through* them; no electricity has passed for instance through the particle at *n* or at *p*, yet more electricity has gone by some kind of conduction to them than to any other of the particles in the sphere. It is not consistent with our understanding of electrolytic conduction to suppose that these particles have been charged by such conduction; for in the exercise of that function it is just as essential that the electricity should *leave* the decomposing particle on the one side, as that it should *go to it* on the other: the mere escape of oxygen and hydrogen into the air is not enough to account for the result, for such escape may be freely permitted in the case of electrodes plunged into water; and yet if the electricity cannot pass from the decomposing particles into the electrodes, and so away by the wires, in a condition enabling it to perform its full equivalent of electric work anywhere else in the circuit, there is no decomposition at the final particles of the electrolyte, nor any electrolytic conduction in its mass. Even in the air cases above referred to, there is a complete transmission of the electricity across the extreme particles concerned in the electrolysis.

If the above reasoning involve no error, but be considered sufficient to show that the particles at *p* and *n* are not electrolyzed, then it is also sufficient to prove that none of the particles between *p* and *n* have been electrolyzed; for though one at *e* or *q* may have had a current of electricity passed through it, it could not give up its elements unless the neighbouring particles were prepared to take them in a fully equivalent degree. To stop the electrolysis at *n* and *p*, or at those parts of the surface where the moving electricity stops, is to stop it at all the intervening parts according to our present views of electrolysis, and to stop the electrolysis is to shut out electrolytic conduction; and nothing at present remains but *conduction proper*, to account for the very manifest effects of conduction which occur in the case.

It may be imagined that a certain polarized state of tension occurs in these cases of static induction, which is intermediate between it and electrolytic conduction (Exp. Res. 1164); or that a certain preparatory and as it were incomplete condition may be assumed, distinguishing the case of static conduction with globes of water, which I have taken as the ground of consideration from the same case when presented by globes of metal. Our further and future knowledge may show some such state; but in respect of our present distinctive views of conduction proper and electrolytic conduction, it may be remarked that such discovery is just as likely to coincide with the former as with the latter view, though it most probably would alter and correct both.

Falling back upon the consideration of the particles between *e* and *n*, we find, that whether we consider them as respects the current which has passed through them, or the charge which they have taken, they form a continuous series; the particle at *e* has had most current, that at *n* none, that at *r* a moderate current; and there are particles which must have transmitted every intermediate degree. So with regard to charge; it is highest at *n*, nothing at *e*, and every intermediate degree occurs between the two. Then with respect to these superficial particles, they hold all the charge that exists, and therefore all the electricity which has been conducted is in them; consequently all the electrolytic results must be there; and that would be the case, even though for the shell we were to substitute a sphere of water. For, if those particles which have had more current through them than others be supposed to have more of the electrolytic results about them than the others, then that electricity which is found associated chiefly, if not altogether, with these others, could have reached them only by conduction proper, which for the moment is assumed to be non-existent. So, to favour the electrolytic argument, we will consider the conduction as ending at, and the electrolytic results as summed up in, these superficial particles, passing for the present the former objection that though the electricity has reached, it has not gone through, these particles. Taking, therefore, a particle at *r*, and considering its electrolytic condition as proportionate to the electricity which has arrived at that particle, and given it charge, we may then assume—for we have the power of diminishing the inductive action in any degree,—that the electricity, the conduction of which has ceased upon the particle that was there, has been just enough to decompose it, and has left what was the under but is now the surface particle, charged. In that case, some other particle, in a higher state of charge, and nearer to *n*, as at *s*, will have had enough electricity conducted towards its place to decompose two particles of water;—but it is manifest that this cannot be the next particle to that at *r*, but that a great number of other particles in intermediate states of charge must exist between *r* and *s*. Now the question is, how can these particles become intermediately charged by virtue of electrolytic conduction only? Electrolytic action is definite, and the very theory of electrolytic conduction assumes that the particles of oxygen and hydrogen as they travel convey not a variable but a perfectly definite amount of power onward in its course, which amount they cannot divide, but must take at once from a like particle, and give at once to another like particle. How then can any number of particles, or any action of such particles, carry a fraction of the force associated with each particle? It is no doubt true, that if two charged particles can throw their power either on to one, or to three or more other particles, then all the difficulty disappears. Conduction proper can do this: but, as we cannot conceive of a particle half decomposed, so I cannot see how this can be performed by electrolytic conduction, *i. e.* how the particle between *r* and *s* can be excited to the intermediate and indefinite degree, conduction without electrolysis being denied both to it and the particles around it.

If the particles between *e* and *n* be supposed to conduct electrolytically by the current which passes *through* them (dismissing for a time, amongst other serious objections, that already given, that the products would not be found at the places to which the electricity has been conveyed), still the present argument would have like force. At *r* enough electricity may have passed through to decompose two particles of water, at *s* only enough to decompose one,—how is a particle between *r* and *s* to change elements with the particles either towards *r* or towards *s*, if electrolytic change only is to be admitted? or how, as before inquired, can two particles throw their power on to, or receive their power from one? Many other considerations spring out of the thought of a water bubble, under static induction; but these just expressed, with those that relate to the *seat* of electrolytic action, whether at the place of current or of charge, create a sum of difficulty fully sufficient, without any others, to make me suspend for the present any conclusions on the matter in question.

The conduction power of water may be considered under another point of view; namely, that which has relation to the absolute charge that can be given to the fluid. A point from the electrical machine can charge neighbouring particles of air, and they issue off in streams. It can do the same to particles of camphine or oil of turpentine;—it can do the same to the particles of water; and if two fine metallic wires connected with Ruhmkorff's apparatus, be immersed in distilled water, about half an inch apart, the motes usually present will soon show how the water receives the charge, and how the charged water passes off in streams, which discharge to each other in the mass. Now such charge is not connected with electrolysis; the condition of electrolyzation is, that the electricity pass through the water and do not stop short in it. The mere charge of the water gives us no idea where any constituents set loose by electrolysis can be evolved, and yet conduction is largely concerned in the act of charging. A shower of rain falls across a space in the atmosphere subject to electric action, and each drop becomes charged; spray may be thrown forth from an electrified fountain very highly charged;—conduction has been eminently active in both cases, but I find it very difficult to conceive how that conduction can be electrolytic in its character.

When drops of water, oppositely electrified, are made to approach each other, they act by convection, *i. e.* as carriers of electricity; when they meet they discharge to each other, and the function of conduction is for the time set up. When the water bubble, described p. 102, is taken out of the sphere of induction, the opposite electricities about *p* and *n* neutralize each other, being conducted through the particles of the water. Are we to suppose in these cases that the conduction is electrolytic? if so, where are the constituents separated, and where are they to appear? It must be a strong conviction that would deny conduction proper to electrolytes in these cases; and if not denied here, what reason is there ever to deny it absolutely?

The result of all the thought I can give to the subject is a suspended judgement. I cannot say that I think conduction proper is as yet disproved in electrolytes; and yet I cannot say that I know of any case in which a current, however weak, being passed by platinum electrodes across acidulated water, does not bring them into a polarized condition. It may be that when metallic surfaces are present, they complete by their peculiarities the condition necessary to the evolution of elements, which, under the same degree of electrification, would not be evolved if the metals were away; and, on the other hand, it also may be that after the metals are polarized, and a consequent state of reactive tension so set up, a degree of conduction proper may occur between them and the electrolyte simultaneously with the electrolytic action. There is now no doubt that as regards electrolysis and its law, all is as if there were but electrolytic conduction; but, as regards static phænomena (which are equally important) and the steps of their passage into dynamic effects, it is probable that conduction proper rules with electrolytes as with other compound bodies; for it is not as yet disproved, is supported by strong presumptive evidence, and may be essential. Yet so distant are the extremes of electric intensity, and so infinitely different in an inverse direction are the quantities that may and do produce the essential phænomena of each kind, that this separation of conductive action may well seem perfect and entire to those whose minds are inclined rather to see conduction proper replaced by electrolytic conduction, than to consider it as reduced, but not destroyed; disappearing, as it were, for electricity of great quantity and small intensity, but still abundantly sufficient for all natural and artificial phænomena, such as those described, where intensity and time both unite in favouring the final results required.

But we must not dogmatise on natural principles, or decide upon their physical nature without proof; and, indeed, the two modes of electric action, the electrolytic and the static, are so different yet each so important, the one doing all by quantity at very low intensity, the other giving many of its chief results by intensity with scarcely any proportionate quantity, that it would be dangerous to deny too hastily the conduction proper to a few cases in static induction, where water is the conductor, whilst it is known to be essential to the many, only because, when water is the electrolyte employed, electrolytic conduction is essential to every case of electrolytic action.

XIV. *On the Molecular Constitution of Crystals.*

By Robert T. Forster, *A.B.*[*]

[With Two Plates.]

Any theories which have been advanced to account for the phænomena of crystallization have been but short lived in their reputation; and, indeed, it may be safely doubted if any occupy such a position as to entitle them to much credit.

The subject is itself one of much interest; and although the consideration that so many able minds have failed in the attempt to investigate it may naturally deter an inquirer, still the prize is sufficient to persuade him to hazard the attempt.

I shall first take a short review of the many theories which have been advanced in explanation of the phænomena, and I trust I shall be able to show that some were unquestionably faulty, and all undoubtedly and confessedly deficient.

Passing by the many absurd theories advanced on this subject in ancient times, the first whom we find deserving of notice is Christian Huygens. In his celebrated work on double refraction, he considered the crystals of Iceland spar to be built up of spheroids, which by their unequal density separated the incident light into two rays. He did not, however, give any reason why the particles are so aggregated.

Hooke, in his *Micrographia*, advanced a similar hypothesis, except that he considered the atoms to be spherical; a supposition which would have accounted for forms in the first system, but which would have utterly failed in case of the third or "rhombohedral," to which he applied it; nor does he perceive that the molecules, if left to themselves, would not assume a definite arrangement. He does not seem to consider these spheres as the ultimate atoms; he says, "that having already shown how a fluid will naturally assume the spherical form, he will proceed to show how these spheres will unite to form a crystal." On the absurdity and complication of such ideas I need not dwell.

The next who commanded attention was M. Prechtl de Brun, whose ideas were to a certain extent those of Hooke; he considered a fluid to be made up of soft molecules, which, while the body was undergoing its change of state, suffered a change of form arising from their mutual pressure, and that they were thus formed into cubes, dodecahedrons, &c.; the cubes by their union forming larger cubes, &c. Not only was this hypothesis quite insufficient, inasmuch as it substituted one difficulty for

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[*] Communicated by the Author; having been read before the Royal Irish Academy, May 14, 1855.

another, but it was totally erroneous in theory, as Dr. Wollaston has ably shown.

Immediately after him, Dr. Wollaston published in the Philosophical Transactions a theory of the manner in which the common octahedron and tetrahedron may be built up of spheres having a mutual attraction for each other. He evidently considered his ideas the same as those of Hooke, as he says he found himself anticipated by that writer. He does not seem to have prosecuted his inquiries very far, indeed he leaves totally unnoticed the innumerable other forms to be met with in nature. He does, however, make some observations with regard to the cube, but he does not appear to have satisfied himself; his words are to this effect:—"If a number of white and an equal number of black balls are so arranged that each white ball shall be equidistant from the four nearest black balls, and also from the adjacent white balls, the form which they would take would be a cube."

I shall now endeavour to show that this theory of the formation of the octahedron is faulty, inasmuch as the molecules, if left to themselves, would never assume such an arrangement.

If any four spherical atoms become attached in consequence of their mutual attraction, they will combine in the manner shown in Plate I. fig. 1, and any fifth atom will attach itself, as shown in fig. 2; but if we examine fig. 3, which represents the arrangement which he supposes them to have, we will find that any five adjacent spheres occupy a position such as shown in fig. 4, a position which they evidently never would naturally assume.

Wollaston himself seems to have some lurking doubts of the validity of this assumption, as he begins by showing how the spheres will become aggregated *if on a plane*, and then from this basis builds up the octahedron; this, however, is the very manner in which crystals do not form.

The first who really investigated this most interesting subject was the Abbé Haüy; his work forms an æra in the history of the science. He perceived that many crystals were liable to cleavage, and that thus in many cases new solids were obtained; he was led from this to consider, that, if this cleavage were continued long enough, he would ultimately arrive at the absolute atom; and he assumed, without sufficient foundation, that this atom should have the same form as the solid obtained by cleavage.

The forms which he considered the molecules to have were the tetrahedron, parallelopiped, and the three-sided prism. He clearly showed that many of the forms could be constructed with these elements, and he pointed out how some of the secondary forms would result from certain decrements or omissions in the regular succession of the particles. He however failed in accounting for the formation of the ordinary octahedron, as such a form could not result unless from a mixture of little tetrahedrons and octahedrons, or unless the tetrahedrons were united by their corners, leaving spaces between them. The necessity for such an arrangement so unaccountable and so totally at variance with the rest of his theory was evidently subversive of it. He moreover does not attempt to explain why the decrements that we have spoken of take place, nor does it seem possible to do so on his hypothesis. He seems to have begun at the wrong end, and instead of investigating how certain particles could unite to form certain crystals, he contented himself with showing that such crystals might be formed by piling up the molecules in a particular manner.

The last and most successful writer is Mr. Dana, the American mineralogist, who first published his views on the subject in the year 1836.

He considers the molecules to be spherical, and possessed of six poles, each 90° distant from the nearest four; three adjacent poles being of one denomination and three of another, those of a like sort repelling, and those of an unlike attracting each other. By means of this hypothesis he very satisfactorily explained the cubical formation, and pointed out, like Haüy, how the many secondaries can be obtained from the cube by certain decrements at the edges or angles. He also accounted for the formation of twin crystals, by showing how two molecules may accidentally unite at a point of equilibrium between two or three poles. He supposed the molecules to be in the first system spheres; in the second spheroids; in the third spheroids, the poles being at the extremities of equal conjugate diameters; in the fourth ellipsoids; in the fifth the same, the poles being at the extremities of axes, two of which are inclined to each other; and in the sixth the same, all the axes of the molecules being inclined. He, however, does not in the least account for the formation of secondary planes (although he wishes us to suppose so), nor of the occurrence of hemihedral forms. In this respect all writers on the subject have erred in not entering sufficiently fully on the investigation; either contenting themselves, like Haüy, with considering how certain solids could be built up to form other solids, or, like Dana, merely observing how molecules, if possessed of certain properties, will unite after certain laws.

The problem, as it appears to me, is of a much more general nature; we have to consider a number of molecules to which we have given certain properties, and we have to investigate what forms they will assume, and what modifying influences may come into play during the process. We cannot expect, that, after several molecules have united and a solid has thus been formed, no change will take place in the law of attraction consequent on the mutual action of all the accumulated particles; nor can we safely suppose, that properties which are themselves transient, and certainly not existing when the body is in all its states, may not be alterable after the body has assumed the solid form.

Dana seems to have given unnecessary complication to his hypothesis, and by this means to have rendered the explanation of some of the phænomena impossible. It appears to me that all the conditions would be satisfied by the molecules having six poles, all exercising a mutual attraction for each other; the form which would result from the union of such molecules would evidently be the cube (fig. 5). But it is natural to suppose, in accordance with similar phænomena in nature, that these poles may be liable to removal or displacement on account of their mutual attraction for each other, and that the attracting fluid is only held in its place by a certain coercive force of the molecule itself, as is the case in the common magnet. As the crystal increases in size, those molecules which are at its extremities will be more and more acted on by this influence, and some may altogether lose their polarities (at the same time it is by no means a consequence that these molecules will drop off, since the attracting fluid need not be removed from the molecule, but only displaced); the consequence will be, that the free particles will not be attached to those which have lost their polarities, and modifying planes will appear.

Thus in case the molecule at each corner of a cube lose its three unattached poles, a plane will appear at each corner, and the octahedron will be thus formed.

If a row of molecules along each edge lose their unattached poles, planes will appear on each edge, which will of course belong to the dodecahedron; but if two rows of molecules lose their poles, the resultant form will be a tetrakishexahedron; if three, another tetrakishexahedron.

Let us now consider under what circumstances these different results will take place. If we consider the manner in which the additional particles are laid on those layers already formed, it is evident that those particles nearest the centre will be the first to which new molecules will become attached, and that in general the particles will attach themselves as nearly as possible to the centre of the crystal, inasmuch as the attraction is strongest there. On the contrary, those molecules which are furthest from the centre will be the first to lose their attractive force, as numbered in fig. 6.

If all the molecules in a face already formed be covered except the external row, and if, while the internal molecules were having new molecules attached, this external row lost its polarities, then dodecahedral faces will appear. In like manner, if only the extreme corner molecule lost its polarities, then all would be covered but that one, and the plane formed would belong to the octahedron. In fact, we have the molecules becoming consecutively attached in one direction, and the polarities of those molecules to which they are to be attached being consecutively removed in exactly an opposite direction; of course, then, after a certain number of molecules have become attached, no more molecules will be attached on that layer, and a new layer will be commenced in exactly the same manner, and exactly the same result will ensue, the same number being dropped as in the last layer, and the consequence being that a plane will be formed being a tangent plane to those molecules to which no others have become attached. It might appear at first sight that this is not true, since the number of molecules in each face are as the squares of the number of molecules along an edge; but we are to remember that all those molecules equidistant from the centre have new molecules attached to them at the same time; therefore the time which elapses while the whole face is being covered is only that which elapses while a single row is being covered.

There is one description of crystals which have been left nearly altogether unnoticed by all writers on this subject, namely hemihedral forms. The only person who mentioned them at all was Wollaston, and he contented himself with proposing a theory for the formation of the tetrahedron, leaving quite unexplained the union of the cube with the tetrahedron (although his own thoughts were elicited by the octahedral cleavage of cubical fluor spar). His theory of the formation of the tetrahedron I have already shown to be faulty. I will now endeavour to show under what circumstances hemihedral forms without parallel faces will be produced. The theory I am about to put forward is not sufficient to explain the formation of crystals with parallel faces; but this does not invalidate it, inasmuch as, from the well-known fact that forms of these two denominations are never found united, we can safely infer that there is something essentially different in their nature; indeed we might say, that if any theory was capable of accounting for both, the fact of its doing so would be an argument for its rejection. If the molecules be spheres, each having twelve poles, or centres of attractive force, on its surface, and these poles were arranged as shown in fig. 7, the form which such molecules would assume is the tetrahedron. (The intersection of the great circles in the figure show the position of the poles; each great circle is divided into six equal parts by the others, and they cut each other at angles of 70° 31′ 44″.)

For if any two of these spheres become united by two of their poles, they will evidently, from the mutual action of all the poles, assume such a position as shown in fig. 8; and a third molecule would be attracted in a similar position, while a fourth would attach itself to the three poles, A, B, C, by its three poles *a, b, c* (fig. 9). We have now a tetrahedron formed, and by precisely analogous reasoning we can continue the process of formation. It will be observed that a fifth molecule, if attached, will be in the same plane with three others, and will only touch two; a result to the necessity of which I have already adverted when speaking of Dr. Wollaston's hypothesis.

If a tetrahedron be thus formed, and if a row of molecules along each edge lose their polarities in each consecutive layer, each additional layer of particles will be deficient by a row, and faces will appear replacing the edge of the crystal tangentially; such faces, it is well known, belong to the cube. We have now a compound form consisting of a cube and tetrahedron. (*Vide* Plate II. figs. 10 and 11.)

If each layer be deficient by one molecule at each corner of the faces, planes will appear truncating the corners of the tetrahedron, which will of course belong to the octahedron. In like manner, if two or more rows be omitted, the hemiikositetrahedron will be formed. And if the same occurrences take place after an octahedron has been formed, the holohedral forms of these solids will be obtained.

It is well known that crystals in the first system are liable to three distinct cleavages, which would evidently indicate three distinct formations; two we have already investigated, namely the cubical and tetrahedral; we will now endeavour to show under what circumstances spherical molecules will assume the form of a dodecahedron, and will then proceed to prove that these three formations will give rise to three distinct cleavages, the directions of cleavage being in every case parallel to the faces of the solid itself, an instance of agreement which is strongly suggestive of truth.

If the spherical molecules have eight poles situated in the same relative positions as the corners of a cube, they will, if under no disturbing influence, assume the form of a dodecahedron; for if any sphere attract eight others, they will be arranged as shown in Pl. I. fig. 12, all the poles of each sphere having the same position as regards the eye; for it is evident, that, if after union they have any other position, the mutual actions of *all* the poles will cause them to rotate on each other till they have that definite position; these eight will be attached simultaneously, and immediately six others will be attached to them, as shown in fig. 13. We have now a dodecahedron formed; and it is evident that as the crystal grows, the same form will be retained (fig. 14). Such a crystal will be liable to modification in the same manner as those already discussed, and thus all other forms may be built up in this manner.

We have thus seen that on these three hypotheses it is possible to explain all forms; the cubical gives rise primarily to the cube, and by means of decrements to all others except the tetrahedron. And here we may mention a fact strikingly confirmatory of these views, namely, that *there is not a single instance in nature of a crystal exhibiting the tetrahedral form, or that of any of its hemihedral derivatives, and possessed of cubical cleavage.* The tetrahedral gives rise to that solid and all others by its modifications, and the dodecahedral in like manner gives rise to all forms; and thus any crystal may have any cleavage.

As regards cleavage, it takes place in every case in whatever direction the least resistance is met with; thus where each molecule has six poles the cleavage is cubical, because such a division separates each atom from one other only, whereas the dodecahedral cleavage would separate each atom from two others, and the octahedral from three, as is at once evident on inspection of fig. 5.

In like manner, in the tetrahedral arrangement the cleavage is parallel to the faces; for it is evident (fig. 3) that such a division would separate each molecule from three others, whereas the only other cleavage which it could possibly undergo, namely that parallel to planes on the edges, would separate each molecule from four others. Lastly, where the poles are eight in number, and the formation dodecahedral, the cleavage planes are parallel to the faces; for by division in such directions we overcome the resistance of two poles in each molecule, while if we divided the crystal by planes parallel to the faces of the cube, we should separate each atom from four others; if by planes parallel to the faces of the octahedron, from three others.

These three formations occur in the other systems also, and give rise to corresponding cleavages; but as some of the poles are stronger than others, in those systems particular cleavages are eminent.

We have now reviewed all the different forms of the first system: it only remains to speak of hemitrope and twin crystals.

Dana has satisfactorily shown that these will arise from the accidental union of two molecules at the middle points between two or more poles; an account of his theory first appeared in the American Journal of Science for 1836. He was certainly the first person who fully investigated this part of the subject; but justice compels me to add, that his ideas were directly borrowed from Sir David Brewster, who some years before, in an article on polarization produced by compression, thus expressed himself:—"The phænomena of cleavage and of hemitrope forms would clearly indicate that the integrant molecules are spheres, each having six poles on its surface." This article was quoted by Dana in the American Journal of Science, and formed the basis of his investigations, without however being acknowledged. Sir David Brewster goes on to say, that it is owing to the different degrees of force possessed by the different poles that the different cleavages are due; and in this he is clearly in error. For if a cube be divided either by the cubical or octahedral cleavage, every molecule will eventually become separated from every other. In fact, to divide a crystal in which the arrangement is cubical by the octahedral cleavage, we must at *each section* separate every molecule from three adjacent molecules, or in other words, we must overcome the cohesion of three of its adjacent poles; but it was to the unequal cohesion of these very three poles that he ascribed the liability to cleavage.

Dana also, as I have already stated, pointed out that in the second and remaining systems the molecules are no longer spheres, but spheroids or ellipsoids; and with this modification, all I have said applies word for word to the other systems.

XV. *On the Felspars occurring in the Granites of the Dublin and Wicklow Mountains. By the* Rev. JOSEPH A. GALBRAITH, *M.A., Erasmus Smith's Professor of Natural and Experimental Philosophy in the University of Dublin.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

A COMMUNICATION from Mr. Jennings which appeared in your last Number, on the Felspars of the Dublin Mountains, makes it necessary for me to request that you will insert in your Journal a short account of a discussion which arose some time since in the Royal Irish Academy on this subject.

In the month of January 1853, Sir Robert Kane brought under the notice of the Academy an analysis of the waters which descend from the sides of the Dublin Mountains. The following is his analysis of the residue after evaporation, of what he considered to be a typical specimen:—

Organic matter	23·30
Carbonic acid	7·40
Muriatic acid	17·99
Sulphuric acid	6·34
Silica	3·81
Lime	3·03
Magnesia	0·85
Potash	2·86
Soda	30·48
Loss	3·94
	100·00

In the discussion of this analysis, he considered that the muriatic acid present existed in the form of common salt, derived from the vapours of the adjoining sea, which leaves of the soda 15·20 to be disposed of; he then allocated all the potash to the sulphuric acid, of which it saturates 2·43, leaving 3·91, which he supposed to be combined with soda, leaving finally of this alkali 12·16 to exist in combination with silica and the organic acids. This large amount of silicate of soda in the waters he attributed to the decomposition of the felspathic materials of the granite; and in conclusion, drew the inference that the felspar of the Dublin granites is, upon the whole, a soda or albitic felspar, although in particular localities orthose or potash felspar may be found. This inference he also stated was verified by a great number of analyses of specimens of granite taken from various parts of the range which extends from Dublin into the county Wicklow; in all these specimens both alkalies were found, but the soda in such excess, as to indicate that the potash should be referred to the mica which the granite contained, and the soda allocated to the felspar.

By this interesting communication to the Irish Academy two important questions were raised; viz. 1, whether the felspar which enters into the constitution of the granite is an albite; 2, whether orthose or potash felspar is a rare mineral, to be found only in special localities, such as Killiney.

In a communication made at a subsequent meeting by Dr. Apjohn, the first of these questions was fully disposed of, as from the analyses which he furnished of specimens of felspars taken from different localities, potash was invariably found in notable quantity, constituting at least one-half of the alkalies present, showing that the felspar could not in any sense be considered as albite or soda felspar.

To the second of these questions I directed my attention, and having procured specimens of felspar, taken from seven localities at about equal distances in a line stretching forty miles from Dublin, I subjected them to careful analysis, and furnished the results to the Academy, and also to your valuable Journal. These results leave nothing to be wished for as far as the second question is concerned, as in every case the specimen appeared to be true orthose, resembling in every particular those which are to be found in Killiney and Dalkey.

In the last Number of your Journal I perceive that a communication has been made by Professor Haughton, in which he gives a number of analyses of *rock specimens* taken from different localities in this range; his results appear to be altogether inconsistent with the opinion expressed by Sir Robert Kane as to the character of the felspars of these mountains, and therefore, of course, with the extended series of analytical investigations of rock specimens on which Sir Robert Kane stated that he founded his opinion, and which, although now nearly three years have elapsed, he has never produced.

Professor Haughton has, in the course of his research, discovered a most interesting fact as to the character of these granites, namely that in every specimen taken from the range itself the potash predominates over the soda, while in the outlying granitic protrusions, the soda prevails, thus indicating a curious constitutional difference, which may, and probably does, accompany a difference of age in these rocks hitherto undiscovered.

As to Mr. Jenning's interference in this discussion, he has not, in my opinion, contributed much to either side; nor do I think he understands the question in all its bearings. In the course of this article it has been stated, that, at the very outset of the discussion, analyses of felspars were produced and relied on by Dr. Apjohn, in which the proportion of the alkalies was nearly equal; it will therefore appear that the production of a single additional, and that but a partial analysis, confirming, so far as it goes, Dr. Apjohn's result, cannot throw much new light on the question at issue.

Immediately after the meeting of the Geological Society, at which I obtained a part of the specimen furnished by Mr. Jennings, I made the following analysis:—

Silica	70·32
Alumina	16·12
Peroxide of iron . .	3·20
Lime	1·34
Potash	4·65
Soda	3·39
Loss by ignition . .	0·96
	99·98

I used the greatest care in obtaining the alkalies *separately,* and have no doubt that the numbers just given accurately represent their relative proportions. It is to be regretted that Mr. Jennings did not obtain from Mr. Campbell a complete analysis, as perfect confidence cannot be placed in quantitative results if all the constituents be not weighed; but if we agree to accept Mr. Campbell's determination of the alkalies as perfectly correct, it is as wholly irreconcileable with Sir Robert Kane's statement as any that has been produced during the controversy.

I regret particularly that Sir Robert Kane has left this question, so far as he is concerned, without any means of solution. And as those who are competent to decide such questions naturally withhold their judgement till all sides are heard, I hope I may be excused if in these pages I express the hope that ere long Sir Robert Kane will favour the public with the investigations on which he has based his opinion. I may, no doubt, be in error in some of my views of this question; and if so, would be glad to have my mistakes corrected by so competent an authority.

I remain, Gentlemen,
Your obedient Servant,
JOSEPH A. GALBRAITH.

Trinity College, Dublin,
July 16, 1855.

XVI. *On two doubtful British Species,* Glottalite *and* Zeuxite.

By R. P. GREG, *Esq.*[*]

I HAVE lately had an opportunity of seeing the original specimens from which, many years ago, the late Dr. Thomson described the rather doubtful species *Glottalite* and *Zeuxite.*

1. The *Glottalite* occurs both crystallized and massive. The massive much resembles, as indeed Dr. Heddle lately suggested it might be, edingtonite. The crystals, which Dr. Thomson rather loosely described as "having *nearly* equilateral faces, either four-sided pyramids or regular octahedrons," are without doubt chabasie. The crystals, though not easy to distinguish, occur in double six-sided pyramids, a form of chabasie sometimes also called phacolite. Another proof of its being in all probability chabasie is the fact, that *glottalite* is described by Thomson as containing 21¼ per cent. of water, which is exactly the proper proportion for chabasie.

2. *Zeuxite.*—This mineral I have long considered as merely a tourmaline or schorl, and my inspection of Dr. Thomson's specimen only confirms me in that opinion. At the time the specimen was found, viz. in 1814, at Huel Unity, little was then known comparatively of the precise composition of tourmaline,

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[*] Communicated by the Author.

or of the laws of isomorphism; and many minerals might then have been considered true and distinct species which would be now united. The following Table of *old* analyses of tourmaline, given in Dr. Thomson's original book, will at once show how highly probable it is that *zeuxite* is in reality nothing but tourmaline, and that at that time no better analysis could have been expected.

	Zeuxite.	Tourmaline.			
	Thomson.	Gmelin. Eibenstock.	Leplay. Mount Rosa.	Gmelin. Greenland.	Gmelin. Devonshire.
$\dddot{\mathrm{Si}}$	33·48	33·05	44·10	38·79	35·20
$\dddot{\mathrm{Al}}$	31·85	38·23	26·36	37·19	35·50
$\dot{\mathrm{Fe}}$	26·01	23·86	11·96	5·81	17·86
$\dot{\mathrm{Ca}}$	2·45	0·85	0·50	....	0·55
$\dot{\mathrm{Mg}}$	....	....	6·97	5·86	1·13
$\dot{\mathrm{Na}}$	....	3·17	....	3·13	2·09
$\dot{\mathrm{K}}$	....	....	2·32	0·22	....
$\dddot{\mathrm{B}}$	....	1·89	5·72	3·63	....
$\dot{\mathrm{H}}$	5·28	....	0·60	1·86	....
	99·07	101·05	98·46	96·48	92·33
Spec. grav.	3·05	3·03	3·14	3·06	3·24

Not only does the specific gravity of *zeuxite* agree with that of tourmaline, but its behaviour before the blowpipe, as described by Dr. Thomson, is also similar; and it is worthy of remark, he mentions that the water in *zeuxite* contained traces of muriatic acid. Had he tried for boracic acid, he would more probably have found it was boracic acid.

XVII. *On the Induction Apparatus and its Effects.*

By J. C. POGGENDORFF.

[Continued from p. 12.]

2. *On the Phænomena observable during the Action of the Apparatus.*

HITHERTO it has not at all, or has not sufficiently been kept in view, that the phænomena manifested by the apparatus are essentially different according as the ends or poles of the induction wire are either (1) connected by means of a good conductor, or (2) separated by air or gas, or (3) separated by means of a liquid or solid insulator. In this peculiarity Volta's induction apparatus differs essentially from the magneto-electric machine.

First case.—When the poles of the induction coil are connected by a metallic wire or by a good conducting liquid, the induction current consists of two parts, or two alternate currents, one in the opposite, and the other in the same direction as the inducing current, corresponding respectively to the moments when the circuit of the latter current is closed and broken.

This can be detected by the galvanometer. When the induction wire is very thin and long, and a galvanometer is introduced, no deviation is observed; but when the said wire is thicker and shorter, or the inducing current stronger, the phænomenon of a twofold deviation, which I described in 1838, is observable*. This is caused, of course, by quick, successive magnetizations of the needle in opposite directions.

If water or dilute sulphuric acid is introduced into the current between platinum plates, oxygen and hydrogen are liberated at each plate, and after the cessation of the current no trace of a polarization of the plates can be detected.

In a similar manner, moistened iodide of potassium paper, when put in contact with the poles, shows a separation of iodide of equal strength at each pole.

An electric thermometer rises, the direction of the current being indifferent to it, but a thermo-electric current receives no charge.

Lastly, the condenser has no influence upon these phænomena.

Second case.—When the poles are separated by a stratum of air or gas, even although the stratum be very thin, only one of the two induction currents acts, *i. e.* the one produced by breaking the galvanic current; the other, produced by closing the current, remains without external action in the coil, which on that account obtains fixed poles.

This is evident in the phænomena of tension, which are exhibited when the induction coil is unconnected, that is to say, when its poles are separated by a great thickness of air. If during its activity an electrometer be brought for a moment into contact with one pole of the apparatus, it will be quite accidental whether it will become charged positively or negatively, as the two electricities change places continually on each pole. If the electrometer be placed near enough to allow sparks to spring over, only one kind of electricity is obtained from each pole, *i. e.* that which belongs to the pole when the current is interrupted.

This is still better shown when one of the poles is connected with the earth by means of a wire. The electrometer, when held

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* Poggendorff's *Annalen*, vol. xlv. p. 353.

at a considerable distance from the other pole (*e. g.* an inch and a half), becomes charged with the electricity which corresponds to that pole and the above-mentioned current; but it is charged by radiation merely, and not by sparks. In accordance with these results, it was found that when a wire connected with the earth was held at a short distance from one of the poles, and sparks taken from it for some time, then, after the cessation of the current, the whole induction coil was charged with that kind of electricity which belonged to the other pole.

The sparks which can be drawn from one of the poles of the induction coil in its unconnected condition are of course stronger when the other pole is connected with the earth, and apparently always stronger at the negative than at the positive pole.

When the two poles of the induction coil are brought so near to one another that sparks pass between the two, the circuit may be considered closed; but this closing of the circuit differs from that produced by a good conductor; in the latter, both currents circulate; in the former, only the one which is produced by breaking the inducing current. The stratum of air between the poles, which may be compared to a filter, insomuch as it keeps one current back, requires to be only very thin. A piece of dry blotting-paper, when placed between two metallic plates connected by wires with the poles of the induction coil, perfectly fulfils the purpose of separating one-half of the electricity, and occasions a current of constant direction, which, its discontinuity excepted, possesses all the properties of a galvanic current of high intensity,—is similar, in fact, to that of a very great number of very small plates.

The interruption by means of points serves, of course, the same purpose; and in many respects this purpose may be still better effected by means of the electric egg, because in rarefied air the discharge between the ends of the metallic conductors takes place much more quietly than by the sudden springing of sparks in air of the common density. The thinner and more rarefied the stratum of air, the less the current will be weakened by this interruption, although, perhaps, a part of the same may always remain behind in the wire.

The *effects* of the current when interrupted by a stratum of air, and its difference from a perfectly closed current, are manifested by the galvanometer. A definite deviation is now obtained which depends upon the direction of the current. It is *greater* with a *thick* than with a *thin* induction wire, and it is also more *continuous* and *regular* when the current is interrupted by discharges in a partial vacuum, than when it is broken by sparks in common air. When we consider the difficulty with which the deviation of a magnetic needle is effected by frictional electricity, the facility with which it is here produced, even without any insulation of the galvanometer, will appear somewhat remarkable.

According to Riess, a thermic action is produced in the wire of the interrupted current, but it is an exceedingly weak one. With my less sensitive air-thermometer I could not detect this action, although the same was sufficiently sensible to show the increase of heat in the perfectly closed wire, especially when the latter was thick. It is worthy of notice, however, that notwithstanding this, a thermo-electric battery was charged in exactly the same manner as with a galvanic current, that is to say, opposite to the direction of the same. I observed it in a combination of two V-formed bismuth and antimony pairs. The common thermo-electric batteries are not here applicable, in consequence of their imperfect insulation.

The sparks at the point where the current is broken have also a thermic action, in fact, a much greater one than that which takes place in the metallic circuit of the current. When very thin platinum wires are used as poles, and when they are brought near one another, it has already been observed by several physicists that the point of the negative wire, which is then surrounded by the positive sparks, becomes red-hot.

Chemical decompositions, decompositions of water, iodide of potassium, &c., are effected in exactly the same manner as with a galvanic current, *i. e.* only *one* electrolyte is separated at each pole. In water the electrodes become polarized; and when they are composed of very *thin* platinum wires, the negative one becomes slightly illuminated at the extremity.

Many of these decompositions are interesting when they are so made that the point where the current is broken is at the liquid itself. For this purpose the wires of the discharger are placed vertical and near one another, and the liquid is made to approach them from below, so that the sparks may be made to strike the liquid from both wires at once, or only from one, the other being kept immersed. In general it is found that the stream of sparks from the positive pole possesses a yellow or reddish-yellow colour at the top, close to the point of the wire, and forms a blue disc below on the surface of the water; whereas the stream of sparks from the negative pole is blue at the top, and surrounds with this light a part of the wire from the point, whilst a yellowish glimmer rests upon the liquid. When the liquid is placed nearer to the wires, about a quarter of a line from them, the phænomenon reduces itself to blue sparks at the negative, and yellow ones at the positive pole.

Further, the phænomenon is different according to the nature of the liquid. Non-conducting liquids, such as turpentine, do not, of course, show the phænomenon at all. With distilled water it is weak, because the sparks are small and have little intensity of light. It is more perfect with weak sulphuric acid, hydrochloric acid, nitric acid, with solutions of hydrate of potash, carbonate of soda, &c.; with these alkaline liquids the middle part of the stream of sparks has a beautiful yellow colour, at the positive pole it is even gold-yellow. The phænomenon, however, is most perfect with concentrated sulphuric acid: not one of the liquids which I examined showed the blue disc on its surface underneath the positive pole so great and clear, and beautifully coloured. All the above-mentioned phænomena are more fully developed when the experiments are made in air rarefied to about 8 inches of mercury pressure.

No chemical decomposition can be seen in any of the above cases when both poles send sparks to the liquid. If, however, sparks are allowed to issue from one wire only, and the other is held immersed, a decomposition is immediately visible at the latter, and it is quite indifferent which pole is immersed, and from which sparks are emitted. This remarkable decomposition at a single actual electrode is certainly weak, but I have distinctly observed it with dilute acids, and still more distinctly with concentrated sulphuric acid.

With a solution of iodide of potassium the action is different, for it is decomposed even when sparks fall upon it from both poles; consequently when there are no real electrodes. Whether nitric acid or ozone is thereby formed, may remain at present undiscussed.

If a piece of blotting-paper be moistened with this solution, put in contact with the negative pole, and the positive pole be brought sufficiently near, a brown spot will immediately make its appearance underneath the latter, and the positive stream of sparks will have a brownish colour unlike the violet colour of the vapour of iodine. Underneath the negative pole no colour is visible. But if, on the contrary, the paper be touched by the positive pole and receive sparks from the negative, a brown colour will be perceptible underneath both, though much weaker underneath the latter. Something similar is observed when the negative pole is connected with the earth, and a piece of moist iodide of potassium paper is held opposite the positive pole only.

Paper and sparks have then a brown colour. If sparks are allowed to strike upon the paper from both poles, a brownish colour will generally be perceptible underneath both, but always weaker under the negative than under the positive.

If the two moist places upon the paper which are struck by the sparks be separated by a dry part of small breadth, sparks will also spring over the latter.

A piece of paper saturated with nitrate of silver, perchloride of platinum or perchloride of gold, is coloured brown underneath the positive stream of sparks. With perchloride of gold and perchloride of platinum the sparks themselves have a brown colour.

Underneath them the paper begins to smoke, it soon dries, and now a hole is burned through it, which, glimmering like a fusee at the edges, rapidly increases in size. Brown filaments rise from the paper, attach themselves to the wire, and form a kind of beard around the same. Underneath the negative stream of sparks these phænomena do not manifest themselves, but a dark spot is there formed, which evidently consists of reduced metal.

The effects of the induction current are now described. The reason why that current is only active which is produced by breaking the inducing current, and why the other, generated by closing the same, has no action and does not possess the requisite tension to break through so thin a stratum of air, is simply to be sought in the fact, that by closing the current a complete conducting circuit is formed, which, like all continuous metallic circuits surrounding the induction coil internally or externally, is unfavourable to the development and the duration of the current induced therein.

Good conducting liquids, like those of a galvanic circuit, are in this respect not different from metals. Of this it is easy to arrive at a conviction if the primary coil possesses two wires. When the galvanic current is sent through one of these wires only, and the other is connected with a pair of metallic plates, the development of sparks by the induction coil is immediately suspended when these plates are immersed in dilute sulphuric acid*.

Under ordinary circumstances, when the current is broken, the destruction of the closed circuit itself removes this impediment, and nothing impedes the full development of the current in the induction coil, consequent upon this act, except what may be termed an accumulation of electricity at the ends of the interrupted inducing current, as well as a similar accumula-

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* For the same reason a former experiment of mine failed. I there attempted to generate a strong induction current by continually reversing the magnetization of the soft iron core of the apparatus. It is easy to produce this reversion *without interruption*. For this purpose it is only necessary to keep one wire of the primary coil *continually* connected with *one* galvanic element, and to connect the other, in opposite direction, with a battery consisting of two such elements, and continually interrupted by a Neef's hammer. This combination, however, did not produce the intended result, because within the induction coil a circuit closed by a good conducting liquid was always present.

tion at the ends of the induction wire: both are produced by the resistance which the air opposes to the passage of electricity.

The first-mentioned accumulation is now prevented by Fizeau's happy idea of employing a condenser, for by it the accumulated electricities can flow quickly out of the inducing wire, without a closed circuit being formed.

The quantity of electricity in the induction wire is not increased by the condenser. This is easily shown by closing the induction circuit after introducing a magnetometer into the same. The deviations which are obtained in the latter instrument at each time of opening and closing the inducing current are not only equal to one another, but are as great without the condenser as with it[*].

The generation of electricity in the induction wire at the moment of breaking the inducing current is, however, accelerated by means of the condenser, and consequently the tension of the

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[*] From the following considerations it is well known that the equality of the quantities of electricity which are generated may be concluded from the equality here mentioned. If, at one time, through a section of the wire, a quantity of electricity equal to *unity* passes in the *unit* of time, so that an intensity of current and a deviation, each equal to *unity*, are obtained, and if, at another time, the *unit* of electricity passes through the same section in *ten* units of time, the intensity of current will be equal to *one-tenth*, and consequently the deviation in the unit of time will be *one-tenth*; therefore in the whole time it will be $=10\cdot\frac{1}{10}$, that is, it will be again $=1$; provided, of course (which has been assumed throughout), that the whole time of passage is so small in comparison to the time of oscillation of a magnetic needle that the single impulses will strike the needle in about the same situation, that is to say, not far distant from its position of rest.

It is otherwise with the quantities of heat which are developed at the opening and closing of the circuit; inasmuch as these are proportional to the squares of the intensities of the current, and not (like the deviations) directly proportional to the same, we cannot conclude the equality of the quantities of heat from the equality of the quantities of electricity which are generated, unless the times in which they are generated are also equal. For example, if at one time through a certain section the *unit* of electricity passes in the *unit* of time, so that an intensity of current and a quantity of heat, each equal to *unity*, are obtained; and if at another time through the same section the *unit* of electricity passes in the time *ten*, then the intensity of current will be equal to *one-tenth*, consequently the quantity of heat generated in the unit of time will be $=\frac{1}{100}$, and in the whole time of passage will be $=10\cdot\frac{1}{100}=\frac{1}{10}$, therefore it will be *ten* times smaller; that is, inversely proportional to the time of passage.

Hence, if the equality of the quantities of electricity which are generated be proved by means of the magnetometer, the relation between the times of generation may be determined by means of the electric thermometer. Instead of the latter instrument, the electric dynamometer, which W. Weber has already employed for such determinations (Pogg. *Ann.* vol. lxxiii. p. 215), could be used. Even a galvanometer with a soft iron needle would determine, approximately, the duration of the times of generation.

generated electricity is thereby increased. Hence a pole of the unconnected induction current acts upon the electrometer at a much greater distance when the condenser is employed than when it is not. From this it follows, that when the ends or poles of the induction wire are separated by a certain thickness of air, the sparks which break through it are increased in number when a condenser is employed; indeed, many sparks appear when this distance between the poles is so great, that, without a condenser, not a single one would be visible. The condenser, therefore, actually increases that part of the electricities which equalize themselves at the poles, or are set in circulation, when the induction wire is interrupted by air.

This is also proved by the galvanometric deviation, which, under these circumstances, is increased; whilst, as has before been mentioned, it is not changed by the condenser when the metallic continuity of the wire is unbroken. It is also proved by the decomposition of water which is effected by the induction current when broken by air; for this action is also increased by the condenser.

What has here been said of tensions, sparks, deviations, and decompositions, is also true of the physiological actions,—of the shocks which are received when part of the body is introduced into the circuit. These are also increased by the condenser. Not only is the action of the *external* or actual induction current more intense, but that of the *inner* or extra current is also increased. I have convinced myself of this in several ways, the least painful of which was the following. Two platinum plates were placed in spring-water, and connected either with the poles of the induction coil or with the hammer on each side of the point of interruption. If the finger be immersed in the water between the plates, the effect of introducing the condenser can be observed without pain. The action is increased with the quantity of the current, as Faraday terms it. With the thin induction wire the action is exceedingly weak, stronger with the thick induction wire, and most sensible with the still thicker wire of the extra current.

These experiments induce me to believe that for most of, if not for all, the medical applications which are now so frequently made of the apparatus, the induction coil is quite superfluous, for the extra current can easily be made so strong that no patient would be able to bear its effects.

Certain it is that the action of the condenser is strengthening only when, between the poles, there is a resistance to overcome. The more this resistance is diminished the less the action of the condenser. On this account its action is much stronger with sparks in free air than with the luminous phænomena in the electric egg. The more the latter is exhausted the less the effect of the condenser, which effect disappears altogether (as in metallic contact) when the exhaustion is carried to a line, or less, of mercury pressure; provided only that the induction current itself is sufficiently intense. For a similar reason an induction coil which possesses a short wire produces much weaker sparks in free air than one with a long wire; whereas with the former the luminous phænomena in a well-exhausted electric egg are very little less developed than with the latter. I observed this with the induction wire only 1200 feet long.

Fizeau does not appear to have thoroughly understood the action of the condenser. Amongst other things, in order to explain the action of the instrument, he only considers a single interruption of the inducing current, consequently only a single charge of the condenser, and never mentions what further occurs. But it is evident, in order that the condenser may continue to serve its purpose, it must be discharged before every interruption of the current; and this actually takes place every time the circuit is closed after it has been broken. At both these times sparks are visible; in fact, when a condenser is employed, Neef's hammer occasions a double series of sparks, which, although they cannot be separated by the eye, owing to their quick succession, are nevertheless *distinguishable* in character. The spark, on breaking contact, is always weakened by the condenser, and the degree in which it is so weakened may be considered as a measure of the condenser's action*; but the spark caused by its discharge is proportionally strengthened.

Thus it appears, that, by the addition of a condenser, the *luminous phænomena* at the hammer are, according to circumstances, apparently *unchanged, increased,* or *diminished.* For example, when a double wire around the primary coil was employed, together with a soft iron core, and a battery of two of Grove's elements, the introduction of the 8-feet condenser caused the luminous phænomenon at the hammer to decrease; with the 2-feet condenser it was, on the contrary, increased, without any particular change in the sparks of the induction coil being perceptible in the two cases. By employing *one* of Grove's elements, with which the sparks at the hammer are always weak, and are still further weakened by the condenser, the discharge sparks are scarcely perceptible, although they can be *heard,* and by their irregular and somewhat sonorous stroke they can be easily distin-

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* When the metallic continuity of the induction circuit is unbroken, the sparks at the hammer are always considerably diminished. Even when the induction circuit is interrupted by a stratum of air so thin that the induction sparks form a continuous line, the sparks at the hammer are always diminished.

guished from the regular beat of the hammer. On the contrary, by employing a strong galvanic current, these sparks are quite visible; they are more steady than the interruption sparks, emit a very clear light, throw out smaller sparks, and, with condensers of small dimensions, assume the appearance of a small flame breaking forth from under the hammer. This is a consequence of the great intensity of the current, which has also the effect of fusing the platinum extremity of the hammer, even when the same is 1 millim. thick, and soldering it to the anvil, so that the action of the instrument is arrested unless the small electro-magnet has sufficient power to overcome the solder. The latter always occurred when a current from one of Grove's elements was conducted through the wire 400 feet long and two-thirds of a millim. thick.

Riess found that this fusion could be avoided by using a long, thin, German silver wire to connect the condenser with the hammer, and thus throwing a body of great resistance into the circuit. The discharge sparks, however, are thereby merely weakened; they can still be observed by employing the current from a battery of two elements, even when 120 or 130 inches of German silver wire, 0·45 millim. in thickness, are introduced between the condenser and the hammer by means of the rheochord, and it is at the same time indifferent whether the current traverses the wires of the primary coil side by side or *one after the other*.

Further, the action of the condenser upon the induction current is weakened by so great a length of German silver wire, as may be best seen by the luminous phænomena in the electric egg. This is most perceptible when the wires of the primary coil are traversed *side by side*; when they are arranged *one after the other*, no diminution in the action can be detected, even when 150 inches of German silver wire are used. In the latter case, however, where the interruption at the hammer is scarcely perceptible, the luminous phænomenon in the egg is, *without a condenser*, somewhat different to what it is in the first case; it is more compact, and more like that which is caused by the addition of a condenser.

The interruption sparks as well as the discharge sparks attack the hammer strongly, and pulverize the parts which beat against one another to a black powder. It is a remarkable fact, which has never yet been sufficiently reconciled with the action of a pure voltaic current, that here, where an induction current (an extra current) also acts, the negative pole is most heated, and that a transfer of platinum from this to the positive pole is the consequence. If the *platinum end* of the hammer be connected with the *negative* or *zinc* pole of the battery, and the tongue with the *positive* or *platinum* pole, the first only will wear away; and on the plate of the tongue an eminence of fused platinum will be formed, so that in time the *platinum end* of the hammer and the *tongue* touch one another. I always adopted this kind of connexion, because it is much easier to replace the platinum end and file the eminence down, than to fill up the holes which, in the opposite case, would be burnt into the plate.

Fizeau has already remarked, that the condenser may, to a certain extent, be replaced by a wire of sufficient intensity connecting the vibrating parts of the hammer. I have tested this substitute and found it to be but an imperfect one, probably because the circuit always remains closed. On this account I was led to the construction of the hammer before described, by means of which the interruptions of the current can be effected within a liquid. In order to study the action of this instrument, several liquids of very different conducting powers were employed,—dilute *sulphuric acid, spring water, alcohol of* 0·863 *spec. grav.*, and *oil of turpentine*. With *sulphuric acid* the instrument had no strengthening influence whatever upon the induction current, and that clearly on account of its great conductibility, which had also the effect of producing a copious decomposition of water even when only a single Grove's element was employed, and thereby causing hydrogen, in accordance with the direction of the current, to be liberated at the pin in connexion with the zinc. With *oil of turpentine* it had also no effect, and evidently for an opposite reason, viz. the complete absence of conductibility. On the contrary, with *alcohol*, with *spring water*, and particularly with *distilled water*, the instrument had a strong action, though still inferior to that of the condenser. It was interesting to see how a single drop of water, placed between the vibrating parts of the current-breaker, immediately caused a vivid stream of sparks between the points of the discharger, even when the distance between these points was so great, that, without the drop, not a single spark would have appeared. Water, alcohol, and oil of turpentine, however, all suffer decomposition between the parts of the current-breaker. With water this decomposition is electrolytic, with oil of turpentine it is electro-thermic (so termed by me in a memoir published in 1847*), that is, it is produced by the heat of the interruption spark; and lastly, with alcohol the decomposition is a mixture of both the former.

The superiority of the condenser, when compared with any of the above liquids, is best proved by combining it with the current-breaker when it is playing in water, alcohol, or oil of turpentine; a strong stream of sparks is immediately produced

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* Poggendorff's *Annalen*, vol. xciv.

between the poles of the induction wire, apparently just as strong as when the current-breaker works in air. This combination is further interesting, because it furnishes an opportunity of more minutely observing the discharge sparks. They are here particularly strong and brilliant; they spurt and hiss, particularly in alcohol and oil of turpentine, and they pulverize the platinum to a black powder, so that in a few moments the liquid has just the appearance of ink. The last also takes place without a condenser, but much more slowly and less intensely.

Third case.—Interruption of the Induction Current by Insulators.

When a glass plate is placed across the stream of sparks passing between the poles of the apparatus, it almost entirely causes their cessation, provided that the poles consist of wires more or less pointed at their extremities. It is otherwise when the poles consist of plates. For example, if a square copper plate connected with one of the poles be placed upon the table and covered with a glass plate; and if upon the latter a second, smaller, round copper plate, connected with the other pole, be placed, a durable, loud, crackling noise will be heard as soon as the apparatus is set in activity; and if the experiment be made in the dark, the small round disc will appear surrounded by a halo of electric light, consisting of an innumerable number of small sparks in unceasing motion. The smaller the round plate, the broader the halo. On the lower great plate, which in this case was 3 inches square, no illumination was visible, although the glass plate projected more than half an inch over it on each side. It is of no importance with which pole the one or the other plate may be connected; it is always the smaller of the two around whose edges the halo is visible. If the latter be moved about over the surface of the glass plate, so that only a part of it remains vertically above the lower metal plate, it is only this part which is surrounded by sparks. If both metal plates are of the same size, and the upper one is placed exactly above the lower one, no halo is visible. When viewed from one side, however, it is evident, from the illuminated parts where the metal is accidentally not in perfect contact with the glass, that from both plates innumerable small sparks pass vertically to the glass.

Instead of *one* glass plate, a number of such can be placed between the metals. I took five, whose total thickness amounted to half an inch, and a rim of sparks was always visible around the upper circular plate. This rim was certainly not so brilliant as with a single glass plate; but, on the other hand, with five plates the limit was not reached at which the halo completely disappeared. To increase the number of glass plates, however, seemed the more unnecessary, as the distance of half an inch was already considerably greater than that at which sparks would pass between the metal plates when they were alone in air without the intervention of glass.

The influence of the glass in transmitting the action of induced electricity was consequently plainly proved. The following experiments will furnish additional evidence of this. The upper round copper plate was kept insulated, and held at a distance of 7 millims. from the lower plate on the table. Now and then a single, broad, short spark darted across where the distance between them was least, for without great care the upper plate could not be placed exactly parallel to the lower one.

A glass plate, 4 millims. thick, was now placed upon the lower metal. Immediately a perfect shower of small sparks fell from the upper plate upon the glass. This change in the nature of the sparks did not, as one might imagine, arise from the fact that the distance was now diminished to 3 millims.; for when the lower system was inverted, *i. e.* when the glass plate was laid upon the table and covered with the copper plate, the sparks between the latter and the upper plate were again of the same kind as before, although, of course, smaller and more frequent than at the original distance of 7 millims.

The above-mentioned change, therefore, is an effect of the glass; and this is also evident when the latter does not at all touch the metals, but is held in free air between them. Small sparks are seen to spring from both metal plates towards the glass, at distances where, in absence of the latter, no sparks would appear.

The same effects are produced when other insulating substances are used instead of glass, *e. g.* plates of marble, gutta-percha, broad columns of liquids, such as distilled water, alcohol, or oil of turpentine, enclosed between glass plates. The electrometer proves that in all these cases the insulating substance receives no, or only a very weak and indefinite, permanent charge.

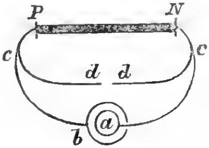
A pointed copper wire was now substituted for the small, round, copper plate, and, like it, connected with one pole of the induction coil. When held at a distance above the greater plate, the point of this wire appeared luminous in the dark; the weaker, of course, the greater the distance. With the power employed by me in all these experiments, viz. that of two of Grove's elements in combination with the shorter of my induction wires, the luminosity at the distance of 2 inches was so weak that it was scarcely perceptible. If now, when the wire is held at this limiting distance, the above-mentioned system of glass plates, half an inch in thickness, be laid upon the lower plate, the luminosity of the point will immediately be again distinctly visible; it increases the nearer the point approaches the glass, until, at a distance of a few lines, a stream of unbroken weak sparks descends. If the point is made to approach still nearer, until it is about a quarter of a line distant, the sparks will not only be clearer, but will disappear in all directions upon the glass, thereby forming a vein-like figure, similar to that of Lichtenberg's, with positive electricity. No difference could be observed in the form of this figure when the point was either positive or negative, except, perhaps, that with a positive point the figure appeared a little larger. Of course it is more perfect and beautiful when the copper plate is covered with *one* thin glass plate instead of with *five*. It may also be remarked, that even when both poles of the induction coil are pointed, the action is not altogether interrupted by the intervention of a glass plate. On the contrary, the points appear luminous in the dark, and emit phosphorescent sparks. In air rarefied to about 6 or 8 inches of mercury pressure, the vein-like figure appears well developed on both sides of the plate, the rays being more than an inch long. By further rarefaction the usual phænomenon occurs, the positive flame curves itself around the glass and forms a brilliant luminous arch.

It is interesting and instructive to examine these phænomena in their reaction on the induction current.

When two glass plates, about a square foot in magnitude, are each coated on one side with tinfoil and placed with their uncoated sides in contact, and the coatings afterwards put in connexion with the poles of the induction coil, a continuous crackling noise is heard so long as the apparatus is in activity, and in the dark the whole intermediate space between the plates, as far as the coatings extend, is seen to be filled with innumerable microscopic sparks. A piece of iodide of potassium paper placed between the plates is coloured brown, on each side equally. If the system is withdrawn from the apparatus during its activity, and then tested, by means of the wire connecting one coating, for a charge, a very weak one will sometimes be detected, though oftener none whatever. This is equally the case whether the poles have actually touched the coatings, or whether they have been separated from them by a small stratum of air, so that sparks might pass from them. It is exactly the same with a Leyden jar. In both cases it will either receive no charge at all or only a very small one, however long it may have been exposed to the current. Even when sparks are taken from one pole upon the knob of the jar, whilst the outer coating and the other pole are connected with the earth, it scarcely receives any charge at all.

If, however, whilst the apparatus is in activity, and its poles are either connected with the coatings of the jar or send sparks to them, a wire be connected with one coating and held sufficiently near to the other, broad, loud sparks will be obtained, which far surpass the thin induction sparks in splendour and strength, although they are inferior to them in length. This difference in the sparks is most perceptible when the poles are within the spark-distance from the coatings; three series of sparks are then obtained, of which two charge the jar and one discharges it. It is evident that charges and discharges are here only apparently simultaneous; actually, they follow one another at intervals too small to be recognized by the eye*.

A curious modification can be made in the experiment, by connecting the discharging wire not immediately with the coatings of the jar, but at any points of the wires which connect the jar with the poles. Let P and N in the adjoining figure be the poles of the induction coil PN, *a* and *b* be the coatings of the jar, and P*b* and N*a* the wires connecting the same with the poles; the discharging wires can be connected at any points *cc* of these latter wires; indeed they may proceed from the poles P and N themselves, and the intense discharge spark will be obtained at the point of interruption *dd*. But, of course, these sparks will be weaker the longer the paths *acd* and *bcd*, or, more correctly, the more resistance these paths oppose. If wires are here inserted which are equal in length or resistance to the induction wire itself, then all difference between the discharge sparks and the induction sparks vanishes. *Cæteris paribus*, the discharge sparks are stronger the more powerful the battery and the smaller the charged surfaces, as might be predicted from the former experiments of Riess with jars laden by the electrical machine.



With respect to the reason why the induction current fails permanently to charge a Leyden jar, even when the former acts only by means of sparks, I was at first inclined to believe that, in consequence of a transmitting action of the glass, a discharge took place; the phænomena with the glass plates coated on one side seemed favourable to this opinion. I have since convinced myself, however, that the cause is rather to be sought in the fact, that, under all circumstances, even when no second connexion is present, the jar is always discharged again by the induction wire

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* This observation was communicated by me to the Berlin Academy, on the 7th of December, after I had communicated it some weeks previous to a few friends. Since then the phænomenon has been described by Grove in the January Number of the Philosophical Magazine. During the publication of the present memoir, I have also learned that it was known to a few persons in Berlin as early as last summer.

itself. The proof of this lies in the fact, that the current of the induction wire always passes to and fro, even when its poles do not touch the coatings, but send sparks to them across a stratum of air; in the latter case, indeed, it is the current consequent upon breaking the inducing current which alone oscillates. If behind the Leyden jar or the coated glass plates, a galvanometer be introduced into the circuit, no deviation is obtained; whereas this never fails when the induction current is simply interrupted by a thickness of air across which sparks can pass*.

This to-and-fro motion of the current is still more perceptibly and beautifully shown by means of the electric egg. So long as it is alone in the circuit, the knob connected with the negative pole is the only one which is surrounded by the well-known beautiful blue light; but as soon as the Leyden jar or the coated glass plates are also introduced into the circuit, both knobs appear blue, and when the vacuum is tolerably good, the positive flame is also visible above the blue light; not, of course, because two opposite currents simultaneously traverse the partial vacuum, but because they follow one another at such short intervals of time that the eye is no longer sensible of the changes†.

Insulating liquids, such as oil of turpentine, when placed between platinum plates and introduced into the circuit, produce the same effects as the Leyden jar.

The experiments with the egg are very instructive, especially when a second circuit also exists. When it is introduced into the primary circuit P*c* or N*c*, or into the branch *cc* at the point *dd*, only one knob is blue. When it is introduced in *ca* or *cb*, both knobs are covered with the blue light.

It is worthy of mention, that the jar *ab* has no influence upon the luminous phænomenon in the branch *cc*, provided the egg be well exhausted, and the induction current itself be sufficiently intense. If air is allowed to enter, however, or if the intensity of the current is diminished, by substituting a long for a short induction wire or by dispensing with the condenser, then the introduction of the jar at *ab* augments the luminous phænomenon at *dd*, though in a much less degree than with sparks in free air.

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* If, instead of the coated glass plates, an oiled silk condenser is introduced into the induction circuit, a continuous crackling is heard, but at the same time a strong deviation is obtained when a galvanometer is also introduced. The crackling in this case, therefore, is caused by an actual passage of electricity, by the breaking through of sparks. At first, when I employed a condenser whose coated surfaces were separated from one another by a double layer of paper, covered on each side with amber varnish, I obtained the phænomena of the glass plates.

† As Riess has already shown (Pogg. *Ann.* vol. xci. p. 290), the same optical illusion is obtained when, by means of the pin already described (Phil. Mag. vol. x. p. 6), the galvanic current is made to traverse the inducing wire alternately in opposite directions.

Mixed Cases.

By mixed cases I understand those wherein a branched communication exists between the poles of the induction wire, and where each branch presents one of the before-mentioned cases. We have just considered a combination of the second and third cases, wherein one branch was interrupted by air, and the other contained an insulating body. A combination of the first and third does not appear to promise much interest; consequently with two branches only, we have but to consider the combination of the first and second cases.

When *cc* is an unbroken metallic circuit, and instead of the jar a well-exhausted egg is introduced at *ab*, no light appears in the latter as long as the wire presents a less resistance. If the resistance be increased, however, either by using a longer or a thinner wire, the current divides itself between both circuits, and the egg becomes luminous. In general this division evidently takes place according to the same laws which hold for the galvanic current, and doubtless if the resistances in the primary and branch circuits, as well as the electromotive force of the induction were known, the intensity of the induction current in both branches could be expressed by the same or similar formulæ to those used in galvanism. The phænomena are here more complicated, merely because the induction apparatus furnishes two alternating currents of opposite directions, of which one has not the power of passing through the egg. Through the primary circuit P*c* or N*c* both currents pass alternately and with equal intensity; through *ca, bc* the current at breaking contact alone passes; and again, through *cc* both currents pass, but with unequal intensities, for the current at closing is greater than the current at breaking contact, because a part of the latter has passed along *ca, bc*. Hence, when the galvanometer is introduced into P*c* or N*c*, no deviation (or an ambiguous one when the current is sufficiently intense) is obtained; in *cb* or *ca* a deviation corresponding to the current at breaking contact is found; and in *cc* the deviation *opposite* to this, corresponding to the current at making contact. These phænomena were very distinct when one of the 10,000-feet wires was employed to generate the induction current, and the other to make the branch *cc*. They were also evident, though accompanied with far less light in the egg, when *cc* consisted of the wire 1200 feet long and 0·25 millim. thick.

Miscellaneous Phænomena.

Many very instructive experiments can be made with two or more instruments, of which the following may be mentioned.

The induction current can be excited in one apparatus and conducted through the coils of the other, in order to study the influence of increased resistance upon the sparks and luminous phænomena in general. For this purpose I conducted the current of the shorter of my induction wires through the 20,000 feet of wire in the other two coils. The sparks were smaller and less brilliant, but there was no perceptible diminution in the distance at which sparks could be obtained. The additional introduction of twelve beakers full of water, and connected by platinum arches, did not alter this result.

This corresponds perfectly with similar phænomena in frictional electricity, formerly studied by Riess. Nevertheless, as far as my own experiments are concerned, I am inclined to doubt whether the tension is the same at all points of the long and well-insulated wire circuit; for when I loosened one end of this circuit from the active induction coil, and by means of another wire, led it alongside the other end of the circuit near to the pole with which it was still connected, sparks were visible between them when the other pole was connected with the earth.

The instruments can be connected alongside one another, or one before the other, in order to study the influence of the thickness and length of the wire.

I had here an opportunity of testing an assertion of Masson's. This physicist states that when two induction instruments are placed alongside one another, but in opposite directions, and are thus connected with the electric egg, both electric lights, the blue glimmering light and the positive flame, are simultaneously seen upon each knob*. From this he arrives at the strange conclusion, that rarefied air possesses the property of simultaneously conducting two electric currents of opposite directions. I obtained a different and far more natural result, viz. that under the circumstances no light whatever is visible. Masson was probably led to his conclusion by allowing two complete instruments to work against one another, and it is more than probable that the strokes of the hammers of both instruments were not exactly isochronous and synchronous. The phænomenon observed by him would then be a natural consequence. Such could not be the case in my experiment, for the inducing wires of both coils formed, one after the other, a single circuit, which was broken by a single hammer.

In conclusion I will notice a combination, the experiments with which are calculated to furnish indications relative to the construction of such apparatus.

Two instruments, A and B, were so combined that the current

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* *Comptes Rendus*, vol. xxxvi. p. 256, and vol. xxxvii. p. 849.

traversed the two primary coils *one after the other*, the induction coils, however, were separate, and A was allowed to give sparks, whilst B remained unclosed, and without a soft iron core; the introduction of the latter into B *weakened* the sparks of A, and the subsequent metallic closing of B *reproduced* their former *strength*. From this it follows that two complete instruments, combined one after the other, mutually disturb one another, and the total effect is not equal to the sum of the effects which each would produce with the same intensity of galvanic current.

Further, if the above experiment be modified so that the current traverses the primary coils of A and B placed *alongside* one another, then the insertion of the soft iron core into B *increases* the sparks of A, and the subsequent metallic closing of B again *destroys* this increase.

Hence, when many induction coils are connected *one after the other*, so as to form a single coil, it is advisable to allow the current to traverse the primary coils of these instruments *side by side*.

With respect to the cause of these phænomena, it is no doubt to be sought in the extra or inner induction current. The outer induction current, with which we are at present solely concerned and which is produced by breaking the inducing current, has the same direction as the latter; whereas the extra current, which is at the same time excited in the inducing circuit, developes a contrary current in the induction wire, whose tendency is to weaken the first induction current, and the more so the stronger the extra current, which latter is especially strengthened by the soft iron core.

Consequently, when the primary coils of A and B are connected one after the other, it is clear that the insertion of the soft iron core into that of B will strengthen the extra current in that of A, and therefore weaken the induction current of the latter instrument.

By connecting the instruments *side by side*, a closed circle is at once formed by them, and the extra current produced by the insertion of the soft iron core into B acts against that in A, and may therefore, when both are equal, completely neutralize it, when the outer induction current of A will be necessarily strengthened. Without the circle formed by this combination, *i. e.* in the wire leading back to the voltaic battery, the two extra currents act, of course, in the same direction and strengthen one another, in consequence of which the sparks at the current-breaker are still very active.

By the metallic closing of B, the action of its soft iron core only will be neutralized. Metallic coverings act similarly, though under certain circumstances they may also act in an opposite manner.

XVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 67.]

April 26, 1855.—Sir Benjamin Brodie, Bart., V.P., in the Chair.

THE following papers were read:—

"Some Observations on the Ova of the Salmon, in relation to the distribution of Species; in a letter addressed to Charles Darwin, Esq., M.A., V.P.R.S. &c." By John Davy, M.D., F.R.SS. Lond. & Edinb.

In this paper the author describes a series of experiments on the ova of the Salmon, made with the intent of ascertaining their power of endurance under a variety of circumstances without loss of life, with the expectation suggested by Mr. Darwin, that the results might possibly throw some light on the geographical distribution of fishes.

The details of the experiments are given in five sections. The results obtained were the following:—

1. That the ova of the Salmon in their advanced stage can be exposed only for a short time to the air if dry, at ordinary temperatures, without loss of life; but for a considerable time, if the temperature be low, and if the air be moist; the limit in the former case not having exceeded an hour, whilst in the latter it has exceeded many hours.

2. That the vitality of the ova was as well preserved in air saturated with moisture, as it would have been had they been in water.

3. That the ova may be included in ice without loss of vitality, provided the temperature is not so low as to freeze them.

4. That the ova, and also the fry recently produced, can bear for some time a temperature of about 80° or 82° in water, without materially suffering; but not without loss of life, if raised above 84° or 85°.

5. That the ova and young fry are speedily killed by a solution of common salt nearly of the specific gravity of sea-water, viz. 1026; and also by a weaker solution of specific gravity 1016.

Finally, in reference to the inquiry regarding the distribution of the species of fishes, he expresses his belief that some of the results may be of useful application, especially those given in the second and third sections; inferring, that as in moist air, the vitality of the ova is capable of being long sustained, they may during rain or fog be conveyed from one river or lake to another adhering to some part of an animal, such as a Heron or Otter, and also during a time of snow or frost; and, further, that other of the results may be useful towards determining the fittest age of ova for transport for the purpose of stocking rivers, and likewise as a help to explain the habitats, and some of the habits of the migratory species.

"Observations on the Anatomy and Affinities of the *Phyllirrhoë bucephala* (Peron)." By John Denis Macdonald, Esq., R.N., Assistant-Surgeon of H.M.S.V. 'Torch.'

As the true position of Peron's genus *Phyllirrhoë*, and even the very existence of the animals composing it, have been matters of doubt to zoologists, during a late cruise to the Fiji Islands I determined to ply the towing-net with a little more diligence than usual, hoping to obtain a few of these almost hypothetical beings, and was rewarded by the capture of many specimens.

Some were taken in the neighbourhood of Lord Howe's Island, S. lat. 31° 31″, E. long. 159° 5″, some near Norfolk Island, S. lat. 29° 2″, E. long. 168° 2″, and others, although in smaller numbers, in different parts of our track. They generally made their appearance after dusk in the evening, and presented a great diversity in size, form and other external characters, which is due to changes in the muscular system, a variable amount of pigment-spots, &c. Indeed at first I fully believed that several distinct species had been brought up together, but this idea was abandoned when I observed the most dissimilar forms gradually assume so close a resemblance to each other, as ultimately to render it difficult to distinguish them.

From these facts I am much inclined to think that the three species described by Quoy and Gaimard, viz. *P. amboinensis*, *P. punctulata* and *P. rubra*, *P. Lichtensteinii* (*Eurydice Lichtensteinii* of Eschscholtz) and *P. rosea* of D'Orbigny, are all referable to Peron's original species *P. bucephala*.

The body of *Phyllirrhoë* is elongated in form and compressed laterally, presenting for description an anterior and posterior extremity, a right and left surface, and a dorsal and ventral border. The head is surmounted by two lengthy, somewhat flattened and acuminate tentacula; the eyes lie beneath the skin, not being visible externally, and the mouth is in the form of a short truncated proboscis, with a vertical opening. The oval-shaped body is on an average about one inch and a half in length, which is something over twice the measurement from the dorsal to the ventral border taken at the middle or broadest part. The tail is quadrilateral in figure, gradually widening towards its posterior border, which is exceedingly thin. The outer integument is perfectly transparent and lined by muscular bundles, disposed longitudinally, and somewhat more than their own breadth apart. These communicate with one another by oblique branching slips, which thus form a kind of network enclosing long lozenge-shaped spaces. Here and there nerve-trunks of considerable size accompany the longitudinal bundles, dividing off into smaller twigs, which distribute themselves at pretty equal distances in a direction more or less perpendicular to that of the muscular fibres. Scattered about at irregular intervals amongst these structures are numerous reddish-brown pigment-spots, in the centre of each of which a clear vesicle is generally distinguishable. As above alluded to, the actual tint of this pigment, and the relative number of spots deposited within a certain space, determine both the general quality and the depth of colour which are found to vary so much in different specimens of *Phyllirrhoë*.

The alimentary canal of this creature consists of a muscular tube lined with mucous membrane, extending without flexure from the mouth to the vent. It commences anteriorly in an oral dilatation, in connexion with which we notice a pair of lateral horny jaws articulated with each other superiorly, and beset with very minute and sharp-pointed teeth along the cutting edge, altogether much resembling those of *Glaucus*, and a lingual ribbon gradually increasing in diameter from before backwards, and supporting a pavement of long, conical, flattened and gracefully curved teeth with fine denticulations at the base. The central series of plates being symmetrical, the large tooth in each takes up a middle position, but in the lateral plates it inclines to the inner side. In some examples I have observed certain lobulated bodies lying in contact with the buccal mass, and which I am disposed to regard as salivary glands. The œsophagus is short, and suddenly expands into a moderately large stomach; and the latter, having received the biliary ducts near its posterior extremity, is continued into the rectum, which passes directly backwards some little distance, and ends in the anus, on the right side of the body, at the union of its posterior and middle thirds. The liver in *Phyllirrhoë* consists of four elongated, tubular, and sacculated portions or lobes, disposed along the borders of the body, two lying above and two below the alimentary canal. Each of the superior hepatic glands opens by a distinct duct into the supero-posterior part of the stomach, while the ducts of the inferior ones unite to form a common tube joining it at its infero-posterior part. The opposite or cæcal extremities of the two anterior hepatic lobes end in the neighbourhood of the head, while those of the others extend to within a short distance of the tail. The secreting cells of these organs are of a rounded or polyhedral form, containing, besides the nucleus, a reddish-brown pigment and fatty globules.

Phyllirrhoë possesses a simple systemic heart, consisting of a single auricle and ventricle. This organ lies upon the stomach, between the ducts of the two superior biliary glands; and a large vessel or sinus, with many circular constrictions in its walls, may be traced towards the auricle, bringing back the aërated blood from the hinder extremity of the body. There are no visible respiratory organs, but it is probable that the cutaneous surface permits of the necessary exposure of the blood to the air contained in the surrounding medium.

The nervous system is well developed. The supra- and subœsophageal ganglia, with their commissural chords, form a close ring round the gullet immediately behind the buccal mass. The auditory sacs, which are filled with vibratory otokonia, appear to lie between both sets of ganglia, and the rudimentary visual organs, consisting each of a simple cell containing a refracting globule imbedded in black pigment, are also in contact with the nervous matter. Besides the actual distribution of the nerves given off from the cephalic ganglia, I noticed nodules of neurine lying at the base of the tentacula, communicating by commissural threads, and sending off each a principal nerve to the corresponding tentacle. The ganglion-globules were lined with a reddish-coloured pigment, deposited round the vesicular nuclei, and when twigs are given off from the smaller nerves, both the homogeneous neurilemma and the contained nervous matter break up like a dividing vessel, without preserving the individuality of distinct nerve-tubes.

The sexes are combined in *Phyllirrhoë*, the male and female generative openings lying close together on the right side of the body in the inferior gastero-hepatic space, and before the anal aperture. The ovaries lie in the inferior recto-hepatic space, varying in number from two to five, in general. They are dark-coloured, subrotund, and finely lobulated bodies, from the fore part of each of which a very delicate duct arises, and all the ducts unite to form a single tube, with a trifling increase in its diameter. This common oviduct, lined by a pavement of transparent epithelial cells, passes forwards beneath the stomach in a flexuous manner; and in the inferior gastero-hepatic space, it first unites with the duct of the testis and again continues its devious course until it ends in the fundus of a much larger tube, whose lining membrane is armed with numerous conical and tooth-like processes, and to this is appended a long cæcal process much resembling the spermatheca of *Helix* for example. The external orifice of the male generative apparatus lies immediately posterior to that of the female organs. The testis is rather small, subglobular in form, and closely connected with a short twisted tube*, much dilated at the middle part, and coated over with a layer of dark pigment-cells. It is with this tube, as above noticed, the small oviduct communicates, in order, as it would seem, to permit of self-impregnation, or to answer some other purpose, with the nature of which we are unacquainted; but there is also an intromittent organ, which, however, I have never seen properly exserted.

As to the affinities of *Phyllirrhoë* with Gasteropods, it may be observed that the animal is bisexual, that the eyes, like those of *Glaucus* and *Ianthina*, are very small and rudimentary, being closely applied to the ganglia of the brain, after the manner of the acoustic sacs, and that both *Phyllirrhoë* and *Glaucus* agree in possessing two lateral horny jaws, articulated with each other superiorly, and bordered with minute conical teeth.

In the *Glaucidæ*, the branchiæ, which consist of simple papillary projections of the skin, are distributed in an equable manner over the dorsal region of the body; and any deviation from this arrangement would naturally tend, either to a more definite localization, or still further dispersion. It is the latter modification which appears to have taken place in *Phyllirrhoë*; so that its respiratory vessels ramify minutely through the common integument, just as the vascular trunks analogous to those which break up in the pectinate gill, adapted for aquatic breathing, are subdivided, and spread themselves over the smooth walls of the lung-chamber in Pulmonifera.

As respects its affinity to the Pteropods, here too the lateral jaws of *Phyllirrhoë* must be borne in mind, together with the almost complete suppression of the organs of vision. It is worthy of note also, that its acoustic capsules contain otokonia, as in Pteropoda, instead of single globular otolithes like those of *Glaucus*, and there is some

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* I have distinctly traced the homologue of this tube in *Pteropoda*, *Heteropoda*, and the *Gasteropoda* proper.

reason to believe that the long tentacula, so called, are the homologues of the cephalic fins of Pteropods.

The particular features of *Phyllirrhoë*, expressed in the last paragraph, also serve to distinguish it from the Heteropoda, but it somewhat approximates this order in the general conformation of its body, which is elongated, laterally compressed, and presents a kind of proboscis at the anterior, and a rudder-fin at the posterior extremity. There is also, as it would appear to be, a small remnant of the foot on the inferior thin margin of the body, and the lateral undulatory motion of the animal in the water exactly resembles that of *Cerophora*, or *Carinaria*.

The heart of *Phyllirrhoë*, in common with that of Heteropods in general, holds a dorsal position. The auricle lies posterior to the ventricle, as in *Cerophora* and *Firola*, but the reverse is the case in *Atlanta* and *Carinaria*, the difference being due to the relation which the respiratory surface bears to the heart itself, lying in every case on the auricular side. Moreover it is remarkable that the rectum is directed backwards in the former instances, but turns forwards in the latter, taking an opposite course to that of the circulation through the heart.

It may be observed in conclusion, that in Heteropoda the viscera are closely packed together so as to occupy the smallest possible space, while they are widely distributed through the abdomen in *Phyllirrhoë*; thus, again, calling to mind its relationship to the Pteropoda.

This paper is illustrated with drawings representing the animal described and some of the details of its internal structure.

"Brief sketch of the Anatomy of a new genus of pelagic Gasteropoda, named *Jasonilla*." By John Denis Macdonald, Esq., R.N.

This communication refers to a remarkable genus of pelagic Gasteropoda, characterized, like *Macgillivraya* and *Cheletropis*, by the presence of ciliated cephalic appendages, but having, as in the present instance, a beautifully transparent, cartilaginous and perfectly symmetrical shell. The author has seen but one species, which was frequently taken between Port Jackson and the Isle of Pines.

The shell resembles that of *Argonauta* in shape, is less than one-eighth of an inch in diameter, and the little animal, when fully retracted, occupies but a small portion of its cavity. The margin of the mantle is of considerable thickness, containing loosely-packed cells, similar to those of the middle or operculigerous lobe of many Pteropods. About eight ciliated arms, identical in character with those of *Macgillivraya*, &c., encircle the head, including the mouth, which is furnished with two massive lateral jaws bearing sharp prominent dental processes on the anterior border, and with a pair of simple tentacula having a dark ocellus at the outer side of the base of each. A well-formed foot arises by a narrow pedicle from the under surface of the body, immediately behind the ciliated collar. The creeping disc is elongated in form, subquadrate in front, and tapers off gradually towards the posterior extremity. The latter part, corresponding to the operculigerous lobe of other species, is speckled with little clusters of dark pigment-cells, disposed so much after the manner of those of the ciliated arms as to lead to the impression that it is one of the same series, or whorl of organs, to use botanical phraseology. A pectinate gill extends beneath the mantle, along the anterior third of the dorsal region, lying, as in most cases, in advance of the heart. The visceral mass of the body, though elongated, is but slightly curved upon itself, not exceeding half a turn. The lobules of the liver, distended with large amber-coloured oil-globules, may be distinctly seen through the transparent outer envelope and shell. Single spherical otolithes are contained in the acoustic sacs, and the lingual ribbon is lengthy and flexuous, presenting a row of uncini on each side, with a series of minute denticulations, pointing backwards on their anterior and posterior borders. The uncini of opposite sides interlock with one another so closely as to conceal the rudimentary segments of the rachis almost completely. The shell is cartilaginous, transparent, planorbicular, and perfectly symmetrical, presenting four rows of minute conical tuberculations on its convex or dorsal surface. The gyri of the involute nucleus are so curved as to leave a central perforation; the mouth of the tube encroaches considerably on the last whorl, and the outer lip is deeply notched between the two lateral rows of tubercles. The author has named the species *Jasonilla M^cLeayiana.* The paper is accompanied with illustrative figures.

Note "On the position of Aluminum in the Voltaic series." By Charles Wheatstone, Esq., F.R.S.

Having, through the kindness of Dr. Hofmann, been permitted to examine a specimen of aluminum prepared by M. Claire-Deville, I availed myself of the opportunity to ascertain one of the physical properties of this extraordinary metal, which it does not appear has been yet determined, viz. its order in the voltaic series. The following are the results of my experiments.

Solution of potass acts more energetically and with a greater evolution of hydrogen gas upon aluminum than it does on zinc, cadmium or tin. In this liquid aluminum is negative to zinc, and positive to cadmium, tin, lead, iron, copper and platina. Employed as the positive metal, the most steady and energetic current is obtained when it is opposed to copper as the negative metal; all the other metals negative to it which were tried became rapidly polarized, whether above or below copper in the series.

In a solution of hydrochloric acid aluminum is negative to zinc and cadmium, and positive to all the other metals above named. With this liquid also copper opposed to it as the negative metal gave the strongest and most constant current.

Nitric and sulphuric acids are known not to act chemically in any sensible manner on aluminum. With the former acid diluted as the exciting liquid aluminum is negative to zinc, cadmium, tin, lead and iron. The current with zinc is strong; with the other metals very weak, and it is probable that their apparent negative condition is the result of polarization. When aluminum is immersed in dilute sulphuric acid, this metal appears negative to zinc, cadmium, tin and iron, but with lead, on which sulphuric acid has no action, the current is insensible. In both these liquids copper and platina are negative to aluminum, and notwithstanding the apparent absence of chemical action on the latter metal, weak currents are produced.

It is rather remarkable, that a metal, the atomic number of which is so small, and the specific gravity of which is so low, should occupy such a position in the electromotive scale as to be more negative than zinc in the series.

May 3.—Charles Wheatstone, Esq., V.P., in the Chair.

"An Experimental Inquiry into the nature of the metamorphosis of Saccharine Matter, as a normal process of the animal œconomy." By Dr. Pavy.

The author begins by observing, that the saccharine matter met with in the animal œconomy is derived from two sources—from the vegetable kingdom, and from the liver of the animal itself; in each case being poured into the general circulation through the hepatic veins. The liver not only enjoys the power of forming sugar, but it likewise exerts (as shown by the experiments of Bernard) some modifying influence over that which is traversing its capillaries and which has been absorbed from the food, by which it is transformed from *vegetable* into *animal* sugar, and thus rendered more apt for serving in the processes of animal life.

The sugar poured into the general circulation through the hepatic veins is conveyed to the capillaries of the lungs, where it in great part disappears, but never entirely so, according to very numerous analyses which the author has made on this subject. If the blood be traced onwards from the arteries through the systemic capillaries into the veins, the small amount of sugar which impregnates arterial blood will be found to be still undergoing a process of destruction; and what appears exceedingly interesting, this process of destruction is not carried on with equal activity in the different parts of the system at large. In the capillaries of the chylo-poietic viscera, the destruction is so complete, that the blood in the portal vein may be entirely free from saccharine principle, when the blood returning from other parts, as that contained in the femoral or jugular veins, remains slightly impregnated. This curious fact has a bearing that will be presently adverted to, with reference to the views to be advanced concerning the nature of the metamorphosis of sugar in the animal œconomy.

The *principal* seat of destruction of saccharine matter in the animal system being located in the respiratory organs, seems at first sight to support the theory of Liebig—that sugar is one of those substances which undergoes a process of combustion, by its direct combination with oxygen and its resolution into water and carbonic acid. Some experiments on the temporary obstruction of the respiration and the examination of arterial blood before and after the operation, led the author to call in question this view, as he observed that notwithstanding the supply of oxygen was cut off to such an extent as almost to occasion death, yet a considerable destruction of sugar took place in the lungs. This, coupled with the fact that a disappearance of sugar takes place in the systemic capillaries, and unequally so in different portions of them, induced him to push his investigations, and see if there might not be some other cause in operation in the living animal to effect the normal destruction of sugar, besides the direct chemical action of the oxygen absorbed in respiration. The results of these investigations, which were first directed towards the changes produced in blood normally containing sugar, injected through the capillaries of lungs removed from the animal, and artificially inflated with atmospheric air or oxygen gas, have induced the author to refer the metamorphosis of sugar in the animal œconomy, to a process which is perfectly consistent and analogous with the well-known chemical bearings of this substance apart from the animal system.

In experiments which the author has now several times repeated, he injected blood removed from the right side of the heart of an animal—and therefore normally containing sugar—through the capillaries of the artificially inflated lungs of another; and found that as long as the blood retains its fibrine, there is as much destruction of its sugar as would take place in the living animal; but that where the fibrine has been separated from the serum and corpuscles, the sugar ceases to be influenced by the presence of oxygen, or ceases to disappear during this process of artificial respiration. It would hence appear, that something besides mere contact with oxygen is requisite for the destruction of sugar. But in other experiments, he has found that oxygen is nevertheless a necessary agent concerned in the process of transformation observed during the arterialization of the blood that has not undergone spontaneous coagulation. It would therefore seem, in fact, that oxygen acts secondarily on the sugar through the medium of the fibrinous constituent of the blood:—that it exerts some changes upon this azotized principle, which are capable of inducing the metamorphosis of sugar.

If we look to the ordinary chemical bearings of saccharine matter apart from the animal system, we find that an azotized substance undergoing the molecular changes of decomposition, placed in contact with sugar readily excites a process of fermentation, and converts it by a mere alteration of the grouping of its elements into another substance, one atom of sugar ($C^{12} H^{12} O^{12}$) being resolved into two atoms of lactic acid ($C^{6} H^{6} O^{6}$). We also find that sugar is not susceptible of oxidation except under the influence of strong chemical reagents. Chemical analogy, therefore, would lead us to look upon the secondary action of oxygen as the more probable process of physiological destruction; especially when we take into consideration, that nowhere do we meet with such a constant series of molecular changes taking place as amongst the azotized constituents of a living animal. In the above-mentioned experiment of injecting fibrinated and defibrinated blood through an artificially inflated lung, when the blood is capable of undergoing the molecular changes of assimilation on contact with oxygen as in the living animal, the sugar in great part disappears, but so soon as the fibrine is separated by spontaneous coagulation, and the blood has thus lost its vital characteristics, oxygen is no longer capable of exerting any metamorphosing influence on its saccharine ingredients.

If the molecular changes occurring during the decomposition of an azotized substance be capable of converting sugar into lactic acid, why should not the molecular changes occurring during the building-up or elaboration of this same nitrogenized compound effect the same? Indeed, we have seen that the process of destruction is carried on to a certain extent in the systemic capillaries, and more especially in those of the chylo-poietic viscera, where the molecular changes of nutrition are also correspondingly carried on with greater activity than elsewhere. So that analogy and experiment would tend to show that the physiological destruction of sugar is owing to a process similar to fermentation induced by the molecular changes occurring in the nitrogenized constituents of the animal during life. And, in accordance with this, we find lactic acid present in the system, and largely separated from arterial blood by the muscular tissue, and the secerning follicles of the stomach.

As regards the lactic acid fermentation, it is well known that the presence of an alkali favours, whilst that of an acid retards the process. In two experiments on animals, the author injected carbonate of soda and phosphoric acid into the circulating current, and observed in the case of the latter that sugar immediately accumulated in the blood.

The preceding observations refer more especially to the changes that take place in the saccharine ingredient of the blood during life; and the author next proceeds to notice some interesting phænomena observable during the decomposition, and even the spontaneous coagulation of blood containing sugar.

If the blood of an animal normally impregnated with sugar be placed aside, and allowed to undergo spontaneous coagulation, on examining separately the serum and clot on the following day, it will be found, that although the serum may be largely saturated with sugar, the clot is entirely, or almost entirely destitute of it. Now, as the clot is moist and remains to a certain extent infiltrated with the serum from which it has partially separated, it would appear that even the molecular changes arising from the spontaneous coagulation of the blood are sufficient to effect the destruction of normal animal sugar. And this conclusion is strengthened by the fact, that in diabetic blood (the sugar of which, as would appear from other considerations also, is not so susceptible of metamorphosis as the healthy variety) the sugar does not disappear to a similar extent in the clot.

Under the changes of the decomposition of blood, normal animal glucose is very readily metamorphosed. The rapidity of the metamorphosis depends on the activity of the decomposition of the animal substances present, and when the destruction of the sugar is complete the blood has assumed an *acid reaction*.

This acid reaction of decomposing blood is only observable in that which was previously pretty largely impregnated with sugar. It appears to be owing to the formation of lactic acid. Certainly, it cannot be due to carbonic acid, for the reaction remains after exposure to a boiling temperature.

The disappearance of sugar in the manner just pointed out does not depend on the oxygen of the air, except in so far as this agent is concerned in exciting the decomposition of the azotized constituents of the blood; for the sugar disappears as rapidly when there is a small, as when there is a large amount of surface exposed to the air. But if the air be carefully and completely excluded, no signs of decomposition of the animal parts of the blood are to be observed, and under these circumstances the sugar also remains. The disappearance of sugar is more rapid where the fibrine and corpuscles are present, than when the serum is exposed alone; and in accordance with this, the blood in the one case undergoes decomposition much sooner than in the other—a fact easily intelligible from the greater amount of azotized ingredients present.

If blood normally impregnated with saccharine matter be placed aside until signs of incipient decomposition are observed, and the sugar is beginning to disappear, exposure to a current of oxygen rapidly completes the total disappearance of the saccharine constituent. In this observation we have a further illustration of the analogy that appears to exist, in the nature of the metamorphosis of sugar as a physiological process, and that which takes place chemically under the influence of an azotized compound, whose elementary particles are in a state of molecular transition. During life, the higher organic constituents of the blood are capable of undergoing the changes of assimilation on exposure to contact with oxygen, and there is a considerable destruction of sugar effected; for a short period after death these azotized constituents remain stationary and uninfluenced by oxygen, and with this, there is a corresponding suspension of the transformation of sugar; but finally, the animal matter of the blood on contact with oxygen, especially during a warm temperature, assumes a state of decomposition, the molecular changes of which again excite the destruction or metamorphosis of saccharine matter.

The sugar *disappears far less rapidly* from diabetic blood under the influence of exposure to the atmosphere, than from healthy right-ventricular blood. From these, and a few other observations which he has as yet been able to make on the blood in Diabetes Mellitus, the author, were he to hazard an opinion on the nature of that obscure disease, would be disposed to say that there appears to be a modification of sugar produced by the liver, which is not susceptible of undergoing the normal process of destruction in the animal system, and which, therefore, accumulating in the blood, is eliminated by the kidneys. The experiments of Bernard have shown that vegetable glucose (grape-sugar) is not susceptible of destruction in the processes of animal life, unless converted into animal glucose by the agency of the liver. Diabetic sugar would therefore seem to bear a resemblance in its physiological relations to vegetable, rather than to animal glucose.

The following communications were in part read:—

"Researches on the Partition of Numbers." By Arthur Cayley, Esq., F.R.S.

The author discusses the following problem:—"To find in how many ways a number q can be made up of the elements $a, b, c \ldots$, each element being repeatable an indefinite number of times." The solution depends upon a peculiar decomposition of an algebraical fraction $\frac{\phi x}{fx}$, where the denominator fx is the product of any number of factors, the same or different of the form $1-x^m$, and upon the expansion by means thereof of the fraction in ascending powers of x. The coefficient of the general term is expressed in terms of circulating functions, such that the sums of certain groups of the coefficients are severally equal to zero; these functions the author calls prime circulators. The investigations show the general form of the analytical expression for the number of partitions, and they also indicate how the values of the coefficients of the prime circulators entering into such expression are to be determined.

"Further Researches on the Partition of Numbers." By Arthur Cayley, Esq., F.R.S.

The memoir contains a discussion of the problem "to find in how many ways a number q can be made up as a sum of m terms with the elements $0, 1, 2, \ldots k$, each element being repeatable an indefinite number of times." The number q may without loss of generality be taken to be equal to $\frac{1}{2}(km-\alpha)$, and the expression for the number of partitions of this number $\frac{1}{2}(km-\alpha)$ is by a peculiar method reduced to the form coeff. x^m in $\frac{\varphi x}{fx}$, where $\frac{\phi x}{fx}$ is an algebraical fraction, the form of which depends on the value of k, *but which does in anywise involve the number m*; the denominator fx is the product of factors of the form $1-x^\theta$, and up to certain limiting values of α the fraction is a proper fraction. The author remarks in conclusion that the researches were made for the sake of their application to the theory developed in his "Second Memoir upon Quantics."

May 10.—The Lord Wrottesley, President, in the Chair.

The following communications were read:—

"An Experimental Inquiry undertaken with the view of ascertaining whether any *force* is evolved during Muscular Contraction analogous to the *force* evolved in the Fish, Gymnotus, and Torpedo." By Henry Foster Baxter, Esq.*

"On a simple Geometrical construction, giving a very approximate Quadrature of the Circle." By C. M. Willich, Esq.

Let AB be a quadrant of a circle A, B, C. In the arc BC place a chord BD equal to the radius, so that the arc BD is one of 60°.

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* This communication will appear in our next Number.

Bisect BD in E; join AE, and produce the joining line to meet the circumference in F. Then AF differs from the side of a square equal in area to the circle by somewhat less than the one four-thousandth part of that side.

May 24.—The Lord Wrottesley, President, in the Chair.

The following communication was read:—

"A Second Memoir upon Quantics." By Arthur Cayley, Esq. F.R.S.

The memoir is intended as a continuation of the author's introductory memoir upon Quantics (vide Phil. Mag. vol. viii. p. 69, and Phil. Trans. 1854, p. 245); the special subject of the memoir is the theorem referred to in the postscript of the introductory memoir, and the numerous developments arising thereout in relation to the number and form of the covariants of a binary quantic. The author, after some remarks as to the asyzygetic integrals and the irreducible integrals of a system of partial differential equations, and after noticing that the number of irreducible integrals is in general infinite, proceeds to establish the above-mentioned theorem, viz. that a function of any order and degree satisfying the necessary condition as to weight, and such that it is reduced to zero by one of the operations $\{xdy\}-xdy$ and $\{ydx\}-ydx$, is reduced to zero by the other of the two operations, *i. e.* that it is a covariant; and he shows how by means of the theorem the actual calculation of the covariants is to be effected. The theorem gives at once (in terms of symbols P, P′, which denote a number of partitions) expressions for the number of the asyzygetic covariants of a given degree and order, or of a given degree only, of a quantic of any order; this enables the discussion of particular cases, but to obtain more general results it is necessary to transform the expressions for the numbers of partitions by the method explained in the author's "Further Researches on the Partition of Numbers." It appears by the resulting formulæ that the number of the irreducible invariants or covariants does in fact become infinite for quantics of an order sufficiently high; the number of the irreducible invariants first becomes infinite in the case of a quantic of the order 7; the number of irreducible covariants first becomes infinite in the case of a quantic of the order 5. In particular, the formulæ show that in the case of a quantic of the order 5, or quintic, there are 4 irreducible invariants of the degrees 4, 8, 12 and 18, respectively connected by an equation of the degree 36; and that in the case of a quantic of the order 6, or sextic, there are 5 irreducible invariants of the degrees 2, 4, 6, 10 and 15 respectively connected by an equation of the degree 30; so that the system of the irreducible invariants of a sextic is analogous to that of the irreducible invariants of a quintic. The memoir concludes with a table of the covariants of a quadric, a cubic, and a quartic, and of certain of the covariants of a quintic.

XIX. *Intelligence and Miscellaneous Articles.*

SOUNDING RAPID CURRENTS.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

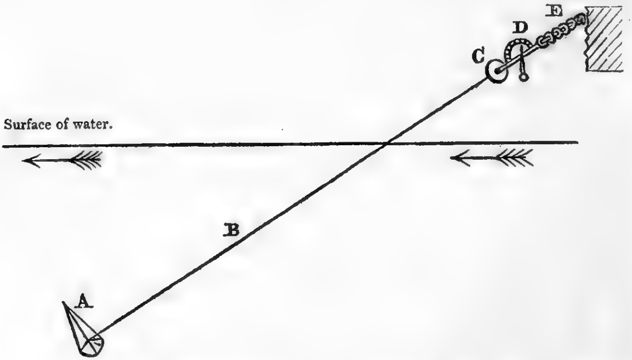
SEEING an account, which is making the tour of the newspapers, of an attempt to sound the depths of Niagara, which failed in consequence of the great velocity of the stream not allowing the plumb to sink, it struck me that that which could not be accomplished by main force might be done by a very simple contrivance.

The principle on which a schoolboy's common kite mounts in the air, overcoming the law of gravity, is well known; and I should think that an apparatus constructed on the same principle, and perhaps of a like form but much smaller, and strongly made of iron, would by the action of the current of water be forced to the bottom of the river. All boys know that the stronger the breeze, the higher the kite flies. The depth might be easily determined by the angle of the detaining chain or wire with the surface of the water. I merely throw out the hint, hoping it may perhaps lead to the invention of some apparatus that will overcome the difficulty.

I enclose a rough diagram, and remain, Sirs,

Your obedient Servant,

W. SOWERBY.



A. Kite, strongly made of thick sheet iron.
B. Detaining chain or wire.
C. Reel fixed at the end of bar carrying the scale D, with a pointing needle weighted to keep perpendicular.
E. Chain and hook to attach the apparatus to the shore or bridge.

ON A FALL OF METEORIC STONES AT BREMEWORDE IN HANOVER. BY M. WÖHLER.

On the 13th of May last, at 5 o'clock in the afternoon, a very remarkable fall of meteoric stones was observed near Bremeworde, not far from Hamburg. This phænomenon was accompanied by thunder and a great hissing noise. The sky was cloudy, so that the meteor was not seen; but three stones were seen falling, and these were found. It is very probable that a greater number fell. The largest of these stones weighs 3 kilogrammes, the second 1½ kilogramme, and the third 325 grammes.

Like most of the aërolites observed, these stones are covered with a black fused crust. Their fracture shows a mixture of several minerals of a gray colour, amongst which a considerable quantity of metallic iron and sulphuret of iron may be distinguished. They present a close resemblance with those which fell in Transylvania on the 4th of September 1852, and which form part of the collection in the Imperial Museum of Vienna.—*Comptes Rendus*, June 25, 1855, p. 1362.

METEOROLOGICAL OBSERVATIONS FOR JUNE 1855.

Chiswick.—June 1. Overcast. 2. Cloudy and fine: very clear. 3. Fine: cloudy: overcast. 4. Heavy clouds: very fine. 5. Cloudy: fine. 6. Hot and dry: rain. 7. Very fine. 8. Cloudy: very fine. 9. Showery. 10, 11. Very fine. 12. Very fine: clear at night. 13. Slight haze. 14. Overcast: rain. 15. Cloudy: fine: rain. 16. Fine: showery: overcast: heavy rain. 17. Cloudy: slight showers. 18. Clear: cloudy: rain. 19. Overcast and fine. 20. Light clouds: fine: clear: *frosty at night*. 21. Clear and fine. 22. Fine: very dry air. 23. Overcast. 24. Very fine. 25. Cloudy and fine. 26. Overcast: very fine. 27. Dry haze: very fine. 28—30. Very fine.

Mean temperature of the month	57°·98
Mean temperature of June 1854	56 ·93
Mean temperature of June for the last twenty-nine years	60 ·39
Average amount of rain in June	1·89 inch.

Boston.—June 1. Cloudy: rain A.M. 2. Cloudy. 3, 4. Fine. 5. Cloudy. 6. Fine. 7. Cloudy: rain A.M. 8. Cloudy. 9. Fine: rain P.M. 10. Fine: rain with thunder P.M. 11. Fine. 12, 13. Cloudy. 14. Rain A.M. and P.M. 15. Cloudy: rain A.M. and P.M. 16. Fine. 17, 18. Cloudy: rain P.M. 19, 20. Cloudy. 21, 22. Fine. 23—27. Cloudy. 28—30. Fine.

Sandwick Manse, Orkney.—June 1. Bright A.M.: rain P.M. 2. Damp A.M.: foggy P.M. 3. Hazy A.M.: cloudy P.M. 4. Hazy A.M.: clear P.M. 5. Bright A.M.: clear P.M. 6. Rain, thunder A.M.: drops P.M. 7. Foggy A.M.: cloudy P.M. 8. Bright, fine A.M.: foggy P.M. 9. Showers A.M.: showers, foggy P.M. 10. Cloudy A.M.: clear P.M. 11. Bright A.M.: cloudy P.M. 12. Rain A.M. and P.M. 13. Foggy A.M. and P.M. 14. Clear A.M. and P.M. 15. Cloudy A.M.: clear P.M. 16. Showers A.M.: drops P.M. 17. Drizzle, showers A.M. and P.M. 18. Bright A.M.: drizzle, showers P.M. 19. Clear A.M. and P.M. 20. Cloudy A.M.: small rain P.M. 21. Foggy A.M. and P.M. 22. Foggy A.M.: bright P.M. 23. Bright A.M. and P.M. 24. Bright A.M.: bright, rain P.M. 25. Rain A.M. and P.M. 26. Drops A.M.: rain P.M. 27. Bright A.M.: clear P.M. 28. Clear, fine A.M.: bright, fine P.M. 29, 30. Bright, fine A.M. and P.M.

Mean temperature of June for twenty-eight previous years	52°·78
Mean temperature of this month	52 ·23
Mean temperature of June 1854	52 ·86
Average quantity of rain in June for fifteen previous years	2·21 inches.

Meteorological Observations made by Mr. Thompson *at the Garden of the Horticultural Society at* Chiswick, *near London;*
by Mr. Veall, *at* Boston; *and by the* Rev. C. Clouston, *at Sandwick Manse,* Orkney.

Days of Month.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
1855. June.	Max.	Min.		9¼ a.m.	8½ p.m.	Max.	Min.		9¼ a.m.	8½ p.m.						
1.	29·924	29·718	29·36	30·12	30·03	55	36	52	51½	46	sw.	s.	ne.	....	·75	....
2.	30·002	29·918	29·55	29·93	29·87	61	35	59	49	46	sw.	ssw.	se.	....	....	·08
3.	29·917	29·874	29·48	29·75	29·68	67	51	58	51	49	sw.	ssw.	sse.	·02	....	....
4.	29·870	29·859	29·38	29·62	29·55	69	50	62	52	49½	sw.	ssw.	se.	....	....	....
5.	29·893	29·876	29·38	29·55	29·64	76	54	60	58½	50½	sw.	s.	sse.	....	....	....
6.	29·809	29·693	29·36	29·72	29·81	86	53	66	53	52	s.	sse.	ese.	·16	....	·09
☾ 7.	29·881	29·826	29·34	29·70	29·79	71	47	60	55	53½	sw.	se.	......	....	·19	·09
8.	29·941	29·929	29·45	29·85	29·83	72	46	63	57	48½	sw.	ssw.	e.	....	....	....
9.	30·019	29·991	29·50	29·79	29·83	66	42	63	51½	49½	sw.	ssw.	nw.	·08	....	·05
10.	30·227	30·191	29·70	30·01	30·12	73	41	58	55½	51	ne.	sw.	wnw.	....	·04	·16
11.	30·195	30·189	29·78	30·16	30·09	72	46	60	55	52	e.	e.	wsw.	....	·20	....
12.	30·107	29·856	29·70	29·95	29·82	70	54	60	51½	50	ne.	e.	sw.	....	....	·30
13.	29·686	29·435	29·30	29·70	29·57	74	51	63	56	50	e.	e.	nw.	·27	....	·47
● 14.	29·517	29·420	28·98	29·40	29·32	65	51	51	53½	50½	sw.	nw.	ne.	·18	·46	·10
15.	29·346	29·280	28·80	29·26	29·39	65	42	61	51	47	sw.	w.	nne.	·11	·30	....
16.	29·434	29·328	28·40	29·52	29·69	65	46	60·5	50½	46½	s.	sw.	n.	·46	·30	....
17.	29·906	29·698	28·30	29·82	29·88	59	36	47	48½	44	nw.	n.	nw.	·02	·02	·02
18.	30·095	30·017	29·60	29·85	29·98	58	50	53	47½	42½	w.	w.	nw.	·18	·02	·06
19.	30·223	30·061	29·70	30·07	30·24	61	36	52	50	46	n.	nne.	se.	....	·06	....
20.	30·366	30·227	29·98	30·27	30·24	56	30	53	55½	55	n.	nne.	wsw.	....	....	....
21.	30·335	30·181	29·94	30·28	30·31	69	38	52	55	54	ne.	nne.	s.	....	....	·07
☽ 22.	30·252	30·104	29·83	30·20	30·17	74	58	58	59½	50	nw.	nw.	w.	....	....	....
23.	30·170	30·110	29·67	30·13	30·11	68	41	63	53	50½	nw.	n.	w.	....	....	....
24.	30·178	30·148	29·73	29·99	29·88	71	43	56	55	50½	nw.	nw.	ssw.	....	....	·07
25.	30·164	30·074	29·61	29·65	29·77	68	52	62	52	49	sw.	w.	e.	....	....	·59
26.	30·224	30·167	29·63	29·94	29·98	77	56	68	52	53½	nw.	w.	wsw.	....	....	·11
27.	30·357	30·297	29·84	30·10	30·29	81	45	70	57½	52	sw.	wnw.	w.	....	....	·38
28.	30·274	30·104	29·80	30·22	30·12	79	53	73	65	57	e.	sse.	e.	....	....	....
○ 29.	30·072	29·874	29·58	30·04	29·96	79	61	65	67½	56	se.	e.	sse.	....	....	....
30.	30·017	29·928	29·34	29·92	29·85	76	52	75	58½	55	sw.	ssw.	se.	....	....	....
Mean.	30·013	29·912	883·01	29·883	29·893	69·43	46·53	60·0	54·26	50·21				1·48	2·34	2·64

THE

LONDON, EDINBURGH AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FOURTH SERIES.]

SEPTEMBER 1855.

XX. *On the Nature of the Force by which Bodies are repelled from the Poles of a Magnet; to which is prefixed, an Account of some Experiments on Molecular Influences. By* JOHN TYNDALL, *Ph.D., F.R.S. &c.**

[With Three Plates.]

INTRODUCTION.

FROM the published account of his researches it is to be inferred, that the same heavy glass, by means of which he produced the rotation of the plane of polarization of a luminous ray, also led Mr. Faraday to the discovery of the diamagnetic force. A square prism of the glass, 2 inches long and 0·5 of an inch thick, was suspended with its length horizontal between the two poles of a powerful electro-magnet: on developing the magnetism the prism moved round its axis of suspension, and finally set its length at right angles to a straight line drawn from the centre of one pole to that of the other. A prism of ordinary magnetic matter, similarly suspended, would, as is well known, set its longest dimension from pole to pole. To distinguish the two positions here referred to, Mr. Faraday introduced two new terms, which have since come into general use: he called the direction parallel to the line joining the poles, the *axial* direction, and that perpendicular to the said line, the *equatorial* direction.

The difference between this new action and the ordinary magnetic action, was further manifested when a fragment of the heavy glass was suspended before a single magnetic pole: the fragment was repelled when the magnetism was excited;

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* From the Philosophical Transactions for 1855, part i.; having been received by the Royal Society October 31, 1854, and read January 25, 1855. The Bakerian Lecture.

and to the force which produced this repulsion Mr. Faraday gave the name of *diamagnetism*.

Numerous other substances were soon added to the heavy glass, and, among the metals, it was found that bismuth possessed the new property in a comparatively exalted degree. A fragment of this substance was forcibly repelled by either of the poles of a magnet; while a thin bar of the substance, or a glass tube containing the bismuth in fragments, or in powder, suspended between the two poles of a horseshoe magnet, behaved exactly like the heavy glass, and set its longest dimension equatorial.

These exhaustive researches, which rendered manifest to the scientific world the existence of a pervading natural force, glimpses of which merely had been previously obtained by Brugmann and others, were made public in 1846; and in the following year M. Plücker announced his beautiful discovery of the action of a magnet upon crystallized bodies. His first result was, that when any crystal whatever was suspended between the poles of a magnet, with its optic axis horizontal, a repulsive force was exerted on the said axis, in consequence of which it receded from the poles and finally set itself at right angles to the line joining them. Subsequent experiments, however, led to the conclusion, that the axes of optically negative crystals only experienced this repulsion, while the axes of positive crystals were attracted; or, in other words, set themselves from pole to pole.

The attraction and repulsion, here referred to, were ascribed by M. Plücker to the action of a new force, entirely independent of the magnetism or diamagnetism of the mass of the crystal*.

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* "*The force which produces this repulsion is independent of the magnetic or diamagnetic condition of the mass of the crystal; it diminishes less, as the distance from the poles of the magnet increases, than the magnetic and diamagnetic forces emanating from these poles and acting upon the crystal.*" —Prof. Plücker in Poggendorff's *Annalen*, vol. lvii. No. 10; Taylor's Scientific Memoirs, vol. v. p. 353.

The forces emanating from the poles of a magnet are thus summed up by M. Plücker:—

1st. The magnetic force in a strict sense.

2nd. The diamagnetic action discovered by Faraday.

3rd. The action exerted on the optic axes of crystals (and that producing the rotation of the plane of polarization which probably corresponds to it). *The second diminishes more with the distance than the first, and the first more than the third.*—Taylor's Scientific Memoirs, vol. v. p. 380.

The crystal (cyanite) does not point according to the magnetism of its substance, *but only in obedience to the magnetic action upon its optic axes.*—Letter to Mr. Faraday, Phil. Mag. vol. xxxiv. p. 451. The italics in all cases are M. Plücker's own.

M. De la Rive states the view of M. Plücker to be:—that the axis in its quality as axis, and independently of the very nature of the substance of the crystal, enjoys peculiar properties, more frequently in opposition to

Shortly after the publication of M. Plücker's first memoir, Mr. Faraday observed the remarkable magnetic properties of crystallized bismuth; and his researches upon this, and other kindred points, formed the subject of the Bakerian Lecture before the Royal Society for the year 1849.

Through the admirable lectures of Professor Bunsen on Electrochemistry in 1848, I was first made acquainted with the existence of the diamagnetic force; and in the month of November 1849 my friend Professor Knoblauch, then of Marburg, now of the University of Halle, suggested to me the idea of repeating the experiments of M. Plücker and Mr. Faraday. He had procured the necessary apparatus with the view of prosecuting the subject himself, but the pressure of other duties prevented him from carrying out his intention. I adopted the suggestion and entered upon the inquiry in M. Knoblauch's cabinet. Our frequent conversations upon the subject led to the idea of our making a joint publication of the results: this we accordingly did in two papers, the first of which, containing a brief account of some of the earliest experiments, appeared in the Philosophical Magazine for March 1850, and some time afterwards in Poggendorff's *Annalen*; while the second and principal memoir appeared in the Philosophical Magazine for July 1850, and in Poggendorff's *Annalen* about January 1851*. I afterwards continued my researches in the private laboratory of Professor Magnus of Berlin, who, with prompt kindness and a lively interest in the furtherance of the inquiry, placed all necessary apparatus at my disposal. The results of this investigation are described in a paper published in the Philosophical Magazine for September 1851, and in Poggendorff's *Annalen*, vol. lxxxiii.

In these memoirs it was shown that the law according to which the axes of positive crystals are attracted and those of negative crystals repelled, was contradicted by the deportment of numerous crystals both positive and negative. It was also proved that the force which determined the position of the optic axes in the magnetic field was not independent of the magnetism or diamagnetism of the mass of the crystal; inasmuch as two crystals, of the same form and structure, exhibited altogether different effects, when one of them was magnetic and the other diamagnetic. It was shown, for example, that pure carbonate of lime was diamagnetic, and always set its optic axis equatorial; but that when a portion of the calcium was replaced by an iso-

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those possessed by the substance itself, or which at least are altogether independent of it."—Treatise on Electricity, vol. i. p. 359.

[*] The memoirs in the Philosophical Magazine were written by me, and the second one has, I believe, been translated into German by Dr. Krönig; the papers in Poggendorff's *Annalen* were edited by my colleague.—J. T.

morphous magnetic constituent, which neither altered the structure nor affected the perfect transparency of the crystal, the optic axis set itself from pole to pole. The various complex phænomena exhibited by crystals in the magnetic field were finally referred to the modification of the magnetic and diamagnetic forces by the peculiarities of molecular arrangement.

This result is in perfect conformity with all that we know experimentally regarding the connexion of matter and force. Indeed it may be safely asserted that every force which makes matter its vehicle of transmission must be influenced by the manner in which the material particles are grouped together. The phænomena of double refraction and polarization illustrate the influence of molecular aggregation upon light. Wertheim has shown that the velocity of sound through wood, *along* the fibre, is about five times its velocity *across* the fibre: De la Rive, DeCandolle and myself have shown the influence of the same molecular grouping upon the propagation of heat. In the first section of the present paper, the influence of the molecular structure of wood upon its magnetic deportment is described: De Senarmont has shown that the structure of crystals endows them with different powers of calorific conduction in different directions: Knoblauch has proved the same to be true, with regard to the transmission of radiant heat: Wiedemann finds the passage of frictional electricity along crystals to be affected by structure; and some experiments, which I have not yet had time to follow out, seem to prove, that bismuth may, by the approximation of its particles, be caused to exhibit, in a greatly increased degree, those singular effects of induction which are so strikingly exhibited by copper, and other metals of high conducting power.

Indeed the mere *à priori* consideration of the subject must render all the effects here referred to extremely probable. Supposing the propagation of the forces to depend upon a subtle agent, distinct from matter, it is evident that the progress of such an agent from particle to particle must be influenced by the manner in which these particles are arranged. If the particles be twice as near each other in one direction as in another, it is certain that the agent of which we speak will not pass with the same facility in both directions. Or supposing the effects to which we have alluded to be produced by motion of some kind, it is just as certain that the propagation of this motion must be affected by the manner in which the particles which transmit it are grouped together. Whether, therefore, we take the old hypothesis of imponderables, or the new, and more philosophic one, of modes of motion, the result is still the same.

If this reasoning be correct, it would follow, that, if the molecular arrangement of a body be changed, such a change will manifest itself by an alteration of deportment towards any force operating upon the body: the action of compressed glass upon light, which Wertheim in his recent researches* has so beautifully turned to account in the estimation of pressures, is an illustration in point; and the inference also receives the fullest corroboration from experiments, some of which are recorded in the papers alluded to, and which show that all the phænomena of magnecrystallic action may be produced by simple mechanical agency. What the crystalline forces do in one case, mechanical force, under the control of the human will, accomplishes in the other. A crystal of carbonate of iron, for example, suspended in the magnetic field, exhibits a certain deportment: the crystal may be removed, pounded into the finest dust, and the particles so put together that the mass shall exhibit the same deportment as before. A bismuth crystal suspended in the magnetic field, with its planes of principal cleavage vertical, will set those planes equatorial; but if the crystalline planes be squeezed sufficiently together by a suitable mechanical force, this deportment is quite changed, and the line which formerly set equatorial now sets axial†.

Thus we find that the influence of crystallization may be perfectly imitated, and even overcome, by simple mechanical agencies. It would of course be perfectly unintelligible were we to speak of any direct action of the magnetic force upon the force by which the powdered carbonate of iron, or the solid cube of bismuth, is compressed; such an idea, however, appears scarcely less tenable than another which seems to be entertained by some who feel an interest in this subject; namely, that there is a direct action of the magnet upon the molecular forces which built the crystal. The function of such forces, as regards the production of the effects, is, I believe, *mediate*; the molecular forces are exerted in placing the particles in position, and the subsequent phænomena, whether exhibited in magnecrystallic action, in the bifurcation and polarization of a luminous ray, or in the modification of any other force transmitted through the crystal, are not due to the action of force upon force, except through the intermediation of the particles referred to‡.

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* Phil. Mag. October and November 1854.

† Phil. Mag. vol. ii. Ser. 4. p. 183.

‡ The influence of molecular aggregation probably manifests itself on a grand scale in nature. The Snowdon range of mountains, for example, is principally composed of slate rock, whose line of strike is nearly north and south. The magnetic properties of this rock I find, by some preliminary experiments, to be very different along the cleavage from what they are across it. I cannot help thinking that these vast masses, in their present position, must exert a different action on the magnetic needle from that which would be exerted if the line of strike were east and west.

The foregoing introductory statement will, perhaps, sufficiently indicate the present aspect of this question. The object I proposed to myself in commencing the inquiry now laid before the Royal Society, was to obtain, if possible, clearer notions of the nature of the diamagnetic force than those now prevalent; for though, in the preceding paragraphs, we have touched upon some of the most complex phænomena of magnetism and diamagnetism, and are able to produce these phænomena at will, the greatest diversity of opinion still prevails as to the real relationship of the two forces. The magnetic force, we know, embraces both attraction and repulsion, thus exhibiting that wonderful dual action which we are accustomed to denote by the term polarity. Mr. Faraday was the first who proposed the hypothesis that diamagnetic bodies, operated on by magnetic forces, possess a polarity "the same in kind as, but the reverse in direction, of that acquired by iron, nickel, and ordinary magnetic bodies under the same circumstances*." M. W. Weber sought to confirm this hypothesis by a series of experiments, wherein the excitement of the supposed diamagnetic polarity was applied to the generation of induced currents—apparently with perfect success. Mr. Faraday afterwards showed, and his results were confirmed by M. Verdet, that effects similar to those described by the distinguished German, were to be attributed, not to the excitement of diamagnetic polarity, but to the generation of ordinary induced currents in the metallic mass. On the question of polarity Mr. Faraday's results were negative, and he therefore, with philosophic caution, holds himself unpledged to his early opinion. M. Weber, however, still retains his belief in the reverse polarity of diamagnetic bodies, whereas Weber's countryman M. von Feilitsch, in a series of memoirs recently published in Poggendorff's *Annalen*, contends that the polarity of diamagnetic bodies is precisely the same as that of magnetic ones. In this unsettled state of the question nothing remained for me but a complete examination of the nature of the diamagnetic force, and a thorough comparison of its phænomena with those of ordinary magnetism. This has been attempted in the following pages: with what success it must be left to the reader to decide.

Before entering upon the principal inquiry, I will introduce one or two points which arose incidentally from the investigation, and which appear to be worth recording.

I. ON THE MAGNETIC PROPERTIES OF WOOD.

No experiments have yet been made, to determine the influence of structure upon the magnetic deportment of this substance;

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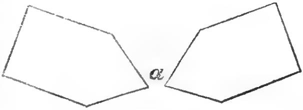
* Experimental Researches, 2429, 2430.

and even on the question whether it is magnetic, like iron, or diamagnetic, like bismuth, differences of opinion appear to prevail. Such differences are to be referred to the extreme feebleness of the force proper to the wood itself, and its consequent liability to be masked by extraneous impurity. In handling the substance intended for experiment the fingers must be kept perfectly clean, and frequent washing is absolutely necessary. After reducing the substance to a regular shape, so as to annul the influence of exterior form, its outer surface must be carefully removed by glass, and the body afterwards suspended by a very fine fibre between the poles of a strong electro-magnet.

The first step in the present inquiry was to ascertain whether the substance examined was paramagnetic* or diamagnetic. It is well known, that, in experiments of this kind, moveable masses of soft iron are placed upon the ends of the electro-magnet, the distance between the masses being varied to suit the experiment. In front of a pointed mass of iron of this kind, a cube of wood was suspended, and if, on exciting the magnet, the cube was repelled by the point, it was regarded as diamagnetic; if attracted, it was considered to be paramagnetic. The force was considerably intensified by placing the two moveable poles as in fig. 1, and suspending the cube at *a* on the same level with the points; a diamagnetic body placed there is, on the development of the magnetic force, forcibly driven *from* the line which unites the points, while a magnetic body is forcibly drawn in between them.

Fig. 1.



Having thus observed the deportment of the mass, the cube was next suspended between the *flat* ends of the poles sketched in fig. 1. The parallel faces were about three-quarters of an inch apart, and in each case the fibre of the suspended wood was horizontal. The specimen first examined was Beef-wood: suspended in the position *a*, fig. 1, the mass was repelled: suspended between the flat poles, on exciting the magnet, the cube, if in an oblique position, turned and set its fibre equatorial.

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* The effects exhibited by iron and by bismuth come properly under the general designation of *magnetic* phænomena: to render their subdivision more distinct Mr. Faraday has recently introduced the word *paramagnetic* to denote the old magnetic effects, of which the action of iron is an example, Wherever the word *magnetic* occurs, without the prefix, it is always the old action that is referred to.

By suitably breaking and closing the circuit, the cube could be turned 180° round and held in this new position. The axial position of the ligneous fibre was one of unstable equilibrium, from which, if it diverged in the slightest degree right or left, the cube turned and finally set its fibre equatorial. The following is a statement of the results obtained with thirty-five different kinds of wood:—

Table I.

Name of wood.	Deportment of mass.	Deportment of structure.	Remarks.
1. Beef-wood	Diamagnetic.	Fibre equatorial.	
2. Black Ebony	"	"	
3. Box-wood	"	"	
4. Second specimen ...	"	"	
5. Brazil-wood	"	"	
6. Braziletto	"	"	Action decided.
7. Bullet-wood	"	"	Action decided.
8. Cam-wood	"	"	
9. Cocoa-wood	"	"	
10. Coromandel-wood ...	"	"	Action strong.
11. Green Ebony	"	"	Action strong.
12. Green-heart	"	"	Action strong.
13. Iron-wood	"	"	
14. King-wood	"	"	Action strong.
15. Locust-wood	"	"	
16. Maple	"	"	Action decided.
17. Lance-wood	"	"	Action decided.
18. Olive-tree	"	"	
19. Peruvian-wood	"	"	Action strong.
20. Prince's-wood	"	"	
21. Camphor-wood	"	"	
22. Sandal-wood	"	"	
23. Satin-wood...........	"	"	
24. Tulip-wood	"	"	
25. Zebra-wood	"	"	
26. Botany Bay Oak ...	"	"	Action strong.
27. Mazatlan-wood	"	"	Action decided.
28. Tamarind-wood	"	"	
29. Sycamore	"	"	Action decided.
30. Beech	"	"	Action decided.
31. Ruby-wood............	"	"	
32. Jacca	"	"	
33. Oak......................	"	"	Action strong.
34. Yew	"	"	Action feeble.
35. Black Oak	Paramagnetic.	"	Action decided.

The term "decided" is here used to express an action more energetic than ordinary, but in no case does the result lack the decision necessary to place it beyond doubt. It must also be remarked that the term "strong" is used in relation to the general deportment of wood; for, compared with the action of many other diamagnetic bodies, the strongest action of wood is but feeble. Simple as the problem may appear, it required considerable time and care to obtain the results here recorded. During a first examination of the cubes eight anomalies presented themselves—in eight cases the fibre set either oblique or axial. The whole thirty-five specimens were carefully rescraped with glass and tested once more; still two remained, which, though repelled as masses, persistently set with the fibre axial, and oscillated round this position so steadily as to lead to the supposition that the real deportment of the substance was thus exhibited. I scraped these cubes ten times successively, and washed them with all care, but the deportment remained unchanged. The cubes, for the sake of reference, had been stamped with diminutive numbers by the maker of them; and I noticed at length, that in these two cases a trace of the figures remained: on removing the whole surface which bore the stamp from each, the cubes forsook the axial position, and set, like the others, with the fibre equatorial.

The influence of the mere *form* of an impurity was here very prettily exhibited. The cubes in question had been stamped (probably by an iron tool) with the numbers 33 and 37, which lay in the line of the fibre; the figures, being dumpy little ones, caused an elongation of the magnetic impurity along the said line, and the natural consequence of this elongation was the deportment above described.

Of the thirty-five specimens examined one proved to be paramagnetic. Now, it may be asked, if the views of molecular action stated in the foregoing pages be correct, how is it that this paramagnetic cube sets its fibre equatorial? The case is instructive. The substance (bog oak) had been evidently steeped in a liquid containing a small quantity of iron in solution, whence it derives its magnetism; but here we have no substitution of paramagnetic molecules for diamagnetic ones, as in the cases referred to. The extraneous magnetic constituent is practically indifferent as to the direction of magnetization, and it therefore accommodates itself to the directive action of the wood to which it is attached.

II. ON THE ROTATION OF BODIES BETWEEN POINTED MAGNETIC POLES.

In his experiments on charcoal, wood-bark and other substances, M. Plücker discovered some very curious phænomena of rotation, which occurred on removing the substance experimented on from one portion of the magnetic field to another. To account for these phænomena, he assumed, that in the substances which exhibited the rotation, two antagonist forces were perpetually active—a repulsive force, which caused the substance to assume one position; and an attractive force, which caused it to assume a different position: that, of these two forces, the repulsive diminished more quickly than the attractive, when the distance of the body from the poles was augmented. Thus, the former, which was predominant close to the poles, succumbed to the latter when a suitable distance was attained, and hence arose the observed rotation.

Towards the conclusion of the memoir published in the September Number of the Philosophical Magazine for 1851, I stated that it was my intention further to examine this highly ingenious theory. I shall now endeavour to fulfil the promise then made, and to state, as briefly as I can, the real law which regulates these complex phænomena.

The masses of soft iron sketched in fig. 1 were placed upon the ends of the electro-magnet, with their points facing each other; between the points the body to be examined was suspended by a fine fibre, which, passing round a groove, the substance could be raised or lowered by turning a milled head. The body was first suspended on the level of the points and its deportment noted, it was then slowly elevated, and the change of position, if any, was observed. It was finally permitted to sink below the points and its deportment there noted also.

The following is a statement of the results; the words 'equatorial' (E.) and 'axial' (A.) imply that the longest horizontal dimension of the substance examined took up the position denoted by each of these words respectively. The manner in which the bars were prepared is explained further on.

Table II.

Name of substance.	Horizontal dimensions.	Deportment of mass.	Position.		
			Between poles.	Above.	Below.
1. Tartaric acid	0·5 ×0·1	Diamagnetic.	E.	A.	A.
2. A second specimen...	0·4 ×0·1	„	E.	A.	A.
3. Red ferrocyanide of potassium	0·6 ×0·1	Paramagnetic.	A.	E.	E.
4. A second prism	0·9 ×0·12	„	A.	E.	E.
5. Citric acid	0·55×0·25	Diamagnetic.	E.	A.	A.
6. A second specimen...	0·48×0·2	„	E.	A.	A.
7. Beryl	0·45×0·1	Paramagnetic.	A.	E.	E.
8. Saltpetre	0·6 ×0·3	Diamagnetic.	E.	A.	A.
9. Nitrate of soda	0·6 ×0·12	„	E.	A.	A.
10. Sulphate of iron	0·7 ×0·15	Paramagnetic.	A.	E.	E.
11. A second specimen...	0·6 ×0·03	„	A.	E.	E.
12. A third specimen ...	1·0 ×0·13	„	A.	E.	E.
13. Calcareous spar	0·5 ×0·1	Diamagnetic.	E.	A.	A.
14. A full crystal.........	....	„	E.	A.	A.
15. Carbonate of iron ...	0·5 ×0·1	Paramagnetic.	A.	E.	E.

(Table II. *continued.*)

Name of substance.	Horizontal dimensions.	Deportment of mass.	Position.		
			Between poles.	Above.	Below.
16. Carbonate of iron powdered and compressed	0·9 ×0·18	Paramagnetic.	A	E.	E
17. Compressed disc	0·8 ×0·08	,,	A	E.	E
18. Bismuth	0·95×0·15	Diamagnetic.	E	A.	A
19. The same compressed	0·7 ×0·05	,,	E	A.	A
20. The same powdered and compressed ...	0·6 ×0·07	,,	E.	A.	A.
21. Cylinder of the same.	1·0 ×0·15	,,	E.	A.	A.
22. Tourmaline	2·1 ×0·1	Paramagnetic.	A.	E.	E.
23. A second specimen ...	1·1 ×0·1	,,	A.	E.	E.
24. A third	0·9 ×0·1	,,	A.	E.	E.
25. Sulphate of nickel ...	0·9 ×0·3	,,	A.	E.	E.
26. A second specimen ...	0·6 ×0·2	,,	A.	E.	E.
27. Heavy spar	0·38×0·18	Diamagnetic.	E.	A.	A.
28. A second specimen ...	0·4 ×0·18	,,	E.	A.	A.
29. Carbonate of tin powdered and compressed	0·34×0·04	,,	E.	A.	A.
30. A second specimen ...	length 6 times width	,,	E.	A.	A.
31. Ammonio-phosphate of magnesia powdered and compressed	0·3 ×0·06	,,	E.	A.	A.
32. A second specimen...	0·5 ×0·07	,,	E.	A.	A.
33. Carbonate of magnesia powdered and compressed	0·45×0·04	,,	E.	A.	A.
34. Sulphate of magnesia	0·32×0·2	,,	E.	A.	A.
35. A second specimen ..	0·25×0·15	,,	E.	A.	A.
36. Flour compressed ...	0·24×0·04	,,	E.	A.	A.
37. Oxalate of cobalt ...	0·6 ×0·08	Paramagnetic.	A.	E.	E.

These experiments might be extended indefinitely, but sufficient are here to enable us to deduce the law of action. In the first place we notice, that all those substances which set equatorial between the points, and axial above and below them, are *diamagnetic*; while all those which set axial between the points, and equatorial above and below them, are *paramagnetic*. When any one of the substances here named is reduced to the spherical form, this rotation is not observed. I possess, for example, four spheres of calcareous spar, and when any one of them is suspended between the points, it takes up a position which is not changed when the sphere is raised or lowered; the crystallographic axis sets equatorial in all positions. A sphere of compressed carbonate of iron, suspended between the points, also sets that diameter along which the pressure is exerted from pole to pole, and continues to do so when raised or lowered. A sphere of compressed bismuth, on the other hand, sets its line of compression always equatorial. The position taken up by the spheres depends upon the *molecular structure* of the substances which compose them; but, when the mass is *elongated*, another action comes into play. Such a mass being suspended with its length horizontal, the *repulsion of its ends* constitutes a mechanical couple which increases in power with the length of the mass; and when the body is long enough, and the local repulsion of the ends strong enough, the couple, when it acts in opposition to the directive tendency due to structure, is able to overcome the latter and to determine the position of the mass. In all the cases cited, it was so arranged that the length of the body and its structure should act in opposition to each other. Tartaric acid and citric acid cleave with facility in one direction, and, in the specimens used, the planes of cleavage were perpendicular to the length of the body. In virtue of the structure these planes tended to set equatorial, but the repulsion of the elongated mass by the points prevented this, and caused the planes to set axial. When, however, the body was raised or lowered out of the sphere of this local repulsion, and into a position where the distribution of the force was more uniform, the advantage due to length became so far diminished that it was overcome, in turn, by the influence of structure, and the planes of cleavage turned into the equatorial position. In the specimen of saltpetre the shortest horizontal dimension was parallel to the axis of the crystal, which axis, when the influence of form is destroyed, always sets equatorial. A full crystal of calcareous spar will, when the magnetic distribution is tolerably uniform, always set its axis at right angles to the line joining the poles; but the axis is the shortest dimension of the crystal, and, between the points, this mechanical disadvantage compels the influence of structure to succumb to the influence of shape. A cube of calcareous spar, in my possession, may be caused to set the optic axis from pole to pole between the points, but this is evidently due to the elongation of the mass along the diagonals; for, when the corner of the cube succeeds in passing the point of the pole, the mass turns its axis with surprising energy into the equatorial position, round which it oscillates with great vivacity. Counting the oscillations, I found that eighty-two were performed by the cube, when its axis was equatorial, in the time required to perform fifty-nine, when the axis stood from pole to pole. Heavy spar and cœlestine are beautiful examples of directive action. These crystals, as is well known, can be cloven into prisms with rhombic bases: the principal cleavage is parallel to the base of the prism, while the two subordinate cleavages constitute the sides. If a short prism be suspended in a tolerably uniform field of force, so that its rhombic ends shall be horizontal, on exciting the magnet the short diagonal will set equatorial, as shown in fig. 2. If the prism be suspended with its axis and the short diagonal horizontal, the long diagonal being therefore vertical, the short diagonal will retain the equatorial position, while the axis of the prism sets axial as in fig. 3. If the prism be suspended with its long diagonal and axis horizontal, the short diagonal being vertical, and its directive power therefore annulled, the axis will take up the equatorial position, as in fig. 4. Now as the line which sets equatorial in diamagnetic bodies is that in which the magnetic repulsion acts most strongly*, the crystal before us furnishes a perfect example of

Fig. 2.

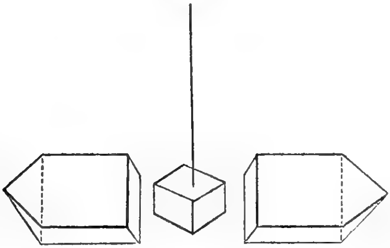


Fig. 3.

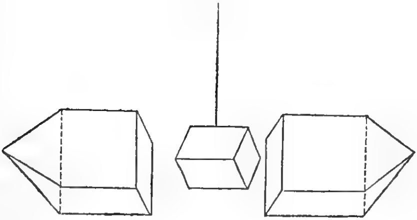
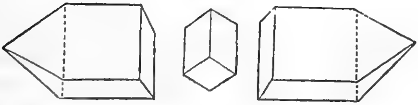


Fig. 4.



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* Phil. Mag. Ser. 4. vol. ii. p. 177.

a substance possessing three rectangular magnetic axes, no two of which are equal. In the experiment cited in Table II., the mass was so cut that the short diagonal of the rhombic base was perpendicular to the length of the specimen. Carbonate of tin, and the other powders, were compressed by placing the powder between two clean plates of copper, and squeezing them together in a strong vice. The line of compression in diamagnetic bodies always sets equatorial, when the field of force is uniform, or approximately so; but between points the repulsion of the ends furnishes a couple strong enough to overcome this directive action, causing the longest dimension of the mass to set equatorial, and consequently its line of compression axial.

The antithesis between the deportment of diamagnetic bodies and of paramagnetic ones is perfect. Between the points the former class set equatorial, the latter axial. Raised or lowered, the former set axial, the latter equatorial. The simple substitution of an attractive for a repulsive force produces this effect. A sphere of ferrocyanide of potassium, for example, always sets the line perpendicular to the crystallographic axis from pole to pole; but when we take a full crystal, whose dimension along its axis, as in one of the cases before us, is six times the dimension at right angles to the axis, the attraction of the ends of such a mass is sufficient to overcome the directive action due to structure, and to pull the crystal into the axial position between the points. In a field of uniform force, or between flat poles, the length sets equatorial, and it is the partial attainment of such a field, at a distance from the points, that causes the crystal to turn from axial to equatorial when it is raised or lowered. Beryl is a paramagnetic crystal, and when the influence of form is annulled, it always sets a line perpendicular to the axis of the crystal from pole to pole; a cube of this crystal, at present in my possession, shows this deportment whether the poles are pointed or flat; but in the specimen examined the dimension of the crystal along its axis was greatest, and hence the deportment described. It is needless to dwell upon each particular paramagnetic body: the same principle was observed in the preparation and choice of all of them; namely, that the line which, in virtue of the internal structure of the substance, would set axial, was transverse to the length of the body. The directive action due to structure was thus brought into opposition with the tendency of magnetic bodies to set their longest dimension from pole to pole: between the points the latter tendency was triumphant; at a distance, on the contrary, the influence of structure prevailed. The substance which possesses this directive action in the highest degree is carbonate of iron: when a lozenge, cloven from the crystalline mass, is suspended from the angle at which the crystallographic axis issues, there is great difficulty in causing the plate to set axial. If the points are near, on exciting the magnetism the whole mass springs to one or the other of the points; and when the points are distant, the plate, although its length may be twenty times its thickness, will set strongly equatorial. An excitation by one cell was sufficient to produce this result. In the experiment cited the residual magnetism was found to answer best, as it permitted the ends of the plate to be brought so near to the points that the mass was pulled into the axial position. When the magnet was more strongly excited, and the plate raised so far above the points as to prevent its springing to either of them, it was most interesting to watch the struggle of the two opposing tendencies. Neither the axial nor the equatorial position could be retained; the plate would wrench itself spasmodically from one position into the other, and, like the human spirit operated on by conflicting passions, find rest nowhere.

The conditions which determine the curious effects described in the present chapter may be briefly expressed as follows :—

An elongated diamagnetic body being suspended in the magnetic field, if the shortest horizontal dimension tend, in virtue of the internal structure of the substance, to set equatorial, it is opposed by the tendency of the longest dimension to take up the same position. Between the pointed poles the influence of length usually predominates; above the points and below them the directive action due to structure prevails.

Hence, the rotation of such a diamagnetic body, on being raised or lowered, is always from the equatorial to the axial position.

If the elongated mass be magnetic, and the shortest dimension of the mass tend, in virtue of its structure, to set from pole to pole, it is opposed by the tendency of the longest dimension to take up the same position. Between the points the influence of length is paramount; above and below the points the influence of structure prevails.

Hence, the rotation of magnetic bodies, on being raised or lowered, is always from the axial to the equatorial position.

The error of the explanation which referred many of the above actions to the presence of two conflicting forces, one of which diminished with the distance in a quicker ratio than the other, lies in the supposition, that the assuming of the axial position proved a body to be magnetic, while the assuming of the equatorial position proved a body to be diamagnetic. This assumption was perfectly natural in the early stages of diamagnetic research, when the modification of magnetic force by structure was unknown. Experience however proves that the total mass of a magnetic body continues to be attracted after it has assumed the equatorial position, while the total mass of a diamagnetic body continues to be repelled after it has taken up the axial one.

III. ON THE DISTRIBUTION OF THE MAGNETIC FORCE BETWEEN TWO FLAT POLES.

In experiments where a uniform distribution of the magnetic force is desirable, flat poles, or magnetized surfaces, have been recommended. It has long been known that the force proceeds with great energy from the edges of such poles: the increase of force from the centre to the edge has been made the subject of a special investigation by M. von Kolke*. The central portion of the magnetic field, or space between two such magnetized surfaces, has hitherto been regarded as almost perfectly uniform, and indeed for all ordinary experiments the uniformity is sufficient. But, when we examine the field carefully, we find that the uniformity is not perfect. Substituting, for the sake of convenience, the edge of a pole for a point, I studied the phænomena of rotation described in the last section, in a great number of instances, by comparing the deportment of an elongated body, suspended in the centre of the space between two flat poles, with its deportment when suspended between the top or the bottom edges. Having found that the fibre of wood, in masses where form had no influence, always set equatorial, I proposed to set this tendency to contend with an elongation of the mass in a direction at right angles to the fibre. For this purpose thirty-one little wooden bars were carefully prepared and examined, the length of each bar being about twice its width, and the fibre coinciding with the latter dimension. The bars were suspended from an extremely fine fibre of cocoon silk, and in the centre of the magnetic field each one of them set its length axial and consequently its fibre equatorial. Between the top and bottom edges, on the contrary, each piece set its longest dimension equatorial, and, consequently, the fibre axial.

For some time I referred the axial setting of the mass, in the centre of the field, to the directive action of the fibre, though, knowing the extreme feebleness of this directive action, I was surprised to find it able to accomplish what the experiments exhibited. The thought suggested itself, however, of suspending the bars with the fibre vertical, in which position the latter could have no directive influence. Here also, to my surprise, the directive action, though slightly weakened, was the same as before: in the centre of the field the bars took up the axial position. Bars of sulphur, wax, salt of hartshorn, and other

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* Poggendorff's *Annalen*, vol. lxxxi. p. 321.

diamagnetic substances were next examined: they all acted in the same manner as the wood, and thus showed that the cause of the rotation lay, not in the structure of the substances, but in the distribution of the magnetic force around them. This distribution in fact was such, that the straight line which connected the centre of one pole with that of the opposite one was the line of weakest force. Ohm represents the distribution of electricity upon the surfaces of conductors by regarding the tensions as ordinates, and erecting them from the points to which they correspond, the steepness of the curve formed by uniting the ends of the ordinates being the measure of the increase or diminution of tension. Taking the centre of the magnetic field as the origin, and drawing lines axial and equatorial; if we erect the magnetic tensions along these lines, we shall find a steeper curve in the equatorial than in the axial direction. This may be proved by suspending a bit of carbonate of iron in the centre of the magnetic field; on exciting the magnet, the suspended body will not move to the nearest portion of the flat pole, though it may be not more than a quarter of an inch distant, but will move equatorially towards the edges, though they may be two inches distant. The little diamagnetic bars referred to were therefore pushed into the axial position by the force acting with superior power in an equatorial direction.

The results just described are simply due to the recession of the ends of an elongated body from places of stronger to those of weaker force; but it is extremely instructive to observe how this result is modified by structure. If, for example, a plate of bismuth be suspended between the poles with the plane of principal cleavage vertical, the plate will assert the equatorial position from top to bottom; and in the centre with almost the same force as between the edges. The cause of this lies in the structure of the bismuth. Its position in the field depends not so much upon the distribution of the magnetic force around it, as upon the direction of the force *through* it. I will not, however, anticipate matters by entering further upon this subject at present.

IV. COMPARATIVE VIEW OF PARAMAGNETIC AND DIAMAGNETIC PHÆNOMENA.

1. *State of Diamagnetic Bodies under Magnetic Influence.*

When a piece of soft iron is brought near to a magnet, it is attracted by the latter: this attraction is not the act of the magnet alone, but results from the mutual action of the magnet and the body upon which it operates. The soft iron in this case is said to be magnetized by influence; it becomes itself a magnet, and the intensity of its magnetization varies with the strength of the influencing magnet. Poisson figured the act of magnetization as consisting of the decomposition of a neutral magnetic fluid into north and south magnetism, the amount of the decomposition being proportional to the strength of the magnet which produces it. Ampère, discarding the notion of magnetic fluids, figured the molecules of soft iron as surrounded by currents of electricity, and conceived the act of magnetization to consist in setting the planes of these molecular currents parallel to each other: the degree of parallelism, or in other words, the intensity of the magnetization, depending, as in Poisson's hypothesis, upon the strength of the influencing magnet.

The state into which the soft iron is here supposed to be thrown is a state of constraint, and when the magnet is removed, the substance returns to its normal condition. Poisson's separated fluids rush together once more, and Ampère's molecular currents return to their former irregular positions. As our knowledge increases, we shall probably find both hypotheses inadequate to represent the phænomena; the only thing certain is, that the soft iron, when acted upon by the magnet, is thrown into an unusual condition, in virtue of which it is attracted; and that the intensity of this condition is a function of the force which produces it.

There are, however, certain bodies which, unlike soft iron, offer a great resistance to the imposition of the magnetic state, but when once they are magnetized they do not, on the removal of the magnet, return to their neutral condition, but on the contrary retain the magnetism impressed on them. It is in virtue of this quality that steel can be formed into compass needles and permanent magnets. This power of resistance and retention is named by Poisson coercive force.

Let us conceive a body already magnetized, and in which coercive force exists in a very high degree—a piece of very hard steel for example—to be brought near a magnet, the strength of which is not sufficient to magnetize the steel further. To simplify the matter let us fix our attention upon the south pole of the magnet, and conceive it to act upon the north pole of the piece of steel. Let the magnetism of the said south pole, referred to any unit, be M, and of the north pole of the steel, M′; then their mutual attraction, at the unit of distance, is expressed by the product MM′. Conceive now the magnet to increase in power from M to nM, the steel being still supposed hard enough to resist magnetization by influence; the mutual attraction now will be

$$nMM',$$

or n times the former attraction; hence when a variable magnetic pole acts on an opposite one of constant power, the attraction is proportional to the strength of the former.

Let us now take a body whose magnetization varies with that of the magnet: a south pole of the strength M induces in such a body a north pole of the strength M′, and the attraction which results from their mutual action is

$$MM'.$$

Let the strength of the influencing south pole increase from M to nM; then, assuming the magnetism of the body under influence to increase in the same ratio, the strength of the above-mentioned north pole will become nM′, and the attraction, expressed by the product of both, will be

$$n^2MM';$$

that is to say, the attraction of a body magnetized by influence, and whose magnetism varies as the strength of the influencing magnet, is proportional to the *square of the strength* of the latter.

Here then is a mark of distinction between those bodies which have their power of exhibiting magnetic phænomena conferred upon them by the magnet, and those whose actions are dependent upon some constant property of the mass: in the latter case the resultant action will be simply proportional to the strength of the magnet, while in the former case a different law of action will be observed*.

The examination of this point lies at the very foundation of our inquiries into the nature of the diamagnetic force. Is the repulsion of diamagnetic bodies dependent merely on the mass considered as ordinary matter, or is it due to some condition impressed upon the mass by the influencing magnet? This question admits of the most complete answer either by comparing the increase of repulsion with the increase of power in the magnet which produces the repulsion, or by comparing the attraction of a paramagnetic body, which we know to be thrown into an unusual condition, with the repulsion of a diamagnetic body, whose condition we would ascertain.

Bars of iron and bismuth, of the same dimensions, were submitted to the action of an electro-magnet, which was caused gradually to increase in power; commencing with an excitation by one cell, and proceeding up to an excitation by ten or fifteen. The strength of the current was in each case accurately measured by a tangent galvanometer. The bismuth bar was suspended between the two flat poles, and, when the magnet was excited,

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* This test was first pointed out in a paper on the Polarity of Bismuth, Phil. Mag. Nov. 1851, p. 333. I have reason, however, to know that the same thought occurred to M. Poggendorff previous to the publication of my paper.—J. T.

took up the equatorial position. The iron bar, if placed directly between the poles, would, on the excitation of the magnetism, infallibly spring to one of them; hence it was removed to a distance of 2 feet 7 inches from the centre of the space between the poles, and in a direction at right angles to the line which united them. The magnet being excited, the bar was drawn a little aside from its position of equilibrium and then liberated, a series of oscillations of very small amplitude followed, and the number of oscillations accomplished in a minute was carefully ascertained. Tables III. and IV. contain the results of experiments made in the manner described with bars of iron and bismuth of the same dimensions.

Table III.

Bar of soft iron, No. 1.
length 0·8 of an inch.
width 0·13 of an inch.
depth 0·15 of an inch.

Strength of current.	Attraction.
168	168^2
214	204^2
248	253^2
274	275^2
323	313^2
362	347^2
385	374^2
411	385^2

Table IV.

Bar of bismuth, No. 1.
length 0·8 of an inch.
width 0·13 of an inch.
depth 0·15 of an inch.

Strength of current.	Repulsion.
78	78^2
136	135^2
184	191^2
226	226^2
259	259^2
287	291^2
341	322^2
377	359^2
411	386^2

These experiments prove, that, up to a strength of about 280, the attractive force operating upon the iron, and the repulsive force acting upon the bismuth, are each accurately proportional to the square of the strength of the magnetizing current. For higher powers, both attraction and repulsion increase in a smaller ratio; but it is here sufficient to show that the diamagnetic repulsion follows precisely the same law as the magnetic attraction. So accurately indeed is this parallelism observed, that while the forces at the top of the tables produce attractions and repulsions exactly equal to the square of the strength of the current, the same strength of 411, at the bottom of both tables, produces in iron an attraction of 385^2, and in bismuth a repulsion of 386^2. The numbers which indicate the strength of current in the first column are the tangents of the deflections observed in each case: neglecting the indices, the figures in the second column express the number of oscillations accomplished in a minute, multiplied by a constant factor to facilitate comparison: the forces operating upon the bars being proportional to the squares of the number of oscillations, the simple addition of the index figure completes the expression of these forces.

In these experiments the bismuth bar set *across* the lines of magnetic force, while the bar of iron set *along* them; the former was so cut from the crystalline mass, that the plane of principal cleavage was parallel to the length of the bar, and in the experiments hung vertical. I thought it interesting to examine the deportment of a bar of bismuth which should occupy the same position, with regard to the lines of force, as the bar of iron; that is to say, which should set its length axial. Such a bar is obtained when the planes of principal cleavage are transverse to the length.

Table V.

Bar of bismuth, No. 2.

Length 0·8 of an inch; width 0·13 of an inch; depth 0·15 of an inch.

Set axial between the excited poles.

Strength of current.	Repulsion.
68	67^2
182	187^2
218	218^2
248	249^2
274	273^2
315	309^2
364	350^2
401	366^2

A deportment exactly similar to that exhibited in the foregoing cases is observed here also: up to about 280 the repulsions are accurately proportional to the squares of the current strengths, and from this point forward they increase in a less ratio.

A paramagnetic substance was next examined which set its length at right angles to the lines of magnetic force: the substance was carbonate of iron. The native crystallized mineral was reduced to powder in a mortar, and the powder was compressed. It was suspended, like the bismuth, between the flat poles, with its line of compression horizontal. When these poles were excited, the compressed bar set the line of pressure from pole to pole, and consequently its length equatorial.

Table VI.

Bar of compressed carbonate of iron.

Length 0·95 of an inch; width 0·17 of an inch; depth 0·23 of an inch.

Set equatorial between the excited poles.

Strength of current.	Attraction.
74	74^2
135	133^2
179	180^2
214	218^2
249	248^2
277	280^2
341	330^2
381	353^2

It is needless to remark upon the perfect similarity of deportment here exhibited to the cases previously recorded.

In the following instances the same law of increase is observable.

Table VII.

Sulphate of iron, No. 1.
length 0·75 of an inch.
width 0·22 of an inch.
depth 0·27 of an inch.

Set axial between the excited poles.

Strength of current.	Attraction.
71	70^2
132	133^2
217	220^2
280	275^2
328	333^2
359	348^2

Table VIII.

Sulphate of iron, No. 2.
length 0·75 of an inch.
width 0·22 of an inch.
depth 0·27 of an inch.

Set equatorial between the excited poles.

Strength of current.	Attraction.
70	68^2
121	123^2
203	207^2
271	268^2
331	308^2
370	334^2

In sulphate of iron there is one direction, which, in virtue of the molecular structure of the substance, sets strongly from pole to pole. The bar No. 1 was so cut that this direction was parallel to its length, which therefore set axial; while No. 2 had the same direction *across* it, thus causing the length of the bar to set equatorial.

Two comparative series were finally made with two prisms of iron and of bismuth, more massive than those previously examined.

Table IX.

Bar of iron, No. 2.
length 1·0 inch.
width 0·3 inch.
depth 0·3 inch.

Strength of current.	Attraction.
70	71^2
122	122^2
167	168^2
206	204^2
268	260^2
322	311^2
356	339^2

Table X.

Bar of bismuth, No. 3.
length 1·0 inch.
width 0·3 inch.
depth 0·3 inch.

Strength of current.	Repulsion.
70	72^2
126	121^2
164	166^2
206	205^2
246	248^2
276	279^2
364	344^2

These experiments can leave little doubt upon the mind, that if a magnetic body be attracted in virtue of its being converted into a magnet, a diamagnetic body is repelled *in virtue of its being converted into a diamagnet.* On no other assumption can it be explained, why the repulsion of the diamagnetic body, like the attraction of the magnetic one, increases in a so much quicker ratio than the force of the magnet which produces the repulsion. But, as this is a point of great importance, I will here introduce corroborative evidence, derived from modes of experiment totally different from the method already described. By a series of measurements with the torsion balance, in which the attractive and repulsive forces were determined directly, with the utmost care, the relation of the strength of the magnet to the force acting upon the substances named in Tables XI., XII., and XIII. was found to be as follows:—

Table XI.

Spheres of native sulphur.

Strength of magnet.	Ratio of repulsions.
96	95^2
153	158^2
222	224^2
265	264^2
316	316^2

Table XII.

Spheres of carbonate of lime.

Strength of magnet.	Ratio of repulsions.
134	134^2
172	173^2
213	212^2
259	264^2
310	311^2
370	374^2

Table XIII.

Spheres of carbonate of iron.

Strength of magnet.	Ratio of attractions.
66	66^2
89	89^2
114	114^2
141	141^2

In confirmation of these results I will cite a series obtained by M. E. Becquerel*, whose experiments first showed that the repulsion of diamagnetic bodies follows the same law as the attraction of magnetic ones.

Bar of sulphur.

Length 25 millims.; weight 840 milligrms.

Squares of the magnetic intensities.	Quotients of the repulsions by the magnetic intensities.
36·58	0·902
27·60	0·929
26·84	0·906
16·33	0·920

The constancy of the quotient in the second column proves that the ratio of the repulsions to the squares of the magnetic intensities is a ratio of equality.

I will also cite a series of experiments by Mr. Joule†, which he adduces in confirmation of the results obtained by M. E. Becquerel and myself.

Bar of bismuth.

Strength of magnet.	Repulsions.
1	1^2
2	2^2
4	4^2

Let us contrast these with the results obtained by the same gentleman, by permitting the magnet to act upon a hard magnetic needle.

Magnetic needle.
Length 1·5 of an inch.

Strength of magnet.	Attraction.
1	1
2	2
4	4

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[*] *Annales de Chimie et de Physique,* 3rd series, vol. xxviii. p. 302.

[†] Phil. Mag. 4th series, vol. iii. p. 32.

Here we find experiment in strict accordance with the theoretical deduction stated at the commencement of the present chapter. The intensity of the magnetism of the steel needle is constant, for the steel resists magnetization by influence; the consequence is that the attraction is simply proportional to the strength of the magnet.

A consideration of the evidence thus adduced from independent sources, and obtained by different methods, must, I imagine, render the conclusion certain that diamagnetic bodies, like magnetic ones, exhibit their phænomena in virtue of a state of magnetization induced in them by the influencing magnet. This conclusion is in no way invalidated by the recent researches of M. Plücker, on the law of induction in paramagnetic and diamagnetic bodies, but, on the contrary, derives support from his experiments. With current strengths which stand in the ratio of 1 : 2, M. Plücker finds the repulsion of bismuth to be as 1 : 3·62, which, though it falls short of the ratio of 1 : 4, as the law of increase according to the square of the current would have it, is sufficient to show that the bismuth was not passive, but acted the part of an induced diamagnet in the experiments. In the case of the soft iron itself, M. Plücker finds a far greater divergence; for here currents which stand in the ratio of 1 : 2 produce attractions only in the ratio of 1 : 2·76.

2. *Duality of Diamagnetic Excitement.*

Having thus safely established the fact that diamagnetic bodies are repelled, in virtue of a certain state into which they are cast by the influencing magnet, the next step of our inquiry is;—Will the state evoked by one magnetic pole facilitate, or prevent, the repulsion of the diamagnetic body by a second pole of an opposite quality? If the force of repulsion were an action on the mass, considered as ordinary matter, this mass, being repelled by both the north and the south pole of a magnet, when they operate upon it separately, ought to be repelled by the sum of the forces of the two poles where they act upon it together. But if the excitation of diamagnetic bodies be of a *dual* nature, as is the case with the magnetic bodies, then it may be expected that the state excited by one pole will not facilitate, but on the contrary prevent, the repulsion of the mass by a second opposite pole.

To solve this question the apparatus sketched in fig. 5*a*. Plate IV. was made use of. AB and CD are two helices of copper wire 12 inches long, of 2 inches internal, and of 5½ inches external diameter. Into them fit soft iron cores 2 inches thick: the cores are bent as in the figure, and reduced to flat surfaces along the line *ef*, so that when the two semicylindrical ends are placed together, they constitute a cylinder of the same diameter as the cores within the helices*. In front of these poles a bar of pure bismuth *gh* was suspended by cocoon silk; by imparting a little torsion to the fibre, the end of the bar was caused to press gently against a plate of glass *ik*, which stood between it and the magnets. By means of a current reverser the polarity of one of the cores could be changed at pleasure; thus it was in the experimenter's power to excite the cores, so that the poles PP′ should be of the same quality, or of opposite qualities.

The bar, being held in contact with the glass by a very feeble torsion, a current was sent round the cores, so that they presented two poles of the same name to the suspended bismuth; the latter was promptly repelled, and receded to the position dotted in the figure. On interrupting the current it returned to the glass as before. The cores were next excited, so that two poles of opposite qualities acted upon the bismuth; the latter remained perfectly unmoved†.

This experiment shows that the state, whatever it may be, into which bismuth is cast by one pole, so far from being favourable to the action of the opposite pole, completely neutralizes the effect of the latter. A perfect analogy is thus established between the deportment of the bismuth and that of soft iron under the same circumstances; for it is well known that a similar neutralization occurs in the latter case. If the repulsion depended upon the *abstract strength* of the poles, without reference to their *quality*, the repulsion, when the poles are of opposite names, ought to be *greater* than when they are alike; for in the former case the poles are greatly strengthened by their mutual inductive action, while, in the latter case, they are enfeebled by the same cause. But the fact of the repulsion being dependent on the quality of the pole, demonstrates that the substance is capable of assuming a condition peculiar to each pole, or in other words, is capable of a *dual* excitation‡. The experiments from which

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* The ends of the semicylinders were turned so as to present the blunted apex of a cone to the mass of bismuth.

† A shorter bar of bismuth than that here sketched, with a light index attached to it, makes the repulsion more evident. It may be thus rendered visible throughout a large lecture-room.

‡ Since the above was written, the opinion has been expressed to me, that the action of the *unlike* poles, in the experiment before us, is "diverted" from the bismuth upon each other, the absence of repulsion being due to this diversion, and not to the neutralization of inductions in the mass of the bismuth itself. Many, however, will be influenced by the argument as stated in the text, who would not accept the interpretation referred to in this note; I therefore let the argument stand, and hope at no distant day to return to the subject.—J. T., May 5, 1855.

these conclusions are drawn are a manifest corroboration of those made by M. Reich with steel magnets.

If we suppose the flat surfaces of the two semicylinders which constitute the ends of the cores to be in contact, and the cores so excited that the poles P and P′ are of different qualities, the arrangement, it is evident, forms a true electro-magnet of the horseshoe form; and here the pertinency of a remark made by M. Poggendorff, with his usual clearness of perception, becomes manifest; namely, that if the repulsion of diamagnetic bodies be an indifferent one of the mass merely, there is no reason why they should not be repelled by the centre of a magnet, as well as by its ends.

[To be continued.]

XXI. *On the Function of Salt in Agriculture.* *By* A. Beauchamp Northcote, *Esq., Senior Assistant in the Royal College of Chemistry**.

THE employment of salt in agriculture has been of late years so much extended, that the question of the advantages derived from its use, which formerly gave rise to so many discussions, can no longer be raised. Such an accumulation of facts with regard to its action has now taken place, that it is only necessary to pass judgement upon the evidence recorded; and as the practical results which have been obtained by its judicious application have been all more or less of a beneficial tendency, the verdict given cannot fail of being in its favour. It seems to be an incontrovertible fact, that the application of salt to certain lands does increase their fertility, and improve the character of the crops grown upon them; and if this is the case, it is most desirable that we should have as clear an idea as possible of the *rationale* of its action. I do not in the present paper profess to supply this explanation, although I hope that the experiments which I am about to detail may serve to throw some light upon a somewhat obscure subject.

It has long been held that the beneficial action which salt exerts upon soils is due to a power which it possesses of fixing ammonia, and with this view it has frequently been spread over the surface of dung-heaps, or other organic matters decomposing into manures (sometimes also being mixed up with them), with the view of preventing the escape of the ammonia produced in the course of eremacausis: in some cases success has attended these trials, and in some, failure. The question of its absorption

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* Communicated by the Author.

of the ammonia eliminated during these changes, is a matter, the investigation of which must necessarily be attended with great difficulty; but with respect to its capability of such absorption, I am informed by my friend Mr. Prideaux, that he has habitually observed that salt mixed with guano retards the exhalation of ammonia from the latter; and he has directed my attention also to some experiments* conducted by M. Barral, one of which proves this point in a remarkable degree. M. Barral exposed to the air for fifteen days, equal weights of guano, and of guano previously mixed with half its weight of salt: the amount of nitrogen in each being determined at the end of that time, he found that the pure guano had lost 11·6 per cent. of its nitrogen, whilst that mixed with salt had lost only 5 per cent.

Before entering, however, upon the experiments which I have made upon this subject, and which I may here state have exclusive reference to the absorption of ammonia, I will give the analyses and table of constituents calculated from them, of three specimens of agricultural salt which I have examined†.

Table of direct results of Analysis calculated to 100 parts of the dry salt.

	No. I.		No. II.		No. III.	
Portion soluble in water... contains—		98·174		99·520		97·803
Soda	49·351		51·841		50·720	
Lime	1·711		·493		·593	
Magnesia	·084		·112		·111	
Chlorine	54·824		58·928		57·217	
Sulphuric acid	4·521		1·424		2·046	
Silica	trace		trace		trace	
Portion insoluble in water contains—		1·826		·480		2·197
Lime	·064		trace		·076	
Magnesia	·038		·013		·037	
Alumina	·026		·017		·472	
Sesquioxide of iron	·161		·116		·319	
Phosphoric acid............	trace		trace		trace	
Silica	·792		·190		1·044	
Carbonic acid	·091		·014		·099	
Organic matter	·654		·130		·150	
		100·000		100·000		100·000

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* Edinburgh Quarterly Journal of Agriculture, April 1855.

† The three specimens of which the analyses are given were all produced from the Worcestershire brine-springs; No. I. being from Mr. Corbett's works at Stoke Prior; Nos. II. and III. from Messrs. Clay and Newman's and Mr. Noak's works at Droitwich, respectively.

Table of Constituents calculated to 100 parts of the dry salt.

	No. I.	No. II.	No. III.
Sulphate of lime ...	4·155	1·197	1·440
Sulphate of magnesia	·252	·336	·330
Sulphate of soda ...	3·388	·880	1·734
Chloride of sodium ...	90·342	97·106	94·287
Soluble in water ...	98·137	99·519	97·791
By experiment...	98·174	99·520	97·803
Carbonate of lime ...	·114	trace	·135
Carbonate of magnesia	·079	·027	·077
Phosphate of alumina	trace	trace	trace
Alumina	·026	·017	·472
Sesquioxide of iron	·161	·116	·319
Silica	·792	·190	1·044
Organic matter	·654	·130	·150
Insoluble in water	1·826	·480	2·197
Total	99·963	99·999	99·988

Since the ammonia which occurs in nature, whether eliminated directly from decaying organic matters, or existing as atmospheric ammonia, is invariably in the immediate presence of an enormous excess of carbonic acid, we may safely conclude that it is always in the state of carbonate; and as, on the one hand, it is brought down to the earth from the atmospheric regions dissolved in rain or dew, and, on the other, when met with in the salt itself, is usually in the presence of abundance of moisture, we may infer that the carbonate of ammonia is presented to absorbent agents in the soil for the most part in a state of solution. I do not think, therefore, that any great departure from the natural course of things can be alleged against the following experiments, which have been made with a solution of the commercial so-called sesquicarbonate of ammonia, containing 4·3 per cent. of ammonia (NH^4O).

Taking, then, these specimens of agricultural salt in the state in which they are supplied at the salt works, the first thing was to ascertain whether any absorption occurred upon mixing them in that condition with this solution of carbonate of ammonia. Quantities of from 2 to 3 ozs. of the salts were placed in wide-mouthed bottles, and small quantities of the standard ammonia solution added, containing an absolute amount of ammonia (NH^4O) varying from ·15 to ·04 of a grain. The contents of the bottle were shaken together, and an extremely delicate red litmus-paper inserted; it was completely blued in a minute or two; and as the same action took place after allowing the salt and ammonia solution to remain all night in contact, the fact that no absorption had occurred was considered to be clearly proved. Water was then added in quantity just sufficient to moisten the salt, and upon the re-insertion of the test-paper, a diminished blueing showed that the ammonia was being absorbed*; water was again added, yet in insufficient quantity to dissolve the whole of the salt, and the action upon the litmus was gone. In this way, by alternating the addition of the ammonia and the solution of the salt, the presence of the ammonia might be rendered more or less distinct, or unrecognizable. The reality of the absorption was thus rendered undeniable; and it was equally evident that the soluble portion of the salt contained the absorbing agents, since the disappearance of the ammonia bore a direct proportion to the completeness of their solution. It then became a question as to which of the constituents of the soluble portion this agency was due.

Now the chloride of sodium being the largest ingredient, it was desirable at once to ascertain the part which it played in the matter: a saturated solution of the pure substance was therefore prepared, measured quantities of it taken, placed in bottles, and shaken with successive portions of carbonate of ammonia solution until a decidedly blue tinge was produced in the litmus-paper, which, for the purpose of having a standard test, was left enclosed in the atmosphere within the bottle for the space of five minutes. As the amount of ammonia added approached the maximum, its absorption became correspondingly slower, and it was necessary to leave it in contact with the salt solution for periods of from one to twelve hours; a point was, however, always reached at which a certain tinge of colour was communicated to the paper, the intensity of which was not lessened if the test was applied again after a lapse of several days.

The following are the results which I have obtained in experimenting with a saturated solution of pure chloride of sodium in the above manner:—

Experiment.	100 grains of saturated solution of pure chloride of sodium will absorb	Hence 100 grains of solid chloride of sodium will absorb
I.	·0324 grain of NH^4O.	·1222 grain of NH^4O.
II.	·0233 do.	·0881 do.
III.	·0292 do.	·1097 do.
Mean	·0283 do.	·1066 do.

These numbers require a slight correction to make them absolutely true expressions for the absorbing power of chloride of

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* With regard to the delicacy of the litmus-paper here employed, I may state that a quantity of the ammonia solution containing ·04 of a grain of NH^4O, being placed in an empty bottle, the paper was intensely blued almost immediately after its insertion.

sodium, because, in addition to the trifling excess unavoidably added in order to obtain the blue tint on the litmus-paper (which is so slight that it need not, and indeed could not, be regarded), the water in which the salt is dissolved exercises a certain amount of absorption: this is not, however, much; the quantity of water contained in the above 100 grains of saturated solution masking ·0046 of a grain of ammonia (NH^4O), which would make the true amount absorbed by 100 grains of salt solution, ·0237 of a grain; and that absorbed by 100 grains of solid salt, ·0893 of a grain. This, however, will not affect the subsequent experiments.

But the amount of ammonia absorbed by this constituent, although really very considerable, and of great importance in an agricultural point of view, did not yet correspond to the apparent absorbing power possessed by the specimens of agricultural salt. The following comparative experiments were therefore made with the view of ascertaining the absolute amount of ammonia which these samples absorbed. The solutions used for these experiments were prepared by digesting great excess of the salt with cold water for some time, so as to approximate the manner of making the solution as closely as possible to that which would occur in nature. The experiments themselves were varied by diluting both the ammonia and salt solutions, without any very great difference of result.

	Agricultural Salt, No. I.	
Experiment.	100 grains of the solution of this salt will absorb	Hence 100 grains of the solid salt (contained in the aqueous solution) will absorb
I.	·1479 grain of NH^4O.	·6207 grain of NH^4O.
II.	·1517 do.	·6361 do.
III.	·1517 do.	·6361 do.
Mean	·1504 do.	·6309 do.
	Agricultural Salt, No. II.	
I.	·1304 grain of NH^4O.	·4918 grain of NH^4O.
II.	·1349 do.	·5049 do.
III.	·1349 do.	·5049 do.
Mean	·1334 do.	·5005 do.
	Agricultural Salt, No. III.	
I.	·1486 grain of NH^4O.	·5533 grain of NH^4O.
II.	·1527 do.	·5685 do.
III.	·1543 do.	·5746 do.
Mean	·1518 do.	·5654 do.

Now as soon as the solution of carbonate of ammonia is dropped into these salt solutions, there occurs (as might have been anticipated) a precipitation of the sulphate of lime which they contain as carbonate; this must be attended with a corresponding fixing of the ammonia, the latter combining with the sulphuric acid to which the lime was previously united; and by this reaction, the immense difference observed between the absorbing power of these solutions and that of a solution of pure chloride of sodium would appear to be accounted for. Whether, then, by its own immediate agency, as in the case of manuring with gypsum, or by the intervention of the absorbent power of the chloride of sodium, the soluble lime-salt present is really the most powerful agent in the absorptive process—a fact which was still further demonstrated by the addition of a few drops of solution of chloride of calcium to some of the pure chloride of sodium solutions, to which, in the previous experiments, the maximum of ammonia solution had been added,—instant absorption, and consequently diminished action on the test-paper, was of course the result.

The absorptive power of the specimens examined ought therefore, if so dependent upon the amount of lime-salt, to be almost in the ratio in which that exists in their soluble portion. The subjoined Table will, however, show a discrepancy in this particular.

Composition of the portion soluble in water, calculated to 100 parts.

	No. I.	No. II.	No. III.
Sulphate of lime	4·234	1·203	1·474
Sulphate of magnesia	·256	·338	·336
Sulphate of soda	3·452	·884	1·773
Chloride of sodium	92·058	97·575	96·417
	100·000	100·000	100·000
Ammonia absorbed by 100 parts of the solid salt	·6309	·5005	·5654

If, now, we compare the quantity of sulphate of lime which these salts respectively contain with the amount of ammonia which they are capable of absorbing, we find no such proportion as might have been expected. This arises from the extreme insolubility of the sulphate of lime compared with the solubilities of the other constituents of the salt; and fresh lime-determinations in the solutions actually employed in the absorption experiments showed, that notwithstanding the different composition of the three salts, the amount existing in the solutions prepared as before described was very much the same. The other constituents, from their possessing a comparatively equal solubility with chloride of sodium, cannot be supposed to vary much; and the true composition of the salts contained in the solutions employed, as given below, will be seen to present a very close coincidence between the per-centage of lime-salt and of ammonia absorbed.

Real composition of the salts dissolved for the absorption experiments, calculated to 100 parts.

	I.	II.	III.
Sulphate of lime (determined).	1·356	1·039	1·182
Sulphate of magnesia	·264	·338	·337
Sulphate of soda	3·555	·886	1·779
Chloride of sodium	94·825	97·737	96·702
	100·000	100·000	100·000
Ammonia absorbed by 100 parts of the solid salt.......	·6309	·5005	·5654

We ought, then, to have an index to the absorbing power of a salt by a knowledge of the quantities of chloride of sodium and of soluble lime-salt which it contains; an equivalent of lime-salt fixing an equivalent of ammonia,—and the chloride of sodium, the per-centage which it has been above shown to absorb. Let us now apply this method of determination to the samples under experiment.

No. I.

Total number of grains of ammonia absorbed by 100 grains of salt	·6309
Deduct the absorbing power of 1·356 grain of sulphate of lime	·5187
	·1122
Deduct the absorbing power of 94·825 grains of chloride of sodium	·1000
	·0122

No. II.

Total number of grains of ammonia absorbed by 100 grains of salt	·5005
Deduct the absorbing power of 1·039 grain of sulphate of lime	·3973
	·1032
Deduct the absorbing power of 97·737 grains of chloride of sodium	·1041
	*

No. III.

Total number of grains of ammonia absorbed by 100 grains of salt	·5654
Deduct the absorbing power of 1·182 grain of sulphate of lime	·4519
	·1135
Deduct the absorbing power of 96·702 grains of chloride of sodium	·1030
	·0105

It will be seen that in Nos. I. and III. a slight excess of ammonia remains unaccounted for; this is partly, of course, due to an unavoidable excess which must in every experiment be added; but I have observed, that throughout the operation, even when the absorption is most active, and even after several hours' contact, there is yet a feeble tinge imparted to the litmus-paper when left in the atmosphere within the bottle for five minutes. This would seem to show, that at all stages of the process there is a certain amount of counteraction going on, and it becomes a great question whether this action—which appears trivial when we deal with a comparatively large amount of ammonia, as in these experiments—would not in practice nullify to a great extent, or at least diminish considerably, the absorbent power of this agent, when, spread on the surface of the soil, minute quantities only of ammonia came within the sphere of its action, and the absorption was distributed over a considerable portion of time.

I believe that this counteraction and liberation of ammonia may be traced to the solubility of the carbonate of lime in the absorbing liquid: upon the addition of the first few drops of carbonate of ammonia solution to the solutions of the salts, the small quantity of precipitate which was produced was immediately and perfectly dissolved by agitating the liquid; this was doubtless partly due to the excess of carbonic acid which was evolved from the sesquicarbonate of ammonia, but a considerable portion of the action must be attributed to another cause. It is some time since Professor Connell has shown, that, upon producing carbonate of lime by the action of carbonate of soda upon chloride of calcium in a medium of distilled water, the carbonate of lime so produced (if produced at all) is held in perfect solution[*]. I have found the same action to operate to a much larger extent in the case of a saturated solution of chloride of sodium: taking two solutions of chloride of calcium and carbonate of soda,

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[*] Phil. Mag. S. 3. vol. xxxi. p. 124.

of which equal bulks contained equivalents, and adding equal portions successively to a known quantity of a saturated solution of pure chloride of sodium, taking care to mix whichever was first added thoroughly with the solution before adding the other, I have obtained the following results, by which it is shown that a saturated solution of chloride of sodium dissolves $\frac{1}{5649}$ of carbonate of lime, or about $5\frac{1}{2}$ times as much as Professor Connell gives as the amount dissolved ($\frac{1}{31323}$) by the common water of St. Andrews. The chloride of sodium was of course perfectly freed from carbonic acid before trying the experiments.

Experiment.	100 grains of saturated solution of pure chloride of sodium will dissolve	Hence 100 grains of the solid salt will decompose?
I.	·0165 of a grain of carbonate of lime.	·0623 of a grain of carbonate of lime.
II.	·0181 do.	·0683 do.
III.	·0186 do.	·0703 do.
Mean	·0177 do.	·0669 do.

The addition of the solutions to the chloride of sodium was continued until a slight film of carbonate of lime was deposited upon the mixture standing for half an hour; a deduction was made for the excess added; the numbers therefore represent the amount of carbonate of lime which can be permanently held dissolved by the chloride of sodium. I say this, because I believe that a far larger amount may be retained in solution for a limited time; for if the first trivial precipitate is disregarded, twice or three times as much of the two solutions may be added without producing any such increase of the precipitate as ought to occur, if at that stage the chloride of sodium absolutely refused any further action. But this somewhat curious result I think of prosecuting further. It must not, however, be supposed that it is only on carbonate of lime in the nascent state (if the expression may be allowed) that the chloride of sodium exercises this power; if the two solutions are allowed to mix by pouring them simultaneously upon the surface of the salt solution, so as to produce there a stratum of recently precipitated carbonate of lime, the chloride of sodium will dissolve this with the utmost facility upon agitation. The clear solution may be boiled without any separation of carbonate of lime occurring; it is partly thrown down by boiling with carbonate of ammonia, partly also by boiling with ammonia, and apparently completely by oxalate of ammonia. Whether there exists in this solution chloride of sodium dissolving carbonate of lime, or chloride of calcium not decomposed by carbonate of soda, we have no present means of determining, although from its partial precipitation by carbonate of ammonia I should almost think that it existed in the latter form; since it is more rational to suppose that carbonate of ammonia, being a less permanent combination than carbonate of soda, should transfer its carbonic acid to chloride of calcium, taking hydrochloric acid in return, than that it should exert any action upon carbonate of lime dissolved as such. The idea of the considerable temporary solubility of recently formed carbonate of lime, receives considerable support from a peculiar phænomenon which took place when I endeavoured to make carbonic acid determinations in the Worcestershire brine-springs. I adopted the ordinary process of adding to the water excess of ammoniacal chloride of calcium,—a very trifling precipitate occurred, which was attributed to the comparative absence of carbonates and carbonic acid; upon examining the bottles at home, however, their interior was covered with most beautiful crystals of selenite, of the most perfect forms,—some complete hemitropes. From their figure and size, these had evidently been deposited slowly; and I do not doubt now that the chloride of sodium at first prevented the precipitation of the carbonate of lime, then that the large quantity of sulphate of soda present parted with its sulphuric acid to the lime so dissolved (by degrees, as the chloride of sodium relaxed its solvent power); the liquid was soon saturated with sulphate of lime, and the surplus was thus gradually deposited. I need not say that the amount of carbonate of lime actually thrown down was quite insufficient to meet the requirements of the analysis; nor is this the only instance in which I have observed a similar result.

The results, then, at which we must arrive are, that agricultural salt is a most energetic absorbent of ammonia, both in virtue of its chloride of sodium and of its soluble lime-salt, and that the proportion of the latter especially most powerfully affects its action;—but that at the same time its agency does not seem to be altogether a permanent one: it will collect the ammonia, but it is questionable whether it can retain it for any great length of time, because in the very decompositions which happen in order to render the ammonia more stable, salts are formed which have a direct tendency to liberate ammonia from its more fixed combinations. It may, however, retain it quite long enough for agricultural purposes: if the young plants are there ready to receive it, its state of gradual liberation may be for them the most advantageous possible; and to this conclusion all experiments on the large scale appear most obviously to tend. It is described as an excellent check to the too forcing power of guano; and from M. Barral's experiment we see that it either prevents the too rapid eremacausis of the latter, or stores up the ammonia as it is formed. As a manure for growing crops, all experience and all theoretical considerations therefore show it to be most valuable; but when employed to mix with manure heaps which have to stand for considerable periods of time, theory would pronounce, as practice has in many cases done, that its power of retaining ammonia under those circumstances is at the best doubtful.

XXII. *An Experimental Inquiry undertaken with the view of ascertaining whether any* Force *is evolved during Muscular Contraction analogous to the* Force *evolved in the Gymnotus and the Torpedo.* *By* H. F. BAXTER, *Esq.*[*]

CONVINCED that a calm inquiry after truth is liable to be lost in controversial disputes, we shall just refer in a general note to the principal sources from whence we have been enabled to obtain views such as are confirmed by experimental evidence, and which are now entertained upon the subject of our present inquiry[†].

The fact long since obtained by Matteucci, viz. that of causing the muscles of a galvanoscopic frog to contract by placing its nerve upon the muscles of another animal during their contraction, is well known; and in his last communication[‡] to the Royal Society, Matteucci has adduced strong reasons for believing that an electric *disequilibrium* is produced during muscular contraction.

By means of the galvanometer, Du Bois-Reymond[§] has satisfactorily shown that an effect occurs upon the needle during muscular contraction. These results have been confirmed by Zantedeschi[||], Buff[¶] and Tyndall[**].

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[*] Communicated by the Author; having been read at the Royal Society May 10, 1855.

[†] *Annales de Chimie et de Physique*, 3^{e} série, vol. xv. p. 64; vol. xxx. pp. 119, 179; vol. xxxix. p. 114. *Comptes Rendus*, vol. xxviii. pp. 570, 641, 653, 663, 782; vol. xxx. pp. 349, 406, 479, 512, 563, 699; vol. xxxi. pp. 28, 91, 318; vol. xxxii. p. 131. *Bibliothèque Universelle de Genève*, Fev. 1850, Juin 1853. On Animal Electricity; being an Abstract of the Discoveries of Emil du Bois-Reymond, &c., edited by H. Bence Jones, M.D., F.R.S. London: Churchill. 1852. 8vo. *Lettre de Charles Matteucci à M. H. Bence Jones, F.R.S.* Florence: Imprimerie Le Monnier, 1853. 8vo. On Signor Carlo Matteucci's Letter to H. Bence Jones, M.D., F.R.S. &c. By Emil du Bois-Reymond. London: Churchill. 1853. 8vo.

[‡] Philosophical Transactions, 1850. Ninth Series.

[§] *Comptes Rendus*, vol. xxviii. p. 641.

[||] *Bibliothèque Universelle de Genève*, Fev. 1850.

[¶] Ibid.

[**] British and Foreign Medical Review for Jan. 1854, p. 141.

The results obtained by Despretz*, Becquerel† and Matteucci‡, have thrown strong doubts upon the conclusions deduced by Du Bois-Reymond. Several experiments undertaken by the author of the present paper, both prior and subsequent to the knowledge of Du Bois-Reymond's researches, tended to confirm in his opinion the doubts expressed by these inquirers. But the positive evidence adduced by Du Bois-Reymond by means of his galvanometer, combined with that of Matteucci obtained by means of the frog, only made it incumbent upon the opponents of Du Bois-Reymond to disprove his conclusions, if capable of disproof, more by stronger experimental evidence than by theoretical arguments; hence the renewal of the present inquiry.

In relating the experiments, to avoid unnecessary prolixity, we shall endeavour to be as concise as possible, incurring, however, some risk of being considered as not having sufficiently eliminated the sources of error. Our great point will be to establish facts; and whatever observations we might think necessary to make in reference to the *origin* of the effect produced, these will form the subject of the concluding remarks, and may be considered as distinct from the main object of the paper. We shall relate in a general manner some results that were obtained by means of a galvanometer consisting of but few coils. To two brass handles of an ordinary medical electro-magnetic machine, were attached thick copper wires, each about 8 inches in length, and bent. Similar copper wires were connected with the galvanometer, their free ends communicating with mercurial cups, these cups forming the means of connexion with the brass handles. Holding the metallic handles firmly, grasping one of them and contracting the muscles of the arm powerfully, and doing this alternately with each arm, we at first failed to obtain any, or if any, but indecisive results. Upon repeating the experiments on another occasion, we ultimately obtained some definite result and a clue to our former failures. It was found that when the muscles of the arms were contracted alternately at definite periods, and continuing this act for some time, that *as the hands became moist* a decided effect upon the needle occurred, indicating the contracting arm to be *positive*§ to the other. We considered that these results were due partly to the reaction of the acid secretions of the hand upon the metallic electrodes, partly to the skin becoming a better conductor than the dry cuticle, and partly perhaps to thermo-electric action.

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* *Comptes Rendus*, vol. xxviii. p. 653.

† Ibid. p. 663.

‡ Ibid. p. 782.

§ If we take an elementary circle, such as zinc, platinum, and dilute acid, the current goes from the zinc, through the fluid, to the platinum, the platinum being the *positive* electrode.

The question now arose, could not the effect which might be due to muscular contraction coincide with that which arose from the action of the cutaneous secretions? From the results obtained by Du Bois-Reymond*, it would appear that the *direction* of the current due to muscular contraction is *inverse,* i. e. *from* the hand *to* the shoulder.

Several other experiments were undertaken in which different solutions were employed to moisten the hands; the effect upon the needle was now much greater. When one hand was moistened with a weak alkaline solution and the other with water, upon contracting the muscles of the arm holding the alkaline electrode, this became *negative* to the other; but by proper management, by not having the solution too strong, we could make this arm become *positive,* the current occurring during muscular contraction overcoming the influence of the alkaline solution. Feeling the force of the objections that might be raised in reference to the use of the metallic electrodes, we do not think it necessary to relate any other experiments made with the use of these instruments.

A galvanometer† of the following construction was now employed. It consisted of two coils, one placed above the other so as to produce the full effect upon both needles, with an index to indicate the amount of deflection. Each coil was 2 inches in length and the same in breadth, consisting of 1500 turns; the thickness of the wire 37 gauge. The needles were rather less than 2 inches in length; the index, of ivory, 3 inches in length. As our object was to ascertain, if possible, the *existence* of the force rather than the *amount,* a further description of its delicacy will not be necessary.

Two copper clamps were made, each 4 inches in length, tapering at one extremity so as to be connected with the binding-screws of the galvanometer, the other extremity being bent down at right angles to the extent of half an inch, presenting a surface $1\frac{1}{4}$ inch in breadth, and to which was also attached a piece of copper by means of two screws; by these clamps the platinum electrodes were attached, and those that were generally used consisted of platinum foil, each $2\frac{1}{2}$ inches in length and 1 inch in breadth.

The vessels usually employed to hold the solutions were two cupping-glasses, each $2\frac{1}{4}$ inches in diameter, $2\frac{3}{4}$ inches in depth, and contained rather more than four ounces of fluid. The

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* Rapport sur les Mémoires rélatifs aux phénomènes électro-physiologiques présentés à l'Académie par M. E. du Bois-Reymond, *Comptes Rendus,* vol. xxxi. p. 28.

† Vide Faraday's mode of employing a galvanometer. Phil. Trans. Series xxviii. 1852.

glasses being half-filled with a weak solution of common salt, covering the electrodes to the extent of an inch, two and sometimes three fingers of each hand were held perpendicularly in the vessels, the middle finger resting upon the bottom of the glass vessel. When first introduced, a slight tremulous motion of the needle was observed; upon taking the fingers out of one vessel and reintroducing them, and repeating this act, a slight effect appeared, at first definite, but this soon subsided. Similar effects were observed when the fingers of the other hand were used in a similar manner. The fingers last introduced were not always *positive* or *negative* to the others. Keeping the fingers in, then moving one of the fingers so as to make the fluid rise and fall upon the surface of the electrode in one vessel, produced but little motion of the needle. After the fingers had been in for some time, and the needle had become quite stationary, upon contracting the muscles of one arm an effect occurred upon the needle indicating the contracted arm to be *positive* to the other 3°; by contracting the muscles of each arm alternately, the effect amounted to 5°. The result was definite, and the motion of the needle steady.

Du Bois-Reymond* considers that there are five circumstances as influential in producing a current; viz. 1, température inégale; 2, durée inégale de l'humectation avec le liquide conducteur; 3, tension inégale de la peau; 4, lésion de l'une des places de la peau; 5, transpiration inégale. We could not refer the effects we obtained to either of these circumstances;—to the tension of the skin, for instance.

Three solutions were now prepared; the first consisting of a concentrated solution of common salt; the second of sulphuric acid, one part of strong acid to six parts of water; and the third of one part of liq. potassæ (Pharm. Lond.) to four parts of water.

It was found necessary that the following precautions should be strictly attended to. The solutions should be prepared and well mixed previous to their use, and the electrodes covered to the same extent. The hands and fingers should be thoroughly *clean*; to attain this object they were first washed with the ordinary curd or Windsor soap, and afterwards well rinsed in common water and wiped comparatively dry with a clean towel. The same towels should not be employed when using the different solutions, and it was found better to work on different days with each solution; for the liquids soaking in between the nails and the fingers became a source of some difficulty to remove, and a cause of some embarrassment in judging of the final result.

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* *Bibliothèque Universelle de Genève*, Juin 1853.

We need hardly state that the surfaces of the electrodes should be clean and the contacts perfect.

With the solution of common salt. Upon the first introduction of the fingers, a slight tremulous motion of the needle occurred. Withdrawing the right-hand fingers and reintroducing them immediately afterwards, and repeating this act a few times, a slight *positive* effect was produced upon the introduction of the fingers. Withdrawing the left-hand fingers in the same manner, a slight *negative* effect was at first obtained, but after a short time the effect became indefinite, and ultimately slightly *positive.* Keeping the fingers in and contracting the muscles of the arm, the fingers of the contracted arm were *positive* 4° to 5°, and made to increase.

The solution in one vessel was now diluted to one-half. Upon the first introduction of the fingers, the strong solution was positive 5°. Keeping the fingers in and waiting until the needle had become motionless, the contracted arm was *positive* to the other; if the arm connected with the strong solution was contracted, the effect was greater than when the other was contracted, the current in the former case rising to 5°, in the latter to 2° or 3°.

With the acid solution. Upon the first introduction of the fingers, the needle would sometimes go 30° or 40°, but generally speaking it would oscillate first to one side and then to the other. Withdrawing the fingers of one hand and then reintroducing them, this hand was *positive* to the other. Keeping the fingers quiescent, and contracting the muscles of the arm, the corresponding fingers were *positive* 4°.

The solution in one vessel was diluted to half the strength of the former; the strong solution was *positive* to the other upon the introduction of the fingers. If the difference between the two solutions was not too great (the exact strength being difficult to state), we might determine a slight current to pass in a constant direction, and then make the current arising during muscular contraction overcome this constant current. Strong solutions were found best for this purpose, from their forming, perhaps, a better conducting liquid than the weaker solutions.

With the alkaline solution. Upon the first introduction of the fingers no definite result, the needle tremulous. Withdrawing the fingers and reintroducing them, the effects varied, but, generally speaking, the reintroduction of the fingers produced a *negative* result. The effect due from muscular contraction was definite from 2° to 4°, the fingers of the contracted arm being *positive.*

The solution in one vessel was diluted to half the strength of the other. Upon the introduction of the fingers, the diluted solution was *positive* to the stronger. We were enabled in this instance, as with the acid solution, to obtain a solution of such strength as to give a constant current in a certain direction, and then make the current due from muscular contraction overpower this current. As we consider this fact of some importance, it will be necessary to state the mode of ascertaining this constant current. If we find, either upon the introduction of the fingers of both hands at the same time, or upon the introduction of the fingers of one hand, that a current existed in one direction to the amount of about 2°, the current occurring during muscular contraction, if it coincided with this constant current, would cause the needle to advance to 4° or 5° or more; but if it has to overcome this constant current, the needle would only move 2° or 3°. We need scarcely add, that the needle of the galvanometer should continue to indicate this current whilst the fingers remain in the solution, which may be indicated by the needle receding to its normal position upon the withdrawal of the fingers. A great difficulty is frequently experienced in obtaining this constant current.

Several other experiments might be related in which the electrodes were made to differ in size, one being narrow, the other broad; or where one was made to dip deeper into the solution than the other. The general results indicated that a difference in extent between the surfaces of the platinum electrodes and the solutions, occasioned an effect upon the needle upon the first introduction of the fingers. In other experiments larger vessels were employed, so as to insert the whole of the hand and wrist; the results coincided with those we have related, the effect being greater.

The solutions were made to vary as to temperature by placing the glass in a vessel containing hot water, and then carefully stirring the solution so as to make it completely uniform. One vessel was at the temperature 65° F., and the other 115°. The results upon the first introduction of the fingers were indecisive. The hot solution was not always positive to the cold solution. We could always obtain the effect during muscular contraction.

We agree with Du Bois-Reymond as to the importance of attending to the five circumstances to which he refers, and which we have already alluded to. The circumstances that appear to us to be the most influential in masking the ultimate result, are,—1st, the action of the fluid upon the surface of the electrodes; and 2ndly, the reaction of the cutaneous secretions upon the fluids. Although we cannot remove these sources of error, we have it nevertheless in our power to counteract their effects by showing that the current consequent upon muscular contraction can be made to overcome that arising from either of the two former actions.

We have not yet alluded to the difference which appears to exist between the results obtained by Du Bois-Reymond and ourselves, viz. the *direction* of the current. Du Bois-Reymond's experiments indicate that the current is *inverse,* i. e. *from* the hand *to* the shoulder during muscular contraction; our results, on the other hand, point out that the current is *direct,* i. e. *from* the shoulder *to* the hand. We do not think that our results differ much from those of Du Bois-Reymond; but as the discussion of this question will involve certain theoretical views, whatever observations we shall have to make will be deferred to our concluding remarks*.

With the assistance of three friends an attempt was made to increase the effect upon the needle by forming a pile, as it were; we could obtain the effect, but there was no decided increase.

Several experiments were undertaken with the *rheoscopic frog*† in lieu of the galvanometer; the results were not so satisfactory as could be desired, to justify their being recorded in the present paper.

The posterior limbs of a frog, separated at the pelvis, but

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* Since this paper was presented to the Royal Society, we have had the opportunity of attending the interesting lectures of Du Bois-Reymond at the Royal Institution. Du Bois-Reymond considers, as far as we could understand, that when the whole of the hands are immersed in the solution, and the muscles of the arm are made to contract, then the current is due to the swollen state of the skin, and perhaps to other circumstances; but should we be justified in concluding, that when the *fingers alone* are in the vessel, then the effect is due to their swollen state? If Du Bois-Reymond maintains that the current due to muscular contraction is *inverse,* i. e. *from* the hand *to* the shoulder, we can only add that we have never been able to obtain that constant effect. We cannot deny that it frequently happens, upon the first repetition of the experiment, that the current may appear in favour of this opinion. Despretz has remarked, that the current may appear first in one direction and then in another. We cannot insist too strongly upon the necessity of having the hands perfectly clean, and we are convinced that the failures and contradictory results which arise are due more to the want of attention on this point than to anything else. The *direction* of the current is a fact of the utmost importance; and as we shall refer to this subject again in the concluding remarks, and as no reasons have occurred to lead us to alter our views, we prefer leaving the paper in its original state. (June 19.)

† The term *rheoscopic* has been recommended in the Report of the Committee of the Academy of Paris, in preference to that of *galvanoscopic.* We have employed both terms. When it is used for the detection of the *current,* the former term is most applicable; but the frog *may* be a test of a force in which the *current* force, in accordance with our present notions of force, cannot be shown to exist. If the *dynamic* condition and the *current* condition of force be considered as equivalent terms, then *rheoscopic* would be unobjectionable.

connected by means of the lumbar nerves and a portion of the vertebral column, were each placed in separate vessels containing the solution of common salt; the muscles of one limb were then made to contract, and an immediate effect upon the needle was observed, the contracted limb being *positive* to the other from 3° to 4°. To prevent the fluid from being scattered upon the electrode, a piece of glass was attached to the part as a weight to avoid too great motion of the limb. In some instances both feet were removed: the effect upon the needle was still the same.

Similar results were obtained with the *acid* solution, but with the *alkaline* solution the effects varied, especially so if the solution were strong. The alkali acted apparently upon the mucous secretion of the skin.

Great care was required to have the cutaneous surfaces clean previous to the introduction of the limbs into the solutions, so that no current could arise therefrom.

Is it necessary to place the nerve of a galvanoscopic frog in contact with the longitudinal and transverse section of a muscle in order to obtain the necessary contractions? Repeating the experiments of Matteucci[*], we found that the contraction might be obtained without placing the nerve in contact with the muscle. At the same time we must add, that the effect upon the galvanoscopic frog appeared greater when its nerve was placed transverse to the muscular fibres than in any other position.

The results of our experiments tend to establish the following conclusions, viz.—

First. That during muscular contraction in man and in frogs, an effect upon the galvanometer may be obtained indicating the manifestation of an electric current.

Secondly. That this manifestation of an electric current is due, in a great measure, to secondary reactions, viz. between the animal secretions and the solutions on the one hand, and between the solutions and the platinum electrodes on the other; but that there nevertheless remains a *residual effect* which we cannot refer to either of these actions, or to those pointed out by Du Bois-Reymond.

Concluding Remarks.

The numerous instances in which an electric current may be shown to exist, naturally renders any conclusion deduced from results obtained by means of the galvanometer extremely doubtful. If these remarks are applicable to physical researches, where

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[*] Phil. Trans. 1850. Ninth Series.

we have such control over our experiments as to afford us facilities favourable for eliminating sources of error, how easily may we conceive that they would apply with far greater force to physiological researches devoid of such facilities. The strong prejudices which exist in reference to electro-physiological inquiries cannot therefore be a matter of much surprise, but must create a great difficulty to any individual who shall attempt the elucidation of any electro-physiological problem experimentally; for not only has he to satisfy, from the mixed character of the inquiry, the extreme views of the physicist on the one hand, and those of the physiologist on the other, but they also afford the indolent inquirer a ready means of apparent refutation, and a powerful weapon to the controversialist. That these opinions are not ill-founded we might easily prove*. Feeling the importance of basing the conclusions in these inquiries upon strictly experimental evidence, in discussing the different views that may be entertained in regard to the evidence, upon which our experiments justify us in concluding that some force *is* evolved during muscular contraction, we shall confine our observations to the experimental results obtained by Matteucci and Du Bois-Reymond, and endeavour to avoid everything of a purely controversial character.

Matteucci† has proved and shown by means of the galvanoscopic frog, that during muscular contraction the muscles of the galvanoscopic frog may be excited to contract, and that for this purpose it is not necessary that the nerve should be brought into contact with *different* portions of the muscular fibre. In repeating these experiments, we have been able to confirm the results obtained by Matteucci.

Matteucci‡, in his endeavours to ascertain whether the muscular current was increased during muscular contraction by forming piles with muscular elements, failed to obtain any decisive result. Du Bois-Reymond, by means of his galvanometer, has satisfactorily shown that when the electrodes are so arranged with a muscular element that the current (muscular) passes through the instrument, that then, upon the contraction of the muscle, the needle of the instrument recedes, and this he calls "the negative variation" of the muscular current. This fact, established by Du Bois-Reymond, and also the results obtained

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* Strange to say, it has been urged that the *time* has not yet arrived for the prosecution of these experiments. This is the only tangible objection that the author has yet had an opportunity of refuting. That objections may be started he is perfectly convinced; but as the inquiry professes to be *experimental*, the objections must be supported by experimental evidence before he can notice them.

† Phil. Trans. 1850. Ninth Series.

‡ Ibid. 1845.

by Matteucci, have been confirmed by some of our own experiments. But we cannot help thinking with Matteucci*, that the falling or the receding of the needle in Du Bois-Reymond's experiment may be attributed in a great measure to the variation in the contacts between the surfaces of the muscle and the electrodes during the act of contraction. We are not aware that the existence of the muscular current is disputed by anyone; it has been attributed to nutritive actions by Matteucci†, and some experiments of our own‡ tended to confirm the views of Matteucci. We now have two distinct questions before us:—1st, *Is* there, or is there *not*, any force evolved during muscular contraction? 2ndly, Is the muscular current affected *during* muscular contraction? and this brings us to speak of the results we have obtained in the present paper.

Du Bois-Reymond's results lead him to consider that in the frog the current is *direct*, and in man *inverse*, as shown by the galvanometer. Our experiments lead us to believe that the current is *direct* in both instances. Upon carefully looking over Du Bois Reymond's§ last paper, it appears to us that he frequently obtains the *direct* current; but laying so much stress, as he does, upon the necessity of a *current* (muscular) circulating through the instrument prior to contraction before he can conclude that the resulting action upon the needle is what he calls "the negative oscillation" of the muscular current, that his object was to ascertain whether the muscular current in the human subject was not affected *during* muscular contraction, as in the frog. We may be mistaken in our opinion. As we have not been able to obtain this *inverse* current, and if Du Bois-Reymond has been endeavouring to obtain this *inverse* current to prove "the negative variation" of the muscular current in the human subject, then the object that each of us has had in view differs: ours has been to ascertain whether any force *is* evolved during muscular contraction; Du Bois-Reymond's, whether the muscular current is affected *during* muscular contraction. The importance of Du Bois-Reymond's researches we do not deny; but it appears to us to be of extreme importance that the two questions should be kept perfectly distinct, although perhaps intimately connected with each other.

The results of our inquiries lead us to believe, that during muscular contraction a force *is* evolved, as in the fish, but that it is only during extraordinary muscular exertion that it can become manifest to the galvanometer. We are perfectly aware

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* Phil. Trans. 1850. Ninth Series.

† Ibid. 1845, p. 301.

‡ Proceedings of the Royal Society, Nov. 25, 1852.

§ *Bibliothèque Universelle de Genève,* Juin 1853.

of the objections that may be urged in reference to the fish being provided with a special apparatus. In our first endeavours also to obtain some result with the galvanometer, and in which we failed to obtain any evidence of the existence of a force being evolved, we were then led to the conclusion as to the improbability of any force becoming free, as it were, during muscular contraction; that whatever force might be present would be expended or converted during the act of contraction; but we could never get over the difficulty which the results of Matteucci, obtained by means of the frog, presented for our consideration, and which is doubly increased by the results we have now related in the present paper. In reference to the fish, also, we must remember that the force evolved bears some relation, according to Matteucci*, "to the activity of the functions of circulation and of respiration, and of every act of nutrition." The apparatus may be a *means* for the evolution of the force, but not a *producer* of the force; and we have some reasons for believing that the electric condition of the blood in the living animal must not be overlooked.

We have purposely avoided referring in this discussion to the influence and relations which the nervous system must bear to muscular contraction; and as it is our wish that these researches should be considered as strictly experimental, whatever remarks we might wish to make must be deferred to a future opportunity, until circumstances will enable us to pursue the inquiry, and with greater advantages than we now possess.

XXIII. *On the Induction Apparatus and its Effects.*

By J. C. POGGENDORFF.

[Concluded from p. 137.]

APPENDIX I.

On the Thermal Effect of the Induction Sparks.

IN the foregoing memoir†, it was incidentally remarked that a greater thermic action evidently exists in the stream of sparks at the place of interruption of the induction wire, than in that wire itself. This assertion is based not only upon the facts there recorded, but also upon the prevalent opinion that electric sparks always contain incandescent material particles, in fact that they cannot otherwise be conceived to exist; an opinion which many phænomena I have observed appear to corroborate. Nevertheless it may appear desirable to produce a direct proof

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* Phil. Trans. 1847, p. 241.

† Phil. Mag. vol. x. p. 122.

of the above assertion, the more so as former observations on the sparks of frictional electricity have led either to no results at all, or only to uncertain ones.

With the induction sparks obtained without trouble from a continually interrupted current, such a proof is without the least difficulty. It is even furnished in a very perceptible manner by a good mercurial thermometer. With this instrument of course no measurements can be made, although comparative results can be obtained by securing a continuous development of sparks, by means of as uniform a motion and condition of the hammer as can be possibly attained, as well as by paying attention to other precautions which will be easily recognized as necessary.

The following are some of the experimental results which were obtained:—

1. When a delicate thermometer is placed in or close to the stream of sparks of the induction apparatus, a considerable rise immediately takes place, contrasting greatly with the small development of heat in the wire itself. This is the more remarkable, because, according to Joule's law, the heat generated in a given time throughout the whole circuit of the induction current is smaller when the circuit is broken by air than when it is continuous.

2. All other things being equal, this elevation of temperature is different according to the nature of the metals or bodies from which the sparks issue.

In this respect, however, platinum, copper, iron, and silver differ little from one another, although amongst them silver appeared to act strongest, and platinum weakest*. But a great difference exists between these metals and those which are but little coherent, and are easily fused or vaporized. On an average, their action is double that of the former.

The following are the elevations of temperature observed during the space of one minute in a series of experiments with different metallic points. The cylindrical reservoir of the thermometer was 11 millims. long and 3·5 millims. in diameter; its axis was placed between the points from which sparks issued, and at a distance of 2¼ millims. from each.

Platinum	18½° C.
Lead	30½ ...
Tin	33 ...
Antimony	34¼ ...
Zinc	35 ...
Bismuth	37 ...

The difference was still greater when the distance between the

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* Graphite alone produced a still smaller degree of heat.

points amounted only to 1½ millim., and both were touched by the thermometer. The elevations of temperature within one minute were then with—

Platinum	23° C.
Copper Iron	24° to 26° C.
Silver	27° C.
Tin	51° C.

When placed near the stream of sparks, though scarcely in contact with it, the thermometer rose 28° C. with tin points.

3. The inequality in the temperature of the two poles of the induction wire may also be clearly shown by the thermometer. When the points were 4·5 millims. apart, the elevation of temperature during one minute was always 6 or 7 degrees more at the negative than at the positive pole, both pole wires being of zinc or tin, and about 1 millim. thick.

4. When the pole wires consist of two different metals, the rise of the thermometer placed in the stream of sparks is greatest when the easily fused and vaporized metal constitutes the negative pole. Within one minute, and when the points were 4·5 millims. asunder, the elevations of temperature were,—

— Pole.	+ Pole.	
Platinum.	Platinum	18½° C.
Platinum.	Tin	23½ ...
Tin.	Platinum	31 ...
Platinum.	Bismuth	18½ ...
Bismuth.	Platinum	30 ...

5. The greater temperature of the sparks with easily fused and vaporized metals appears to be caused by the volatilization of particles of the same. This is clearly shown by the thermometer suspended in the stream of sparks; when zinc, tin, bismuth, and lead are used, the positive side is coloured white, and the negative brown (at least with bismuth and lead).

6. This volatilization appears also, by improving the conductibility between the poles, to produce a greater transfer of sparks, and consequently a greater intensity of current. It is true the galvanometric deviations caused by the induction wire during the time that sparks are produced are very irregular, so much so that it is exceedingly difficult to determine their magnitude, nevertheless I am inclined to think that the deviations are greater with tin than with platinum points.

7. This suggested a method of proving whether the facility with which sparks pass from one pole to the other is modified by the

nature of the points which may be employed. The method consists in the division of the current between two pairs of points of different metals. The experiments (like the similar ones of Faraday's with different gases) involve some difficulties, and require to be carefully interpreted; because if by accident the stream of sparks should be better conducted through one pair of points than through the other, the former may, by the air becoming warm, and by the volatilization of metallic particles, obtain an excess over the latter which is not natural to it. In general, when the distances between points are equal, sparks which are simultaneously obtained from both pairs of points differ in colour and brilliancy according to the nature of the metals, but apparently they do not differ in the spark-distance. Very often the one or the other stream of sparks ceased for a while without any very apparent cause; on the other hand, however, when zinc, or still better tin, was compared with platinum, it was always found that at small distances (1 or 2 millims.) sparks only passed between the former metals, at greater distances (3 or 5 millims.) only between the latter metal. Without doubt at small distances the volatilized zinc or tin particles conduct so well that they usurp the whole current.

8. The luminous discharges in partial vacuum present similar phænomena to sparks in air. The increase of temperature thereby is very considerable, although not so great as with sparks. Although the strength of the current increases with increasing rarefaction, the elevation of temperature decreases, owing to the decreased resistance.

9. In partial vacuum, too, the inequality in the temperature of the poles is maintained, although it does not appear to be so great as in free air.

In one experiment the air was rarefied to about one inch of mercury pressure, at which time, although the electric light was continuous, it still produced a jingling noise, like that at the commencement of the crackling of the sparks in free air. Two thermometers stood at the distance of about a line from the brass knobs between which the discharge took place, and which were 1¼ inch asunder. During one minute the thermometer at the negative pole rose 15° or 15°·5 C., the one at the positive pole 12° or 13° C.

All the above results were obtained by employing the thicker and shorter of the described induction coils, a battery of two of Grove's elements, and the condenser.

APPENDIX II.

On a new method of increasing the Induction Current[*].

Amongst the reasons which induced me to construct Neef's hammer as an independent instrument[†], I may mention that I intended to use it under the air-pump, because I anticipated that the induction current would be influenced by interrupting the inducing current in a partial vacuum, instead of in free air.

Recent experiments have fully satisfied my anticipations, by proving that the partial vacuum increases the action of the current-breaker to such an extent, that in those classes of phænomena wherein the action of the condenser was greatest, the latter may now be dispensed with.

As the effects produced by this manner of using Neef's hammer are in other respects interesting, I may here be allowed to enter into a few particulars.

I placed the hammer upon the extra plate, before described, in an atmosphere of about 1 or 1½ lines of mercury pressure, and connected it with the inducing apparatus in such a manner that the galvanic current could traverse the wires of the primary coil (which were 100 feet long and 1 millim. thick, and encircled a core of soft iron), either alongside or behind one another in the same direction. The current was excited sometimes by one, and sometimes by two of Grove's elements, and allowed to act alternately upon the shorter and upon the longer of my induction wires. Arrangements were made, lastly, to connect the hammer vibrating in the partial vacuum with the large oiled-silk condenser, if necessary.

The *same* hammer was afterwards employed in air under the same circumstances, and also supported by the same condenser, so that its action here could be compared with that which was observed when it worked in a vacuum[‡].

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[*] From Poggendorff's *Annalen* for May 1855, p. 156.

[†] Phil. Mag. vol. x. p. 12.

[‡] In order to make this comparison with two different hammers, it is necessary that they should be exactly alike. Under otherwise equal circumstances, the sparks, and particularly the spark-distance, of the induction coil are very much influenced by the motion of the hammer; it is not so essential that it should vibrate quickly as that it should vibrate with a certain force, *i. e.* that the tongue should separate itself quickly from the pin, and that the amplitude of its oscillation should not be too small. For this purpose it is therefore convenient to have a rather heavy armature at the end of the tongue; in one of my instruments, where the armature is too light, I usually attach a piece of brass to it by means of wax. On the other hand, in order to obtain very strong galvanometric or voltametric actions, when the induction current is interrupted by a piece of dry blotting-paper, it is convenient to employ a light armature, so that the tongue may vibrate very quickly.

As a general result of this comparison, it was quite evident that the action of the *sparks* of the induction coil was in all cases quite as strong when *the hammer worked in a vacuum without the condenser, as when it worked in air with a condenser*; in some cases, indeed, it was considerably stronger.

This was particularly the case with the thicker and shorter induction wire, when the wires of the primary coil were at the same time connected side by side, so as to form a wire 100 feet long and $\sqrt{2}$ millims. in diameter.

When the wires of the primary coil were connected in this manner, the combination of the condenser with the hammer in a vacuum had apparently no influence upon the spark-distance of the induction coil, at least no increasing influence; the sparks were merely stronger sometimes. On the contrary, when the primary wires were connected one after the other, so as to form a wire 200 feet long and 1 millim. in diameter, the combination of the hammer and oiled-silk condenser considerably diminished the spark-distance; a phænomenon which at first sight appeared strange. At the same time, however, a continual crackling could be heard in the condenser. The remarkable phænomenon, therefore, was caused by the extra current, owing to its increased intensity, penetrating the condenser in the form of sparks*.

The luminous phænomena of the induction current in the electric egg are also very well developed when the hammer vibrates in a vacuum without the condenser; apparently just as well as when the hammer works in air, and is connected with the condenser. It is here of importance to what degree the rarefaction is carried; if it amounts to about 1 line of mercury pressure, and at the same time a great intensity is given to the current by developing it in a wire 10,000 feet long, the combination of the condenser with the hammer vibrating in a vacuum is without influence upon the phænomenon; in the opposite case it increases the phænomenon in the same manner as when, at a former time†, the hammer played in free air.

With reference to the cause of the action of the hammer in a vacuum, I will only remark that it appears to me to be of the same nature as that of the condenser.

The vibrations of the hammer in a vacuum are very interesting and instructive in the study of the extra current which is developed in the proper circuit of the galvanic current, partly by the

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* The condenser is of course perforated by this, so that I have always discontinued the experiment after a very short time. Small holes, however, appear to injure the action of the condenser but little, if at all.

† Phil. Mag. vol. x. p. 127.

mutual action of the coils of wire, partly also and principally through the influence of the soft iron core in connexion with the interrupting hammer.

For this purpose it is best to remove the induction coil for the time, and to retain only the primary coil in connexion with the galvanic battery and the hammer.

If, in the first place, the wires of the primary coil be connected either side by side, or one after the other, in such a manner that the current must traverse them in opposite directions, instead of in the same direction, as is usually the case, the production of the extra current will be effectually prevented, and on this account only a very small spark of a purely galvanic nature is observed at the point of interruption where the hammer vibrates in a vacuum*. The insertion of a soft iron core into the primary coil has no influence upon this result, for the iron, as will be readily seen, is not magnetized. The addition of a condenser is equally without effect.

Passing from this combination to that wherein the current traverses the wires in the same direction, the influence of the extra current shows itself immediately and in a remarkable manner. The scarcely perceptible galvanic spark expands itself into a luminous phænomenon, whose magnitude is actually surprising when we consider that it is obtained from a wire only 100, or at most 200 feet long. Notwithstanding its magnitude, however, it is explained by the fact, that, previous to its development, the above wire constituted a complete circuit to a current of comparatively great intensity.

In general this phænomenon consists of two parts: a more or less luminous spark at the place where the current is interrupted, and a soft, clear, blue light surrounding the neighbouring parts of the hammer. The formation of both, however, is different according as the galvanic current is excited by one, two, or three of Grove's elements; according as the wires are connected side by side or one after the other, or a soft iron core inserted in the coil or not; and lastly, according as a condenser is used or not. I have generally employed a battery of two of Grove's elements, and connected them with the hammer so that the pin formed the negative pole. Under these circumstances the following was observed.

By combining the primary wires side by side, the spark is pretty strong, but the blue light very weak, provided no soft

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* This method of connecting the wires is perhaps the only practical means of excluding the influence of induction currents when measuring the velocity with which electricity is propagated; an influence to which must be chiefly ascribed the extraordinary disparity between the results already obtained.

iron is employed. When the soft iron core is inserted in the coil, the intensity of both increases, but it is the spark which especially gains magnitude and brilliancy, and in these respects is at least three times greater than the spark in free air. It actually assumes the form of a small flame bursting forth on all sides from under the pin, in the midst of which the point of the pin may be distinguished as a brilliant luminous point. The blue light, on the contrary, covers all the neighbouring parts of the pin, which forms the negative pole, with its beautiful brightness, and would undoubtedly spread itself more were the hammer suitably constructed*.

When employing soft iron, the condenser has scarcely any influence upon the phænomenon; without soft iron it weakens the spark a little, and destroys the blue light completely.

The primary wires being connected end to end, the spark becomes smaller and less luminous. The blue light, on the contrary, is more developed, if not more beautifully blue.

When the current is reversed, it is the vibrating hammer which becomes covered with the blue light; this is not only beautiful to observe, but also gives rise to a remarkable occurrence: the anterior part of the tongue between the pin and the armature, which is not traversed by the current, receives a luminous covering.

The above-mentioned phænomena are well calculated to render perceptible the influence of the soft iron core in its several forms. A massive soft iron bar introduced into the primary coil weakens the action which this coil exerts when alone, in exactly the same manner as it decreases the spark when the hammer vibrates in free air. On the contrary, a hollow bundle of wires, whose walls are tolerably thick, is just as effectual as a full bundle, and its action is little or not at all diminished when a solid bar is introduced into the hollow axis. The most effectual means of increasing the luminous phænomena, however, is to place the coil in the middle of a very long bundle of soft iron wires; for this purpose I employed the one before described†, which is three times as long as the coil itself.

The insertion of the induction coil into the primary coil has no influence upon any of the phænomena here described, so long as the former remains unclosed, and does not present a closed circuit in its interior. When closed, however, either by metallic contact or by a well-exhausted electric egg, its insertion weakens the luminous phænomena at the hammer, vibrating in a vacuum

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* Neef himself allowed the hammer to vibrate in a vacuum (Pog. *Ann.* vol. lxvi. p. 432), but the small intensity of his apparatus did not, of course, permit him to see the phænomenon in its full development.

† Phil. Mag. vol. x. p. 6.

very much. This is a very clear proof of the reaction of the induction current upon the extra current.

It may also be mentioned, that when the hammer vibrates in a vacuum, and when the parts which beat against one another are of platinum, the latter are very much attacked. The platinum part of the tongue, the brass part above it, the screw of the pin, and the mountings are all greatly blackened. This black powder evidently consists of finely pulverized platinum, which, when the hammer is in activity, can be seen scattered in small sparks from the point of the pin around in all directions.

Owing to this strong action upon the hammer, the above-described method of increasing the intensity of the induction current is scarcely to be recommended in practice; nevertheless, theoretically considered, it is certainly not less interesting than the three methods already known; by the condenser, by allowing the hammer to vibrate under water, and by interrupting the current between the poles of a magnet, which last method was investigated by Rijke.

The action upon the hammer, however, can be considerably diminished by substituting a silver in place of the platinum pin. The sparks then assume a green colour, which is particularly visible in daylight.

XXIV. *On an Electrical Apparatus which acts the part of a Valve.*

By M. J. M. Gaugain.

M. GAUGAIN has recently communicated to the Academy of Sciences in Paris, a Note on an Electrical Apparatus which acts the part of a valve[*]. The subject has been further examined by M. Riess, an account of whose researches we intend to appear in a subsequent Number of the Philosophical Magazine. M. Gaugain remarks, that there is a numerous class of electric currents which are considered as being formed by the succession of several other currents, with directions alternately opposed to each other. To fix definitely the true constitution of these composite currents, it was thought useful to isolate the partial currents which form them; and to attain this end, he proposed to devise an apparatus which should possess (like a valve) the power of arresting currents having a certain direction, while it should leave to those flowing in an opposite direction perfect freedom of passage. Several combinations which fulfil this condition more or less completely were successively studied; they were based on the known properties of points, and on the experiment of the pierced card. We will here, however, limit

[*] *Comptes Rendus*, vol. xl. No. 12.

ourselves to the description of an apparatus wherewith M. Gaugain has obtained results much more satisfactory than with any other, and which is founded upon an observed fact which he believes to be new.

Let an ordinary *electric egg** be taken, and let the upper knob be coated with an insulating substance, the stem which supports the knob being similarly coated, leaving naked only an excessively small portion of the surface of the knob: let the egg thus prepared be placed in the circuit of a Ruhmkorff's coil: introducing a galvanometer also into the same circuit, the following facts may be observed:—When the induced currents which correspond to the interruption of the inducing current (the only ones which traverse the vacuum of the egg) pass from the covered to the naked knob, the intensity of the current indicated by the galvanometer goes on increasing as the air within the egg is more and more rarefied; but this is not the case when the induced currents pass through the egg from the naked to the covered knob. In this case the intensity of the current goes on, at first advancing as the pressure is diminished; but when this pressure descends below a certain limit, the deviation of the galvanometer diminishes; for a certain pressure it becomes zero, and finally changes the sign when the vacuum is as perfect as can be obtained with a good air-pump. M. Gaugain limited himself to the establishment of this fact without seeking an interpretation; this sufficed for the object he had in view, namely, to show that the currents traversed the egg freely when they passed from the covered knob to the naked one, and could not pass in the opposite direction, when the vacuum was suitably made. From this it follows that the electric egg, disposed as indicated, can act, in relation to certain currents, the part of a valve with respect to liquids.

M. Gaugain believes that this electric valve may be useful in a certain number of researches, and he has already applied it to the solution of a question raised by M. du Moncel in one of the last communications made by the latter to the Academy of Sciences. When a condenser is interposed in the induced circuit of the apparatus of Ruhmkorff, the motion of the electricity continues, as is proved by the physiological and luminous effects produced on the circuit: but two different hypotheses may be made as to the nature of this movement. It might be supposed that the electricity propagates itself across the insulating film of the condenser in the same manner as it passes through a conducting body, and in this case its direction would be constantly the same; it might, on the contrary, be supposed that the two

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* A glass receiver shaped like an egg.

electricities, developed by the induction apparatus, accumulate on the two surfaces of the condenser during the time the electromotive force is active, and that they recombine as soon as this force ceases to act. According to this latter hypothesis, the current ought to pass in opposite directions alternately; the rigorous discussion of the facts M. Gaugain believes sufficient to decide which of those hypotheses is the true one; but the question may be resolved in a decisive manner by means of the electric valve.

To render our language definite, let us suppose the condenser employed to be a Franklin's plate, placed horizontally, and that its inferior surface is in communication with the negative pole of the induced circuit of the induction apparatus; let two different communications A and B be established between the positive pole of the apparatus and the superior armature of the condenser: in each of these portions of the circuit let a galvanometer and valve be introduced, and let the two valves be disposed in such a manner that the currents can pass in the circuit A from the pole to the condenser, and that, on the contrary, they can only pass in the circuit B when they are directed from the condenser towards the pole: it is easy to see what must take place acording to each of the hypotheses between which it is our wish to decide. If the direction of the currents be constant, they will pass exclusively in the circuit A, or exclusively in the circuit B; if, on the contrary, the motion of the electricity consists of a succession of two currents alternately opposed to each other, the two circuits A and B will be traversed simultaneously by the opposed currents; and the direction of these currents, determined only by the disposition of the valve, will be independent of the direction of the inducing current. Now it is in this latter way that the facts exhibit themselves; the existence of currents which traverse at the same time the circuits A and B may be proved, either by the simultaneous appearance of light in the electric eggs, or by the deviation of the galvanometers. The intensities of the two currents differ very little from each other; this may be inferred from the following numbers: in one of the experiments the deviation corresponding to the current which charged the condenser was 63 degrees; the deviation due to the current which discharged the condenser was 61 degrees. It evidently results from this experiment, that the motion of the electricity in a circuit interrupted by the interposition of an insulating film is formed by the succession of two alternate currents.

In a recent number of the *Comptes Rendus*, M. Gaugain has described further experiments of this nature; we shall take occasion to refer to them in an early Number of this Journal.

XXV. *On the Law of Electro-chemical Equivalents.*

By M. I. Soret*.

IN a previous memoir I have shown that equal weights of copper are obtained, by submitting to the action of the same current different electrolytes in which copper forms the positive element, or what is called the *cation*. I proposed also to study the different electrolytes of which hydrogen is the electro-positive radical, and had even commenced some experiments; but I have given up this investigation, which has become useless since M. Despretz published his researches upon this subject, and announced his intention of pursuing them. I shall here limit myself to the relation of the following observation upon the decomposition of water.

When distilled water is submitted to the action of a somewhat powerful battery (sixty small Grove's elements), the gaseous bubbles which are disengaged at one of the electrodes are attracted by the other electrode, probably in consequence of the great difference of tension of the electricity at the surface of the two poles. The molecules of gas, and perhaps of water, electrized by their contact with one of the poles, discharge themselves upon the other.

It is possible that, by this *mechanical* transfer, if I may call it so, there may be a passage of a certain quantity of inefficacious electricity, which will necessarily be greater the nearer the electrodes are to each other. But I do not know whether the amount of this may become considerable.

The same thing does not take place in acidulated water, the conductibility of which is much greater, because the difference of tension of the two electrodes is much less considerable.

To return to the law of electro-chemical equivalents. We may, I think, consider that the above-mentioned researches have demonstrated, that, in taking separately the groups of different electrodes from which the current separates the same electro-positive radical or *cation*, the law of Faraday is exact, within the limits of errors of observation. Buff has proved a second point; he has shown with nitrate of silver that the point of silver separated is exactly in proportion to the quantity of electricity which traverses the electrolyte, whatever may be the intensity of the current†.

To complete the verification of Faraday's law, it is necessary also to show that a similar current traversing electrolytes, the electro-positive elements of which are different, separates weights of these elements proportionate to their chemical equivalents.

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* From the *Comptes Rendus* for August 6, 1855, p. 220.

† Liebig's *Annalen*, vol. lxxxv. p. 1.

It is with this view that I have undertaken the following experiments, by means of which I have compared the quantities of copper, hydrogen, and silver separated by the current. I have also tried some experiments upon lead, but they did not lead to a satisfactory result.

I. *Comparison of the quantities of copper and hydrogen separated by the same current.*—The comparison of the quantities of copper and hydrogen separated by electrolysis has been effected by means of a somewhat complicated apparatus, which it would be difficult to describe completely without the aid of a figure. I shall therefore confine myself to indicating briefly the mode in which I have operated.

An electrical current traversed simultaneously a solution of sulphate of copper, and some acidulated water. The copper deposited on a platinum wire was weighed directly. The hydrogen was determined by combustion, as in an organic analysis.

In preparing the sulphate of copper, in its electrolysis and the weighing of the deposit, I operated exactly in the same manner as in the researches upon the salts of copper previously referred to. Considerable difficulty is met with in the exact determination of the quantities of water decomposed by the pile. In fact it is to be feared that a portion of the gas set free combines afresh. The circumstances which may be in favour of this recombination are,—1, the mixture of the hydrogen and oxygen in the presence of the platinum wires, especially if a part of the oxygen is in the state of ozone; 2, the contact of the hydrogen dissolved in the water with oxygen in the nascent state; 3, the formation of binoxide of hydrogen, that is to say, of a very oxidizing body which may burn the hydrogen. These causes of error are avoided principally by separating the gases from the moment of their formation, and raising the temperature of the electrolyte. The best mode of separating the gases appeared to me to consist in plunging the electrodes in two separate test-tubes, united by a siphon filled with acidulated water. They were kept at a temperature of 140° to 158° F. The hydrogen disengaged from one of these test-tubes passed first through a series of desiccating apparatus, and then traversed a combustion-tube. The water was absorbed in weighed tubes. A current of atmospheric air which traversed the apparatus at the same time, served to draw the whole of the hydrogen into the combustion-tube at the conclusion of the experiment, and to prevent the detonations which would inevitably have been produced if the proportion of hydrogen had been considerable.

I made a certain number of preliminary experiments to determine the strength of the pile which it would be necessary to employ, and to ascertain the limit of exactitude in the observations;

I believe that we cannot hope in these experiments to measure the quantity of water absorbed with certainty within 0·001.

The definitive experiments were made with twenty Bunsen's elements. 395·6 was taken as the equivalent of copper, and 112·5 as that of water. The results are given in the following Table:—

Numbers of the experiments.	Duration of the experiments.	Weight of copper deposited.	Weight of water absorbed.		Absolute differences.	Relative differences.
			Calculated.	Found.		
	hours.	gr.	gr.	gr.		
1	6	0·4133	0·1176	0·1180	+0·0004	$+\frac{1}{295}$
2	6	0·4856	0·1381	0·1382	+0·0001	$+\frac{1}{1382}$
3	12	1·0133	0·2882	0·2871	−0·0011	$-\frac{1}{261}$
			0·5439	0·5433	−0·0006	$-\frac{1}{905}$

It appears that the absolute differences do not exceed the limits of exactitude to be expected. The greatest difference is $\frac{1}{261}$ of the total quantity of water absorbed, and the average relative difference only extends to $\frac{1}{905}$. Faraday's law is therefore verified by these experiments.

II. *Comparison of the quantities of silver and copper separated by the same current.*—The comparison of the weights of copper and silver separated by the action of the pile is much easier. The electrolytes which I have employed are pure sulphate of copper and nitrate of silver.

The electrolysis of the sulphate of copper was effected as in the preceding experiments, and that of the nitrate of silver nearly in the same manner. A neutral solution of nitrate of silver was placed in a tube closed at one end. In this a platinum wire was immersed as a negative electrode, and a silver wire as the positive one. The silver deposited was washed with distilled water, then left to dry in the open air, and weighed. My first experiments did not present complete exactness, from certain causes which were afterwards avoided. 1350 was taken as the equivalent of silver. The following Table contains the results of the final experiments:—

Numbers of experiments.	Duration of the experiments.	Weight of silver deposited.	Weight of copper deposited.		Absolute differences.	Relative differences.
			Calculated.	Found.		
	hours.	gr.	gr.	gr.		
1	$1\frac{1}{4}$	0·8781	0·2573	0·2572	−0·0001	$-\frac{1}{2572}$
2	,,	0·3999	0·1172	0·1168	−0·0004	$-\frac{1}{292}$
3	8·20	1·6084	0·4713	0·4708	−0·0005	$-\frac{1}{942}$
			0·8458	0·8448	−0·0010	$+\frac{1}{845}$

The absolute differences may be entirely referred to possible errors in the weighing. We may observe that the values of the chemical equivalents themselves are not determined with greater accuracy, for if we only admit 395·1 as the equivalent of copper instead of 395·6, the law is mathematically verified by the average of these experiments.

Conclusion.—The weights of copper, hydrogen and silver, separated by the same electrical current, are in proportion to the chemical equivalents of these bodies, and Faraday's law is verified within the limits of errors of observation.

XXVI. *Notices respecting New Books.*

A Treatise on the Calculus of Operations. By the Rev. ROBERT CARMICHAEL, *A.M., Fellow of Trinity College, Dublin, and Member of the Royal Irish Academy.* London: Longman, Brown, Green, and Longmans. 1855.

THIS is a work which will be welcome to every mathematical student, for it will, in a great measure, supply a want which has long been felt. The Calculus of Operations ranks amongst the most important improvements which modern mathematicians have introduced. It originated in a profound investigation of the essential nature of all existing operations, whereby it was found, that, although they differ considerably in their objects, they have nevertheless so much in common that the results obtained by one operation may, with a few modifications, be translated into the language of another; that each, in fact, is a particular interpretation of far more general results, which are susceptible of an unknown number of interpretations.

A wide field of discovery is thus opened to the mathematician. Not only are operations in differential calculus, in integral calculus, and in the calculus of finite differences, in some cases, immensely facilitated, but an intimate relation between these operations is established, and the key obtained to still higher and more complicated ones.

Such being the importance of the subject, no doubt can be entertained that the calculus of operations might be introduced into our elementary course of instruction at a far earlier period than it is at present; but in order to do so, its elementary principles must be first demonstrated with all that lucid precision and completeness which is necessary to interest and carry conviction to the young student, and thus enable him to employ the calculus with confidence and facility. This is scarcely the object of the book under review. The author has furnished an admirable text-book to those who are already, to some extent, acquainted with the calculus. He has studiously avoided "all prolixity of detail as alike unnecessary and wearisome" to them, but we may add, important and *not necessarily* wearisome to the less advanced student. To such intentions, of course, no one can take objection; and the less so, as in this instance they have been faithfully executed. The researches of other mathematicians on the subject have been carefully collected, and not unfrequently considerably improved; besides which the author has added much of his own, and treated the subject throughout with great ability and elegance. Nevertheless it is worthy of consideration, whether, by expanding the second chapter on 'Elementary Principles,' and by entering more fully into those details of which we have before spoken, the sphere of the book's usefulness could not be considerably increased.

The Elements of Plane Practical Geometry, with Illustrative Applications. By E. W. Dallas, *F.R.S.E.* London: John W. Parker and Son, West Strand. 1855.

The author's object in writing this work was "not so much to educate mathematicians as to provide the ordinary workman with an intelligent reason for many of those problems of geometry that occur in the operations in which he is daily employed, and a knowledge of the principles of which must tend to correctness and excellence of execution." It is a difficult task to avoid embarrassing such a class of readers with too much theory, and at the same time to preserve the necessary mathematical precision and sequence. This task the author has accomplished with praiseworthy skill; for although a mathematician would certainly detect imperfections, they are of such a nature as not to diminish the value of the book to those for whose perusal it was written.

The number of theorems in the first part is as small as the 'workman' can desire, and they are demonstrated concisely and clearly. The practical problems in the second part are, for the most part, well chosen and skilfully solved. Amongst them the mathematical student will recognize many 'deducibles' whose solutions have cost him much labour, and he will certainly find it instructive to compare his own solutions with those here given. He must not forget, however, that in his case the principal value of these problems is to be found in *solving* them, rather than in knowing their solutions.

The 'observations and applications' appended to many of these problems are not so skilfully chosen as we could desire; they are intended to "afford some indication to the reader of the very general application of geometry," but they do so very inadequately, and it it is quite possible that their perusal may be a source of some disappointment to the ardent student. Many of them he will certainly find interesting, others superfluous, because he would undoubtedly have made them for himself; others, again, he may find trivial, and a few, such as the remark about diverging rays in the sky (p. 65), are of doubtful accuracy. Nevertheless the work will be a welcome one to those for whom it was intended; and the fact that it was undertaken at the suggestion of the Honourable the Board of Trustees for the Encouragement of Manufactures, &c. in Scotland, is a good guarantee that such a work was needed.

Recent Cambridge Mathematical Works.

A Treatise on the Differential Calculus, and the Elements of the Integral Calculus, with numerous Examples. By I. TODHUNTER, *M.A., Fellow of St. John's College, Cambridge.* Cambridge: Macmillan and Co. 1852.

A Treatise on Analytical Statics, with numerous Examples. By I. TODHUNTER, *M.A. &c.* Cambridge: Macmillan and Co. 1853.

Elementary Mechanics. By J. B. PHEAR, *M.A., Fellow and Mathematical Lecturer of Clare Hall, Cambridge.* Cambridge: Macmillan and Co. 1850.

Elementary Hydrostatics, with numerous Examples. By J. B. PHEAR, *M.A. &c.* Cambridge: Macmillan and Co. 1852.

Arithmetic and Algebra, in their Principles and Application, with numerous systematically arranged Examples, taken from the Cambridge Examination Papers, with especial reference to the ordinary Examination for B.A. degree. By BARNARD SMITH, *M.A., Fellow of St. Peter's College, Cambridge. Second Thousand, with an additional Appendix.* Cambridge: Macmillan and Co. 1854.

The first thing that strikes us in all these publications of Messrs. Macmillan and Co., is the beautiful way in which they are all printed and "got up." Those which contain diagrams and figures have them on the same page as the text, which is a very great convenience to the reader. Most mathematical books of late years have all the plates at the end of the volume (for the sake of cheapness), an arrangement which gives constant trouble and annoyance to the student. We would gladly pay a shilling or two extra, if necessary, to save all this trouble of turning to the plates every time the figure has to be looked at. We come now to the merits of the works as text-books on their respective subjects.

The 'Differential Calculus' of Mr. Todhunter is an excellent one, and has already taken its place at Cambridge as *the* text-book on that subject. We are far from thinking that the author has done all that can be done towards making the principles of the Calculus clear and satisfactory; but he has done a great deal. We can strongly recommend the work to all students, whether at Cambridge or elsewhere.

The 'Analytical Statics,' by the same author, is, as he states in the preface, little more than a reprint of those portions of Pratt's 'Mechanical Philosophy' which treat of the same subject. The well-known work of Pratt has been the text-book at Cambridge on all the subjects, almost, of which it treats (from elementary mechanics up to the planetary theory), ever since its first publication. And few books have ever deserved their reputation better. It is a first-rate book, and we cannot understand why it should be allowed to go out of print, as it now is. The publishers, indeed, announce their intention of issuing the remaining portions of the work under other editors, which may certainly afford an opportunity of improving the book in various respects (for instance, placing the diagrams on the same page as the text instead of at the end); but the whole work will thus become much more expensive. There is one argument in favour of this arrangement, viz. that it will enable students to purchase only those parts which they immediately want; and those, for example, who only want the elementary subjects, need not buy the part on the lunar and planetary theories, &c.

Notwithstanding the typographical beauty of this new edition, there are misprints and errors in the examples which the editor will probably correct in a future edition. The additional examples form, indeed, the chief recommendation of this new edition over the original; but the greatest care ought to be bestowed to ensure the perfect *accuracy* of these examples, which otherwise will only serve to confuse and perplex the student, especially if he has no tutor. The few additions to the original text of 'Pratt' are derived chiefly from MSS. which have long been familiar to the Cambridge student.

Mr. Phear's 'Elementary Mechanics' is also a good elementary text-book. The examples are numerous, and apparently well chosen. We extract two simple 'questions,' one from his 'Mechanics,' and the other from his 'Hydrostatics,' as good specimens of a *class* of questions which we should be glad to meet with more frequently in examinations and books:—

"A shopkeeper has a false balance, and thinks to make his customers' consequent losses and gains balance each other by weighing the goods which he sells alternately in the one scale and in the other; does he succeed?" (Mechanics, p. 107.)

"Is it advantageous to a buyer of diamonds that the weighing of them should be made when the barometer is high or when it is low, supposing their specific gravity to be less than that of the substance used as the weight?" (Hydrostatics, p. 39.)

The great fault of all Cambridge mathematical books is, that they do not bear sufficiently on *practical* subjects. Ingenious problems are framed, having no earthly connexion with any practical purpose or application, whilst hundreds of questions might be proposed quite as good in the way of illustrating mathematical principles and calculations, and much more *useful*. Engineers and other practical men would often be led to consult mathematical works, and to study mathematics, if they found *what they wanted* in such a course; but unfortunately they find scarcely anything of practical utility to them. Cambridge men have themselves acknowledged this defect in their system, but few have done anything towards correcting it. The following quotation from Whewell's preface to the first part of his 'Dynamics' (published in 1832), is just as applicable now as ever: "Analytical as well as geometrical speculations may be unprofitable. The really important applications of mathematics are so numerous, that it is by no means desirable to employ the student's time on detached and useless problems. . . . . . There can be no doubt of the advantage which students derive from working out the results of these and similar problems as exemplifications of the application of their principles. But those who would really use their mathematical acquirements for the improvement of their fellow-students in this place, may easily find better subjects for their skill. There are at present a number of branches of natural science to which mathematical calculation has been so far successfully applied, that they might form a portion of the subjects of study here, if we had clear and convenient treatises upon them." (Preface, pp. xvii and xviii.)

We may also quote the following from a really practical work by one of the most distinguished Cambridge men of the present day, whose example, it is much to be regretted, has not been followed by more men of his own university:—

"I am not without hopes, that, in addition to its principal object of giving a scientific and systematic form to its subject, the results of the volume which I now venture to present to the world may be found a useful addition to mathematical studies in general, by affording simple illustrations of the application and interpretation of formulæ, and by suggesting new subjects for problems, and for further investigation." (Willis's 'Principles of Mechanism,' Preface.)

Mr. Phear's 'Hydrostatics' is not sufficiently copious and explanatory. He (like so many other Cambridge writers) has sacrificed clearness to brevity. But there are some good examples, and the work, like all the others, is admirably printed.

Of Mr. Barnard Smith's 'Arithmetic and Algebra,' we need say no more than repeat the encomium of Dr. Peacock, and recommend it as the best book we know on the subject. We may state that the 'Arithmetic' is published separately for the use of schools.

XXVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 149.]

May 24, 1855.—The Lord Wrottesley, President, in the Chair.

THE following communications were read:—

"On a Decimal Compass Card." By James M. Share, Esq., Master R.N.

The mariners' compass-needle having of late years received great improvements, I am of opinion it is high time the card, as at present arranged, should take its place by the side of such things as are superseded by others better adapted to the advancing spirit of the times.

I venture to make an attempt to innovate on an old custom, by suggesting the substitution of a compass card containing thirty-six points of ten degrees each—every degree being one-tenth of a point.

By the use of this card the mariner will avoid the constantly recurring trouble of turning degrees into points, and *vice versâ.*

The ship's course having been worked out in degrees, the deviation and local attraction have but to be applied to adapt it to the decimal steering card, thus rendering the "traverse table for points" no longer necessary to those steering by it; the course N. 35° E. being the same as "north three and a half points east," &c. The same remark applies also to astronomical bearings, azimuths, amplitudes, &c.

Should the decimal card be adopted, the old-fashioned method of "boxing the compass," which takes young people so long to become familiar with, will be entirely superseded, and I think the sooner such method becomes obsolete the better it will be for the interests of the mariner, for, together with other advantages, the tedious operation of a "day's work" will be divested of half the usual trouble.

When giving a course to the "quarter-master," or "man at the wheel," no mistake, so liable to be the case at present, can well occur; it will merely be necessary to direct him to steer, for instance, "north five points east," or more briefly, "north five east," "south six west," &c. &c.

I recollect an instance of a vessel steering N.W. by N. ½ N., instead of W. by N. ½ N. during thick weather in the Bristol Channel, thus running into danger from the similarity of sound between the courses alluded to.

The practical application of the decimal card would not materially affect the charts previously published, which could have printed compasses containing thirty-six points pasted over the others. Such might be sold by any chart-seller.

"Observations on the Human Voice." By Manuel Garcia, Esq.

The pages which follow are intended to describe some observations made on the interior of the larynx during the act of singing. The method which I have adopted is very simple. It consists in placing a little mirror, fixed on a long handle suitably bent, in the throat of the person experimented on against the soft palate and uvula. The party ought to turn himself towards the sun, so that the luminous rays falling on the little mirror, may be reflected on the larynx. If the observer experiment on himself, he ought, by means of a second mirror, to receive the rays of the sun, and direct them on the mirror, which is placed against the uvula. We shall now add our own deductions from the observations which the image reflected by the mirror has afforded us.

Opening of the Glottis.

At the moment when the person draws a deep breath, the epiglottis being raised, we are able to see the following series of movements:—the arytenoid cartilages become separated by a very free lateral movement; the superior ligaments are placed against the ventricles; the inferior ligaments are also drawn back, though in a less degree, into the same cavities; and the glottis, large and wide open, is exhibited so as to show in part the rings of the trachea. But unfortunately, however dexterous we may be in disposing these organs, and even when we are most successful, at least the third part of the anterior of the glottis remains concealed by the epiglottis.

Movement of the Glottis.

As soon as we prepare to produce a sound, the arytenoid cartilages approach each other, and press together by their interior surfaces, and by the anterior apophyses, without leaving any space, or intercartilaginous glottis; sometimes even they come in contact so closely as to cross each other by the tubercles of Santorini. To this movement of the anterior apophyses, that of the ligaments of the glottis corresponds, which detach themselves from the ventricles, come in contact with different degrees of energy, and show themselves at the bottom of the larynx under the form of an ellipse of a yellowish colour. The superior ligaments, together with the aryteno-epiglottidean folds, assist to form the tube which surmounts the glottis; and being the lower and free extremity of that tube, enframe the ellipse, the surface of which they enlarge or diminish according as they enter more or less into the ventricles. These last scarcely retain a trace of their opening. By anticipation, we might say of these cavities, that, as will afterwards appear clearly enough in these pages, they only afford to the two pair of ligaments a space in which they may easily range themselves. When the aryteno-epiglottidean folds contract, they lower the epiglottis, and make the superior orifice of the larynx considerably narrower.

The meeting of the lips of the glottis, naturally proceeding from the front towards the back, if this movement is well managed, it will allow, between the apophyses, of the formation of a triangular space, or inter-cartilaginous glottis, but one which, however, is closed as soon as the sounds are produced.

After some essays, we perceive that this internal disposition of the larynx is only visible when the epiglottis remains raised. But neither all the registers of the voice, nor all the degrees of intensity, are equally fitted for its taking this position. We soon discover that the brilliant and powerful sounds of the chest-register contract the cavity of the larynx, and close still more its orifice; and, on the contrary, that veiled notes, and notes of moderate power, open both so as to render any observation easy. The falsetto register especially possesses this prerogative, as well as the first notes of the head-voice*. So as to render these facts more precise, we will study in the voice of the tenor the ascending progression of the chest-register, and in the soprano that of the falsetto and head-registers.

Emission of the Chest-voice.

If we emit veiled and feeble sounds, the larynx opens at the notes do$_2$, ré$_2$, mi$_2$,  †; and we see the glottis agitated by large and loose vibrations throughout its entire extent. Its lips comprehend in their length the ante-

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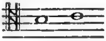
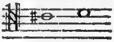
[*] Let us here observe, that three registers of voice are generally admitted,—chest, falsetto, and head. The first begins lower in a man's voice than in a woman's; the second extends equally in both voices; the third reaches higher in the female voice.

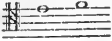
Table of the Human Voice in its full extent.

[†] The musical limits we establish in the course of these pages vary a little in each individual.

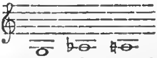
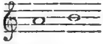
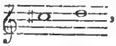
rior apophyses of the arytenoid cartilages and the vocal cords; but, I repeat it, there remains no triangular space.

As the sounds ascend, the apophyses, which are slightly rounded on their internal side, by a gradual apposition commencing at the back, encroach on the length of the glottis; and as soon as we reach the sounds si$_3$, do$_3$, , they finish by touching each other throughout their whole extent; but their summits are only solidly fixed one against the other at the notes do♯$_3$, ré$_3$, . In some organs these summits are a little vacillating when they form the posterior end of the glottis, and the two or three half-tones which are formed show a certain want of purity and strength, which is very well known to singers. From the do♯$_3$, ré$_3$  the vibrations, having become rounder and purer, are accomplished by the vocal ligaments alone, up to the end of the register.

The glottis at this moment presents the aspect of a line slightly swelled towards its middle, the length of which diminishes still more as the voice ascends. We also see that the cavity of the larynx has become very small, and that the superior ligaments have contracted the extent of the ellipse to less than one-half.

When instead of veiled and feeble sounds, we make use of full and vibrating ones, the glottis becomes visible only at the sounds mi$_3$, fa$_3$, , and those above them, a limit which depends to a certain extent on the dexterity of the singer. For all the rest, the organs act as we have just said, but with a double difference: 1. The cavity of the larynx contracts itself more when the voice is intense, than when it is feeble. 2. The superior ligaments are contracted so as to reduce the small diameter of the ellipse to a width of two or three lines. But however powerful these contractions may be, neither the cartilages of Wrisberg, nor the superior ligaments themselves, ever close sufficiently to prevent the passage of the air, or even to render it difficult. This fact, which is verified also with regard to the falsetto and head-registers, suffices to prove that the superior ligaments do not fill a generative part in the formation of the voice. We may draw the same conclusion by considering the position occupied by the somewhat feeble muscles which correspond to these ligaments; they cover externally the extremity of the diverging fibres of the thyro-arytenoid muscles, and take part especially in the contractions of the cavity of the larynx during the formation of the high notes of the chest- and of the head-registers.

Production of the Falsetto.

The low notes of the falsetto,  sol$_2$, la♭$_2$, la♮$_2$, show the glottis infinitely better than the unisons of the chest-voice and produce vibrations more extended and more distinct. Its vibrating sides, formed by the anterior apophyses of the arytenoid cartilages, and by the ligaments, become gradually shorter as the voice ascends; at the notes la$_3$, si$_3$, , the apophyses take part only at their summits; and in these notes there results a weakness similar to that which we have remarked in the chest-notes an octave below. At the notes do♯$_4$, ré$_4$, , the ligaments alone continue to act; then begins the series of notes called *head-voice.* The moment in which the action of the apophyses ceases, exhibits in the female voice a very sensible difference at once to the ear and in the organ itself. Lastly, we verify, that, up to the highest sounds of the register, the glottis continues to diminish in length and in width.

If we compare the two registers in these movements, we shall find some analogies in them: the sides of the glottis, formed at first by the apophyses and the ligaments, become shorter by degrees, and end by consisting only of the ligaments. The chest-register is divided into two parts, corresponding to these two states of the glottis. The register of falsetto-head presents a complete similarity, and in a still more striking manner.

On other points, on the contrary, these same registers are very unlike. The length of the glottis necessary to form a falsetto note, always exceeds that which produces the unison of the chest. The movements which agitate the sides of the glottis are also augmented, and keep the vibrating orifice continually half opened, which naturally produces a great waste of air. A last trait of difference, is in the increased extent of that elliptic surface.

All these circumstances, which we shall refer to again, show in the mechanism of the falsetto, a state of relaxation, which we do not find in the same degree in the chest-register.

Manner in which the sounds are formed.

As we have just said, and what we have seen proves it, the inferior ligaments, at the bottom of the larynx, form exclusively the voice, whatever may be its register or its intensity; for they alone vibrate at the bottom of the larynx*. But by virtue of what principle is the voice formed? It seems to me, that the answer to this question can be but this; the voice is formed in one unique manner,—*by the compressions and expansions of the air, or the successive and regular explosions which it produces in passing through the glottis.*

The ligaments of the glottis are situate about the mean level of the upper border of the cricoid, close the passage, and present a resistance to the air. As soon as the air has accumulated sufficiently, it parts these folds and produces an explosion. But at the same instant, by virtue of their elasticity, and the pressure from below being relieved, they meet again to give rise to a fresh explosion. A series of these compressions and expansions, or of explosions, occasioned by the expansive force of the air and the reaction of the glottis, produces the voice.

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* We gladly acknowledge that this most important fact has been already announced by J. Müller, although we have our objections to the theory which accompanies it.—*Handbuch der Physiologie des Menschen.*

This theory, though now generally admitted for reeds, and undoubtedly evident in the liquid vein, the toothed-wheel of Savart, the syrène of the Baron Cagnard Latour &c., has not to my knowledge, been yet applied to the glottis*. If we consider that the lips of this aperture, taken separately, can give no kind of sound, however we may try to make them speak, we must admit that the sounds which they give forth by their mutual action, are only owing to the explosions of the air produced by their strokes†. It is not necessary in order to obtain the explosion of sound, that the glottis should be perfectly closed each time after its opening; it suffices that it should oppose an obstacle to the air capable of developing its elasticity. In this case the rushing of the air is heard accompanying the sounds, and they take a veiled, and sometimes an extremely muffled character; an observation which we have already presented to the reader's notice in speaking of the falsetto.

Conjectures on the Formation of the different Registers.

As the entire system of vibrations arises solely from the inferior ligaments, it is evident that the cause of the different tones called registers, must be sought for in the muscles which set these ligaments in motion; and that the other parts of the larynx must be considered only as apparatus for strengthening the sounds obtained, and for modifying their quality. In our efforts to discover the more intimate processes of the vocal organs which produce the sounds, we shall recur at once to the observations already mentioned, to some anatomical remarks which we are going to make, and to the sensations which we feel in the organ itself whilst it is producing sounds.

If we detach one of the halves of the thyroid cartilage, we shall see a large muscular surface of oblique fibres, which fills all the space between the arytenoid and thyroid cartilages. At its upper end is to be seen the muscle corresponding to the superior vocal ligaments, and which sometimes extends to the notch in the thyroid. After detaching this generally frail muscle, all the fibres constituting this muscular surface seem to start from two opposite centres, viz. the anterior surface of the arytenoid, and the re-entering angle of the thyroid. These centres, occupying the extremities of a diagonal line, send their fibres towards each other in parallel lines. Those which start from the anterior face of the arytenoid descend obliquely; the most external ones go to the cricoid, whose posterior half they cover at the side; the most internal ones descend to the vocal membrane‡, which they cover entirely. The fibres which terminate at the membrane become longer, as they become more internal. Those which start from the re-entering angle

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* I find that Dr. Müller hints at the possibility of the voice being thus formed, but only to attack and reject the notion.—*Handbuch der Physiologie des Menschen.*

† Many controversies have arisen respecting the sounds sometimes emitted by animals after the section of the superior and recurrent laryngeal nerves; sounds which have been perhaps occasioned by the struggling of the animal causing a swelling of the neck and a mechanical contact of the vocal ligaments. However, without doubt, after the section of these nerves, voice, as a voluntary act, can no longer take place.

‡ We thus designate that part of the membrane which goes from the bottom of the vocal ligament, to the edge of the cricoid.

of the thyroid, reascend obliquely to the summit of the arytenoid, then diverge in order to form the sides of the ventricles, and then disappear in the aryteno-epiglottidean folds and even the under surface of the epiglottis. If we cut it away in successive layers, proceeding *from* the outside *to* the in, we reach a thick bundle of fibres, perfectly horizontal, which line the outer aspect of the vocal ligament, and which go from the anterior apophyses of the arytenoid to the re-entering angle of the thyroid*.

This bundle has its posterior half covered by the lateral crico-arytenoid muscle, and its anterior half by the diverging fibres which start from the thyroid. If we cut away the horizontal bundle in successive layers, we see that the fibres are not all of the same length; the most external fibres are the longest, and the succeeding ones get gradually shorter as they become more internal; but they all originate in the anterior cavity of the arytenoid, and the muscle is inserted in the manner above explained throughout the whole length of the vocal ligaments, the thyro-arytenoid portion of it excepted. As the fibres all begin from the arytenoid, and terminate successively at more distant points of the membrane, we see that the muscle is thicker behind than before.

Thus the vocal ligament, and the membrane which depends from it, the sole sources of all vocal sounds, are under the direct action of the fibres which come from the anterior cavity of the arytenoid; the ligament under the action of the horizontal bundle, the membrane under that of the oblique fibres. The long horizontal fibres, extending from one cartilage to the other, are placed at the exterior of the short horizontal fibres, and at the interior of the oblique fibres. The diverging fibres which start from the thyroid, acting only on the superior vocal ligaments and the folds, seem to influence by their contractions only the quality and volume of the voice.

The remarkable arrangement of the fibres which we have just examined, enables us to explain a fundamental fact,—the elevation of the voice. The fibres of the horizontal bundle being placed over each other, in layers, one covering the other, and getting gradually longer and longer, as they become more external, extend their action to the more anterior parts of the edges of the glottis. This progressive action from the back to the front, encroaches gradually on the length of the vibrating portion of the ligament, and likewise increases its tension, and its faculty of accelerating its pulsations.

Another portion of the thyro-arytenoid muscle at the same time stretches and raises the vocal membrane more and more, causing a lesser depth of the ligaments to be in contact, in proportion as the sounds become higher, and thus assists by increasing the mobility of the ligaments.

We shall see in a few moments that the rotatory movement, which the external fibres of the lateral crico-arytenoid muscles give to the arytenoid, by making the vocal membrane deeper, partly counteracts the above effect, and produces the chest-register.

The crico-thyroid muscle, on the contrary, is a powerful auxiliary in the elevation of the voice. This muscle, which at the same time

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* Another portion of the thyro-arytenoid muscle.

causes the thyroid to come forwards and downwards, gives rise to a mechanical tension, not only in the vocal ligament, but even in the whole vocal membrane. The meeting of the thyroid and cricoid cartilages, which we can feel by the touch, becomes especially marked when the inter-ligamentous glottis alone produces the sounds, which takes place as we have seen at the notes do$_3$♯, ré$_3$,  in the chest-register, and an octave above for that of the head; with this difference, however, that for the latter a more vigorous and complete connexion is necessary.

Let us now see what we may learn from the sensations we feel in the vocal organ. When we produce a chest-note, the least attention enables us to distinguish a "*pinching*" at the posterior part of the glottis, which becomes more vigorous as the notes ascend. This pinching seems to be formed by extension of the depth of the touching surfaces, and may become very painful; whilst the notes of falsetto, when higher than chest ones, give comparatively great relief to this part, and the surfaces in contact seem to have become thinner.

If we combine these sensations with the different remarks which have been furnished to us by the examination of the muscles, we can fix the particular mechanism of each register.

Chest Register.

In fact, when the arytenoid muscles have brought in contact the arytenoid cartilages, and closed the glottis, the voice may take two very different characters; nay, more, it will be produced in pitches widely apart from one another, and will give forth the chest, or falsetto registers, according as the fibres of the thyro-arytenoid attached to the vocal membrane are active or not. By the action of these fibres, as we have seen, this muscle raises the vocal membrane, and makes its apposable part thinner; whereas the lateral crico-arytenoid gives a rotatory movement to the cartilage, which brings the apophyses into deep contact. This deep contact, which continues even after the apophyses no longer partake in the vibrations, gives a deep tension to the membranes, increases the depth of their contact*, and, as a necessary consequence, augments the resistance they present to the air. It is to the extent of this resistance that we attribute the formation of the chest-register, so distinct by its particular amplitude. To it we attribute also the slowness of the beats of the glottis, and the consequent low pitch of the sounds, a pitch which, even in the highest tenor voices, is at least an octave lower than the head notes of ordinary soprani.

Register of Falsetto.

When, on the contrary, the external fibres of the lateral crico-arytenoid muscle remain inactive, we produce the falsetto. The lips of the glottis, stretched by the horizontal bundle of the thyro-arytenoid, come in contact by their edge alone, formed at once by the ligament and the apophyses, and offer little resistance to the air. Hence arises the great loss of this agent, and the general weakness of the sounds produced here.

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* It is then that we feel the pinching of which we have spoken.

But as soon as we reach the sound do$_4$, the beats are produced by the ligaments exclusively, and we have attained the head-register. It is certain, as we may deduce from the movement of the ligaments, that then the vocal membrane is raised by the action of the fibres of the thyro-arytenoid muscle, and its surface is diminished to an edge; but we think that the external fibres of the lateral crico-arytenoid, which would prevent this movement, remain inactive. Then also the very decided tension, which the crico-thyroid muscle effects on the vocal tendons, and which accelerates their movements, takes place.

During the chest-register, therefore, the vocal ligaments are stretched, and are in contact to an extent corresponding with the depth of the anterior apophyses of the arytenoid, whilst in the falsetto the edges alone of the ligaments are stretched and apposed; in both cases the sounds being formed, not by the actual vibrations of either the whole or part of the tendons, but by the successive explosions which they allow.

Pressure of the Air.

Until now, in our remarks on the manner in which the voice is formed, we have only referred to the rigidity of the glottis, a rigidity necessary to accomplish the 1056 vibrations in one second*, which form the do$_4$ of the chest-voice, and to accomplish the double number which produces the octave above in the head-voice. There is, notwithstanding, another indispensable element for the production of vocal sounds, the pressure of the air. Pressure, as is well known, developes an elastic force in this agent, in a degree inverse to the volume which it occupies. It is by means of this power that the intensity of the sounds is obtained. The intensity of the sound can only depend on the quantity of air which goes to each *sharp* explosion. I say *sharp* explosion, as an express condition: the glottis should close itself perfectly after every vibration; for if the air found a constant passage, as in the notes of falsetto, then the greatest movements of the glottis, and the greatest waste of air, would produce precisely the weakest notes. To reject this theory would be to attribute the intensity of the sound to the extent of the vibrations accomplished by the lips of the glottis, and to suppose that these lips, each taken separately, possess the power of producing sounds, suppositions quite contrary to the facts.

The elastic force of the air arises not only from the compression of the lungs, but also from the contractions of the trachea, which adjusts its calibre to the different dimensions of the glottis. It is by means of this force that the air conquers the continually-increasing obstacle presented by the lips of the glottis when they produce sounds more and more intense.

Thus the problem of the elevation of the voice, always complicated with that of its intensity, in order to be complete, ought to show the connexion which exists between the tension of the lips of the glottis, the pressure of the air, and the number and intensity of the explosions obtained. As a consequence, we may state that the

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* Pouillet, *Physique*, Sixth Edition, vol. ii. page 77.

greater pressure of air necessary to produce the greater intensity, would at the same time increase the number of pulsations, and so raise the tone; but to prevent this, the glottis must at the same time be lengthened, and *vice versâ*; or, in other words, that the different lengths of the glottis can, under different degrees of pressure, produce the same number of shocks, but at different degrees of intensity.

Of the Qualities of the Voice.

Various simultaneous causes modify the qualities of the voice:—1, according as the glottis partially or entirely closes the passage between the explosions, it produces veiled or brilliant sounds; 2, the tube which surmounts and surrounds it also greatly affects the quality of the voice; by its contractions it gives brilliancy to it and its widening volume; 3, the epiglottis also plays a very important part, for every time that it lowers itself, and nearly closes the orifice of the larynx, the voice gains in brilliancy; and when, on the other hand, it is drawn up, the voice immediately becomes veiled.

ROYAL INSTITUTION OF GREAT BRITAIN.

June 1, 1855.—"On the Currents of the Leyden Battery." By Professor Tyndall, F.R.S.

In our conceptions and reasonings regarding the forces of nature, we perpetually make use of symbols, which, when they possess a high representative value, we dignify with the name of theories. We observe, for example, heat propagating itself through a bar of metal, and help ourselves to a conception of the process by comparing it with water percolating through sand, or travelling by capillary attraction through a lump of sugar. In some such way we arrive at what is called the material theory of heat. The thing seen is thus applied to the interpretation of the thing unseen, and the longing of the human mind to rest upon a satisfactory reason, is in some measure satisfied. So also as regards the subject of the present evening's discourse; we are not content with the mere facts of electricity; we wish to look behind the fact, and, prompted by certain analogies, we ascribe electrical phænomena to the action of a peculiar fluid. Such conceptions have their advantages and their disadvantages: they afford peaceful lodging to the intellect for a time, but they also circumscribe it; and by and by, when the mind has grown too large for its mansion, it often finds a difficulty in breaking down the walls of what has become its prison instead of its home. Thus, at the present day, the man who would cross the bounds which at present limit our knowledge of electricity and magnetism finds it a work of extreme difficulty to regard facts in their simplicity, or to rid them of those hypothetical adornments with which common consent has long invested them.

But though such is the experience of the earnest student of Natural Philosophy at the present—though he may be compelled to refuse his assent to the prevalent theoretic notions, he may nevertheless advantageously make use of the language of these theories in bringing the facts of a science before a public audience; and in speaking of electricity, the speaker availed himself of the convenient hypothesis of two fluids, without at all professing a belief in their existence. A Leyden jar was charged. The interior of the jar might be figured as covered with a layer of positive electricity, and the exterior by a layer of negative electricity; which two electricities, notwithstanding their mutual attraction, were prevented from rushing together by the glass between them. When the exterior and interior coating are united by a conducting body, the fluids move through the conductor and unite; thus producing what is called an electric current. The mysterious agent which we darkly recognise under this symbol is capable of producing wonderful effects; but one of its most miraculous characteristics is its power of arousing a transitory current in a conductor placed near it. The phænomena of voltaic induction are well known; and it is interesting to inquire whether frictional electricity produces analogous phænomena. This question has been examined by Dr. Henry, and still more recently by that able and experienced electrician M. Riess, of Berlin.

A wooden cylinder was taken, round which two copper wires, each 75 feet in length, were wound; both wires being placed upon a surface of gutta-percha, and kept perfectly insulated from each other. The ends of one of these wires were connected with a universal discharger, whose knobs were placed within a quarter of an inch of each other; when the current of a Leyden battery was sent through the other wire, a secondary current was aroused in that connected with the discharger, which announced itself by a brilliant spark across the space separating the two knobs.

The wires here used were covered externally with a sheet of gutta-percha; and lest it should be supposed that a portion of the electricity of the battery had sprung from one wire to the other, two flat discs were taken. Each disc contained 75 feet of copper wire, wound in the form of a flat spiral, the successive convolutions of which were about two lines apart. One disc was placed upon the other one, the wire being so coiled that the convolutions of each disc constituted, so to say, the impress of those of the other, and the coils were separated from each other by a plate of varnished glass. The ends of one spiral were connected with the universal discharger, between whose knobs a thin platinum wire, 10 inches long, was stretched. When the current of the Leyden battery was sent through the other spiral, the secondary current, evoked in the former, passed through the thin wire, and burnt it up with brilliant deflagration. A pair of spirals were next placed 6 inches apart, and a battery was discharged through one of them; the current aroused in the other was sufficient to deflagrate a thin platinum wire 4 inches in length.

We have every reason to suppose that the secondary current thus developed is of the same nature as the primary which produced it; and hence we may infer, that if we conduct the secondary away and carry it through a second spiral, it, in its turn, will act the part of a primary, and evoke a *tertiary* current in a spiral brought near it. This was illustrated by experiment. First, two spirals were placed opposite to each other, through one of which the current of the battery was to be sent; the other was that in which the secondary current was to be aroused. The ends of the latter were connected by wires with a third spiral placed at a distance, so that when the secondary current was excited it passes through the third spiral. Underneath the latter, and separated from it by a sheet of varnished glass, was a fourth spiral, whose two ends were connected with the universal discharger, between the knobs of which a quantity of gun-cotton was placed. When the battery was discharged through the first spiral, a secondary current was aroused in the second spiral, which completed its circuit by passing through the third spiral: here the secondary acted upon the spiral underneath, developed a tertiary current which was sufficiently strong to pass between the knobs, and to ignite the gun-cotton in its passage. It was shown that we might proceed in this way and cause the tertiary to excite a current of the fourth order, the latter a current of the fifth order, and so on; these children, grandchildren, and great grandchildren of the primary being capable of producing all the effects of their wonderful progenitor.

The phænomena of the *extra current*, which exists for an instant contemporaneously with the ordinary current in a common voltaic spiral, were next exhibited; and the question whether a spiral through which a Leyden battery was discharged exhibited any similar phænomena, was submitted to examination. It was proved, that the electric discharge depended upon the *shape* of the circuit through which it passed: when two portions of such a circuit are brought near each other, so that the positive electricity passes in the same direction through both of them, the effect is that the discharge is *weaker* than if sent through a straight wire; if, on the contrary, the current flow through both portions in opposite directions, the discharge is *stronger* than if it had passed through a straight wire. A flat spiral was taken, containing 75 feet of copper wire; one end of the spiral was connected with a knob of the universal discharger, and the other knob was connected with the earth: between the knobs of the discharger about 4 inches of platinum wire were stretched; on connecting the other end of the spiral with the battery, a discharge passed through it of such a strength that it was quite unable to raise the platinum wire to the faintest glow. The same length of copper wire was then bent to and fro in a zigzag manner, so that on every two adjacent legs of the zigzag the current from the battery flowed in opposite directions. When these 75 feet of wire were interposed between the battery and the platinum wire, a discharge precisely equal to that used in the former instance, raised the platinum wire to a high state of incandescence, and indeed could be made to destroy it altogether.

When a primary and a secondary spiral are placed opposite to each other, a peculiar reaction of the secondary upon the primary is observed. If the ends of a secondary (50 feet long) be connected by a thick wire, the effect upon the primary current is the same as when the ends of the secondary remain wholly unconnected. If the ends of the secondary be joined by a long thin platinum wire, the reaction of the secondary is such as to enfeeble the primary.

This enfeeblement increases up to a certain limit as the resistance is increased, from which forwards it diminishes until it becomes insensible. This would appear to prove, that to react upon the primary the secondary requires to be retarded; and that the greater the amount of the retardation, up to a certain limit, the greater is the enfeeblement. But by increasing the resistance we diminish the strength of the secondary, and when a certain limit is attained, this diminution is first compensated for by the influence of retardation, from which point forwards with every increase of the resistance, the enfeeblement of the primary is diminished. A primary current which fuses a certain length of platinum wire where the ends of the secondary are disunited, or where they are united by a thick wire, fails to do so when they are united with a thin wire. But if, instead of a thin wire, a body of much greater resistance, a column of water for example, be introduced, the platinum wire is fused as before.

XXVIII. *Intelligence and Miscellaneous Articles.*

ON TABASHEER. BY M. GUIBOURT.

TABASHEER is a siliceous concretion found in the interior of the stem of the large Indian Bamboo (*Bambusa arundinacea*, Schreb.). The attention of the orientals was first directed to this substance by the writings of the Arabian physicians. The Turks and Arabs only know it under the name of Tabasheer, which is of Persian origin. In India it has other names, such as *Vedroo-paloo* (bamboo-milk), *Vedroo-carpooram* (bamboo-camphor), and *Mangil-upoo* (bamboo-salt). The orientals regard it as one of the most valuable medicines, whilst it is only interesting to us from its being a concretion of silica deposited in a vegetable organism, which has some resemblance to the hydrophane of Haüy.

Towards the end of the last century, Macie stated that tabasheer was nothing but silica. As, however, Vauquelin found it to contain 30 per cent. of potash and lime, Macie's determination was regarded with doubt. Brewster has examined its optical and physical properties.

The author has received a quantity of this substance from Professor Royle, and submitted it again to analysis. The analysis gave—

Silica	96·94
Water	2·93
Potash and lime	0·13
Organic matter	traces

Tabasheer is imperfectly transparent, and sometimes perfectly opake. When immersed in water a quantity of air-bubbles escape from it, and it becomes more transparent; hydrophane behaves in the same manner. When completely saturated with water its spec. grav. is as follows:—

2·158, Macie; 2·169, Cavendish; 2·059 to 2·412, Brewster; and 2·148, Guibourt.

When tabasheer is weighed after soaking in water, it is found to have more than doubled its weight. The statements put forward upon this point are as follows:—

	Weight.	Increased to.	Absorption.
Macie	75·70 grms.	154·20 grms.	79·50 grms. water.
Brewster..	6·65 ..	14·10 ..	7·45 ..
Guibourt..	2·526 ..	5·224 ..	2·695 ..

If we calculate from these numbers the density of the porous tabasheer, by adding the weight of the absorbed water (by which the volume of the pores is determined) to the loss of weight undergone by the body in water (which gives the volume of the solid matter), and then divide the sum by the weight of the dry body, so as to obtain the density of the porous body in the natural state, we get 0·727, 0·6227, 0·6520.

But the tabasheers are heavier than water; they sink in it immediately, and have consequently a greater specific gravity than that thus calculated. The author shows that Brewster observed this contradiction, but without troubling himself about the cause, determined the space occupied by the silica and the pores from this incorrect specific gravity, so that the space occupied by the pores in proportion to that filled by the silica was, according to him, as 2·307 : 1; whilst, from the author's experiments upon the natural density of tabasheer, it appears that this proportion is 0·0689 : 1, which agrees with the behaviour of the body in water.

Tabasheer which has been completely permeated by water and become transparent, becomes perfectly opake in the air. This behaviour, which Brewster believed to occur only with this body, induced him to set up a peculiar theory upon the small refractive power of this substance, which he found to be less than that of any other known body. But without taking into consideration that the number 976·10, which Brewster adopted for the refractive power of tabasheer, is too small, as the author calculates it at 1500·5, we must also take into account the opacity which is peculiar to the molecular state of this substance.

Tabasheer is evidently a mass produced from a jelly by desiccation. To obtain an insight into its production, the author has investigated the inorganic constituents of the bamboo. The small quantity of pith which is seen when a bamboo is split longitudinally, contains iron, potash, lime and silica. The ashes of the wood consist of—

Soluble salts.		Insoluble salts.	
Carbonate of potash..	1·9872	Phosphate of lime..	0·0928
Sulphate of potash ..	0·2905	Phosphate of iron..	0·0130
Phosphate of potash..	0·1593	Silica	0·0408
Chloride of potassium.	0·0766	Loss	0·0371
Silica	0·0204		0·1837
	2·5340		

The author found that the silica occurred in different quantities in different parts of the cane. The pith contained 0·448 per cent. The inner wood held much less, and the greatest proportion occurred in the external wood. Consequently, the author thinks the formation of tabasheer is easily explained in the following manner: at the time when the straw is developed, the outer wood has no longer any necessity for silica, which is carried inwards and deposited in the cavity of the straw.—*Journ. de Pharm.* vol. xxvii. pp. 61 and 252.

NOTE ON THE INFLAMMABILITY OF HYDROGEN.

BY M. BAUDRIMONT.

It is said by Berzelius and others, that hydrogen prepared in the dry way does not possess the property of inflaming by contact with spongy platinum; this phænomenon, first indicated by Faraday, having been attributed by Berzelius to an allotropic condition of the hydrogen, we have thought it worth while to repeat the experiment. For this purpose vapour of water was decomposed by red-hot iron, and the hydrogen thus produced (which is that said to be prepared in the dry way) was directed upon spongy platinum, which, as we had ascertained, possessed the power of igniting the ordinary gas. We then found that it also inflamed the gas obtained from the vapour of water. On collecting some of this same gas in a bottle with a ground glass stopper, and afterwards driving it out by means of a current of water, so as to direct it upon a small mass of spongy platinum, the hydrogen was again ignited. If, therefore, this gas may acquire various allotropic conditions, these cannot be proved by the fact advanced by Faraday, and which we have not been able to realise.—*Comptes Rendus*, August 5, 1855, p. 177.

METEOROLOGICAL OBSERVATIONS FOR JULY 1855.

Chiswick.—July 1. Very fine. 2. Heavy clouds: slight rain. 3, 4. Very fine. 5. Clear: hot and dry. 6. Foggy: very fine. 7, 8. Very fine. 9. Hazy: very fine: thunder, lightning and rain at night. 10. Fine: cloudy: very fine. 11. Constant and very heavy rain from early in the morning till night. 12, 13. Very fine. 14. Cloudy: very fine. 15. Cloudy and fine: rain. 16. Cloudy: heavy rain at night. 17. Fine: cloudy. 18. Very fine. 19. Rain. 20. Very fine. 21. Exceedingly fine. 22. Very fine. 23. Slight fog: sultry: rain. 24. Rain: cloudy. 25. Rain. 26. Densely overcast: constant and very heavy rain. 27. Overcast: heavy clouds and showers. 28. Cloudy: heavy showers. 29. Very fine. 30. Slight haze: very fine: overcast: rain at night. 31. Heavy rain: showery: clear and fine at night.

Mean temperature of the month	62°·99
Mean temperature of July 1854	61 ·59
Mean temperature of July for the last twenty-nine years	63 ·12
Average amount of rain in July	2·428 inches.

Boston.—July 1—4. Fine. 5—7. Cloudy. 8. Fine. 9. Cloudy. 10, 11. Cloudy: rain A.M. 12. Fine. 13. Cloudy. 14. Rain and hail A.M. 15. Fine. 16, 17. Cloudy: rain A.M. and P.M. 18. Cloudy. 19, 20. Cloudy: rain, with thunder A.M. and P.M. 21—23. Cloudy. 24, 25. Rain A.M. and P.M. 26. Fine: rain P.M. 27. Cloudy: rain P.M. 28. Fine: rain P.M. 29, 30. Fine. 31. Rain A.M. and P.M.

Sandwick Manse, Orkney.—July 1. Damp A.M.: showers P.M. 2. Showers A.M.: drops P.M. 3. Rain A.M.: damp P.M. 4. Bright A.M.: cloudy P.M. 5. Cloudy A.M.: fog P.M. 6. Bright, fine A.M.: clear, fine P.M. 7. Bright, fine A.M. and P.M. 8. Bright, fine A.M.: clear, fine P.M. 9. Clear, fine A.M.: vapour, fine P.M. 10. Hazy A.M.: clear, fine P.M. 11. Fine A.M. and P.M. 12. Bright, fine A.M.: fine, fog P.M. 13. Fog A.M.: fine, fog P.M. 14. Fog A.M.: thunder showers, fog P.M. 15. Fog A.M.: showers P.M. 16. Rain A.M.: showers P.M. 17. Damp A.M.: cloudy P.M. 18. Cloudy A.M. and P.M. 19. Bright A.M.: hazy, fine P.M. 20. Bright A.M.: clear, fine P.M. 21. Clear A.M.: drops P.M. 22. Cloudy A.M. and P.M. 23. Cloudy A.M.: showers P.M. 24. Bright A.M.: fine, fog P.M. 25. Cloudy A.M.: fine, fog P.M. 26. Rain A.M.: fog P.M. 27. Rain, fog A.M.: showers, thunder and lightning, clear P.M. 28. Bright A.M.: fine, drops P.M. 29. Rain A.M.: damp P.M. 30. Cloudy A.M.: fine, vapour P.M. 31. Cloudy A.M.: fine P.M.

Mean temperature of July for twenty-eight previous years	55°·08
Mean temperature of this month	59 ·19
Mean temperature of July 1854	55 ·25
Average quantity of rain in July for fifteen previous years	2·41 inches.

Meteorological Observations made by Mr. Thompson *at the Garden of the Horticultural Society at* CHISWICK, *near London;*
by Mr. Veall, *at* BOSTON; *and by the* Rev. C. Clouston, *at Sandwick Manse,* ORKNEY.

Days of Month.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
1855. July.	Max.	Min.		9¼ a.m.	8½ p.m.	Max.	Min.		9¼ a.m.	8½ p.m.						
1.	30·163	30·161	29·52	29·78	29·96	76	60	68	59	53	sw.	ssw.	wsw.	....	....	·32
2.	30·193	30·153	29·58	29·97	29·97	78	57	72·5	55	53	sw.	ssw.	w.	·01	....	·17
3.	30·181	30·082	29·63	30·00	30·10	79	56	61	52	52½	n.	nw.	nw.	....	....	·08
4.	30·100	30·091	29·58	30·18	30·18	77	37	64	55½	52	n.	ne.	nne.	....	....	....
5.	30·121	30·038	29·63	30·14	30·09	81	38	57·5	52½	50½	ne.	nnw.	e.	....	....	....
☾ 6.	30·045	30·027	29·62	30·07	30·07	77	44	59·5	56½	54	w.	n.	e.	....	....	....
7.	30·079	30·064	29·64	30·08	30·07	74	50	64	67	59	e.	nne.	se.	....	....	....
8.	30·025	29·867	29·55	30·04	29·97	79	55	65	68½	57½	se.	e.	se.	....	....	....
9.	29·775	29·599	29·43	29·89	29·85	80	59	57	64	53	e.	e.	e.	·14	....	....
10.	29·608	29·585	29·13	29·79	29·77	82	59	73	57	54	sw.	s.	ese.	·48	·05	....
11.	29·541	29·516	29·12	29·77	29·83	71	55	58·5	70½	60	n.	n.	se.	1·07	·40	....
12.	29·840	29·747	29·27	29·83	29·90	81	52	66	74½	60	nw.	n.	nw.	....	....	....
13.	29·959	29·931	29·44	29·92	29·87	79	55	71	58	55	sw.	sw.	ese.	....	....	....
● 14.	29·858	29·822	29·33	29·82	29·70	76	53	61	66	57	sw.	nne.	sse.	·29	·10	....
15.	29·919	29·772	29·34	29·67	29·70	75	55	68	61	56	sw.	ssw.	nw.	·41	·40	·01
16.	29·617	29·464	29·10	29·59	29·58	73	46	68	59½	57	sw.	s.	ne.	·40	....	·12
17.	29·648	29·543	29·00	29·61	29·65	66	51	58	58	56	sw.	w.	n.	....	1·35	·05
18.	29·744	29·724	29·20	29·62	29·59	76	46	65	58	56	sw.	w.	......	·01	·13	....
19.	29·654	29·567	29·12	29·55	29·56	64	46	60	58	55	s.	s.	......	·36	....	....
20.	29·951	29·745	29·22	29·71	29·92	73	41	62·5	60	55	w.	nnw.	......	....	·28	....
21.	30·107	30·049	29·58	29·92	29·91	80	45	59	62	60	w.	nw.	s.	....	·16	....
☽ 22.	30·156	30·114	29·62	29·95	30·04	78	52	72	70	62	w.	w.	se.	....	....	....
23.	30·020	29·765	29·47	29·98	29·80	83	59	75	72	62½	sw.	se.	se.	·68	....	....
24.	29·696	29·606	29·10	29·70	29·71	71	52	67	65	62	sw.	sw.	e.	·14	·10	·15
25.	29·716	29·672	29·13	29·76	29·82	73	49	63·5	65	60	sw.	ene.	e.	·20	·21	....
26.	29·777	29·720	29·28	29·82	29·82	63	50	63	60	60	s.	sw.	e.	1·22	·26	·14
27.	29·842	29·782	29·29	29·62	29·78	75	49	64	60	59	sw.	sw.	ese.	·45	·29	·78
28.	29·876	29·870	29·34	29·83	29·83	72	50	64	65½	58	sw.	ssw.	w.	·06	·19	·03
○ 29.	29·927	29·899	29·34	29·68	29·79	77	53	63	58	55½	w.	w.	se.	....	·03	1·08
30.	29·872	29·828	29·34	29·87	29·83	75	55	64	63	56	sw.	nw.	se.	·24	....	·04
31.	29·750	29·736	29·20	29·76	29·70	71	42	65	61	58	sw.	w.	e.	·14	·15	....
Mean.	29·895	29·823	29·36	29·836	29·850	75·32	50·67	64·5	61·67	56·72				6·30	4·10	2·97

THE
LONDON, EDINBURGH AND DUBLIN
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AND
JOURNAL OF SCIENCE.

[FOURTH SERIES.]

OCTOBER 1855.

XXIX. *On a new and advantageous Method of preparing Aluminium.* By H. Rose, *Professor of Chemistry in the University of Berlin*[*].

SINCE the discovery of aluminium by Wöhler, Deville has recently taught us the means of procuring this metal in large solid masses, in which condition it exhibits properties with which we were previously unacquainted in its more pulverulent form as procured by Wöhler's method. While, for instance, in the latter state it burns vividly to white earthy alumina on being ignited, the fused globules may be heated to redness without being perceptibly oxidized. The differences may be ascribed to the greater amount of division on the one hand and of density on the other. According to Deville, however, Wöhler's metal contains platina, by which he explains its difficulty of fusion (although it affords white alumina by combustion).

Upon the publication of Deville's researches, I also tried to procure aluminium by the decomposition of the chloride of aluminium and sodium by means of sodium. I did not follow exactly the method of Deville, but placed the salt in alternate layers with sodium and applied heat. I did not, however, obtain satisfactory results.

Moreover, Professor Rammelsberg, who followed exactly the method of Deville, obtained but a very small product, and found it very difficult to prevent the cracking of the glass tube in which the experiment was conducted by the action of the vapour of sodium on the chloride of aluminium. It appeared to me that a great amount of time, trouble and expense, as well as long practice, was necessary to obtain even small quantities of this remarkable metal.

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[*] From Poggendorff's *Annalen* for September, p. 152. Communicated by T. H. Henry, Esq., F.R.S.

The employment of chloride of aluminium and its compounds with the chlorides of alkaline metals is particularly inconvenient, owing to their volatility, to their deliquescence, and to the necessity of preventing all access of air during their treatment with sodium.

It very soon occurred to me that it would be better to use the fluoride of aluminium instead of the chloride, or rather the combinations of the fluoride with the alkaline fluorides, such as we know them through the investigations of Berzelius, who pointed out the strong affinity of fluoride of aluminium for the fluorides of sodium and potassium, and that the mineral occurring in nature under the name of kryolite, was a pure compound of fluoride of aluminium with fluoride of sodium.

This compound is equally fitted for the preparation of aluminium by means of sodium with the chloride of aluminium, or its combination with chloride of sodium. Moreover, as kryolite is not volatile, is readily reduced to the most minute state of division, is free from water and does not attract moisture from the atmosphere, it offers peculiar advantages over the above-named compounds*.

In fact, I succeeded with much less trouble in preparing aluminium by exposing it together with sodium to a strong red heat in an iron crucible, than by using the chloride of aluminium and its compounds. The scarcity of the mineral, however, prevented my pursuing the experiments.

In consequence, however, of receiving very recently from Prof. Krantz of Bonn a considerable quantity of the purest kryolite at a very moderate price (2 dollars the kilogramme), I was enabled to renew the investigation. I was particularly stimulated by finding most unexpectedly that kryolite was to be obtained here in Berlin commercially at an inconceivably low price.

Prof. Krantz had already informed me that he had heard that kryolite occurred in commerce in bulk, but could not learn where. Shortly after, M. Rudel, the manager of the chemical works of H. Kunheim, gave me a sample of a white coarse powder, large quantities of which were brought from Greenland by way of Copenhagen to Stettin under the name of *mineral soda,* and at the price of *three dollars the centner* (about 9*s*. per cwt.). Samples of 40 lbs. had been sent to the soap-boilers, and a soda ley had been extracted from it by means of quicklime, especially adapted to the preparation of many kinds of soap, probably from its containing alumina.

I found this powder to be kryolite, of equal purity to that received from M. Krantz. It dissolved without residue in hy-

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* Dr. Percy also suggested kryolite as a source of aluminium, and some specimens prepared from this mineral were exhibited by Dr. Faraday at one of the Friday evening meetings of the Royal Institution last season.—T. H. H.

drochloric acid (in platina vessels); the solution evaporated to dryness with sulphuric acid and heated until the excess of acid was dissipated, gave a residue which dissolved completely in water with the aid of a little hydrochloric acid. From the solution ammonia precipitated a considerable quantity of alumina. The solution filtered from the precipitate furnished, on evaporation, a residue of sulphate of soda free from potash. Moreover, the powder gave the well-known reactions of fluorine in a marked degree.

This powder is kryolite of great purity; therefore, the coarse powder I first obtained was not the form in which it is originally introduced. It is now to be obtained in Berlin in great masses; for the preparation of aluminium, however, it must be reduced to a very fine powder.

In my experiments on the preparation of this metal, which were performed in company with M. Weber and with his most zealous assistance, I made use of small iron crucibles of 1$\frac{3}{4}$ inch in height, and 1$\frac{3}{8}$ inch upper diameter, which I had cast here. In these I placed the finely-powdered kryolite between thin layers of sodium, pressed it down tight, covered it with a good layer of chloride of potassium, and closed the crucible with a well-fitting porcelain cover.

I found chloride of potassium the most advantageous flux to employ; it has the lightest specific gravity of any which could be used, an important point when the slight density of aluminium is taken into consideration; it also increases the fusibility of the fluoride of sodium. I usually employed equal weights of kryolite and chloride of potassium, and for five parts of kryolite I employed two parts of sodium; the most fitting quantity for the crucible was found to be 10 grms. of powdered kryolite. The whole was raised to a strong red heat, by means of a blowpipe supplied with a mixture of atmospheric air and coal-gas, through tubes, on the principle of Daniel's tube for the oxy-hydrogen blowpipe.

It was found most advantageous to maintain the heat for about half an hour and not longer, the crucible being kept closely covered the whole time; the contents are then found to be well fused. When quite cold, the melted mass is removed from the crucible by means of a spatula; this is facilitated by striking the outside with a hammer. The crucible may be employed several times; at last it is broken by the blows from the hammer.

The melted mass is treated with water, when at times only, a very minute evolution of hydrogen gas is observed, which has the same unpleasant odour as the gas evolved during the solution of cast iron in muriatic acid. The carbon contained in this gas is derived from the very slight trace of naphtha adhering to the sodium after drying it. On account of the difficult solubility of the fluoride of sodium, the mass is very slowly acted upon by the water, although the insolubility is somewhat diminished by the presence of the chloride of potassium. After twelve hours the mass has become so far softened that it may be removed from the liquid and broken down in a porcelain mortar. Large globules of aluminium are then discovered, weighing from 0·3 to 0·4 grm. (5 to 6 grains), which may be separated; latterly I have found them of 0·5 grm. (7·7 grs.).

The smaller globules cannot well be separated from the undecomposed kryolite and the alumina always produced by washing, owing to their being specifically lighter than the latter. The whole is treated with diluted nitric acid in the cold; the alumina is not dissolved thereby, but the little globules then first assume their true metallic lustre. They are dried and rubbed on fine silk muslin, the finely-powdered undecomposed kryolite and alumina pass through, while the globules remain on the gauze.

The melted mass should be treated with water in a platinum or silver vessel; a porcelain capsule would be powerfully acted upon by the fluoride of sodium. The solution, after standing till clear, may be evaporated to dryness in a platinum capsule in order to obtain the fluoride of sodium, mixed, however, with much chloride of potassium.

The small globules of aluminium may be united by fusion in a small, well-covered porcelain crucible under a layer of chloride of potassium. They cannot be united without a flux. They cannot be united by mere fusion, like globules of silver for instance, for though they do not appear to oxidize on ignition in the air, they become coated with a scarcely perceptible film of oxide which prevents their running together into a mass. This fusion with chloride of potassium is always attended with a loss of aluminium. A button weighing 3·85 grms. lost, when so treated, 0·05 grm. The chloride of potassium when dissolved in water, left a small quantity of alumina undissolved, but the solution contained none. Another portion of the aluminium had undoubtedly decomposed chloride of potassium, and a portion of chloride of aluminium and chloride of potassium must have been volatilized during the fusion. Other metals, as copper and silver*, behave in a similar manner.

I therefore followed the instructions of Deville, and melted the globules under a stratum of chloride of aluminium and sodium in a covered porcelain crucible. The salt was first melted, and then the globules of metal added to the melted mass. There is no loss, or a very trifling one of a few milligrams of metal, by this process.

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* Poggendorff's *Annalen*, vol. lxviii. p. 287.

When the aluminium is fused under chloride of potassium, its surface is not perfectly smooth, but exhibits minute concavities; with chloride of aluminium and sodium, this is not the case.

The readiest method of preparing the chloride of aluminium and sodium for this purpose, is by placing the mixture of alumina and carbon in a glass tube as wide as possible, and in this a tube of less diameter, open at both ends, and containing chloride of sodium; if the spot where the mixture is placed be very strongly heated, and that where the chloride of sodium is situated more moderately, while a current of chlorine is passed through the tube, the vapour of the chloride of aluminium is so eagerly absorbed by the chloride of sodium, that no chloride of aluminium, or at most a trace, is deposited in any other part of the tube.

If the smaller tube be weighed before the operation, the amount absorbed is readily determined. It is not uniformly combined with the chloride of sodium, for that part which is nearest to the mixture of the charcoal and alumina will be found to have absorbed the most.

I have varied in many ways the process for the preparation of aluminium, but in the end have returned to the one described. I often placed the sodium at the bottom of the crucible, the powdered kryolite above it, and then the chloride of potassium above all. On proceeding in this manner, it was observed that much sodium was volatilized, burning with a strong yellow flame, which never occurred when the sodium was cut into thin slices and placed in alternate layers with the kryolite, in which case the process goes on very quietly. When the crucible begins to get red-hot, the temperature suddenly rises, owing to the commencement of the decomposition of the compound; no lowering of the temperature should be allowed, but the heat should be steadily maintained, not longer, however, than half an hour. By prolonging the process a loss would be sustained, owing to the action of the chloride of potassium on the aluminium. Nor does the size of the globules increase on extending the time even to two hours; this effect can only be produced by obtaining the highest possible temperature. If the process be stopped, however, after five or ten minutes of a very strong heat, the produce is very small, as the metal has not had sufficient time to conglomerate into globules, but is in a pulverulent form and burns to alumina during the cooling of the crucible. No advantage is gained by mixing the kryolite with a portion of the chloride before placing it between the layers of sodium. Neither did I increase the product by using the chloride of sodium and aluminium to cover the mixture instead of the chloride of potassium.

I repeatedly employed chloride of sodium (common salt decrepitated) as a flux in the absence of chloride of potassium without remarking any important difference in the amount of metal produced, although a higher temperature is required in that case.

The operation may also be conducted in unglazed crucibles of difficultly fusible stone-ware of the above dimensions, although they do not resist the action of fluoride of sodium at very high temperatures so well, but fuse in one or more places. The iron crucibles fuse, however, when exposed to a very high temperature in a charcoal fire.

The produce in metal was found to vary much, even when operating exactly in the manner recommended, and with the same quantities of the materials. I never succeeded in reducing the whole amount of metal contained in the kryolite; it contains only 13 per cent. of aluminium; by operating on 10 grms. of the mineral, the quantity I always employed in the small iron crucibles, the most successful result was 0·8 grm. But 0·6 grm., or even 0·4 grm. may be considered favourable. Many times I obtained only 0·3 grm., or even less.

These very different results depend on various causes, more particularly, however, upon the degree of heat obtained. The greater the heat, the greater the amount of larger globules, and the less amount of minutely divided aluminium to oxidise during the cooling of the crucible. I succeeded once or twice in reducing nearly the whole of the metal obtained to one single button, weighing 0·5 grm. at a very high temperature in a stoneware crucible.

I could not always attain the same heat with the blowpipe, as it depended in some degree upon the pressure in the gasometer at the gas-works, which varies at different periods of the day. The following experiment will show how great the loss of aluminium may be owing to the oxidation of the metal during the slow cooling of the crucible and contents.

In a large iron crucible were placed 35 grms. of kryolite in alternate layers with 14 grms. of sodium, and the whole covered with a thick stratum of chloride of potassium. The crucible closed with a porcelain cover, was placed in a larger earthen one also covered, and the whole exposed to a good heat in a draft furnace for one hour, and cooled as slowly as possible. The produce in this case was remarkably small, for 0·135 grm. of aluminium was all that could be obtained in globules.

The difference in the amount reduced depends also in some degree on the more or less successful stratification of the sodium with the powdered kryolite, as much of the latter sometimes escapes decomposition. The greater the amount of sodium employed, the less likely is this to be the case; however, owing to the great difference in the price, I never employed more than 4 grms. of sodium to 10 grms. of kryolite. In order to avoid this loss by oxidation during cooling, I tried another method of preparation.

20 grms. of kryolite were heated intensely in a gun-barrel in a current of hydrogen, and then the vapour of 8 grms. of sodium passed over it. This was effected simply by placing the sodium in a little iron tray in a part of the gun-barrel, without the fire, and pushed forward when the kryolite had attained the maximum temperature. The operation went on very well, the whole being allowed to cool in the current of hydrogen. After the treatment with water, in which the fluoride of sodium dissolved very slowly, I obtained a black powder, consisting for the most part of iron; its solution in hydrochloric acid gave small evidence of alumina.

The small amounts I obtained, however, should not deter others from pursuing these experiments. These are the results of first experiments, on which I have not been able to expend much time. Now that kryolite can be procured at so very moderate a price, and sodium, by the improvement in the preparation of which Deville has performed a most important service, will in future become so much cheaper, it is in the power of every chemist to employ himself in the preparation of aluminium, and I have no doubt in a short time methods will be found affording a much more profitable result.

For the rest, I am of opinion that kryolite is the best adapted of all the compounds of aluminium for the preparation of this metal. It deserves the preference over chloride of aluminium and chloride of aluminium and sodium, and it might still be employed with great advantage even if its price were to rise considerably.

The attempts at preparing aluminium direct from alumina have as yet been unattended with success. Potassium and sodium appear only to reduce metallic oxides when the potash and soda produced are capable of forming a compound with a portion of the oxide remaining as such. Pure potash and soda, with whose properties we are very slightly acquainted, do not appear to be formed in this case. Since, however, alumina combines so readily with the alkalies to form aluminates, one would be inclined to believe that the reduction of alumina by the alkaline metals should succeed.

But even were it possible to obtain the metal direct from alumina, it is very probable that kryolite would long be preferred should it remain at a moderate price; for it is furnished by nature in a rare state of purity, and the aluminium is combined in this substance with sodium and fluorine only, which exercise no prejudicial influence on the preparation of the metal, whereas alumina is rarely found in nature in the pure state, and in a dense and compact condition; and to prepare alumina on a large scale, freeing it from those substances which would act injuriously on the preparation of the metal, would be attended with great difficulties.

The buttons of aluminium which I have prepared are so malleable that they may be beaten and rolled out into the finest foil without cracking on the edges; they have a strong metallic lustre. Some small pieces, not globular, however, which were found at the bottom of the crucible and occasionally adhering to it, cracked on being hammered, and were different in colour and lustre. They were evidently not so pure as the greater number of the globules and contained iron.

On sawing through a larger button of aluminium weighing 3·8 grms., it could readily be observed that the metal for about half a line from the exterior was brittle, while in the interior it was soft and malleable; sometimes the interior of the globules contained concavities.

With Deville I have occasionally obtained the aluminium crystalline. A large button became striated and crystalline on the under side on cooling. Deville believes he has observed regular octahedra, but does not state this positively. According to my brother's examination, these crystalline indications do not belong to the regular form.

As I chanced on one occasion to attempt the fusion of a large button of rather impure aluminium after flattening it out, without a flux, I observed before the heat was sufficient to fuse the mass small globules sweating out of the surface. The impure aluminium being more infusible than the pure metal, the latter expands on fusing, and comes to the surface. A similar phænomenon has been remarked by Schneider with impure bismuth.

I have mentioned above that kryolite is used under the name of mineral soda here in Berlin, for the preparation of soda-ley, which, on account of the alumina it contains, is admirably adapted to the manufacture of soap. In fact, powdered kryolite is completely decomposed when boiled with quicklime and water. The fluoride of calcium formed contains no alumina, which is all dissolved in the caustic soda solution, and this, on its side, is free from fluorine or contains only a very minute trace.

XXX. *On Alloys.* *By* F. Crace Calvert, *F.C.S., M.R.A. of Turin, &c.; and* Richard Johnson, *M.Ph.S. Manchester*[*].

THERE are few subjects in chemistry which have been less examined than alloys, and yet when we reflect on the important services which they render to manufactures and the arts,

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[*] Communicated by the Authors.

we must come to the conclusion that all information which may throw light on their composition and properties must enhance their value and applications.

Alloys have been made, up to the present time, according to habit; and the proportions used to compose them have been the results of arbitrary judgements, instead of fixed and determined chemical proportions. It seemed to us probable, that as all the compounds of chemistry result from the combination of their elements in definite proportions, the same must occur with metals operated upon under circumstances which would allow them to combine freely. If, in ordinary circumstances, definite compounds are not formed, it is owing to the presence of an excess of one or more of the metals which enter into the composition of the alloys, and to the real definite compound being solidified in the mass of the metal in excess; just in the same way as when bismuth or sulphur are melted in a crucible and allowed to cool, as we all know, the well-defined crystals of bismuth or sulphur are immersed in the excess of bismuth or sulphur which has not been able to crystallize.

We were therefore inclined to believe, that by producing alloys having a definite composition, we should point the way to much cheaper and better alloys than hitherto; for there can be no doubt that the irregularity of the quality of the alloys of commerce is owing, not only to an excess of one of the metals, but also to a portion of this excess combining with a definite alloy and modifying the composition; thus, if it is a very fusible metal which is in excess, it will remain liquid and combine with the last portion of the real alloys, and form one which will have a different composition from that which was formed at first on the outside of the mass; whilst, on the contrary, if it is a less fusible metal which is used in excess, it will solidify before the predominant alloy of the mass of the casting and prevent its homogeneity. It is to avoid this serious evil that bronze cannons are cooled at the present day shortly after being cast, so as to keep the mass as uniform as possible; and the result has been, that instead of having, as formerly, one-third of the bronze pieces of ordnance defective, now only about one-tenth are so.

Before entering into the details of our researches, it is our duty to state, that during our experiments, Messrs. Levol, Rieffel, and Joule have published papers on some alloys and amalgams having definite proportions.

The alloys of gold and silver, of gold and copper, and of silver and lead, analysed by M. Levol, are very interesting, and many of them, we have no doubt, are definite compounds, as their respective equivalent proportions are low; but as to those analysed by M. Rieffel, we have doubts as to their definite composition, as some of them have the following constitution:—

1 equivalent of copper,
48 equivalents of tin,

or

98 equivalents of copper,
1 equivalent of tin.

Our object was not to examine a few isolated cases, or only the alloys existing in commerce, but to produce a great variety of new alloys, in definite proportions, and to examine their physical and chemical properties, &c.

The first class of alloys which we shall describe is not only quite new but highly interesting, as being the first alloys which have been prepared with iron in definite proportions. It is certainly remarkable, considering the low price of iron, that no efforts have been made to alloy this useful and cheap metal with those of a more expensive nature, such as alloys of iron and tin, or iron and copper, &c., which, if effected, would have given to commerce cheaper alloys than those now in use.

Our primary object in preparing the following alloys of iron, was to render this metal less oxidizable by alloying it with a more electro-positive metal than itself. We were convinced that if we effected this, it would be the most practical method of obtaining the long-looked for object of diminishing the injury which that valuable metal sustains by exposure to the atmosphere; but we were deceived in our anticipations, for the affinity of iron for oxygen is such that it is not diminished, except in one case, when combined with potassium or aluminium.

Alloys of Iron and Potassium.

The first trial we made consisted in heating to a very high temperature some bitartrate of potash and iron, mixed with excess of bitartrate of potash; the result was only a melted mass of carbonate of potash, with a button of cast iron, no doubt owing to the fact that the iron, being in a state of minute division, united first with carbon, and then was unable to unite with potassium.

We then made a mixture of fine iron filings and bitartrate of potash in the following proportions:—

12 equivs. of iron . . .	336 grs.	or 3 equivs. of iron,
8 equivs. bitartrate of potash,	1504 ...	2 ... potassium;

with a view to produce an alloy having the composition of the sesquioxide, and submitted the mixture to a high temperature in a crucible, and obtained a large button, which was found on analysis to be composed as follows:—

Iron	74·60
Potassium	25·40
	100·00

which leads to the following formula:—

4 equivalents of iron	112	=	74·17
1 equivalent of potassium	39	=	25·83
	151	=	100·00

Therefore, instead of obtaining an alloy having the definite proportions we had employed, we obtained one containing much more iron. This alloy had all the appearance of malleable iron, and could be forged and welded; but what was very extraordinary, and which much surprised us, was its extreme hardness, so much so that at natural temperatures it was hardly dented when hammered with a heavy sledge hammer, and was barely affected by the file; and what was not less interesting, was, that although this alloy contained upwards of 25 per cent. of potassium, or one of the most powerful electro-positive substances, still the iron of the alloy oxidized rapidly in the atmosphere and under water. It is certainly to be much regretted that such is the result; for if, by alloying with a metal more electro-positive than itself, we had succeeded in obtaining an alloy of iron not oxidizable, we should have discovered a most valuable fact for manufactures, and one which has occupied so deeply the thoughts of all those connected with such improvements.

We made another experiment with the same proportions of bitartrate of potash and iron, only we added some charcoal finely pulverized, and obtained the following result:—

Iron	81·42
Potassium	18·58
	100·00

which leads to the formula:—

6 equivalents of iron	168	or	81·16
1 equivalent of potassium	39	...	18·84
	207	...	100·00

We are led to think that this alloy contained an excess of iron, for there was on its surface a thin layer of cast iron; and although we filed it off as carefully as possible, still no doubt some of it had introduced itself into the mass. As this alloy presented all the properties of the former, we shall not enter into details respecting it. With the view of obtaining this alloy, if possible, by a cheaper process, we submitted for several hours to an intense heat a mixture of fine iron filings with a quantity of carbonate of potash, containing as much potassium as exists in the bitartrate used in the previous experiments, or—

336 of iron,
552 of pure carbonate of potash,

but no results were obtained.

Alloys of Iron and Aluminium.

We were induced to make a great many experiments to produce these new alloys, in consequence of the remarkable properties which aluminium possesses, as discovered by M. St. Clair Deville; and what increased our desire was, that we hoped they would not only present useful properties, but might be the means of obtaining aluminium cheaper than by the process already discovered.

We shall not describe all the fruitless essays we made, but shall confine our observations only to those which gave us satisfactory results.

The first alloy of aluminium and iron was obtained by heating to a white heat, for two hours, the following mixture:—

8 equivalents of chloride of aluminium .	1076
40 equivalents of fine iron filings . . .	1120·0
8 equivalents of lime	224·0

The lime was added to the mixture with the view of removing the chlorine from the chloride of aluminium, so as to liberate the metal aluminium; by forming a fusible chloride of calcium, and subtracting the lime from the above proportions, we ought to have obtained an alloy having the composition of—

1 equivalent of aluminium . .	14	=	9·09
5 equivalents of iron	140	=	90·91
	154	=	100·00

whilst the alloy which we found at the bottom of the crucible was composed of 100 parts as follows:—

Aluminium	12·00
Iron	88·00
	100·00

which leads to the following formula:—

1 equivalent of aluminium .	11·11
4 equivalents of iron	88·89
	100·00

This alloy, as it will be remarked, has the same composition as one of those of potassium and iron, and, like it, it was extremely hard, and rusted when exposed to a damp atmosphere; still it could be forged and welded.

We obtained a similar alloy on adding to the above mixture some very finely-pulverized charcoal, and submitting the whole to a high temperature in a forge furnace for two hours. This alloy gave on analysis the following composition:—

Aluminium . . .	87·91
Iron	12·09
	100·00

But in the mass of chloride of calcium and charcoal remaining in the crucible, there were a great number of globules varying in size from that of a large pea to that of a pin-head. These globules were as white as silver, extremely hard; and what rendered this alloy particularly interesting, was the property which it possessed of not rusting when exposed to a moist atmosphere, or even to hyponitric fumes; its analysis gave these results on 100 parts:—

Aluminium . . .	24·55
Iron	75·45
	100·00

which correspond to:—

2 equivalents of aluminium . .	28	=	25·00
3 equivalents of iron	84	=	75·00
	112	=	100·00

Therefore this alloy has the same composition as alumina, the iron taking the place of the oxygen which exists in the latter.

We treated these globules with weak sulphuric acid, which removed the iron and left the globules of aluminium; these had exactly the same form as before removing the iron, and the aluminium thus obtained had all the properties which have been ascribed to that curious metal.

We have made trials with the following mixture, but although they have yielded results, still they are not sufficiently satisfactory for us to describe in this paper, which is the first of a series we intend publishing on alloys:—

Kaolin or silicate of alumina . .	1750 parts.
Chloride of sodium	1200 ...
Iron	875 ...

This mixture has yielded a metallic mass and globules.

Alloys of Aluminium and Copper.

To obtain these alloys, we had recourse to the same chemical reaction as we employed to prepare those of iron, namely,—

20 equivalents of copper	640
8 equivalents of chloride of aluminium	1076
10 equivalents of lime	280

We mixed these substances intimately together, and after having submitted them to a high temperature for one hour, we found at the bottom of the crucible a melted mass covered with chloride of copper, and in this mass small globules, which on analysis gave the following results:—

Copper	91·53
Aluminium	8·47
	100·00

which correspond with the formula:—

5 equivalents of copper	160	=	91·96
1 equivalent of aluminium	14	=	8·14
	174	=	100·00

We made another mixture of chloride of aluminium and copper in the proportions above stated, but left out the lime; we also obtained an alloy, which yielded the following results:—

Copper	87·18
Aluminium	12·82
	100·00

which lead to the formula:—

3 equivalents of copper	96	=	87·27
1 equivalent of aluminium	14	=	12·73
	110	=	100·00

Alloys of Iron and Zinc.

We also analysed a deposit formed constantly at the bottom of a metallic bath, composed of melted zinc and tin, and which is used for galvanizing iron. This deposit was found to be composed of—

Iron	6·06
Zinc	93·94
	100·00

which corresponds with the formula:—

1 equivalent of iron	28	=	6·79
12 equivalents of zinc	384	=	93·21
			100·00

This alloy had not the lamellar appearance of zinc, but a crystalline one, and was extremely hard and slightly fusible. We thought, perhaps, that the bath was saturated with iron, which had combined with the zinc and had gradually deposited. We accordingly took some of the melted alloy of zinc and tin in various parts of the bath and examined them, but found only traces of iron. It is certainly remarkable that the iron does not remain diffused in the metallic bath, which is constantly kept in a state of fusion, but combines in definite proportions with zinc, forming a crystalline compound, and that it deposits at a temperature of at least 800° F.

This fact induced us to examine the composition of the metallic bath, which being a very large one, gave us a good opportunity of ascertaining a very interesting problem, viz. whether when we employed definite proportions of zinc and tin to compose it, and melted them together, the mass of the bath would have a uniform composition, or would vary according to the depth. The bath we used was 2½ feet wide, 10 feet long, and 3½ feet deep, and it contained fourteen tons of melted zinc and tin. As this definite compound of iron and zinc existed in large quantities at the bottom of the metallic bath, in order to take our samples we made a wrought iron tube, of two sections, with flanges the entire length, joined together by means of screws, which brought the parts of the tube into close contact. This tube, which had a tap fitted on the upper end, being air-tight when closed, was plunged into the melted metallic bath, and after having reached a given depth, a little alloy was allowed to run in by slightly opening the tap and then closing it. By this means we obtained the following samples; one at the top, one at from 21 to 24 inches deep, and one at the bottom, and we found these to have the following composition:—

	Top.	21 to 24 in.	Bottom.
Zinc . .	81·48	87·72	90·04
Tin . .	13·60	10·03	8·64
Lead . .	4·92	2·25	1·32
	100·00	100·00	100·00

These numbers therefore clearly show that the metallic bath of zinc and tin (for the lead may be considered as an impurity of the zinc) had a different composition in its various parts, and that the chemical affinity was not powerful enough to maintain a uniform composition through the mass. It is also curious to observe that the heaviest metals, viz. tin and lead, are in larger proportions at the top of the bath than at the bottom. We shall not, however, venture to give any explanation of this curious fact until we have further data.

There is a fact which appears to us also worth notice; it is, that if we suppress from the above per-centages the proportion of lead which they contain, and then calculate the zinc and tin into new per-centages, we find that the results obtained perfectly correspond with the quantities required to represent definite compounds, or alloys having an equivalent composition; in fact:—

		Found.	Calculated.	Formula.
Top:—				
	Tin . .	14·30	13·89	1Sn.
	Zinc . .	85·70	86·11	11Zn.
Middle:—				
	Tin . .	10·26	9·98	1Sn.
	Zinc . .	89·74	90·02	16Zn.
Bottom:—				
	Tin . .	8·76	8·54	1Sn.
	Zinc . .	91·24	91·46	19Zn.

Although the quantities found by analysis so nearly correspond with those obtained by calculations, still we hesitate to admit that the metals composing the metallic bath exist in it in a state of definite compounds, the more so as the equivalent of zinc is so high comparatively with that of tin.

Our object being to obtain cheaper alloys than those now in use called brass and bronze, in which copper predominates, we prepared several alloys in definite proportions, in which zinc predominated. To obtain these alloys we melted tin, and added gradually to it the zinc, or the zinc and lead, and then poured this mixture into a melted mass of copper; stirring the mixture well and casting the whole into bars, we succeeded in obtaining the following alloys:—

No. 1.

	Found.	Calculated.
6 equivalents of zinc . .	68·32	68·55
1 equivalent of tin . . .	20·62	20·34
1 equivalent of copper .	11·06	11·11
	100·00	100·00

No. 2.

	Found.	Calculated.
10 equivalents of zinc . .	62·64	62·85
1 equivalent of tin . . .	11·32	11·18
1 equivalent of lead . .	19·94	19·86
1 equivalent of copper .	6·10	6·11
	100·00	100·00

No. 3.

	Found.	Calculated.
20 equivalents of zinc	69·56	69·77
2 ... tin	12·58	12·41
1 ... lead	11·06	11·04
2 ... copper	6·80	6·78
	100·00	100·00

Alloys of Copper.

We also prepared a series of alloys in which copper predominated; the *modus operandi* was the same as that adopted to prepare the above, and we found them to be composed of,—

No. 1.

	Found.	Calculated.
4 equivalents of copper	56·25	56·45
3 ... zinc	43·75	43·55
	100·00	100·00

No. 2.

18 equivalents of copper	87·05	86·29
1 ... zinc	5·07	4·93
1 ... tin	7·88	8·78
	100·00	100·00

No. 3.

10 equivalents of copper	77·45	77·77
3 ... zinc	14·39	14·23
1 ... tin	8·16	8·00
	100·00	100·00

The first alloy of this series, or one much approaching to it, is already in commerce, and has been analysed by M. Rieffel.

The second alloy has been introduced into commerce of late, and is much valued by locomotive manufacturers for its extreme hardness.

The third alloy is one not yet in commerce, but we believe, from its physical properties, that it will replace the second alloy in many of its principal applications, and if so, it will prove an advantage to trade, as it is a much cheaper alloy.

We were induced to try the action of various acids on the above alloys; for if they were simple mixtures of metals, there was no reason why the metals composing them should not be attacked, though mixed together, as they would if they existed in a free state; whilst, if they were chemically combined, the action of acids would be modified. The results, as far as we have proceeded, perfectly corroborate these views, and we hope that when we have completed this new field of investigation, we shall have practical results to offer.

Action of Hydrochloric Acid, full strength 1·24, *for two hours.*

	Composition of alloys.	Loss per cent.	Action on the metals composing the alloy.
No. 1:—			
Cu	18 eq. 86·29	0·18	Zinc, violent.
Zn	1 ... 4·93		Tin, violent.
Sn	1 ... 8·78		Copper, slight.
No. 2:—			
Cu	10 eq. 77·77	0·12	
Zn	1 ... 14·23		
Sn	1 ... 8·00		
No. 3:—			
Cu	4 eq. 56·45	0·2	
Zn	3 ... 43·55		

Therefore these alloys are much less attacked than the metals which compose them; and it is certainly interesting to find that the alloy No. 3, which contains nearly 50 per cent. of zinc, is attacked so slightly by the hydrochloric acid. This inertness of an acid, having a most powerful action on one or more of the metals composing an alloy, is most curious; and what increases its interest is, that it appears to be general, as seen in the following tables, which illustrate the action of sulphuric acid:—

	Composition of alloy.	Action of sulphuric acid, spec. grav. 1·50.	Action of sulphuric acid on the metals.
No. 1:—			
Cu	18 eq. 86·29	None.	Tin, slight.
Zn	1 ... 4·93		Copper, slight.
Sn	1 ... 8·78		Zinc, violent.
No. 2:—			
Cu	10 eq. 77·77	None.	
Zn	1 ... 14·23		
Sn	1 ... 8·00		
No. 3:—			
Cu	4 eq. 56·45	None.	
Zn	3 ... 43·55		

Action of Nitric Acid on the above Alloys.

	Composition of alloy.	Nitric acid, sp. gr. 1·100. Loss.	Action of nitric acid on the metals.
No. 1:—			
Cu	18 eq. 86·29	·02	Copper, most violent.
Zn	1 ... 4·93		Tin, do.
Sn	1 ... 8·78		Zinc, do.

	Composition of alloy.	Nitric acid. sp. gr. 1·100. Loss.	Action of nitric acid on the metals.
No. 2:—			
Cu .	10 eq. 77·77	·06	Copper, most violent.
Zn .	1 ... 14·23		Tin, do.
Sn .	1 ... 8·00		Zinc, do.
No. 3:—			
Cu .	4 eq. 56·45	·03	
Zn .	3 ... 43·55		

The results contained in the last table illustrate our views completely, viz. that alloys having a definite composition offer a most extraordinary resistance to the action of acids, as we have alloys the metals of which are most violently attacked by nitric acid, and still we find but a comparatively small amount of alloy dissolved by this powerful acid.

We hope at a future time to publish the result of our labour, which will not only consist in examining the comparative tenacity, hardness, &c. of certain alloys, as compared with the metals composing them, but the influence which, say one, two, or three equivalents of a metal entering into the composition of an alloy, may have on the physical and chemical properties of the said alloy.

XXXI. *On the Application of Photography to Experiments on Diffraction.* *By* John Bridge, *M.A. University College, London*[*].

IN addition to the difficulty of giving an explanation at once accurate and popular of the principles of the interference of light, the want of some cheap and convenient means of exhibiting its phænomena has hitherto prevented the subject from being generally known. It is not my concern here to insist that the former difficulty may be removed, but to describe a means by which the phænomena may, at a trifling cost, be illustrated in as great variety as may be desired.

If we look at a line of light through a series of equidistant lines ruled on glass, lateral spectra are produced, whether a telescope or only the naked eye be employed. Here is then an experiment which may be performed in a simple manner; and to make it a popular experiment, it is only necessary to produce these lines cheaply. It occurred to me to do this by taking a collodion picture, as small as desired, of a series of lines ruled on a scale as large as may be necessary to ensure accurate equidistance. When I had succeeded (to a considerable extent) in this, it seemed to me that by bringing the circles, triangles, or

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[*] Communicated by the Author.

other figures used in Sir John Herschel's experiments within the space of the pupil of the eye on a collodion plate, that series of beautiful phænomena might be produced without the employment of a telescope, or at any rate by the employment of a telescope of very low power.

Several circumstances tend to prevent perfect accuracy, and claim attention, either to be obviated as we best can, or to be dismissed as incapable of doing material harm.

1. Inequality in the surface and in the substance of the glass which is to receive the collodion film.

2. Inequality in the collodion film; in its thickness from pouring off, or wrinkles on its surface left by the evaporation of the æther.

3. The difficulty of placing the glass to receive the image in a plane exactly parallel to that of the object.

With respect to the second difficulty, it is to be considered that the thickness of the collodion film is of an order quite comparable with the length of a wave of light; in fact, I found that the film of an average collodion from a hundred square inches, when dried and lightly pressed, occupied considerably less than $\frac{1}{64}$th of a cubic inch, so that the thickness was certainly not more (and probably very much less) than $\frac{1}{6400}$dth of an inch. The inequality arising from wrinkles is a small fraction of this. The inequality arising from pouring off must also, for the small surface I would use, be a very small fraction of the whole thickness; and such as it is, approximates to that of a very acute-angled prism, whose effect would be quite inappreciable. By cementing a piece of thin glass over the surface of the picture, it is probable that the effect of the wrinkles would be neutralized, at the same time that protection would be afforded to the film.

The third source of error is the most important, but is more in the power of care and delicacy than the others. To find the effect of a small error of this sort in displacing the points of the figure, let *ab* be the correct, and AB the actual position of the surface of the film, C the centre of the picture, O of the lens. Then the point which should be at the distance C*p* from C is at the distance CP.

Now

$$\mathrm{CP}-\mathrm{C}p=\frac{\mathrm{C}p^2}{\mathrm{CO}}\times\angle\mathrm{AC}a.$$

If we are to have, say 500 lines in $\mathrm{C}p=\frac{1}{2}$ of an inch, the error AC*a*, which would remove the 500th line to the place of the 501st, would be $\frac{1}{25}$, or rather more than 2° for a focal length of 10 inches.

The degree of success which I have obtained without the opportunity of satisfying these requirements, is sufficient to assure me that these beautiful experiments may thus be placed within the reach of all, and to justify the expectation that any one who possesses the necessary skill and other advantages may convert this, not only into a popular, but an accurate and scientific experiment.

Besides the lines giving lateral spectra, I have produced regular series of a large number of circles, triangles, &c. within the space of the pupil of the eye, so that the phænomena may be seen in a variety of forms by the unaided eye*. This is, I suppose, scarcely possible by any other means than photography.

XXXII. *Notes on Mineralogy.*—No. III. *On Serpentines and Soapstones. By the* Rev. SAMUEL HAUGHTON, *Professor of Geology in the University of Dublin*†.

THE following additional analyses of serpentines and soapstones may be considered of interest by some of your readers, particularly those relating to the serpentine porphyry of the Lizard, respecting which there has been some difference of opinion among mineralogists.

The serpentine porphyry of Cornwall consists of greenish crystals imbedded in a reddish paste; the green mineral has been pronounced diallage by Dr. Boase and Sir H. De la Beche, and the reddish paste considered by the same authorities as a species of felspar: both these opinions seem to me to be erroneous, as the porphyritic serpentine of Landewednack and Kynance Cove must be considered as composed altogether of serpentine. I have not found alumina in sensible quantity in the Cornish porphyry, and therefore its presence in the veins of steatite which intersect the serpentine porphyry is highly interesting. During a visit which I paid to this interesting locality in 1854, I ascertained the cause of the presence of alumina in the veins of soapstone. Both at Kynance Cove and Gue Grease, the serpentine porphyry is intersected by dykes of granite, and the celebrated veins of soapstone lie spread out in sheets at the junction of the serpentine and granite; the soapstone must therefore be regarded as the result of the contact of these rocks at an elevated temperature; the serpentine supplying the magnesia; and the felspar of the granite, the alumina, necessary for the formation of the soapstone.

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* But much better with a common telescope.

† Communicated by the Author.

Analyses of Serpentine.

	No. 1.	No. 2.	No. 3.	No. 4.
Silica	38·29	40·12	42·88	41·24
Alumina	......	trace		
Protoxide of iron	13·50	3·47	3·80	7·41
Magnesia	34·24	40·04	40·52	36·28
Water	12·09	13·36	12·64	14·16
Carbonic acid	......	2·00	......	
	98·12	98·99	99·84	99·09

No. 1. The red earthy, sometimes semi-crystalline base of the serpentine porphyry of Kynance Cove.

No. 2. Serpentine, carefully picked out from the verd antique of Ballinahinch, co. Galway. It is intimately mixed up with white crystalline marble, and is the result of metamorphic action. The carbonic acid present is due to small particles of limestone which could not be completely separated.

No. 3. Eruptive pale green (passing into gray) serpentine, containing abundance of magnetic iron oxide, from Zermat Thal, Switzerland.

No. 4. Dark green, glossy serpentine from Syria, precise locality unknown.

From the foregoing analyses we readily obtain the following atomic equivalents:—

Number of Atoms.

	Silica.	Protoxides.	Water.	Integers.
No. 1.	0·832	2·087	1·343	10 : 25·08 : 16·14
No. 2.	0·872	2·098	1·484	10 : 24·06 : 17·02
No. 3.	0·932	2·131	1·404	10 : 22·86 : 15·06
No. 4.	0·896	2·020	1·573	10 : 22·54 : 17·65

Notwithstanding the differences apparent in the foregoing results, they all approximate to the formula $5\text{Si O}^3, 12\text{MgO}, 8\text{HO}$, which gives for rational formula,—

$$5\{2\text{MgO}, \text{SiO}^3+\text{HO}\}+\{2\text{MgO}, 3\text{HO}\}.$$

The following Table contains the analyses of two specimens of soapstone; the first taken from the vein at Kynance Cove, the second from the vein at Gue Grease.

Analyses of Soapstones.

	Kynance.	Gue Grease.
Silica	42·47	42·10
Alumina	6·65	7·67
Magnesia	28·83	30·57
Water	19·37	18·46
	97·32	98·80

These analyses furnish us with the following:—

Number of Atoms.

	Silica.	Alumina.	Magnesia.	Water.	Integers.
Kynance	0·923	0·129	1·441	2·152	6·4 : 0·9 : 10 : 14·9
Gue Grease	0·915	0·149	1·528	2·051	6· : 0·9 : 10 : 13·4

From this table we deduce the formula $6SiO^3$, Al^2O^3, $10MgO$, $14HO$, which gives as rational formula probably the following:—

$$5\{2MgO, SiO^3\}+\{Al^2O^3, SiO^3\}+14HO.$$

XXXIII. *On Pressures of Saturated Vapours.*
By W. J. Macquorn Rankine, *C.E., F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

The perusal, this day, of a recent paper by Sir John Lubbock on Astronomical Refractions, has made me aware of the fact, that in 1840 that author published a general formula for the pressures of saturated vapours of the following form:—

$$\log P=\frac{1}{\beta}\left(\log E+\log\left(1+\frac{F}{\tau}\right)\right); \quad . \quad . \quad . \quad (1)$$

P being the pressure, τ the absolute temperature, and β, E, F constants.

Sir John Lubbock points out the resemblance between the above formula and that which was published by me in the Edinburgh Philosophical Journal for July 1849, viz.

$$\log P=A-\frac{B}{\tau}-\frac{C}{\tau^2}- \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

That resemblance I freely admit; inasmuch as the development of the formula (1) produces a series in terms of the powers of $\frac{1}{\tau}$.

The *differences* between the two formulæ appear to me to be the following.

The formula (1) is deduced from certain reasonings as to relations between the pressure of vapour and its total heat of evaporation (which reasonings it would be premature for me to discuss after so brief an examination of them as I have yet been able to make), and it contains essentially but three independent coefficients; so that all the coefficients of its development must be functions of the first three.

The formula (2) was rather *suggested by*, than deduced from, certain hypothetical views as to the condition of a vapour at and near the surface of its liquid; with respect to which views I may refer to the Transactions of the Royal Society of Edinburgh, vol. xx., and the Philosophical Magazine for December 1851 and December 1854. In this formula the number of independent coefficients to be determined empirically is *indefinite*, although three coefficients have been found to give a sufficiently close agreement with such experiments as have yet been made. On this point see § 10 of a paper in the Philosophical Magazine for December 1854. The computation of the coefficients is a very easy process.

I may observe, that, from the following principle in the theory of thermo-dynamics (Phil. Trans. 1854, part 1),—

Latent heat of evaporation of unity of weight of a fluid

$$=\tau \cdot \frac{dP}{d\tau}(V-v)$$

> (expressed in units of work at the rate of 772 foot-pounds per Fahrenheit-unit of heat), where v and V are the volumes of unity of weight of the fluid in the liquid and vaporous states respectively,—

it is easily deduced that *if the vapour be a perfect gas,*

$$\log_e P = a - \frac{\tau_0}{P_0 V_0}\left\{(k-K)\log_e \tau + \frac{c}{\tau}\right\}, \quad . \quad . \quad (3)$$

in which τ_0, P_0, V_0 refer to a fixed standard absolute temperature, K is the dynamical specific heat of the vapour under constant pressure (or the mechanical work required to heat unity of weight of it one degree under constant pressure by friction), k that of the liquid, and a and c are constants to be found empirically. But as few saturated vapours are even approximately in the perfectly gaseous state, the formula (3) refers to an ideal case, and I therefore refrain from enlarging upon it.

The ratio $\frac{K\tau_0}{P_0V_0}$ corresponds to $\frac{\gamma}{\gamma-1}$ in Poisson's notation.

I have the honour to be, Gentlemen,
Your most obedient Servant,
W. J. MACQUORN RANKINE.

Glasgow, September 21, 1855.
59 St. Vincent Street.

XXXIV. *On the Nature of the Force by which Bodies are repelled from the Poles of a Magnet; to which is prefixed, an Account of some Experiments on Molecular Influences.* By JOHN TYNDALL, *Ph.D., F.R.S. &c.*

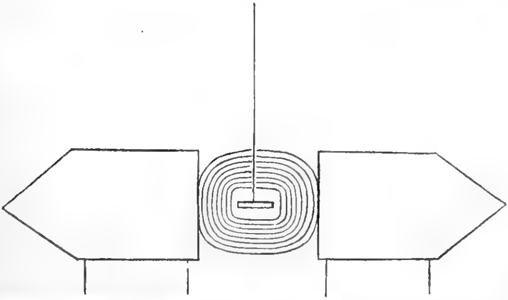
[Concluded from p. 179.]

3. *Separate and joint action of a Magnet and a Voltaic Current on Paramagnetic and Diamagnetic Bodies.*

IN operating upon bars of bismuth with the magnet, or the current, or both combined, it was soon found that the gravest mistakes might be committed if the question of structure was not attended to; that it is not more indefinite to speak of the volume of a gas without giving its temperature, than to speak of the deportment of bismuth without stating the relation of the form of the mass to the planes of crystallization. Cut in one direction, a bar of bismuth will set its length parallel to an electric current passing near it; cut in another direction, it will set its length perpendicular to the same current. It was necessary to study the deportment of both of these bars separately.

A helix was formed of covered copper wire one-twentieth of an inch thick: the space within the helix was rectangular, and was 1 inch long, 0·7 inch high, and 1 inch wide: the external diameter of the helix was 3 inches. Within the rectangular space

Fig. 6.



the body to be examined was suspended by a fibre which descended through a slit in the helix. The latter was placed between the two flat poles of an electro-magnet, and could thus be caused to act upon the bar within it, either alone or in combination with the magnet. The disposition will be at once understood from fig. 6, which gives a front view of the arrangement.

Action of Magnet alone: Division of bars into Normal and Abnormal.—A bar of soft iron suspended in the magnetic field will set its longest dimension from pole to pole: this is the normal deportment of paramagnetic bodies. A bar of bismuth, whose planes of principal cleavage are throughout parallel to its length, suspended in the magnetic field with the said planes vertical, will set its longest dimension at right angles to the line joining the poles: this is the normal deportment of diamagnetic bodies. We will therefore, for the sake of distinction, call the former a *normal paramagnetic bar*, and the latter a *normal diamagnetic bar*.

A bar of compressed carbonate of iron dust, whose shortest dimension coincides with the line of pressure, will, when suspended in the magnetic field with the said line horizontal, set its length equatorial. A bar of compressed bismuth dust, similarly suspended, or a bar of bismuth whose principal planes of crystallization are transverse to its length, will set its length axial in the magnetic field. We will call the former of these an *abnormal paramagnetic bar*, and the latter an *abnormal diamagnetic bar*.

Action of Current alone on normal and abnormal bars.—*A normal paramagnetic bar* was suspended in the helix above described; when a current was sent through the latter, the bar set its longest horizontal dimension parallel to the axis of the helix, and consequently perpendicular to the coils.

An *abnormal paramagnetic bar* was suspended in the same manner; when a current was sent through the helix, the bar set its longest dimension perpendicular to the axis of the helix, and consequently parallel to the coils.

A *normal diamagnetic bar* was delicately suspended in the same helix; on the passage of the current it acted precisely as the abnormal magnetic bar; setting its longest dimension perpendicular to the axis of the helix and parallel to the coils. When a fine fibre and sufficient power are made use of, this deportment is obtained without difficulty.

An *abnormal diamagnetic bar* was suspended as above; on the passage of the current it acted precisely as the normal magnetic bar: it set its length parallel to the axis of the helix and perpendicular to the coils. Here also, by fine manipulation, the result is obtained with ease and certainty.

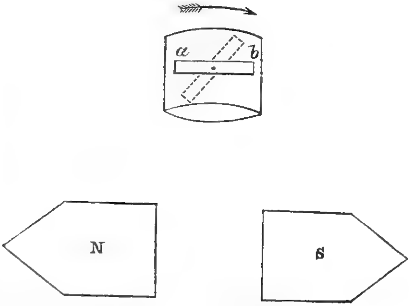
Action of Magnet and Current combined.—In examining this subject, eight experiments were made with each particular bar; it will be remembered that fig. 6 gives a general view of the arrangement.

1. Four experiments were made in which the *magnet* was excited first, and after the suspended bar had taken up its position of equilibrium, the deflection produced by the passage of a current through the surrounding helix was observed.

2. Four experiments were made in which the *helix* was excited first, and when the bar within it had taken up its position of equilibrium, the magnetism was developed and the consequent deflection observed.

Normal Paramagnetic Bar.—In experimenting with the soft iron it was necessary to place it at some distance from the magnet, otherwise the attraction of the entire mass by one or the other pole would completely mask the action sought. Fig. 7 represents the disposition of things in these experiments: N and S indicate the north and south poles of the magnet; *ab* is the bar of iron; the helix within which the bar was suspended is shown in outline around it; the arrow shows the direction of the current in the *upper half* of the helix; its direction in the under portion would, of course, be the reverse.

Fig. 7.

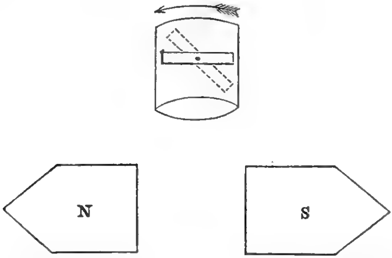


On exciting the magnet, the bar of soft iron set itself parallel to the line joining the poles, as shown by the unbroken line in fig. 7.

When the direction of the current in the helix was that indicated by the arrow, the bar was deflected towards the position dotted in the figure.

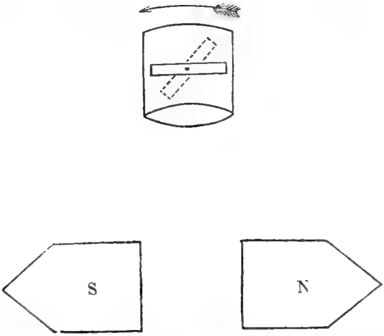
Interrupting the current in the helix, and permitting the magnet to remain excited, the bar returned to its former position: the current was now sent through the helix in the direction of the arrow, fig. 8; the consequent deflection was towards the dotted position.

Fig. 8.



Both the current which excited the magnet and that which passed through the helix were now interrupted, and the polarity of the magnet was reversed. On sending a current through the helix in the direction of the arrow, the deflection of the bar was from the position of the defined line to that of the dotted one, fig. 9.

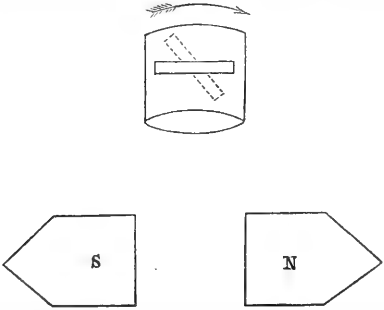
Fig. 9.



Interrupting the current through the helix, and permitting the bar to come to rest under the influence of the magnet alone, a current was sent through the helix in a direction opposed to its former one: the deflection produced was that shown in fig. 10.

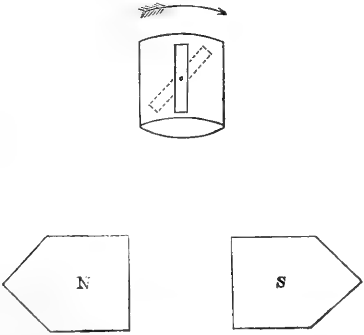
The position of equilibrium finally assumed by the bar depends, of course, upon the ratio of the forces acting upon it: in these experiments, the bar, in its final position, enclosed an angle of about 50 degrees with the axial line.

Fig. 10.



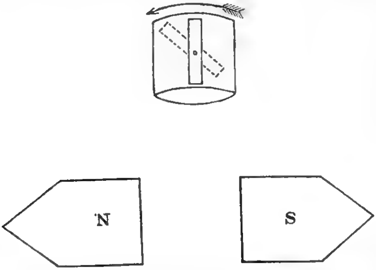
A series of experiments was next made, in which the bar was first acted on by the current passing through the helix, the magnet being brought to bear upon it afterwards. On the passage of the current through the helix in the direction shown in fig. 11, the bar set its length parallel to the axis of the latter. On exciting the magnet so that its polarity was that indicated by the letters N and S in the figure, the deflection was towards the dotted position.

Fig. 11.



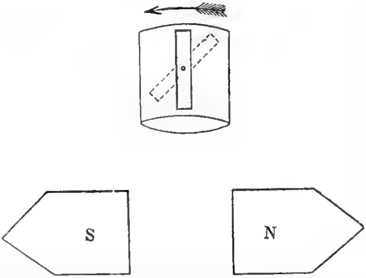
Interrupting the current through both magnet and helix, and reversing the current through the latter, the bar came to rest, as before, parallel to the axis: on exciting the magnet, as in the last case, the deflection was that shown in fig. 12.

Fig. 12.



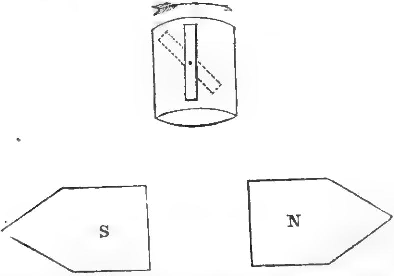
Preserving the same current in the helix, and reversing the polarity of the magnet, the deflection was that shown in fig. 13.

Fig. 13.



Preserving the magnet poles as in the last experiment, and reversing the current in the helix, the deflection was that shown in fig. 14.

Fig. 14.



In these cases, the bar, in its final position of equilibrium, enclosed an angle of about 40 degrees with the axial line.

Normal Diamagnetic Bar.—The above experiments exhibit to us the deportment of the normal paramagnetic body under a great variety of conditions, and our next step is to compare with it the deportment of the normal diamagnetic body under the same circumstances.

For the sake of increasing the force, the helix was removed from its lateral position and placed between the two poles, as in fig. 6, p. 257. The normal diamagnetic bar was suspended within the helix and submitted to the self-same mode of examination as that applied in the case of the paramagnetic body.

The polarity first excited was that shown in fig. 9, Plate III., and the position of rest, when the magnet alone acted, was at right angles to the line joining the poles; on sending a current through the helix in the direction of the arrow, the deflection was towards the dotted line.

Preserving the magnetic polarity as in the last experiment, the direction of the current through the helix was reversed, and the deflection was that shown in fig. 10.

Reversing the polarity of the magnet, and sending the current through the helix in the direction of the last experiment, the deflection was that shown in fig. 11.

Preserving the last magnetic poles, and sending the current through the helix in the opposite direction, the deflection was that shown in fig. 12.

In the following four experiments the helix was excited first.

Operated upon by the helix alone, the suspended bar set its length parallel to the convolutions, and perpendicular to the axis of the coil: the direction of the current was first that shown in fig. 13: when the magnet was excited, the bar was deflected towards the dotted position.

Interrupting both currents, and reversing the current in the helix; when the magnet was excited, as in the last experiment, the deflection was that shown in fig. 14.

Preserving the helix current as in the last experiment; when the polarity of the magnet was reversed, the deflection was that shown in fig. 15.

Interrupting both, and reversing the current in the helix; when the magnet was excited as in the last experiment, the deflection was that shown in fig. 16.

In a paper on the Polarity of Bismuth* published in the Philosophical Magazine, ser. 4. vol. ii., and in Poggendorff's *Annalen*, vol. lxxxvii., an experiment is recorded in which the

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* From the notices of this paper which have appeared in the continental journals, I am obliged to infer that it is in some respects obscurely written. The conclusion I intended to express is that bismuth possesses a polarity opposed to that of iron.—J. T.

deportment exhibited by fig. 11 of the present series was obtained. In a recent memoir on the same subject, M. v. Feilitsch* states that he has sought this result in vain. Sometimes he observed the deflection at the moment of closing the circuit, but conceived that it must be ascribed to the action of induced currents; for immediately afterwards a deflection in the opposite direction was observed, which deflection proved to be the permanent one.

I have repeated the experiment here referred to with all possible care; and the result is that described in the remarks which refer to fig. 11. This result agrees in all respects with that described in my former paper. To enable myself, however, to appeal to quantitative measurement, a small graduated circle was constructed and placed underneath the bar of bismuth suspended within the helix. The effect, as will be seen, is not one regarding which a mistake could be made on account of its minuteness: operating delicately, and choosing a suitable relation between the strength of the magnet and that of the spiral†, on sending a current through the latter as in fig. 11, the bar was deflected so forcibly that the limit of its first impulsion reached 120° on the graduated circle underneath. The permanent deflection of the bar amounted to 60° in the same direction, and hence the deportment could in no wise be ascribed to the action of induced currents, which vanish immediately. Before sending the current through the helix, the bar was acted on by the magnet alone, and pointed to zero.

Though it was not likely that the shape of the poles could have any influence here, I repeated the experiment, using the hemispherical ends of two soft iron cores as poles: the result was the same.

A pair of poles with the right- and left-hand edges rounded off, showed the same deportment.

A pair of poles presenting chisel edges to the helix showed the same deportment.

Various other poles were made use of, some of which appeared to correspond exactly with those figured by M. v. Feilitsch; but no deviation from the described deportment was observed. To test the polarity of the magnet, a magnetic needle was always at hand: once or twice the polarity of the needle became reversed, which, had it not been noticed in time, would have introduced confusion into the experiments. Here is a source of error against which, however, M. v. Feilitsch has probably guarded himself. Some irregularity of crystalline structure may, perhaps, have

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* Poggendorff's *Annalen*, vol. xcii. p. 395.

† In most of these experiments the spiral was excited by ten cells, the magnet by two.

influenced the result. With "chemically pure zinc" M. v. Feilitsch obtained the same deflection that I obtained with bismuth: now chemically pure zinc is *diamagnetic**, and hence its deportment is corroborative of that which I have observed. M. v. Feilitsch, however, appears to regard the zinc used by him as magnetic; but if this be the case, it cannot have been chemically pure. It is necessary to remark that I have called the north pole of the electro-magnet that which attracts the south, or unmarked end, of a magnetic needle; and I believe this is the custom throughout Germany.

Abnormal Paramagnetic Bar.—This bar consisted of compressed carbonate of iron dust, and was suspended within the helix with the line of compression, which was its shortest dimension, horizontal. As in the cases already described, it was first acted upon by the magnet alone; having attained its position of equilibrium, a current was sent through the helix, and the subsequent deflection was observed.

The magnet being excited as in fig. 17, Plate III., the bar set its length equatorial; on sending a current through the helix in the direction of the arrow, the bar was deflected to the dotted position.

Reversing the current in the helix, but permitting the magnet to remain as before, the deflection was that shown in fig. 18.

Interrupting all, and reversing the polarity of the magnet; on sending the current through as in the last case, the deflection was that shown in fig. 19.

Reversing the current, but preserving the last condition of the magnet, the deflection was that shown in fig. 20.

In the subsequent four experiments the helix was excited first. Whatever might be the direction of the current through the helix, the bar always set its length perpendicular to the axis of the latter, and parallel to the coils.

When the direction of the helix current, and the polarity of the magnet, were those shown in fig. 21, the deflection was to the dotted position.

Interrupting all, and reversing the current in the helix; on exciting the magnet the deflection was that shown in fig. 22.

Changing the polarity of the magnet, and preserving the helix current in its former direction, the deflection was that shown in fig. 23.

Interrupting all, and reversing the current through the helix; when the magnetism was developed the deflection was that shown in fig. 24.

Abnormal Diamagnetic Bar.—This bar consisted of a prism of bismuth whose principal planes of crystallization were perpendi-

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* Phil. Mag. vol. xxviii. p. 456.

cular to its length: the mode of experiment was the same as that applied in the other cases.

Acted upon by the magnet alone, the bar set its length from pole to pole: the magnetic excitation being that denoted by fig. 29, a current was sent through the helix in the direction of the arrow; the bar was deflected to the dotted position.

Reversing the current through the helix, the deflection was that shown in fig. 30.

Interrupting both currents and reversing the magnetic poles; on sending a current through the helix as in the last experiment, the deflection was that shown in fig. 31.

Reversing the current through the helix, the deflection was that shown in fig. 32.

In the subsequent four experiments the helix was excited first.

Sending a current through the helix in the direction denoted by the arrow, the bar set its length at right angles to the convolutions, and parallel to the axis of the helix; when the magnetism was excited as in fig. 25, the deflection was to the dotted position.

When the current was sent through the helix in an opposite direction, the deflection was that shown in fig. 26.

Interrupting both currents, and reversing the poles of the magnet; on sending a current through the helix as in the last experiment, the deflection was that shown in fig. 27.

Reversing the current in the helix, the deflection was that shown in fig. 28.

In all these cases the position of equilibrium due to the first force was attained before the second force was permitted to act.

It will be observed, on comparing the deportment of the normal paramagnetic bar with that of the normal diamagnetic one, that the position of equilibrium taken up by the latter, when operated on by the helix alone, is the same as that taken up by the former when acted on by the magnet alone: in both cases the position is from pole to pole of the magnet. A similar remark applies to the abnormal para- and diamagnetic bars. It will render the distinction between the deportment of both classes of bodies more evident, if the position of the two bars, before the application of the second force, be one and the same. When both the bars, acted on by one of the forces, are axial, or both equatorial, the contrast or coincidence, as the case may be, of the deflections from this common position by the second force will be more strikingly evident.

To effect the comparison in the manner here indicated, the figures have been collected together and arranged upon Plate III. The first column represents the deportment of the normal paramagnetic bar under all the conditions described; the second column, that of the normal diamagnetic bar; the third shows the deportment of the abnormal paramagnetic bar, and the fourth that of the abnormal diamagnetic bar.

A comparison of the first two columns shows us that the deportment of the normal magnetic bar is perfectly antithetical to that of the normal diamagnetic one. When, on the application of the second force, an end of the former is deflected to the right, the same end of the latter is deflected to the left. When the position of equilibrium of the magnetic bar, under the joint action of the two forces, is from N.E. to S.W., then the position of equilibrium for the diamagnetic bar is invariably from N.W. to S.E. There is no exception to this antithesis, and I have been thus careful to vary the conditions of experiment in all possible ways, on account of the divergent results obtained by other inquirers. In his recent memoirs upon this subject, M. v. Feilitsch states that he has found the deflection of diamagnetic bodies, under the circumstances here described, to be precisely the same as that of paramagnetic bodies: this result is of course opposed to mine; but when it is remembered that the learned German worked confessedly with the "roughest apparatus," and possessed no means of eliminating the effects of structure, there seems little difficulty in referring the discrepancy between us to its proper cause.

The same perfect antithesis will be observed in the case of the abnormal bars, on a comparison of the third and fourth columns. In all cases then, whether we apply the magnet singly, or the current singly, or the magnet and current combined, the deportment of the normal diamagnetic bar is opposed to that of the normal paramagnetic one, and the deportment of the abnormal paramagnetic bar is opposed to that of the abnormal diamagnetic one. But if we compare the normal paramagnetic with the abnormal diamagnetic bar, we see that the deportment of the one is identical with that of the other*. The same identity of action is observed when the normal diamagnetic bar is compared with the abnormal paramagnetic one. The necessity of taking molecular structure into account in experiments of this nature could not, I think, be more strikingly exhibited.

For each of the bars, under the operation of the two forces, there is an oblique position of equilibrium: on the application

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* Identical to the eye, but not to the mind. The notion appears to be entertained by some, that, by changing molecular structure, I had actually converted paramagnetic substances into diamagnetic ones, and *vice versâ*. No such change, however, can cause *the mass* of a diamagnetic body suspended by its centre of gravity to be *attracted*, or the mass of a paramagnetic body to be *repelled*. But by a change of molecular structure, one of the forces may be so caused to apply itself that it shall present to the eye all the *directive* phænomena exhibited by the other.—J. T., May 5, 1855.

of the second force, the bar swings like a pendulum beyond this position, oscillates round it, and finally comes to rest there. Hence, if before the application of the second force the bar occupy the axial position, the deflection, when the second force is applied, appears to be from the axis to the equator; but if it first occupy the equatorial position, the deflection appears to be from the equator to the axis.

We have already shown that the repulsion of diamagnetic bodies is to be referred to a state of excitement induced by the magnet which acts upon them: it has been long known that the attraction of paramagnetic bodies is due to the same cause. The experiments just described exhibit to us bars of both classes of bodies moving in the magnetic field: such motions occur in virtue of the induced state of the body, and the relation of that state to the forces which act upon the mass. We have seen that in all cases the antithesis between both classes of bodies is maintained. Whatever, therefore, the state of the paramagnetic bar under magnetic excitement may be, a precisely antithetical state would produce all the phænomena of the diamagnetic bar. If the bar of iron be polar, a reverse polarity on the part of bismuth would produce the effects observed. From this point of view all the movements of diamagnetic bodies become perfectly intelligible, and the experiments to be recorded in the next chapter are not calculated to diminish the probability of the conclusion that diamagnetic bodies possess a polarity opposed to that of magnetic ones.

The phænomena to which we have thus far referred consist in the rotations of elongated bars about their axes of suspension. The same antithesis, however, presents itself when we compare the *motion of translation* of a paramagnetic body, within the coil, with that of a diamagnetic one. A paramagnetic sphere was attached to the end of a horizontal beam and introduced into the coil: the magnet being excited, the sphere could be made to traverse the space within the coil in various directions, by properly varying the current through the coil. A diamagnetic sphere was submitted to the same examination, and it was found that the motions of both spheres, when operated on by the same forces, were always in opposite directions.

V. FURTHER COMPARISON OF PARAMAGNETIC AND DIAMAGNETIC PHÆNOMENA:—DIAMAGNETIC POLARITY.

When an iron bar is placed within a helix, it is well known that on sending a current through the latter the bar is converted into a magnet, one end of the bar thus excited being attracted, and the other end repelled by the same magnetic pole. In this *twoness* of action consists what is called the *polarity* of the bar: we will now consider whether a bar of bismuth exhibits similar effects.

Fig. 39 Plate IV. represents the disposition of the apparatus used in the examination of this question. AB is a helix of covered copper wire one-fifteenth of an inch in thickness: the length of the helix is 5 inches, external diameter 5 inches, and internal diameter 1·5 inch. Within this helix a bar of bismuth 6½ inches long and 0·4 of an inch thick was suspended. The suspension was effected by means of a light beam, from two points of which, sufficiently distant from each other, depended two silver wires each ending in a loop: into these loops, *ll′*, the bar of bismuth was introduced, and the whole was suspended by a number of fibres of unspun silk from a suitable point of support. Fig. 39 *a* is a side view of the arrangement used for the suspension of the bar. Before introducing the latter within the helix, it was first suspended in a receiver, which protected it from air currents, and in which it remained until the torsion of the fibre had exhausted itself: the bar was then removed, and the beam, without permitting the fibre to twist again, was placed over the helix so as to receive the bar introduced through the latter. From the ends of this helix two wires passed to a current reverser R, from which they proceeded further to the poles of a voltaic battery. CD and EF are two electro-magnetic spirals, each 12 inches long, 5½ inches external and 2 inches internal diameter. The wire composing them is one-tenth of an inch thick, and so coiled that the current could be sent through four wires simultaneously. Within these spirals were introduced two cores of soft iron 2 inches thick and 14 inches long: the ends of the cores appear at P and P′. The spirals were so connected together that the same current excited both, thus developing the same magnetic strength in the poles P P′. From the ends of the spirals proceeded wires to the current reverser R′, and thence to a second battery of considerably less power than the former. By means of the reverser R′ the polarity of the cores could be changed; P′ could be converted from a south pole to a north pole, at the same time that P was converted from a north pole to a south pole. Lastly, by a change of the connexions between the two spirals, the cores could be so excited as to make the poles of the same quality, both north or both south.

The diameter of the cylindrical space, within which the bismuth bar was suspended, was such as to permit of a free play of the ends of the bar through the space of an inch and a half. Having seen that the bar swung without impediment, and that its axis coincided as nearly as possible with the axis of the helix, a current from the battery was sent through the latter. The magnetism of the cores P and P′ was then excited, and the action upon the bismuth bar observed. M. v. Feilitsch has attempted a similar experiment to that here described, but without success: when, however, sufficient power is combined with sufficient delicacy, the success is complete, and the most perfect mastery is obtained over the motions of the bar.

The helix above described is the one which I have found most convenient for the experiments; various other helices, however, were tried with a result equally certain, if less energetic. The one first made use of was 4 inches long, 3 inches exterior diameter, and three-quarters of an inch interior diameter, with wire one-fifteenth of an inch in thickness, the bar being suspended by a fibre which passed through a slit in the helix: sending through this helix a current from a battery of 10 cells, and exciting the cores by a current from 1 cell, the phænomena of repulsion and *attraction* were exhibited with all desirable precision.

I shall now proceed to describe the results obtained by operating in the manner described. The bismuth bar being suitably suspended, a current was sent through the helix, so that the direction of the current *in the upper half* was that indicated by the arrow in fig. 40, Pl. IV. On exciting the magnet, so that the pole N was a north pole and the pole S a south pole, the ends of the bar of bismuth were *repelled*. The final position of the bar was against the side of the helix most remote from the magnets: it is shown by dots in the figure.

By means of the reverser R the current was now sent through the helix in the direction shown in fig. 41: the bar promptly left its position, crossed the space in which it could freely move, and came to rest as near the magnets as the side of the helix would permit it. *It was manifestly attracted by the magnets.*

Permitting the current in the helix to flow in the last direction, the polarity of the cores of soft iron was reversed: we had then the state of things sketched in fig. 42; the bismuth bar instantly loosed from the position it formerly occupied, receded from the magnet, and took up finally the position marked by the dots.

After this new position had been attained, the current through the helix was reversed: the bar promptly sailed across the field towards the magnets, and finally came to rest in the dotted position, fig. 43. In all these cases, when the bar was freely moving in any direction, under the operation of the forces acting upon it, the reversion either of the current in the helix or of the polarity of the cores arrested the motion; approach was converted into recession, and recession into approach.

The ends of the helix in these experiments were not far from the ends of the soft iron cores; and it might therefore be supposed that the action was due to some modification of the cores by the helix, or of the helix by the cores. It is manifest that the magnets can have no *permanent* effect upon the helix; the current through the latter, measured by a tangent galvanometer, is just as strong when the cores are excited as when they are unexcited. The helix may certainly have an effect upon the cores, and this effect is either to enfeeble the magnetism of the cores or to strengthen it; but if the former, and the bar were the simple bismuth which it is when no current operates on it, the action, though weakened, *would still be repulsive*; and if the latter, the increase would simply augment the repulsion. The fact, however, of the ends of the bar being *attracted*, proves that the bar has been thrown into a peculiar condition by the current circulating in the surrounding coil. Changing the direction of the current in the coil, we find that the self-same magnetic forces which were formerly attractive are now repulsive; to produce this effect the condition of the bar must have changed with the change of the current; or, in other words, the bar is capable of accepting *two different states* of excitement, which depend upon the direction of the current.

In order, however, to reduce as far as possible the action of the helix upon the cores, I repeated the experiments with the small helix referred to in fig. 6, page 27. It will be remembered that this helix is but an inch in length, and that the bismuth bar is 6½ inches long. I removed the magnets further apart, so that the centres of the cores were half an inch beyond the ends of the bismuth bar, while the helix encircled only an inch of its central portion: in this position, when the helix was excited, there was no appreciable magnetism excited by it in the dormant cores; at least, if such were excited, it was unable to attract the smallest soft iron nail. Here then we had cores and helix sensibly independent of each other, but the phænomena appeared as before. The bar could be held by the cores against the side of the helix, with its ends only a quarter of an inch distant from the ends of the cores; on reversing either current the ends instantly receded, but the recession could be stopped by again changing the direction of the current. With a tranquil atmosphere, and an arrangement for reversing the current without shock or motion, the bar obeyed in an admirable manner the will of the experimenter, and, under the operation of the same forces, exhibited all the deflections sketched in figs. 40, 41, 42 and 43.

The motion of the bar cannot be referred to the action of induced currents. The bar was brought into the centre of the hollow cylinder in which it swung, and held there; the forces were all in action, and therefore all phænomena of induction passed: the arrangement of the forces being that shown in fig. 40, on releasing the bar it was driven from the cores, whereas when the arrangement was that shown in fig. 41, it was drawn towards them.

But it does not sufficiently express the facts to say that the bar is capable of two different states of excitement; it must be added, that both states exist simultaneously in the excited bar. We have already proved that the state necessary for the action of one pole is not that which enables an opposite pole to produce the same action; hence, when the two ends of the bar are attracted or repelled, at the same time, by two opposite poles, it is a proof that these two ends are in different states. But if this be correct, we can test our conclusion by reversing one of the poles; the direction of its force being thereby changed, it ought to hold the other pole in check and prevent all motion in the bar. This is the case: if, in any one of the instances cited, the polarity of either of the cores be altered; if the south be converted into a north, or the north into a south pole, thus making both poles of the same quality, the repulsion of the one is so nearly balanced by the attraction of the other, that the bar remains without motion towards either of them.

To carry the argument a step further, let us fix our attention for an instant upon fig. 40. The end of the bar nearest to the reader is repelled by a south pole; the same end ought to be *attracted* by a north pole. In like manner, the end of the bar most distant from the reader is repelled by a north pole, and hence the state of that end ought to fit it for *attraction* by a south pole. If, therefore, our reasoning be correct, when we place a north pole opposite to the lower end of the bar, and on the same side of it as the upper north pole, and a south pole opposite the upper end of the bar and on the same side of it as the lower south pole, the simultaneous action of these four poles ought to be more prompt and energetic than when only two poles are used. This arrangement is shown in Plate V.: the two poles to the right of the bismuth bar must be of the same name, and the two to the left of the bar of the opposite quality. If those to the right be both north, those to the left must be both south, and *vice versâ*. The current reverser for the magnets appears in front, that for the helix is hidden by the figure. The above conclusion is perfectly verified by experiments with this apparatus, and the twofold deflection of the bismuth bar is exhibited with remarkable energy*.

The bar used in these cases is far heavier than those commonly

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* These experiments, and almost all the others mentioned in this memoir, may be exhibited in the lecture-room. By attaching indexes of wood to the bars of bismuth, and protecting the indexes from air currents by glass shades, the motions may be made visible to several hundreds at once. See a description of a Polymagnet, Phil. Mag. June 1855.—J. T.

made use of in experiments on diamagnetism, but the dimensions stated do not mark the practical limit of the size of the bar. A solid bismuth cylinder, 14 inches long and 1 inch in diameter, was suspended in a helix 5·7 inches long, 1·8 inch internal diameter, 4 inches external diameter, and composed of copper wire 0·1 of an inch in thickness: when a current of twenty cells was sent through the helix, and the magnets (only two of them were used) were excited by one cell, all the phænomena exhibited by figs. 40, 41, 42 and 43, were distinctly exhibited.

A considerable difference is always necessary between the strength of the current passing through the helix and that which excites the cores, so as to prevent the induction of the cores, which of itself would be followed by repulsion, from neutralizing, or perhaps inverting, the induction of the helix. When *two* magnets were used and the helix was excited by ten cells, I found the magnetic excitement by one or two cells to be most advantageous; when the cores were excited by ten, or even five cells, the action was always repulsive[*]. When four magnets were applied and the helix was excited by a battery of ten or fifteen cells, a power of five cells for the magnets was found efficient.

The deportment of paramagnetic bodies is so well known, that it might be left to the reader to discern that in all the cases described it is perfectly antithetical to that of the diamagnetic body. I have nevertheless thought it worth while to make the corresponding experiments with an iron bar; to facilitate comparison, the results are placed side by side in Plate IV. with those obtained with the bar of bismuth. It must be left to the reader to decide whether throughout this inquiry the path of strict inductive reasoning has been adhered to: if this be the case, then the inference appears unavoidable, *that the diamagnetic force is a polar force, the polarity of diamagnetic bodies being opposed to that of paramagnetic ones under the same conditions of excitement*[†].

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[*] The perfect similarity of this deportment to that of soft iron under the same circumstances is evident.

[†] I would gladly refer to M. Plücker's results in connexion with this subject had I been successful in obtaining them; I will here, however, introduce the description of his most decisive experiment in his own words. (See Scientific Memoirs, New Ser. p. 336.)

"From considerations of which we shall speak afterwards, it appeared to me probable that bismuth not only assumes polarity in the vicinity of a magnetic pole, but that it also retains the polarity for some time after the excitation has taken place; or, in other words, that bismuth retains a portion of its magnetism permanently, as steel, unlike soft iron, retains a portion of the magnetism excited in it by induction. My conjecture has been corroborated by experiment.

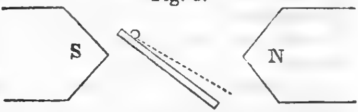
"I hung a bar of bismuth, 15 millims long and 5 millims. thick, between the pointed poles of the large electro-magnet; it was suspended horizon-

VI. CONCLUDING OBSERVATIONS: ON M. WEBER'S THEORY OF DIAMAGNETIC POLARITY*, AND ON AMPÈRE'S THEORY OF MOLECULAR CURRENTS.

It is well known that a voltaic current exerts an attractive force upon a second current, flowing in the same direction; and that when the directions are opposed to each other the force exerted is a repulsive one. By coiling wires into spirals, Ampère was enabled to make them produce all the phænomena of attraction and repulsion exhibited by magnets, and from this it was but a step to his celebrated theory of molecular currents. He supposed the molecules of a magnetic body to be surrounded by such currents, which, however, in the natural state of the body

tally from a double cocoon-thread, fig. 1. The distance between the points was diminished until the bar could barely swing freely between them. A little rod of glass was brought near to one of the points, so that the bismuth bar, before the magnetism was excited, and in consequence of the torsion, leaned against the glass rod. On exciting the magnet by a current of three of Grove's elements, the bismuth, prevented from assuming the equatorial position, pressed more forcibly against the glass rod; when the current was interrupted, the bar remained still in contact with the rod, while its free end vibrated round its position of equilibrium. The current was closed anew and then reversed by a gyrotrope. In consequence of this reversion, the bar of bismuth, loosening from the glass rod, moved towards the axial position, but soon turned and pressed against the glass as before, or in some cases having passed quite through the axial position was driven round with the reversed ends into the equatorial. . . . . . This experiment, which was made with some care, proves that the bismuth requires time to reverse its polarity."

Fig. 1.



I have repeated this experiment with great care, and have obtained in part the effect described: it is perfectly easy to produce the rotation of the bar. The cause of this rotation, however, was in my case as follows:—When the magnet was unexcited, the position of equilibrium of the axis of the bar acted upon by the torsion of the fibre was that shown by the dotted line in the figure; when the magnetism was developed, the repulsive force acting on the free end of the bar necessarily pushed it beyond the dotted line—an action which was perfectly evident when the attention was directed towards it. On reversing the current, a little time was required to change the polarity of the iron masses; during this time the free end of the bismuth *fell* towards its former position, and the velocity acquired was sufficient to carry it quite beyond the pole points. The only difference between M. Plücker and myself is, that I obtained the same result by simply *intercepting* the current as by reversing it. I may remark that I have submitted ordinary bismuth to the most powerful and delicate tests, but as yet I have never been able to detect in it a trace of that retentive power ascribed to it by M. Plücker.

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* Poggendorff's *Annalen*, vol. lxxxvii. p. 145, and Taylor's Scientific Memoirs, New Ser. p. 163.

mutually neutralized each other, on account of their confused grouping. The act of magnetization he supposed to consist in setting these molecular currents parallel to each other; and starting from this principle, he reduced all the phænomena of magnetism to the mutual action of electric currents.

If we reflect upon the experiments recorded in the foregoing pages from first to last; on the inversion of magnecrystallic phænomena by the substitution of a magnetic constituent for a diamagnetic; on the analogy of the effects produced in magnetic and diamagnetic bodies by compression; on the antithesis of the rotating actions described near the commencement; on the indubitable fact that diamagnetic bodies, like magnetic ones, owe their phænomena to an induced condition into which they are thrown by the influencing magnet, and the intensity of which is a function of the magnetic strength; on the circumstance that this excitation, like that of soft iron, is of a dual character; on the numerous additional experiments which have been recorded, all tending to show the perfect antithesis between the two classes of bodies;—we can hardly fail to be convinced that Mr. Faraday's first hypothesis of diamagnetic action is the true one—that diamagnetic bodies operated on by magnetic forces possess a polarity "the same in kind as, but the reverse in direction of that acquired by magnetic bodies." But if this be the case, how are we to conceive of the *physical mechanism* of this polarity? According to Coulomb's and Poisson's theory, the act of magnetization consists in the decomposition of a neutral magnetic fluid; the north pole of a magnet, for example, possesses an attraction for the south fluid of a piece of soft iron submitted to its influence, draws the said fluid towards it, and with it the material particles with which the fluid is associated. To account for diamagnetic phænomena this theory seems to fail altogether: according to it, indeed, the oft-used phrase, 'a north pole exciting a north pole, and a south pole a south pole,' involves a contradiction. For if the north fluid be supposed to be *attracted* towards the influencing north pole, it is absurd to suppose that its presence there could produce *repulsion*. The theory of Ampère is equally at a loss to explain diamagnetic action; for if we suppose the particles of bismuth surrounded by molecular currents, then according to all that is known of electro-dynamic laws, these currents would set themselves parallel to, and in the same direction as those of the magnet, and hence attraction, and not repulsion, would be the result. The fact, however, of this not being the case proves that these molecular currents are not the mechanism by which diamagnetic induction is effected. The consciousness of this, I doubt not, drove M. Weber to the assumption that the phænomena of diamagnetism are produced by molecular currents, not *directed*, but actually *excited* in the bismuth by the magnet. Such induced currents would, according to known laws, have a direction *opposed* to those of the inducing magnet, and hence would produce the phænomena of repulsion. To carry out the assumption here made, M. Weber is obliged to suppose that the molecules of diamagnetic bodies are surrounded by channels, in which the induced molecular currents, once excited, continue to flow without resistance.

This theory, notwithstanding its great beauty, is so extremely artificial, that I imagine the general conviction of its truth cannot be very strong; but there is one conclusion flowing from it which appears to me to be in direct opposition to experimental facts. The conclusion is, "*that the magnetism of two iron particles in the line of magnetization is increased by their reciprocal action; but that, on the contrary, the diamagnetism of two bismuth particles lying in this direction is diminished by their reciprocal action.*" The reciprocal action of the particles varies inversely as the cube of the distance between them: at a distance expressed by the number 1, for example, the enfeeblement is eight times what it would be at the distance 2.

The conclusion, as regards the iron, is undoubtedly correct; but I believe experiment proves that the mutual action of diamagnetic molecules, when caused to approach each other, *increases* their repulsive action. I have had massive iron moulds made and coated with copper by the voltaic current; into these fine bismuth powder has been introduced and submitted to powerful hydraulic pressure. No sensible fact can, I think, be more certain, than that the particles of this dust are brought into closer proximity along the line in which the pressure is exerted, and this is the line of *strongest diamagnetization*. If a portion of the compressed mass be placed upon the end of a torsion beam and the amount of repulsion measured, it will be found that the repulsion is a maximum when the line of magnetization coincides with the line of compression; or, in other words, with that line in which the particles are packed most closely together: if the bismuth were fixed, and the magnet moveable, the former would repel the latter with a maximum force with the line of compression parallel to the direction of magnetization: it is a stronger *diamagnet* in this direction than in any other. Cubes of bismuth, which, in virtue of their crystallization, possessed a line of minimum magnetization, have been placed in those moulds and pressed closely together in the direction of the said line: the approximation of the particles thus effected has converted the direction spoken of from one of minimum into one of maximum magnetization. It would be difficult for me to say how many diamagnetic bodies I have submitted to compression, some massive, some in a state of powder, but in no single instance have I discovered an exception to the law that the line of compression of purely diamagnetic bodies is the line of strongest diamagnetization. The approximation of diamagnetic particles is therefore accompanied by an augmentation of their power, instead of a diminution of it, as supposed by the theory of M. Weber.

Any hypothesis which involves the idea of the diminution of the diamagnetic action of a body by the approximation of its particles, is, I believe, opposed to facts. Such a hypothesis must, I imagine, form the basis of the following remark of Prof. W. Thomson:—referring to "a thin bar or needle of a diamagnetic substance," he says, "such a needle has no tendency to arrange itself across the lines of magnetic force; but, as will be shown in a future paper, if it be very small compared with the dimensions and distance of the magnet, the direction it will assume, when allowed to turn freely round its centre of gravity, will be that of the lines of force*." I have not found in any of the subsequent numbers of the Philosophical Magazine the proof here promised†. But I doubt not the conclusion involves the assumption that the mutual action of diamagnetic particles is to weaken each other, and hence to produce a more feeble magnetization *along* a thin diamagnetic bar than *across* it—an assumption which, as already shown, is contradicted by experiment.

It is scarcely possible to reflect upon the discovery of Faraday in all its bearings, without being deeply impressed with the feeling that we know absolutely nothing of the physical causes of magnetic action. We find the magnetic force producing, by processes which are evidently similar, two great classes of effects. We have a certain number of bodies which are attracted by the magnet, and a far greater number which are repelled by the same agent. Supposing these facts to have been known to Ampère, would he have satisfied his profound mind by founding a theory which accounts for only the smaller portion of them? This theory is admirable as far as it goes, but the generalization is yet to come which shall show the true relationship of phænomena, towards whose connexion the theory of Ampère furnishes at present no apparent clue.

Royal Institution, October 1854.

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* Phil. Mag. vol. xxxvii. p. 244.

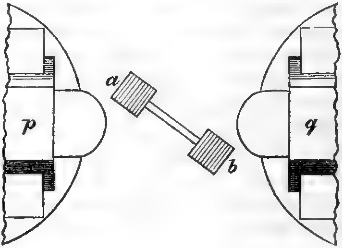
† This remark appears to have induced Mr. Thomson to publish the proof referred to in the last Number of the Philosophical Magazine. The arguments there brought forward have been long familiar to me, but I regret to say that I cannot attach much real value to them. At some future day I hope to be able to justify the scepticism which I here venture to express.—J. T., May 5, 1855.

Note on M. Matteucci's *Objections.*

The foregoing memoir was on the point of leaving my hands for the Royal Society, when accident, backed by the kindness of Mr. Faraday, placed the *Cours Spécial* of M. Matteucci, recently published in Paris, in my hands. An evening's perusal of this valuable work induces me to append the following remarks to the present paper.

M. Matteucci honours the researches which bear my name, and those which I published in connexion with M. Knoblauch, with a considerable share of his attention. He corroborates all the experimental facts, but at the conclusion states three objections to the manner in which these facts have been explained. "La faveur," writes the learned Italian, "avec laquelle les idées de MM. Tyndall et Knoblauch ont été accueillies m'imposent le devoir de ne pas vous laisser ignorer les objections qui s'élèvent contre elles. La première consiste dans la différence très-grande et constant dans la force qui fait osciller entre les poles un aiguille de bismuth cristallisé, suivant que ses clivages parallèles à sa longueur sont suspendus verticalement ou dans un plan horizontal : cette différence me parait inconciliable avec le résultat déjà rapporté de l'expérience de M. Tyndall, sur lequel se fonde l'explication des phénomènes magneto-cristallisés. Mais une objection encore plus grave est celle du mouvement d'*attraction** vers les poles qui se manifeste dans les prismes de bismuth cristallisé dont les clivages sont perpendiculaires à leur longueur. Pour rendre la conséquence de cette dernière expérience encore plus évidente, j'ai fixé deux cubes de bismuth, qui ont deux faces opposées naturelles et parallèles aux plans de clivage, aux extrémités d'un petit levier de verre, ou de sulphate de chaux, suspendu par un fil de cocon au milieu du champ magnétique entre les extrémités polaires d'un electro-aimant (fig. 27*a*) ; lorsque les deux cubes ont les clivages verticaux et perpendiculaires à la longueur à l'aiguille, au moment où le circuit est fermé, l'aiguille est attiré, *quelle que soit la position* qu'elle occupe dans le champ magnétique, et se fixe en equilibre dans la ligne polaire. . . . . . Il me semble impossible d'expli-

Fig. 27*a*.



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* This is in reality not a 'movement of *attraction*,'—see Appendix to the present paper.—J. T., May 1855.

quer ces mouvements du bismuth cristallisé, comme on a essayé de la faire, par la force repulsive de l'aimant, qui, suivant l'expérience de M. Tyndall*, s'exerce avec plus d'intensité parallèlement aux clivages que dans la direction perpendiculaire à ces plans.

"Remarquons encore qu'on ne trouve pas constamment l'accord qui devrait exister, selon les idées de MM. Tyndall et Knoblauch, entre les phénomènes magneto-cristallisés et les effets produits par la compression dans le bismuth, si l'on considère ces plans de clivages et la ligne suivant laquelle la compression a eu lieu comme jouissant des même propriétés†."

With regard to the first objection, I may say that it is extremely difficult to meet one so put; it is simply an opinion, and I can scarcely say more than that mine does not coincide with it. I would gladly enter upon the subject and endeavour to give the objection a scientific form were the necessary time at my disposal, but this, I regret to say, is not the case at present. I shall moreover be better pleased to deal with the objection after it has assumed a more definite form in the hands of its proposer, for I entertain no doubt that it is capable of a sufficient answer. The second objection M. Matteucci considers to be a more grave one. The facts are as follows:—the repulsion of a mass of crystallized bismuth depends upon the direction in which the mass is magnetized. When the magnetizing force acts in a certain direction, the intensity of magnetization, and the consequent repulsion of the mass, is a maximum. This is proved by placing the mass upon the end of a torsion beam and bringing its several directions successively into the line of the magnetic force. Poisson would have called such a direction through the mass a principal axis of magnetic induction, and I have elsewhere called it a line of elective polarity. When a sphere or cube of bismuth is freely suspended in the magnetic field, with the direction referred to horizontal, in all positions except two the forces acting on the mass tend to turn it; those positions are, when the line of maximum magnetization is axial and when it is equatorial, the former being a position of unstable, and the latter a position of stable equilibrium. When the above line is oblique to the direction of magnetization, the sphere or cube will turn round its axis of suspension until the direction referred to has set itself at right angles to the line joining the poles. Now if the direction of maximum magnetization be transverse to an elongated mass of bismuth, such a mass must, when the said direction recedes to the equator, set its length from pole to pole.

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* This was first proved by Mr. Faraday.—J. T.

† *Cours spécial sur l'induction, &c.*, p. 255.

The facts observed by M. Matteucci seem to me to be a simple corroboration of this deduction*.

The third objection is directed against an imaginary case, "si l'on considère les plans de clivage et la ligne de compression comme jouissant des même propriétés." It must be evident that a crystal like bismuth, possessing a number of cleavages of unequal values, cannot be compared in all respects with a body which has suffered pressure *in one direction* only. I have no doubt whatever, that, by a proper application of force in different directions, a compressed mass might be caused to imitate to perfection every one of the actions exhibited by crystallized bismuth. Indeed I would go further, and say, that I shall be happy to undertake to reproduce, with bismuth powder, the deportment of any diamagnetic crystal whatever that M. Matteucci may think proper to name.

In looking further over M. Matteucci's instructive book, I find another point alluded to in a manner which tempts me to make a few remarks in anticipation of a fuller examination of the subject. The point refers to the reciprocal action of the particles of magnetic and diamagnetic bodies. It is easy to see, that if the attraction of a bar of iron varies simply as the number of the particles attracted, then, inasmuch as the weight of the body varies in the same ratio, and the moment of inertia as the weight, the times of oscillation of two masses of the same length, but possessing different numbers of attracting particles, must be the same. Coulomb indeed mixed iron filings with wax, so as to remove the particles out of the sphere of their mutual inductive action, and proved that when needles of equal lengths, but of different diameters, were formed from the same mixture, the duration of an oscillation was the same for all. From this he inferred that the attractive force is simply proportional to the number of ferruginous particles; but this could not be the case if these particles exerted any sensible reciprocal action, either tending to augment or diminish the induction due to the direct action of the magnet. On account of such a mutual action, two bars of soft iron, of the same length, and of different diameters, have not the same time of oscillation.

In examining the question whether the particles of diamagnetic bodies exert a similar reciprocal action, M. Matteucci fills quills of the same length, and of different diameters, with powdered bismuth, and finds that there is no difference between the duration of an oscillation of the thick ones and the slender ones; from this he infers that there can be no reciprocal action among the particles of the bismuth.

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* For a more complete examination of this subject see the Appendix to this paper.—J. T., May 1855.

Now it is not to be imagined that even in Coulomb's experiments with the iron filings the molecular induction was absolutely nothing, but simply that it was so enfeebled by the separation of the particles that it was insensible in the experiments. This remark applies with still greater force to M. Matteucci's experiments with the bismuth powder; for the enfeeblement of a force already so weak, by the division of the diamagnetic mass into powder, must of course practically extinguish all reciprocal action of the particles, even supposing a weak action of the kind to exist when the mass is compact.

I will not here refer to my own experiments on compressed bismuth, but will take a result arrived at by M. Matteucci himself while repeating and corroborating these experiments. "I made," says M. Matteucci, "two cylinders of bismuth precisely of the same dimensions, the one compressed, the other in its natural state, and found that the compressed mass had a diamagnetic power *distinctly superior* to that of natural bismuth*." Now M. Matteucci, in his *Cours Spécial,* has made his own choice of a test of reciprocal molecular action; he assumes that if cylinders of the same length, but of different masses, have equal times of oscillation, it is a conclusive proof that there is no action of the kind referred to. This necessarily implies the assumption, that were the times of oscillation *different,* a reciprocal action would be demonstrated. Now, according to his experiments described in the Association Report, the times of oscillation *are* different; the diamagnetism of the compressed cylinder is "*distinctly superior*" to that of the uncompressed one: *the diamagnetic effect increases in a greater proportion than the quantity of matter*; and hence, on M. Matteucci's own principles, the result negatived by his experiments on powdered bismuth is fairly established by those which he has made with the compressed substance.

APPENDIX†.

Reflecting further on the subject of diamagnetic polarity, an experiment occurred to me which constitutes a kind of crucial test to which the conclusions arrived at in the foregoing memoir may be submitted.

Two square prisms of bismuth, 0·43 of an inch long and 0·2 of an inch wide, were laid across the ends of a thin plate of cedar wood, and fastened there by white wax. Another similar plate of wood was laid over the prisms, and also attached to them by

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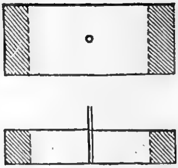
* Report of British Association for 1852, Transactions of Sections, p. 7.

† Received December 21, 1854.

wax; a kind of rectangular box was thus formed, 1 inch long and of the same width as the length of the prisms, the ends of the box being formed by the prisms, while its sides were open. Both plates of wood were pierced through at the centre, and in the aperture thus formed a wooden pin was fixed, which could readily be attached to a suspending fibre. Fig. 1 represents the arrangement both in plan and section.

Fig. 1.



The prisms first chosen were produced by the compression of fine bismuth powder, without the admixture of gum or any other foreign ingredient, the compressed mass being perfectly compact and presenting a surface of metallic brilliancy. If such a mass be placed on the end of a torsion balance and a magnetic pole is brought to bear upon it, I have proved the repulsion to be maximum when the direction in which the mass has been compressed is in the continuation of the axis of the magnet. A comparative view of the repulsion in this direction, and in another perpendicular to it, is given in the following Table:—

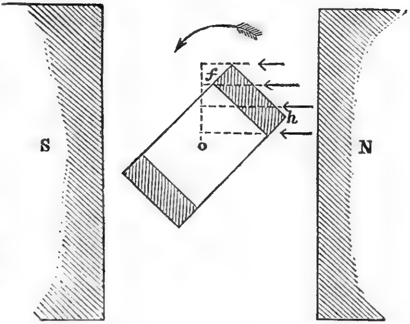
Compressed bismuth powder.

	Repulsion.	
Strength of magnet.	Line of pressure axial.	Line of pressure equatorial.
5·8	22	13
8·4	46	31
10·0	67	46
11·9	98	67

We see here that the repulsion, when the line of pressure is axial, exceeds what occurs when the same line is equatorial by fully one-half the amount of the latter. Now this can only be due to the more intense magnetization, or rather diamagnetization, of the bismuth along the line of pressure; and in the experiment now to be described I availed myself of this fact to render the effect more decided.

The prisms of bismuth were so constructed that the line of pressure was parallel to the length of each. The rectangular box before referred to was suspended from its centre of gravity O in the magnetic field, so that the two prisms were in the same horizontal plane. Let the position of the box thus suspended horizontally be that shown in fig. 2. For the sake of simplicity, we will confine our attention to the action of one of the poles N, which may be either flat or rounded, upon the prism *hf* adjacent to it, as indeed all the phænomena to be described can be produced before a single pole. The direction of the force emana-

Fig. 2.



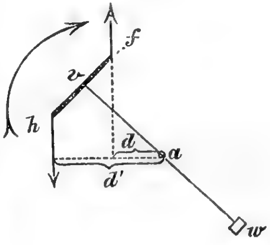
ting from N is represented by the arrows; and if this force be *purely repulsive*, the action upon every single particle of the diamagnetic mass furnishes a moment which, in the position here assumed, tends to turn the rectangular box in the direction marked by the arrow. It is perfectly impossible that such a system of forces could cause the box to turn in a direction opposed to the arrow; yet this is the precise direction in which the box turns when the magnetic force is developed.

Here, then, we have a mechanical effect which is perfectly inexplicable on the supposition that the diamagnetic force is purely repulsive. But if the conclusions arrived at in the foregoing memoir be correct, if the diamagnetic force be a polar force, then we must assume that attraction and repulsion are developed simultaneously, as in the case of ordinary magnetic phænomena. Let us examine how this assumption will affect the analysis of the experiment before us.

The marked end of a magnetic needle is pulled towards the north magnetic pole of the earth; and yet, if the needle be caused to float upon a liquid, there is no motion of its mass towards the terrestrial pole referred to. The reason of this is known to be, that the south end of the needle is repelled by a force equal to that by which the north, or marked end, is attracted. These two equal and opposite forces destroy each other as regards *a motion of translation*, but they are effective in producing *a motion of rotation*. The magnetic needle, indeed, when in a position oblique to the plane of the magnetic meridian, is solicited towards that plane by a mechanical couple, and if free to move, will turn and find its position of equilibrium there.

Let such a needle, *fh*, be attached, as in fig. 3, to the end of a light wooden beam, *vw*; let the beam and needle be suspended horizontally from the point *a*, round which the whole system is free to turn, the weight of the needle being ba-

Fig. 3.



lanced by a suitable counterpoise, w; let the north pole of the earth be towards N. Supposing the beam to occupy a position oblique to the magnetic meridian, as in the figure, the end f, or the marked end, of the needle is solicited towards N by a force ϕ, and the tendency of this force to produce rotation in the direction of the arrow is expressed by the product of ϕ into the perpendicular drawn from the axis of rotation upon the direction of the force. Setting this distance $=d$, we have the moment of ϕ in the direction stated,

$$=\phi d.$$

The end h of the needle is repelled by the earth's magnetic pole with a force ϕ': calling the distance of the direction of this latter force from the axis of rotation d', we have the moment of ϕ' in a direction opposed to the arrow,

$$=\phi' d'.$$

Now as the length of the needle may be considered a vanishing quantity, as compared with its distance from the terrestrial pole, we have practically

$$\phi=\phi',$$

and consequently

$$\phi d<\phi' d'.$$

The tendency to turn the lever in a direction opposed to the arrow is therefore predominant; the lever will obey this tendency, and move until the needle finds itself in the magnetic meridian: when this position is attained, the predominance spoken of evidently ceases, and the system will be in equilibrium. Experiment perfectly corroborates this theoretic deduction.

In this case, the centre of gravity of the needle recedes from the north magnetic pole as if it were repelled by the latter; but it is evident that the recession is not due either to the attraction or repulsion of the needle considered as a whole, but simply to the mechanical advantage possessed by the force ϕ', on account of its greater distance from the axis of rotation. If the force acting upon every particle of the needle were purely *attractive*, it is evident that no such recession could take place. Supposing, then, that we were simply acquainted with the fact, that the end f of the needle is attracted by the terrestrial pole, and that we were wholly ignorant of the action of the said pole upon the end h, the experiment here described would lead us infallibly to the conclusion that the end h must be repelled. For if it were attracted, or even if it were neither attracted nor repelled, the motion of the bar must be *towards* the pole N instead of in the opposite direction.

Let us apply this reasoning to the experiment with the bismuth prisms already described. The motion of the magnetic needle in the case referred to is not more inexplicable, on the assumption of a purely attractive force, than is the motion of our rectangular box on the assumption of a purely repulsive one; and if the above experiment would lead to the conclusion that the end h of the magnetic needle is repelled, the experiment with the bismuth leads equally to the conclusion that the end f of the prism hf, fig. 2, must be *attracted* by the pole N. The assumption of such an attraction, or in other words, of diamagnetic polarity, is alone capable of explaining the effect, and the explanation which it offers is perfect.

On the hypothesis of diamagnetic polarity, the prism hf turns a hostile end h to the magnetic pole N, and a friendly pole f away from it. Let the repulsive force acting upon the former be ϕ, and the attractive force acting upon the latter ϕ'. It is manifest that if ϕ were equal to ϕ', as in the case of the earth's action, or in other words, if the field of force were perfectly uniform, then, owing to the greater distance of ϕ' from the axis of rotation, from the moment at which the rectangular box quits the equatorial position, which is one of unstable equilibrium, to the moment when its position is axial, the box would be incessantly drawn towards the position last referred to.

But it will be retorted that the field of force is not uniform, and that the end h, on account of its greater proximity to the magnet, is more forcibly repelled than the end f is attracted: to this I would reply, that it is only in "fields" which are approximately uniform that the effects can be produced; but to produce motion towards the pole, it is not necessary that the field should be perfectly uniform: setting, as before, the distance of the direction of the force ϕ from the axis of rotation $=d$, and that of the force $\phi'=d'$, a motion towards the pole N will always occur whenever

$$\frac{d'}{d} > \frac{\phi}{\phi'}.$$

To ascertain the diminution of the force on receding from a polar surface such as that here used, I suspended a prism of bismuth, similar to those contained in the rectangular box, at a distance of 0·9 of an inch from the surface of the pole. Here, under the action of the magnet excited by a current of ten cells, the number of oscillations accomplished in a second was 17; at 0·7 of an inch distant the number was 18; at 0·5 of an inch distant the number was 19; at 0·3 distant the number was 19·5; and at 0·2 distant the number was 20. The forces at these respective distances being so very little different from each other, it follows that a very slight deviation of the box from the equatorial position is sufficient to give the moment of ϕ' a preponderance over that of ϕ, and consequently to produce the exact effect observed in the experiment.

The consistency of this reasoning is still further shown when we operate in a field of force which diminishes speedily in intensity as we recede from the magnet. Such a field is the space immediately in front of pointed poles. Suspending our rectangular box between the points, and causing the latter to approach until the box has barely room to swing between them, it is impossible to produce the phænomena which we have just described. The intensity with which the nearest points of the bismuth bar are repelled so much exceeds the attraction of the more distant end, that the moment of attraction is not able to cope successfully with the moment of repulsion; the bars are consequently repelled *en masse*, and the length of the box takes up a position at right angles to the line which unites the poles.

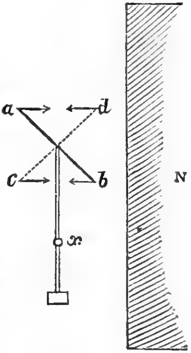
It is manifest, however, that by increasing the distance between the bismuth bar and the points acting upon it, we diminish the difference of action upon the two ends of the bar. When the distance is sufficient, we can produce, with the pointed poles, all the phænomena exhibited between flat or rounded ones.

All the effects which have been described are produced with great distinctness when, instead of compressed bismuth, two similar bars of the crystallized substance are used, in which the planes of principal cleavage are parallel to the length. Such bars are not difficult to procure, and they ought to hang in the magnetic field with the planes of cleavage vertical. It is unnecessary to describe the experiments made with such bars; they exhibit with promptness and decision all the effects observed with the compressed bismuth.

We have hitherto operated upon elongated masses of bismuth; but with the compressed substance, or with the substance crystallized uniformly in planes, as in the case last referred to, an elongation of the mass is not necessary to the production of the effects described. Previous, however, to the demonstration of this proposition, I shall introduce a kind of lemma, which will prepare the way for the complete proof.

Diamagnetic bodies, like paramagnetic ones, vary considerably in the intensity of their forces. Bismuth or antimony, for example, exhibits the diamagnetic force with greater energy than gold or silver, just as iron or nickel exhibits the magnetic force with greater energy than platinum or chromium. Let two thin bars, *ab*, *cd*, fig. 4, of two bodies of different diamagnetic powers be placed at right angles to each other, so as to form a cross; let the cross be attached to the end of a lever and suspended horizontally from the point x, before the flat or rounded pole N of a magnet. Let the continuous line ab represent the needle of the powerful diamagnetic body, and the broken line cd that of the feeble one. On the former a mechanical couple acts in the direction denoted by the arrows at its ends; and on the latter a couple operates in the direction of the arrows at *its* ends. These two couples are evidently opposed to each other; but the former being, by hypothesis, the more powerful of the two, it will overcome the latter. The mechanical advantage possessed by the *attracted* end a of the more powerful bar, on account of its greater distance from the axis of suspension x, will, in an approximately uniform field of force which we here assume, cause the centre of gravity of the cross to move towards the pole N.

Fig. 4.



In the formation of such a cross, however, it is not necessary to resort to two different substances in order to find two needles of different diamagnetic powers; for in crystallized bodies, or in bodies subjected to mechanical pressure, the diamagnetic force acts with very different energies in different directions. Let a mass of a diamagnetic body which has been forcibly compressed in one direction be imagined; let two needles be taken from such a mass, the one with its length parallel, and the other with its length perpendicular to the line of pressure. Two such needles, though composed of the same chemical substance, will behave exactly as the two bars of the cross in the experiment last described; that needle whose length coincides with the line of pressure will bear the same relation to the other that the needle of the powerfully diamagnetic substance bears to that of the feeble one. An inspection of the table at page 282 will show that this must be the case.

It is also shown in the following table, that in masses of crystallized bismuth the diamagnetic repulsion acts with very different energies in different directions. Cubes were taken from a mass of bismuth with the planes of principal cleavage parallel throughout to two opposite faces of each cube. The cubes were placed upon the ends of a torsion balance, and the diamagnetic repulsion was accurately measured when the force acted parallel to the planes of cleavage. The cubes were then turned 90° round, and the repulsion was measured when the force acted perpendicular to the planes referred to.

Cubes of crystallized bismuth.

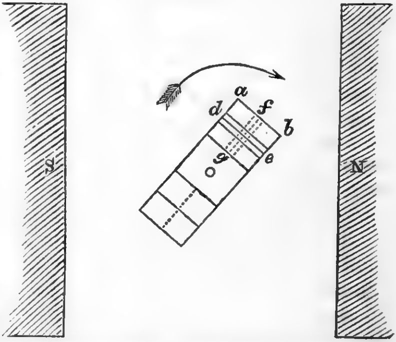
Strength of magnet.	Repulsion when the force was directed	
	along the cleavage.	across the cleavage.
3·6	11·7	8
5·7	34·8	23
8·4	78	53
10·0	111	76·5
11·9	153	110

It is manifest from this table that bismuth behaves as a body of considerably superior diamagnetic power when the force acts *along* the planes of cleavage.

Let two indefinitely thin needles be taken from such a mass, the one with its length parallel, and the other with its length perpendicular to the planes of cleavage; it is evident that if two such needles be formed into a cross and subjected to experiment in the manner above described, the former will act the part of the more powerfully diamagnetic needle, and produce similar effects in the magnetic field.

We now pass on to the demonstration of the proposition, that it is not necessary that the crystallized masses should be elongated to produce the effects exhibited by the prisms in the experiments already recorded. Let us suppose the ends of our rectangular box to be composed of cubes, instead of elongated masses, of crystallized bismuth, and let the planes of principal cleavage be supposed to be parallel to the face *ab*, fig. 5. Let the continuous line *de* represent an indefinitely thin slice of the cube passing through its centre, and the dotted line *gf* a similar slice in a perpendicular direction. These two slices manifestly represent the case of the cross in fig. 4; and were they alone active, the rectangular box, in a uniform field of magnetic force, must turn in the direction of the arrow. Comparing similar slices *in pairs* on each side of those two central slices, it is manifest that every pair parallel to the line *de* represents a stronger mechanical couple than every corresponding

Fig. 5.



pair parallel to *fg*. The consequence is, that a cube of crystallized bismuth suspended in the manner described, in a sufficiently uniform field of magnetic force, will move in the same direction as the cross in fig. 4: its centre of gravity will therefore *approach* the pole N, which was to be demonstrated.

This deduction is perfectly illustrated by experiment. It is manifest that the effect of the pole S upon the cube adjacent to it is to increase the moment of rotation of the rectangular box: the same reasoning applies to it as to the pole N.

Referring to fig. 27*a*, page 278, it will be seen that we have here dealt with the second and gravest objection of M. Matteucci, and converted the facts upon which the objection is based into a proof of diamagnetic polarity, so cogent that it alone would seem to be sufficient to decide this important question. Holding the opinion entertained by M. Matteucci regarding the nature of diamagnetic force*, his objection must have appeared to him to be absolutely unanswerable: I should be glad to believe that the remarks contained in this Appendix furnish, in the estimation of the distinguished philosopher referred to, a satisfactory explanation of the difficulty which he has disclosed.

Let me, in conclusion, briefly direct the reader's attention to the body of evidence laid before him in the foregoing pages. It has been proved that matter is repelled by the pole of a magnet in virtue of an induced condition into which the matter is thrown by such a pole. It is shown that the condition evoked by one pole is not that which is evoked by a pole of an opposite quality —that each pole excites a condition peculiar to itself. A perfect antithesis has been shown to exist between the deportment of paramagnetic and diamagnetic bodies when acted on by a magnet alone, by an electric current alone, or by a magnet and an electric current combined. The perplexing phænomena resulting from molecular structure have been laid open, and the antithesis between paramagnetic and diamagnetic action traced throughout. It is further shown, that whatever title to polarity the deportment of a bar of soft iron, surrounded by an electric current, and acted on by other magnets, gives to this substance, a bar of bismuth possesses precisely the same title: the disposition of forces, which in the former case produces attraction, produces in the latter case repulsion, while the repulsion of the iron finds its exact complement in the attraction of the bismuth. Finally, we have a case adduced by M. Matteucci which suggests a crucial experiment to which all our previous reasoning has been submitted, by which its accuracy has been proved, and the insufficiency of the assump-

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* "Il ne peut exister dans les corps diamagnétiques une polarité telle qu'on la concoit dans le fer doux."—*Cours Spécial*, p. 201.

tion, that the diamagnetic force is not polar, is reduced to demonstration. When we remember that against all this no single experimental fact or theoretic argument* which can in any degree be considered as conclusive, has ever been brought forward, nor do I believe can be brought forward, the conclusion seems irresistible, that we have in the agency by which bodies are repelled from the poles of a magnet, a force of the same dual character as that by which bodies are attracted; that, in short, "diamagnetic bodies possess a polarity the same in kind but the opposite in direction to that possessed by magnetic ones."

XXXV. *Experiments on some of the Compounds of Tin with Arsenic. By* Ed. Haeffely *of Mulhouse*†.

By pouring an excess of nitric acid into a solution of stannate of soda and arseniate of soda, in which the arseniate predominates, and bringing this mixture to a state of ebullition, a white gelatinous precipitate is produced, composed of arsenic acid, peroxide of tin, and water; when washed and dried at the ordinary temperature, it forms transparent fragments.

To analyse the above compound, the delicate process described by M. Levol‡ was employed, viz. placing within a glass tube heated to redness 100 parts in an anhydrous state, and passing through it a current of hydrogen, when the tin is left in the metallic form, and the arsenic sublimed in the tube. Three analyses gave,—

Tin . . .	45	46·3	45·21
Arsenic . .	27·2	26·3	

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* I ought perhaps to except an argument of Professor W. Thomson's, which professes to prove that an absolute creation of force, and the setting up of a perpetual motion, would follow, if diamagnetic polarity were conceded. While expressing my admiration of the ingenuity of Mr. Thomson's reasoning, it appears to me to labour under the disadvantage of proving too much, his conclusion being equally fatal to polarity of all kinds. The argument, I believe, was first publicly urged against myself at the Belfast Meeting of the British Association; but at the Liverpool Meeting last year Professor Thomson himself admitted "that he had not perfect confidence in the truth of the conclusion, as one of the assumptions on which the reasoning was founded admitted of doubt."—See Athenæum, 1854, p. 1204. Indeed, from many of his published papers, it might be inferred that Mr. Thomson actually *assumed* what I, in the present memoir, have attempted to *prove*.

I refrain from alluding to the negative results obtained by Mr. Faraday in repeating M. Weber's experiments; for though admirably suited to the exhibition of certain effects of ordinary induction, Mr. Faraday himself has shown how unsuitable the apparatus employed would be for the investigation of the question of diamagnetic polarity. See Experimental Researches (2653, 2654), vol. iii. p. 143.—J. T., May 9, 1855.

† Communicated by the Author.

‡ *Annales de Chimie et de Physique*, 3 sér. vol. xvi.

By adding to these numbers the respective quantities of oxygen required for their maximum oxidation, we arrive at the formula—

$$AsO^5+2SnO^2,$$

which requires—

Tin . .	44·52	Arsenic . .	28·31

Dried in the stove at 120° C. the loss amounted to 25·3 per cent., corresponding to 10 equivalents.

The formation of an insoluble arseniate of tin is nothing new of itself, for it has long been known that metalloidic acids form insoluble precipitates with certain insoluble metallic acids. I only point out the favourable circumstance by which a constant compound is formed, namely, by taking care to operate with an excess of the metalloidic acid. Phosphoric acid in excess gives with peroxide of tin,—

$$PO^5\,2SnO^2+10HO.$$

M. Levol, in seeking for an easy method to ascertain the quantity of arsenic in the metals and their alloys, operated in all his experiments with an excess of the metallic oxide, by which he obtained but imperfect precipitates of tin with arsenic (or antimony with arsenic). This led him to abandon the hope of determining this element by precipitation; he says, "the increase of weight which arsenic acid obtains by its combination with hydrated peroxide of tin (or antimony) did not appear to me a sufficient warrant for determining the arsenic from it in a rigorous analysis."

M. Levol's combining proportion varies from 1 of arsenic to 15, and 20 of tin: but operating under different circumstances, I am able to combine 12·72 of the metalloid with 20 of the metal. The above result may be applied in quantitative analysis, when a mere calculation will replace the delicate and somewhat complicated operation of M. Levol. I turned this formation of the constant compound $AsO^5\,2SnO^2$ to account in the analysis of the commercial stannates of soda, containing large admixtures of arseniate of soda.

To a known weight of a commercial stannate I add a *known quantity of arseniate of soda*, boil with an excess of nitric acid, collect the insoluble arseniate of tin $AsO^5\,2SnO^2$, and from its weight calculate the amount of *arsenic acid* and peroxide of tin.

The liquid filtrates are treated with sulphuretted hydrogen, to throw down as sulphuret of arsenic the *excess of arsenic acid* added intentionally. The amount of arsenic acid found, and the amount added intentionally being known, it will be easy to calculate the original quantity which the stannate contained.

Decomposition of $AsO^5\ 2SnO^2\ 10HO$.

In presence of an excess of a fixed alkali (soda used) this arseniate of tin is split into two different soda salts, one of which contains all the arsenic, and crystallizes at first in silky needles, while the mother-liquors consist of ordinary stannate of soda: the reaction takes place according to the equation—

$$2(AsO^5\ 2SnO^2)+9NaO=\underbrace{2(AsO^5,6NaO,SnO^2)}_{}+3(SnO^2\ NaO).$$

This same salt is also obtained by boiling 2 equivalents of terbasic arseniate of soda with 1 equivalent of hydrated peroxide of tin. (When hydrate of alumina is taken, an arseniate of alumina and soda is obtained.) To analyse the tin salt, I determined each constituent by direct processes.

A current of hot air deprived of its carbonic acid passed over 100 parts of the salt, gave a loss of 48 per cent. of water.

100 parts treated with an excess of boiling nitric acid gave 13·95 parts of the insoluble arseniate of tin, $AsO^5\ 2SnO^2$, corresponding to—

$$\begin{array}{l} 7{\cdot}887\ SnO^2=2\ \text{equivs.} \\ \underline{6{\cdot}063}\ AsO^5=1\ \text{equiv.} \\ 13{\cdot}95 \end{array}$$

100 parts acidulated with hydrochloric acid, treated with a current of sulphuretted hydrogen, gave 41·5 parts of double sulphuret of tin and arsenic.

These two weights being ascertained, a mere calculation may be substituted for the tedious operation of M. Levol, for it remains only to calculate the corresponding amount of double sulphurets which 13·95 parts of arseniate of tin would form, to be deducted from the total amount of double sulphuret, 41·5, obtained; as follows:—

$$13{\cdot}95 \text{ of } AsO^5\ 2SnO^2=15{\cdot}825=AsS^5\ 2SnS^2;$$

and 15·825 $AsS^5\ 2SnS^2$ deducted from 41·5 double sulphuret, leaves 25·675 of $AsS^5=19{\cdot}05\ AsO^5$.

By evaporating to dryness the hydrochloric acid filtrates, I get 37 of chloride of sodium $=19{\cdot}6$ soda.

Recapitulation of the Numbers.

	By experiment.	Calculated.	Equivalent numbers.	
AsO^5 . .	25·113	24·442	115	1 eq.
SnO^2 . .	7·887	7·970	93	3 ...
NaO . .	19·6	19·766	37·5	½ ...
HO . .	48	47·822	225	25
	100·6	100·000	470·5	

The formula of this salt is therefore—

$$2(AsO^5\,6NaO\,SnO^2)\,50HO.$$

Stannate of soda is used as a mordant in print and dye works. I have made numerous experiments on a large scale to decide whether in calico printing or dyeing, the hydrated peroxide of tin alone would be preferable to an arseniate of peroxide of tin as a mordant. The result of comparative trials leads me to give the preference to hydrated peroxide of tin alone, the various shades being more brilliant and less liable to unevenness than when arsenic acid is present. It would be desirable to substitute in commerce a purer stannate of soda for that at present sold, as well as for that containing arseniate of soda.

These experiments were made in the laboratory of Harrison Blair, Esq., near Manchester, who kindly afforded me every opportunity for carrying out the same. They have led at least to this practical fact, that the danger arising from the employment of arseniates in stannates of soda may be obviated by the use of a pure stannate of soda alone.

XXXVI. *Improvements on a Dew-point Hygrometer lately described by the Author**. *By* Professor CONNELL†.

A FEW simple alterations have greatly facilitated the employment of this instrument, and at the same time made it considerably less liable to injury. It consists, as will be recollected, of a little bottle of thin brass, into which the bulb of a thermometer is introduced so as to fit air-tight, and the neck of which is attached laterally to a small exhausting syringe. Æther having been previously introduced into the bottle, the temperature is by a gradual process of exhaustion reduced, until moisture begins to be condensed on the polished surface of the bottle, when the temperature of deposition is indicated by the thermometer. A little collar of ivory is introduced between the bottle and the syringe to prevent the heat produced by the friction of the piston and cylinder from being communicated to the bottle. The valves of the syringe are made of gold-beater's leaf, as the vapour of the æther was found to act on valves of oiled silk.

As washers of leather required to be introduced on both sides of the ivory collar to make the connexions air-tight, considerable difficulty was experienced, from the varying thickness of these

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* Edinburgh Philosophical Transactions for 1854, and Philosophical Magazine, vol. viii. p. 81.

† Communicated by the Author, having been read to the Physical Section of the British Association at Glasgow, September 17, 1855.

washers, in bringing the bottle with the inserted thermometer into the proper vertical position. This inconvenience has been completely remedied by connecting the neck of the bottle with the syringe by means of a coupling screw, instead of an ordinary screw. In this way the bottle and thermometer can be at once brought into any required position, without the use of any washer at all on that side; and as the screw projecting from the ivory collar on that side is now made of brass, instead of being of ivory as before, it is not subject to fracture when any little additional force is necessary in order to unscrew it. In this way both additional facility of manipulation and additional security from accident are attained. This alteration also creates much greater facility of attachment, according to the varying nature or position of the fixtures to which the clamp which secures the instrument when in operation is made fast. The form of the key employed in screwing and unscrewing the bottle and ivory collar has also been altered in a way which does not risk the fracture of the collar as the previous one did. It now grasps the collar externally, like that used for an air-pump or gas-holder. The new forms of screw, ivory valve-piece, and key, can easily be adapted to any instrument which has been constructed in the original manner.

It is also satisfactory to be able to state, that during this summer I have had ample experience of the instrument being in all its parts quite calculated to stand the ordinary concussions of travelling, having carried it with me in its proper case in my ordinary baggage, during a tour on the Continent, without injury to any part of it. On this occasion I have also found that it worked equally well as at home, in the various situations in which I employed it in Germany, Switzerland and France*.

It may be mentioned that the gold-beater's leaf valve is constructed by folding a piece of that material, 2 inches long by $\frac{1}{2}$ an inch wide, first longitudinally and then transversely. This fourfold portion is then placed across the valve-piece and secured in its place by two or three coils of silk thread, and the superfluous leaf and thread cut away by a sharp pen-knife. A drop of sperm oil on the point of the knife is then passed carefully under the leaf and the knife instantly withdrawn. Gentle pressure by the point of the finger causes the oil to pass over the whole surface under the leaf. The proper state of the valve is ascertained both by blowing and suction.

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* In order to meet the wishes of those who may desire to find their own bottles for æther and olive oil, Mr. Kemp of Edinburgh has fitted up all the essential parts of the instrument in a smaller and less expensive case, which he finds he can afford to sell at two-thirds of the cost of the more complete arrangement; those, however, who may wish to have everything necessary at their hand, can still obtain from him the complete collection in their proper case.

In the use of this hygrometer, the person of the experimenter can easily be kept at a sufficient distance from the condensing surface; and in the ordinary mode of operating at an open window whilst the little brass bottle is in the open air, the body of the experimenter is of course in the room. Were it thought necessary, a small telescope or opera-glass might be used to observe the surface, whilst the handle of the piston-rod might be lengthened so as to admit of still greater distance.

XXXVII. *Notices respecting New Books.*

Experimental Researches in Electricity. *By* MICHAEL FARADAY, *D.C.L., F.R.S. &c. &c.* 3 vols. 8vo. London: Taylor and Francis.

THREE volumes of Faraday's Experimental Researches are now before the public. They embrace his contributions to the Philosophical Transactions, the Philosophical Magazine, and other journals during a period of upwards of thirty years. The first of these volumes opens with the celebrated memoir on Voltaic and Magneto-electric Induction. In the year 1820 Arago and Humboldt were making experiments on terrestrial magnetism on the slope of Greenwich Hill: Arago observed that the magnetic needle came more quickly to a state of rest when in proximity with certain bodies, and following up this observation he founded upon it his so-called magnetism of rotation. The investigation of the force which produced the rotation of the magnetic needle occupied for years the attention of English and continental philosophers; exact experiments were made and explanations given, all of which, however, were overthrown by the subsequent experiments of Arago himself. The fact stood forth as a challenge to the natural philosophers of the day, until in the year 1831 Faraday crushed the difficulty, and embraced the discovery of Arago in that of magneto-electric induction—a discovery which rivals in importance that of the pile by Volta, and, indeed, already threatens to supersede the latter in its most important practical applications.

There are men, who, having made a great discovery—having passed the trying furnace of thought through which such a result is approached,—rest contented with their achievement, and evince no desire to renew the struggle by which their laurels were won. The great Italian whose name we have mentioned, suffers in this respect by a comparison with Faraday. The latter drew from his discovery courage and strength to go on to others, and the volumes now before us are the practical record of a life of scientific activity unexampled in result since the time of Newton. Faraday has not been disciplined in the accredited schools of science: he has figured the operations of nature in his own fashion before his mental eye, and the very richness of his imagination in this respect often places his readers in difficulty. We question much whether those who dwell most upon Faraday's simplicity of expression are really aware of the effort sometimes required to understand him. Such a man is not to be skimmed over superficially; he cannot make his thoughts an alphabet for children, and those who would accompany him to the depths from which he speaks must be prepared to encounter difficulties in the way. Often indeed he speaks in a language not generally known even among scientific men. As Kant in the 'Critic of Pure Reason' was almost necessitated to elaborate a new language for himself, so the incessant contact with new truths which it has been Faraday's fortune to experience has rendered a divergence from the accredited forms of expression necessary to him. He has seen theories fade before the stern realities which he has brought to light, and it is therefore impossible for him to make use of the language of those theories with the undoubting confidence of those who believe in them. An apparent vagueness may sometimes be the result of this scepticism, but this is inevitable with the man, who, working on the boundaries of science, discerns the inadequacies of received notions, and has to cross a chasm of doubt and darkness in his transition from the defective to the true.

The salient qualities of Faraday's scientific character manifest themselves from the beginning to the end of these volumes,—a union of ardour and patience, the former prompting the attack, the latter holding him to his subject until he has obtained complete possession of it. Every experiment is a wedge which opens a way before it to other results. He places his subject in all imaginable positions, looks round it, and observes all its outlying relations. Give him hold of a fact and the probability is that he will draw a hundred after it, and so master its dependencies as to leave his followers nothing to find. How strikingly is he in this respect contrasted with Oersted, who, having made a great discovery, seemed to have a wall of adamant built round his intellect to prevent him from pursuing it to its consequences. Fructified by this man's intellect, each fact becomes a seedling which ever sprouts and blossoms into new results.

The memoir to which we have referred as commencing the first volume is followed by others of great importance;—on the Identity of Electricity, &c.,—on the Laws of Electric Conduction, wherein the remarkable influence of fusion upon conducting power is made manifest,—on Electro-Chemical Decomposition,—on the Power of Platinum to induce Combination,—on the Definite Nature and Extent of the Chemical Forces—a memoir of incalculable value, which excited the thoughts and admiration of even metaphysicians at the time of its publication, and at the present moment stirs profoundly the minds of scientific men. Faraday has introduced many new terms into science, and the utility of some may not be manifest to superficial thought. But scientific terms, arising as they often do out of theoretic contemplations, often introduce something more than, or different from, the fact into the mind, and hence the possible injury arising from their use. Faraday's constant effort has been to displace those terms which carry with them foregone conclusions by others of a character so neutral that the thought shall be uninfluenced by the term. Passing over many minor papers we arrive at the important memoirs on Static Induction, on Conduction, and on Induction in Curved Lines. The latter idea reveals perhaps as clearly as any other the profound and subtle character of Faraday's mind. The question he proposes to himself is: "How is the electric power propagated? Does it act like the force of gravity at a distance without the help of intervening matter, or does it act in virtue of such intervention?" He reflects thus: If it be a force acting at a distance, this force will act in the straight line which unites each pair of active points, but if it be a force which propagates itself by the agency of material particles, there is every reason to suppose that it may act in curved lines, as sound, for example, which depends on the atmosphere for its propagation, can turn round a corner, or waves of water round a rock. Reasoning in the inverse way, if it can be established that the electric force propagates itself in curved lines, it may certainly be inferred from this that it requires a material medium for its propagation. Faraday believes that his experiments establish the latter point; others doubt it; but however this may be, the profound and philosophic nature of the thought which guided him is manifest. We may not be able to make Faraday's mode of symbolizing the operations of nature our own, but when we reflect upon the success which has attended his efforts, on the fact that his peculiar symbols enabled him to foresee results of which no other electrician dreamed, and which after a lapse of twenty years accident has brought to light, we are warned against being dogmatic on theoretic matters, and learn the salutary lesson, that the competent and earnest searcher after truth has other avenues than our licensed scientific turnpikes into nature.

The second volume of the Experimental Researches contains valuable papers on the Electricity of the Gymnotus, on the Source of Power in the Voltaic Pile, on the Electricity evolved by the Friction of Water and Steam, in which the principles of the hydro-electric machine of Armstrong are fully developed, a paper on Magnetic Rotations, dating as far back as 1821, some speculative observations, and the writer's letters in the only controversy which has ever disturbed his peaceful life. The memoir on the Source of Power in the Voltaic Pile is of peculiar importance. The arguments which it contains appear to us to overthrow wholly the hypothesis of contact as maintained by Volta. The doctrine of the conservation of force has received formal expression at the hands of many recent investigators, but the idea appears to have been a guiding light to Faraday throughout his entire scientific career, and the explicit statement of this principle at page 103 of the present volume places a difficulty in the way of the theory of contact which has never yet been overcome.

The third volume opens with an account of one of the most remarkable discoveries of modern times, entitled by the author, "the Magnetization of Light and the Illumination of Magnetic Lines of Force," and expressed in the theoretic language usual in our day, "the Rotation of the Plane of Polarization of a Luminous Ray. Up to the present time the value of this discovery has been principally suggestive; no new fact has been added to the subject since it left the hands of Faraday. A connexion between light and magnetism is here indicated which none of our theories would have enabled us to predict. None of our hypotheses would have prompted us to seek this result; they would rather have dissuaded us from the attempt, but this man sees the generally fleeting nature of those images by which the operations of nature are rendered tangible to the human mind, and is not to be warped from his path by a symbol. The method of true scientific investigation is probably incommunicable; it belongs to the individual rather than to the system, and our contemporaries, we think, miss their mark when they point at Faraday's researches as simply illustrative of the power of inductive philosophy. Faraday's researches are illustrative of the power of a strong and independent soul, expressing itself after its own methods, and acknowledging no mediator between it and Nature. You may fill your brain with inductive philosophy, but without the God-given force which this man possesses, and which signalizes him from all others at the present day, you will never accomplish what he has accomplished. Hotly following the discovery of the rotation of the plane of polarization is that of diamagnetism. At the end of the last century Bruggmanns had actually shown that a magnetic needle was repelled by a mass of bismuth: here he stopped. Le Baillif repeated the experiment with bismuth and antimony: here *he* stopped. Seebeck and others have touched upon the same subject; the cherry struck against their lips, but they failed to take it in. These fragmentary glances excited a momentary curiosity and were almost forgotten, until Faraday, without knowing anything of the experiments alluded to, rediscovered the same facts, but instead of stopping, he made them the inlet to a new and vast scientific domain, in which philosophers of all countries now find occupation. The value of Faraday's discoveries consists in a great degree in the amount of intellectual power which they call into action. It has been his good fortune to alight, not upon the isolated nuggets of science, which, while enriching himself might have left others without a share, but to strike the golden lodes at which the highest scientific spirits of the present age have worked, and at which thousands yet to come will work with incalculable benefit to mankind.

The present volume contains numerous offshoots from the great discovery just alluded to,—the author's experiments and speculations on Magne-crystallic Action,—the Magnetism of the Atmosphere, and its possible Influence upon the Diurnal Variation of the Magnetic Needle,—on the Nature of the Lines of Magnetic Force,—on Electric Induction in Subterranean Telegraph Wires, and papers on other kindred subjects. The mention of the telegraph reminds us of the application of the author's own discovery to telegraphic purposes, whereby the grave difficulties presented to the transmission of ordinary voltaic electricity through subterranean wires are practically overcome. The public never hear Faraday's name mentioned in connexion with the electric telegraph—scarcely that of Volta; the public contemplate the flower, but see not the roots from which its petals are fed; the men who have given the telegraph its present practical form are deserving of public esteem, and have obtained both it and more substantial marks of public appreciation; and yet the merits of Faraday, in comparison with such men, are about in the ratio of Prometheus to Dr. Kahn. Down deep in the mysteries of Nature, he and his great *confrère* Volta found the forces which pulse through the telegraphic wire. Honour to those who by their mechanical skill have brought these great discoveries home to human hearths, and made them the ministers of society; but a higher and nobler meed be his who brought us the fire from heaven which imparts life and activity to the telegraphic mechanism. This is the position of Faraday, this the position of Volta, although neither of them even derive the poor breath of popular applause in return for their transcendent services.

This leads us into sober reflections. The public never can know and appreciate the national value of such a man as Faraday. He does not work to please the public, nor to win its guineas; and the said public, if asked its opinion as to the practical value of his researches, can see no possible practical issue there. The public does not know that we need prophets more than mechanics in science,—inspired men, who, by patient self-denial and the exercise of the high intellectual gifts of the Creator, bring us intelligence of His doings in Nature. To them their pursuits are good in themselves. Their chief reward is the delight of being admitted into communion with Nature, the pleasure of tracing out and proclaiming her laws, wholly forgetful whether those laws will ever augment our banker's account or improve our knowledge of cookery. Such men, though not honoured by the title of 'practical,' are they which make practical men possible. They bring us the tamed forces of Nature, and leave it to others to contrive the machinery to which they may be yoked. The labour of the prophet, as we have called him, is unpurchaseable. Nevertheless he is a man, has the needs of a man, and, as age advances, feels the infirmities of a man. Has such a man no claim upon his country? We know not what provision has been made for him whose labours are now before us: we only know his services to science and to the public. If we are rightly informed, it was Faradaic electricity which shot the glad tidings of the fall of Sebastopol from Balaklava to Varna. Had this man converted his talent to commercial purposes, as so many do, we should not like to set a limit to his professional income. As before stated, we do not know the equivalent provided by Government for such sacrifices and such services as his,—where he is to rest his noble head when years have showered their snows upon it. The quality of his services cannot be expressed by pounds, but that brave body which for forty years has been the instrument of that great soul, is a fit object for a nation's care, as the achievements of the man are, or will one day be, the object of a nation's pride and gratitude.

XXXVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 226.]

June 14, 1855.—The Lord Wrottesley, President, in the Chair.

THE following communications were read:—

"Remarks on the Rev. H. Moseley's Theory of the Descent of Glaciers." By James D. Forbes, D.C.L., F.R.S.

In a paper "On the Descent of Glaciers," communicated to the Royal Society on the 19th of April, 1855, and printed in their Proceedings, the Rev. Henry Moseley has proposed an explanation of that phenomenon.

The first part of his paper contains a lucid description of the gradual motion of a sheet of lead covering the roof of Bristol Cathedral, which he ascribes (I have no doubt justly) to the successive expansions and contractions of the lead by atmospheric temperature. He explains the influence of the slope of the roof and of the measure of friction upon the motion with his customary precision and clearness. He also finds for the probable measure of the effect or creeping motion of the lead, a quantity which, considering the imperfect nature of the data with regard to temperature, agrees sufficiently well with observation.

In the latter and shorter part of the paper is a transition to the case of glaciers, whose motion over their beds may, he thinks, be accounted for in the same way, namely, by the alternate contraction and expansion of the ice by diurnal changes of temperature, and he then enters into certain calculations founded principally on data contained in my 'Travels in the Alps of Savoy' in confirmation of this view.

Entertaining as I do the highest respect for Mr. Moseley's eminent attainments as a theoretical mechanician, it is with extreme regret that I find it necessary, in maintenance of the views regarding glacier motion which I have elsewhere advanced, and in the interest of scientific truth, to show (as I believe I can) that Mr. Moseley has been led, apparently by a sudden inadvertency, to uphold an opinion completely indefensible.

I must first object to Mr. Moseley's description or definition of a glacier, as calculated to mislead the inquirer: he says (p. 339), "glaciers are, on an increased scale" [compared to the sheet lead covering of a roof], "sheets of ice placed upon the slopes of mountains." There are certainly some inconsiderable glaciers of the second order to which this description might possibly apply, with the exception of the small thickness inferred by the word "sheet;" but the true glaciers, whose theory has been so often discussed (which theory must evidently likewise include that of glaciers of the second order), cannot fairly be called either *sheets* of ice or be accurately described as lying *on* the slopes of mountains. They are vast icy accumulations whose depth bears a considerable proportion to their breadth, and which fill mountain ravines or valleys, Glaciers are very generally hemmed in by precipitous rocks which determine their *contour* or ground plan; they have often to make their way through contracted gorges where the ice occupies (as in the case of the Mer de Glace of Chamouni), within a short distance, a channel but half as wide as it did before. Yet the glacier, preserving its continuity as a whole, expands or contracts in conformity with the irregularities, not only of its lateral walls, but of its bed, forcing itself over obstacles, or even occasionally allowing itself to be cleft into two branches by them, and closing again into a united mass after the insular obstruction has been past. To speak of such resistances of the channel to the progress of the ice as mere *friction*, or of a glacier considered as a solid body and in its whole extent (or in any considerable part of it) as having an *angle of repose*, as in the case of a substance with a flat base resting on an inclined plane, is evidently inadmissible and tends to mislead. The valley of the Mer de Glace might have almost any possible inclination before the ice would tend to slide out of it *en masse*, for it is moulded to every sinuosity or protuberance of the bed, whether vertical or horizontal. Let Mr. Moseley imagine a sheet of lead having the ground plan of the *Mer de Glace* and confined by margins of wood accurately adapted to it, and he will see that unless lead were so ductile as to be entitled to the appellation of a semifluid, no motion could possibly result, however great might be the slope on which it lay.

I am sorry to find that Mr. Moseley denies entirely (p. 341) the viscous or plastic structure of a glacier as "not consistent with the fact that no viscosity can be traced in its parts when separated." The answer to this objection seems to be merely this; that the viscosity, though it cannot be "traced" in the parts, *if very minute*, nevertheless *exists* there, as unequivocally proved by experiment on the large scale, or even on spaces several yards or fathoms in extent*. The plastic condition of a glacier is, as I have repeatedly stated, no longer an hypothesis, but a *fact*, since I have in many places demonstrated that, account for it as we may, different portions of the same continuous mass of ice are moving at the same moment with different velocities. That a small piece of ice is not sensibly plastic, is not more strange than that the fine blue colour so perceptible in the glacier totally vanishes in its constituent fragments. That *ductility* and *fragility* are not incompatible qualities, is shown by the fact, that sealing-wax at moderate atmospheric temperatures will mould itself (*with time*) to the most delicate inequalities of the surface on which it rests, under a pressure of not more than half an inch of its substance, but may at the same time be shivered to atoms by a blow with a hammer.

The question of plasticity, however, affects only *mediately* Mr. Moseley's theory of the primary cause of motion by dilatation and contraction. According to the views I support, the dilatation and contraction of the ice of glaciers (assuming it to exist) would be inefficient to move the mass unless it moved plastically; and if it moves plastically, the supposition of its thermal expansion is, at all

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* See Phil. Trans. 1846, p. 162, and Phil. Magazine (1845), xxvi. p. 414.

events, superfluous, since gravity is in that case a sufficient moving force.

But, it will be argued, if the ice be really acted on by heat and cold as Mr. Moseley supposes, it is a *vera causa* of motion and cannot be neglected. And here we join issue respecting the physical theory proposed.

Mr. Moseley's explanation of the descent of the lead on a roof at an angle much below that at which motion could take place by gravity, friction being allowed for (the *angle of repose*), amounts to this, that every increase of temperature of the mass by the heat of the day expanding it, pushes the lower end downwards *more* than it pushes the upper end upwards; whilst the cold of the night retracts a little the lower end, but (being favoured by the slope) it pulls down the upper end more than it had been pushed up during the heat of the day, and thus by a species of vermicular motion impels the body down the inclined plane. The motion is calculated from a formula including the absolute expansibility of lead, the slope of the roof, the angle of repose, and the *diurnal range* of temperature. Taking then corresponding data for the Mer de Glace of Chamouni, assuming 30° to be the angle of repose of a glacier upon its bed, taking the expansion of ice to be nearly double that of lead (according to experiments made at St. Petersburgh), and the daily range of temperature *of the ice* to be the same as that *of the air* observed by De Saussure on the Col du Géant in the month of July, Mr. Moseley calculates the daily descent of the glacier opposite the Montanvert and compares it with my observations.

Waiving for the moment all other objections, *can we possibly attribute to the ice of the entire mass of this vast glacier an average daily range of temperature of* $4\frac{1}{4}°$ *of Reaumur or* $9\frac{1}{2}°$ *of Fahrenheit?* The idea seems to me to be perfectly untenable.

The expansion and contraction of ice by heat and cold can of course only take place *below* the freezing-point, or 32°. Let it be percolated by water as it may, it cannot rise *above* that temperature nor expand in the smallest degree. But it is a matter perfectly notorious, that, at least in summer, and throughout the whole extent of the Glacier Proper, and even far into the region of the *névé*, the glacier is charged with percolating water derived from superficial fusion. Mr. Moseley admits this, and even attributes the diurnal oscillation of temperature which he assumes, to the action of water, as in the following passage: "Glaciers are, on an increased scale, sheets of ice placed upon the slopes of mountains, and subjected to atmospheric variations of temperature throughout their masses by variations in the quantity and the temperature of the water, which flowing from the surface everywhere percolates them" (p. 339). This action therefore clearly brings the temperature of the ice up to 32° during the day. But how is the cold of the night to operate in reducing the temperature of a mass of ice certainly from 300 to 600 or more feet in thickness through the enormous average depression of $9\frac{1}{2}$ degrees? The water so efficient by its percolation in *raising* the temperature (if necessary) to 32°, being frozen, is now powerless. Cold can be conveyed downwards, or to speak more correctly, heat can be transmitted upwards through the ice only by the slow process of conduction, and this on the supposition that the depression of superficial temperature is all that the theory might require. But how stands the fact? Mr. Moseley quotes from De Saussure the following *daily ranges* of the temperature of the air in the month of July at the Col du Géant and at Chamouni, between which points the glacier lies.

At the Col du Géant	4°·257 Reaumur.
At Chamouni	10°·092 Reaumur.

And he assumes "the same mean daily variation of temperature to obtain throughout the length" [and depth?] "of the Glacier du Géant which De Saussure observed in July at the Col du Géant." But between what limits does the temperature of the air oscillate? We find, by referring to the third volume of De Saussure's Travels, that the mean temperature of the coldest hour* (4 A.M.) during his stay at the Col du Géant was 0°·457 Reaumur, or 33°·03 Fahrenheit, and of the warmest (2 P.M.) 4°·714 Reaumur, or 42°·61 Fahrenheit†. So that even upon that exposed ridge, between 2000 and 3000 feet above where the glacier can be properly said to commence, the air does not, on an average of the month of July, reach the freezing-point at any hour of the night. Consequently the *range of temperature attributed to the glacier is between limits absolutely incapable of effecting the expansion of the ice in the smallest degree.* This would of course be still more applicable if we take the mean of the temperatures at Chamouni and the Col du Géant to present the general atmospheric conditions to which the glacier is exposed.

It is in summer that the glacier moves fastest: it is with my observations of motion *in July* that Mr. Moseley compares the results of his theory: and therefore it is of no avail to say that there are periods of the year when congelation penetrates at night some inches, or even it may be some feet into the ice, and when therefore the sensible heat of the glacier may be considered to vary, though, if regard be had to its vast thickness, it must be on an average and in the most extreme circumstances to an absolutely inappreciable degree.

Lastly, Mr. Moseley, whilst condemning in the following passage the theory of glacier motion by the dilatation of water in the interstices of the ice, clearly passes sentence on his own, which could not come into action until the other had already produced its effects: "The theory of Charpentier, which attributes the descent of the glacier to the daily congelation of the water which percolates it, and the expansion of its mass consequent thereon, whilst it assigns a cause which, so far as it operates, cannot, as I have shown, but cause a glacier to descend, appears to me to assign one inadequate to the result; for the congelation of the water which percolates the

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* The observations were made every two hours day and night.

† The corresponding extremes at Chamouni are 53°·25 and 75°·96 Fahr.

glacier does not, according to the observations of Professor Forbes, take place at all in summer more than a few inches from the surface. Nevertheless it is in summer that the daily motion of the glacier is greatest." (Moseley, Proc. R.S. vol. vii. p. 341.)

"Researches on the Foraminifera.—Part I. General Introduction, and Monograph of the Genus *Orbitolites*." By William B. Carpenter, M.D., F.R.S., F.G.S. &c.

The group of *Foraminifera* being one as to the structure and physiology of which our knowledge is confessedly very imperfect, and for the natural classification of which there is consequently no safe basis, the author has undertaken a careful study of some of its chief typical forms, in order to elucidate (so far as may be possible) their history as living beings, and to determine the value of the characters which they present to the systematist. In the present memoir, he details the structure of one of the lowest of these types, *Orbitolites*, with great minuteness; his object having been, not merely to present the *results* of his investigations, but also to exhibit the *method* by which they have been attained; that method essentially consisting in the minute examination and comparison of a *large number* of specimens.

The *Orbitolite* has been chiefly known, until recently, through the abundance of its *fossil* remains in the Eocene beds of the Paris basin; but the author, having been fortunate enough to obtain an extensive series of *recent* specimens, chiefly from the coast of Australia, has applied himself rather to these as his sources of information; especially as the *animals* of some of them have been sufficiently well preserved by immersion in spirits, to permit their characters to be well made out.

As might have been anticipated from our knowledge of their congeners, these animals belong to the *Rhizopodous* type; the soft body consisting of *sarcode*, without digestive cavity or organs of any kind; and being made up of a number of segments, equal and similar to each other, which are arranged in concentric zones round a central nucleus. This body is invested by a calcareous shell, in the substance of which no minute structure can be discerned, but which has the form of a circular disk, marked on the surface by concentric zones of closed cells, and having minute pores at the margin. Starting from the central nucleus,—which consists of a pear-shaped mass of sarcode, nearly surrounded by a larger mass connected with it by a peduncle,—the development of the Orbitolite may take place either upon a *simple*, or upon a *complex* type. In the former (which is indicated by the *circular* or *oval* form of the cells which show themselves at the surfaces of the disk, and by the *singleness* of the row of marginal pores), each zone consists of but a single layer of segments, connected together by a single annular stolon of sarcode; and the nucleus is connected with the first zone, and each zone with that which surrounds it, by radiating peduncles proceeding from this annulus, which, when issuing from the peripheral zone, will pass outwards through the marginal pores, probably in the form of *pseudopodia*. In the *complex* type, on the other hand (which is indicated by the *narrow* and *straight-sided* form of the superficial cells, and by the *multiplication* of the horizontal rows of marginal pores), the segments of the concentric zones are elongated into vertical columns with imperfect constrictions at intervals; instead of a single annular stolon, there are two, one at either end of these columns, between which, moreover, there are usually other lateral communications; whilst the radiating peduncles, which connect one zone with another, are also multiplied, so as to lie in several planes. Moreover, between each annular stolon and the neighbouring surface of the disk, there is a layer of superficial segments, distinct from the vertical columns, but connected with the annular stolons; these occupy the narrow elongated cells just mentioned, which constitute two *superficial* layers in the disks of this type, between which is the *intermediate* layer occupied by the columnar segments.

These two types seem to be so completely dissimilar, that they could scarcely have been supposed to belong to the same species; but the examination of a large number of specimens shows, that although one is often developed to a considerable size upon the simple type, whilst another commences even from the centre upon the complex type, yet that many individuals which begin life, and form an indefinite number of annuli, upon the simple type, then take on the more complex mode of development.

The author then points out what may be gathered from observation and from deduction respecting the *Nutrition* and mode of *Growth* of these creatures. He shows that the former is probably accomplished, as in other Rhizopods, by the entanglement and drawing in of minute vegetable particles, through the instrumentality of the pseudopodia; and that the addition of new zones probably takes place by the extension of the sarcode through the marginal pores, so as to form a complete annulus, thickened at intervals into segments, and narrowed between these into connecting stolons, the shell being probably produced by the calcification of their outer portions. And this view he supports by the results of the examination of a number of specimens, in which *reparation of injuries* has taken place. Regarding the *Reproduction* of Orbitolites, he is only able to suggest that certain minute spherical masses of sarcode, with which some of the cells are filled, may be *gemmules*; and that other bodies, enclosed in firm envelopes, which he has more rarely met with, but which seem to break their way out of the superficial cells, may be *ova*. But on this part of the inquiry, nothing save observation of the animals in their living state can give satisfactory results.

The regular type of structure just described is subject to numerous *variations*, into a minute description of which the author next enters; the general results being, that neither the shape nor dimensions of the entire disk, the size of the nucleus or of the cells forming the concentric zones, the surface-markings indicating the shape of the superficial cells, nor the early mode of growth (which, though typically *cyclical*, sometimes approximates to a *spiral*), can serve as distinctive characters of *species*; since, whilst they are all found to present most remarkable differences, these differences, being strictly gradational, can only be considered as distinguishing *individuals.* It thus follows that a very wide *range of variation* exists in this type; so that numerous forms which would be unhesitatingly accounted specifically different, if only *the most divergent examples* were brought into comparison, are found, by the discovery of those *intermediate links* which a large collection can alone supply, to belong to one and the same specific type.

After noticing some curious *monstrosities*, resulting from an unusual outgrowth of the central nucleus, the author proceeds to inquire into the *essential character* of the Orbitolite, and its relations to other types of structure. He places it among the very lowest forms of Foraminifera; and considers that it approximates closely to sponges, some of which have skeletons not very unlike the calcareous net-work which intervenes between its fleshy segments. Of the *species* which the genus has been reputed to include, he states that a large proportion really belong to the genus *Orbitoides*, whilst others are but varieties of the ordinary type. This last is the light in which he would regard the *Orbitolites complanata* of the Paris basin; which differs from the fully-developed Orbitolite of the Australian coast in some very peculiar features (marking a less complete evolution), which are occasionally met with among recent forms, and which are sometimes distinctly transitional towards the perfect type.

The author concludes by calling attention to some general principles, which arise out of the present inquiry, but which are applicable to all departments of Natural History, regarding the *kind* and *extent* of comparison on which alone specific distinctions can be securely based.

June 21.—The Lord Wrottesley, President, in the Chair.

The following communications were read:—

"On the Magnetism of Iron Ships, and its accordance with Theory, as determined externally, in recent Experiments." By the Rev. W. Scoresby, D.D., F.R.S.

The magnetic condition of iron ships is a subject of so much importance, practically and scientifically, that I have been induced to submit to the Society a few characteristic facts (hastily indeed brought together) derived from recent experiments.

In a work in the Society's library, entitled 'Magnetical Investigations,' it was shown, by deductions from an elaborate series of experiments on plates and bars of malleable iron, that the magnetic condition of iron ships should, theoretically, be conformable to the direction of terrestrial induction whilst on the stocks; and the *retentive* quality, which is so highly developed by virtue of the hammering and other mechanical action during the building, should be so far fixed in the same direction, as to remain after the ships might be launched, until disturbed by fresh mechanical action in new positions of their head or keel. In this view, taking, for instance, the condition of the middle, or the main-breadth section of a ship on the stocks, the magnetic polar axis should assume the direction of the dipping-needle (with an equatorial plane, or plane of no-attraction at right angles to it), passing through or proximate to the centre of gravity of the iron material in such section. Thus every ship should have a characteristic magnetic distribution, primarily, dependent on her position whilst on the stocks; so that, being built with the head north or south, the *equatorial plane* should appear externally on the same horizontal level, the *polar axis* only being inclined from the vertical, in correspondence with the direction of the axis of terrestrial magnetism (Magnetical Investigations, vol. ii. pp. 331, 332).

It was also inferred, that whilst such individuality of the magnetic distribution would be rendered *retentive* on the same principles as this quality of magnetism is developed in bars or plates of iron by mechanical action, so the axial direction of the ship's magnetism would be *liable to change*, under mechanical action, in new positions of the ship's head, or under new relations of terrestrial magnetism, just as the retentive magnetism is liable to change in bars or plates of iron if hammered, vibrated, or bent whilst held in new positions.

And it was further inferred, that the analogy with plates and bars might be expected to hold, notwithstanding the numerous pieces of which the ship's hull might be composed, because, in experiments on combining short magnets into long series, or submitting piles of short bars of iron to terrestrial induction, it was found that no material difference in the effects occurred betwixt a *single* steel magnet of a given length, or a *single* bar of iron, and the same substance and dimensions combined in short or small pieces in contact. Hence it was considered that an iron ship should exhibit in its general fabric the characteristic phenomena of one undivided mass, or a unity as a magnetic body.

These anticipations, it will be seen (published betwixt three and four years ago), have, so far as we have now time to elucidate them, had verifications, in actual experiments on iron ships, as beautiful as they are conclusive.

In the case of the 'Elba,' an iron ship of 200 feet in length, built recently on the Tyne, the magnetic condition before launching, which I was invited to investigate by Mr. Robert Newall, the owner, was found satisfactorily accordant with theory. Her head pointed south a few degrees westerly, and her lines of no-deviation (indicative of the position of the magnetic equatorial plane) were at a small distance in elevation different on the two sides, that of the starboard side being the highest. Proceeding in a direction at right angles to the dip, and passing through, or near to, the ship's general centre of gravity, the lines of no-attraction (descending forward) came out near the junction of the stem with the keel. And *there*, it was remarkable, as I had suggested as probable to Mr. James Napier of Glasgow, before making any experiment, there was found a departure from the ordinary regularity of the lines of no-attraction in a considerable downward bend.

Towards the stern, the equatorial lines rose out of and came above the iron plating of the *top-sides*, about 40 feet from the tafrail; thus giving to the *after-part* of the ship a uniform northern polarity. The ship, consequently, had become a huge simple magnet—the north pole at the stern and the south at the head. The attractive power, as was expected, was highly energetic. At the distance of 50 feet, a compass on the level of the keel, at right angles to it abreast of the stern, was deviated to an extent of above 10°; at 100 feet distance the ship's magnetism caused a deflection of about half a point; and at 150 and even 200 feet there was a very sensible disturbance!

In the case of the 'Fiery Cross,' built at Glasgow and launched in January last (a case which I have elsewhere referred to), the lines of no-deviation, as taken for me by Mr. James Napier, were still more rigidly in accordance with theory,—the difference of elevation of the observed lines of no-deviation at the main-breadth section agreeing with calculation, theoretically, to within an inch or two. In the other case, that of the 'Elba,' a slight discrepancy as to the comparative level of the lines of no-attraction on the two sides, might, perhaps, be satisfactorily explained by the proximity and somewhat disturbing influence of another iron ship (advanced only to the frames or angle irons) on the port side of her.

In the case of the 'Elizabeth Harrison,' a large iron ship built at Liverpool, the first I had carefully examined, the correspondence of the magnetic polar axis and equatorial plane with those of terrestrial action was equally characteristic and conclusive.

Hence we may perceive a sufficient reason for some of the peculiar phenomena in iron ships of the compass disturbances and their changes. We may see why a ship built with her head easterly or westerly, and having the polar axis inclined over 18° or 20° to the starboard or port side, should be *particularly liable to compass changes*, if severely strained or struck by the sea with her head in an opposite direction. We see why the compasses of the 'Tayleur' should have been exposed to such a change as appears to have taken place in her lamentable case. We see in the case of the 'Ottawa*' (one I have elsewhere referred to), why a heavy blow of the sea, with the ship heeling and her head pointing eastward, would be likely to produce a change in her magnetism, when her previous magnetic distribution was solicited by terrestrial action in an angle of 30° or 40° of difference. And we see why the *deviations* of the compass in iron ships should, differently from those of wooden ships, be sometimes *westerly* and sometimes *easterly* in ships built and trading in home latitudes; for here, whilst in wooden ships, where the iron work is in detached masses, the ship can have but little, externally, of the character of a true magnet, and can possess but small comparative differences from the position of her head whilst building; in iron ships, on the converse, where the ship is rendered by percussive action a powerful and, *retentively*, true magnet, her deviating action must be expected to be different, as the polarity of the head or stern

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* Where the compass suddenly changed two points.

may differ in denomination, or as the ship's magnetic polar axis may happen to lie over to starboard or port.

As an objection might be made to deductions from experiments on simple individual bars or plates of iron being applied to the case of iron ships built up of thousands of pieces, I have repeated the experiments, substituting for an entire plate or bar of iron a plate about 18 inches long and 3 broad, made up of numerous separate plates, and combined in the manner of the plating of iron ships. The compound or combined plate of some eighteen or twenty pieces yielded, under percussion, vibration or bending, results precisely similar to those obtained by the use of single plates or bars.

XXXIX. *Intelligence and Miscellaneous Articles.*

EXAMINATION OF THE GREEN MATTER CONTAINED IN GREEN INFUSORIA. BY THE PRINCE OF SALM-HORSTMAR.

IF the green Infusoria of large ponds be collected upon a filter, then extracted with alcohol whilst fresh, and the green extract evaporated to dryness at a temperature of about 104° F., the body obtained has the following properties :—

1. When slowly heated upon platinum, it does not fuse, but soon evolves vapours (not smoke) which possess the peculiar odour of ponds, whilst the greater part of the mass is volatilized. The production of vapour then ceases, and a small brownish residue is left, which, however, is also slowly volatilized at a temperature below that at which the platinum becomes red-hot. No carbonaceous residue is left.

2. It dissolves readily both in alcohol and acetic æther.

3. It dissolves in hot water with a yellowish-olive colour; the water first becomes milky.

4. It dissolves in ammonia with a yellow colour.

5. It is soluble with a greenish-yellow colour in solution of potash, and the solution shows a black stripe in the red of the spectrum.

6. The alcoholic solution has no reaction upon litmus.

It is consequently neither a wax-like body nor chlorophyll; for chlorophyll extracted by alcohol from freshly-dried leaves of *Lolium perenne* and evaporated at 104° F. until no more fluid is present, forms a sticky mass which does not dissolve in water, ammonia, or solution of potash. Its behaviour when heated in a platinum crucible is the same as that of the green of the Infusoria.

The two alcoholic solutions are very different in colour when freshly prepared; that of chlorophyll from grass is emerald-green, that from the Infusoria olive or bottle-green.

Angström's exact optical investigations of the two substances had also presupposed their distinctness.—Poggendorff's *Annalen*, vol. xciv. p. 466.

ON THE RED DISPERSED LIGHT IN A SOLUTION OF CHLOROPHYLL. BY THE PRINCE OF SALM-HORSTMAR.

From the statements of Stokes it appears as though the red dispersion was only to be seen in directions perpendicular to the incident ray; this, however, applies only partially to the phænomenon. I have observed that the red light can be distinctly seen, even when the test-glass containing the solution of chlorophyll is held so that the flame of a lamp is placed nearly between the glass and the eye of the observer. In order to avoid mistake, I put a piece of black velvet into the fluid so as to prevent the hinder surface of the glass from giving any reflexion; but the red dispersion remained as before. The red light consequently radiates also in the opposite direction to that of incidence.

It is remarkable, however, that this fluid should also show the red light in the opposite direction; that is to say, in the direction of the incident ray, as soon as the solution is so concentrated that the directly visible light becomes more obscured.

As the so-called dispersed light promises to do much to elucidate the phænomena of light, the smallest circumstance deserves to be kept in mind.

From the well-known fact that the red dispersion is only seen with the help of a burning-glass in the centre of the solution of chlorophyll, it would seem that those invisible rays of sun-light which are rendered visible by the chlorophyll are separated in the sun-light by heterogeneous spaces, just as though they passed through a lattice, which only causes those atoms lying close to the surface to become luminous, where it is permitted by the lattice, and complete their course at that point. Thus in the parallel state the red dispersion is visible at the surface; in the converging state, also, in the centre. The question then arises, whether this might not be employed in measuring these parallel heterogeneous intervals?—Poggendorff's *Annalen*, vol. xciv. p. 467.

MOLECULAR CONSTITUTION OF CRYSTALS.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN, Dublin, August 4, 1855.

IN my paper on the Molecular Constitution of Crystals, which appeared in the Philosophical Magazine for August, I represented Sir David Brewster as having said that the cleavage of crystals arises from the unequal cohesion of the different poles of each molecule.

Quoting his words from memory, I erroneously considered them to apply to the first system of crystals of which I was then speaking;

I have fortunately since discovered my error, and hasten to make the only reparation in my power—that of acknowledging it.

I can assure Sir David Brewster that there is nothing I regret more than having made, even inadvertently, any misrepresentation, particularly of one whom I must ever look on as one of the fathers of science, and whose opinions, even though they differ from my own, I will ever regard with respect.

Your obedient Servant,
ROBERT T. FORSTER.

METEOROLOGICAL OBSERVATIONS FOR AUG. 1855.

Chiswick.—August 1. Very fine. 2. Very fine: heavy rain. 3—5. Very fine. 6. Overcast. 7. Very fine. 8. Rain: thunder: heavy rain. 9. Cloudy and fine: clear at night. 10. Foggy, with heavy dew: very fine: slight haze: very fine. 11. Slight haze: very fine. 12. Overcast: fine: clear. 13. Very fine. 14. Cloudy and fine. 15, 16. Very fine. 17. Slight fog: very fine. 18. Very fine. 19. Very fine: slight rain at night. 20. Slight rain: cloudy and boisterous. 21. Fine: clear and windy. 22. Very fine. 23. Very fine: much sheet and forked lightning at night. 24—26. Very fine. 27. Very fine: cloudy at night. 28, 29. Very fine. 30. Very fine: clear at night. 31. Very fine.

Mean temperature of the month	61°·63
Mean temperature of Aug. 1854	60 ·55
Mean temperature of Aug. for the last twenty-nine years ...	61 ·98
Average amount of rain in Aug.	2·446 inches.

Boston.—Aug. 1. Fine: rain A.M. 2—4. Fine. 5, 6. Cloudy. 7. Cloudy: rain A.M. and P.M., with thunder. 8. Cloudy: rain A.M and P.M. 9. Rain A.M. 10. Fine. 11. Cloudy. 12. Cloudy: rain A.M. 13. Fine. 14. Cloudy. 15—18. Fine. 19. Fine: rain A.M. 20. Cloudy: rain A.M. 21, 22. Cloudy. 23. Cloudy: rain A.M. and P.M. 24. Fine. 25. Fine: rain P.M. 26—28. Fine. 29. Cloudy. 30, 31. Fine.

Sandwick Manse, Orkney.—Aug. 1. Cloudy A.M.: fine, fog P.M. 2. Rain A.M.: fine, cloudy P.M. 3. Clear A.M.: fine, drops, thunder and lightning P.M. 4. Rain A.M.: drizzle, showery P.M. 5. Bright A.M.: cloudy P.M. 6. Clear A.M. and P.M. 7. Rain A.M.: showers P.M. 8. Rain A.M.: drizzle P.M. 9. Drizzle A.M.: clear, vapour P.M. 10. Hazy A.M.: damp P.M. 11. Cloudy A.M.: showers P.M. 12. Cloudy A.M.: damp P.M. 13. Showers A.M.: cloudy P.M. 14. Bright A.M.: cloudy P.M. 15. Drizzle A.M.: cloudy P.M. 16. Cloudy A.M.: rain P.M. 17. Cloudy A.M. and P.M. 18, 19. Cloudy A.M.: cloudy, thunder and lightning P.M. 20. Showers A.M.: rain P.M. 21. Bright A.M.: cloudy P.M. 22. Showers A.M. and P.M. 23. Showers, bright A.M.: clear P.M. 24. Cloudy A.M.: rain P.M. 25. Clear A.M. and P.M. 26, 27. Rain A.M.: cloudy P.M. 28. Clear A.M.: showers P.M. 29. Bright A.M.: clear P.M. 30. Drops A.M.: clear, drops P.M. 31. Cloudy A.M. and P.M.

Mean temperature of Aug. for twenty-eight previous years ...	54°·99
Mean temperature of this month	56 ·10
Mean temperature of Aug. 1854	55 ·06
Average quantity of rain in Aug. for fifteen previous years ...	2·95 inches.

Meteorological Observations made by Mr. Thompson *at the Garden of the Horticultural Society at* CHISWICK, *near* **London**; *by* Mr. Veall, *at* BOSTON; *and by the* Rev. C. Clouston, *at Sandwick Manse*, ORKNEY.

Days of Month.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
1855. Aug.	Max.	Min.		9¼ a.m.	8½ p.m.	Max.	Min.		9¼ a.m.	8½ p.m.						
1.	29·853	29·773	29·32	29·65	29·58	74	54	61·5	61	56	sw.	sw.	e.	·02	·04	....
2.	29·824	29·728	29·27	29·52	29·49	76	50	63	59	58	sw.	sw.	e.	·17	·02	·11
3.	29·780	29·639	29·23	29·43	29·44	74	52	63	65	57	sw.	sw.	ne.	....	....	·29
☾ 4.	29·713	29·664	29·08	29·48	29·69	72	53	64·5	55	50½	sw.	sw.	n.	·01	....	·64
5.	30·022	29·929	29·43	29·90	30·05	73	41	61	53½	50½	sw.	nw.	n.	....	....	·05
6.	30·062	29·866	29·58	30·06	30·01	73	55	65	54½	50	s.	nw.	e.	·11	....	....
7.	29·755	29·664	29·28	29·82	29·74	75	56	66	52½	52½	sw.	s.	ne.	·44	·04	·10
8.	29·650	29·626	29·12	29·63	29·65	72	54	63	58	57	sw.	nw.	ese.	·50	·37	·20
9.	29·918	29·828	29·30	29·71	29·85	71	38	59	56½	54	w.	wnw.	calm	....	·35	·23
10.	30·151	30·043	29·57	29·89	29·99	76	49	57	61	58½	w.	wnw.	s.	....	....	....
11.	30·201	30·145	29·68	30·02	30·07	80	56	66	65	54½	w.	wnw.	w.	....	....	·05
● 12.	30·205	30·181	29·65	30·07	30·03	70	42	65	59	55	w.	n.	sw.	·01	·20	·44
13.	30·223	30·213	29·73	29·96	29·99	72	41	60	57	55	n.	wnw.	w.	....	....	·04
14.	30·189	30·174	29·66	30·06	30·12	73	57	63	57	54	nw.	w.	nw.	....	....	·01
15.	30·231	30·224	29·70	30·01	30·10	76	47	65	58	56	n.	nw.	w.	....	....	....
16.	30·297	30·274	29·75	30·14	30·18	79	45	63	58	56	nw.	nw.	sw.	....	....	·08
17.	30·282	30·244	29·78	30·14	30·09	83	45	61	59	58	n.	wnw.	wsw.	....	....	·06
18.	30·232	29·760	29·60	29·94	29·74	79	53	65	68	60	se.	s.	sse.	....	....	....
19.	29·791	29·689	29·10	29·40	29·35	76	56	70	61½	54	w.	ssw.	wsw.	·02	·02	....
☽ 20.	29·929	29·809	29·34	29·42	29·38	66	58	65	54½	52	sw.	w.	ese.	·05	·08	·07
21.	29·878	29·817	29·23	29·38	29·54	74	42	64	54	52	w.	sw.	w.	....	....	·14
22.	29·994	29·961	29·46	29·55	29·62	76	52	63	54½	51½	w.	w.	w.	....	....	·15
23.	29·898	29·691	29·44	29·74	29·83	81	56	63	55	48	se.	s.	calm	·12	....	·11
24.	29·876	29·725	29·20	29·76	29·47	74	40	68	53	56	sw.	ssw.	se.	....	·12	....
25.	30·006	29·969	29·47	29·53	29·50	73	49	59	57	55½	sw.	ssw.	s.	....	....	·61
26.	30·047	29·775	29·52	29·50	29·68	74	39	58·5	55½	53	n.	sw.	w.	....	·06	·30
○ 27.	29·983	29·878	29·43	29·44	29·46	77	49	63	55	56½	sw.	s.	wsw.	....	....	·06
28.	29·858	29·777	29·24	29·48	29·53	81	44	64	61	55	sw.	ssw.	calm	....	....	·08
29.	30·025	29·991	29·46	29·78	29·97	75	42	63	56	52½	w.	sw.	wsw.	....	....	·13
30.	30·257	30·250	29·58	30·13	30·08	72	40	60	56	55	ne.	s.	sse.	....	....	·03
31.	30·210	30·181	29·75	29·95	30·18	78	41	59	62	53½	e.	ssw.	sw.	....	....	....
Mean.	30·011	29·919	29·45	29·757	29·787	75·00	48·26	63·0	57·80	54·41				1·45	1·30	3·98

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XL. *On the Passage of Electric Currents through Rarefied Air.*

By P. Riess[*].

AN interesting observation has lately been made by M. Gaugain in Paris. He allowed a magneto-induction current to pass between metallic knobs enclosed in a glass receiver containing rarefied air, one of which knobs, with the exception of a very small portion, was coated with an insulating substance[†]. Besides the glass receiver, or electric egg, a galvanometer was introduced into the circuit, the needle of which was strongly deflected when the naked knob was negative, *i. e.* when the current passed from the covered to the naked knob, but suffered no deflection when the current was reversed. Thus to the interruption current[‡] of the induction apparatus was ascribed the property of passing through the electric egg in one direction only, that is, from the coated to the naked knob, and on this account the egg was called an *electric valve.* The following investigation will show that this conclusion is unfounded, and that the name, at least in the above sense, is inappropriate.

In order to repeat Gaugain's experiment, I provided myself with a short glass cylinder closed at each end with metallic plates; the lower plate, as well as its attached rod and knob, with the exception of a small portion of the latter about the size of a pin's head, were coated with melted sealing-wax: about three-quarters of an inch above the coated knob was a similar

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[*] From the Report of the Academy of Science at Berlin, June 18, 1855.

[†] *Comptes Rendus,* vol. xl. p. 640. Phil. Mag. vol. x. p. 207.

[‡] The induced currents caused by interrupting and closing the circuit of the primary or inducing current, are for the sake of brevity called interruption current and contact current, respectively.

one, naked, at the end of a rod attached to the upper plate. To generate the magneto-induction current, I used a very efficient apparatus constructed by Siemens and Halske, and formerly described by me*. The induction coil of this apparatus was $2\frac{1}{2}$ inches long and $2\frac{1}{4}$ inches broad; the voltaic current in the primary coil, in which lay a bundle of loose iron wires, was closed and broken by means of the tongue invented by Wagner, and a long time ago introduced by Neef. The circuit of the induction coil was closed after introducing into it a galvanometer with an astatic system of needles, as well as the before-described glass cylinder, in which the air was rarefied to one line of mercury pressure. By means of a commutator, the coated knob in the cylinder could be made positive or negative by connecting it immediately with the one or the other pole of the induction coil. That pole of the induction coil will be called *positive* which developes iodine when both poles are connected by a strip of paper moistened with a solution of iodide of potassium, and when the primary current is broken. This pole can be easily detected by first fixing two pointed elastic strips of metal with their points opposite one another, so that the distance between the points can be changed, and then fastening a strip of thin writing-paper between the points. When the points are connected with the poles of the induction coil, and made to approach one another until a continuous series of sparks ensues between the two, the sparks will be seen to pass on one side of the paper only, provided the latter is not perceptibly displaced. It follows from a known property of positive electricity, that the point in contact with this side of the paper is connected with the positive pole of the induction coil. To set the induction apparatus in action, I used one of Grove's elements; but this current being too strong, I found it necessary to pull the iron wire half-way out of the primary coil. When the coated knob in the cylinder was connected with the positive pole of the induction coil, the needle of the galvanometer was instantly deflected towards a certain side, and remained oscillating about 30°. On the other hand, when the naked knob was positive, the needle remained for some moments motionless, and then moved to a few degrees on one or on the other side of the zero of the scale. Hence it really appeared as if the interruption current passed easily and completely from the covered to the naked knob; but not at all, or only partially and with difficulty, from the naked to the covered knob. This explanation of the experiment, however, was contradicted by the light in the cylinder, which could be seen in both cases, and, in full day with shaded cylinder, appeared to have the same form; further, in the case where the needle was deflected, the light

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* Poggendorff's *Annalen*, vol. xci. p. 290.

appeared more quiet and uniform than in the other case. Afterwards I examined the luminous phænomena more minutely. Before having some experience with respect to the above phænomena in the case of the simplest electric current, such as that of a Leyden battery, it appeared to me useless to experiment further upon that most complicated of all electric currents which the induction apparatus furnishes.

Experiments on the Leyden Battery.

I had the following simple and convenient apparatus prepared. A cylinder of very thick glass, $3\frac{3}{4}$ inches high and 1·46 inch in interior diameter, was ground perfectly plane at one end, and closed at the other by a brass mounting, which carried a cock and a screw for the purpose of being screwed on to the air-pump. Projecting into the glass vessel and attached to this mounting is a slight brass cylinder, in which a brass rod, carrying a knob at one end, can move up and down. The cylinder can be closed and made air-tight by placing on the top a glass plate smeared with lard. The glass plate has a small hole in its centre, and upon it an ivory rod an inch long is cemented, which carries a clamp for fixing the conducting wire. A piece of platinum wire half a millim. thick descends from the clamp through the ivory and is cemented into the glass plate, the latter and the end of the wire being ground into one plane. In the plane of the lower surface of the glass, therefore, there is a disc of platinum half a millim. in diameter, between which and the brass knob beneath it electricity can pass. After placing the knob ($\frac{3}{8}$ths of an inch in diameter) at a distance of $\frac{5}{6}$ths of an inch from the platinum surface, the air in the cylinder was rarefied to about one line of mercury pressure, and the apparatus, together with a sensitive electric thermometer, brought into the circuit of an electric battery consisting of three jars. The apparatus was so connected with the discharging wire, that during the discharge the knob in the cylinder could be made either the positive or the negative electrode. In order that the manner of charging the battery may be seen, the part of the cylinder which was in connexion with the interior of the battery is given in the following table. The words "surface +" show, therefore, that the battery was charged with positive electricity; "knob −" that it was charged with negative; with respect to the direction of the current, it will be seen that it was the same in both these cases.

The following single observations show the degree to which the thermometer was heated with different charges of the battery; the knobs of the instrument for measuring the quantity of electricity in the battery were half a line distant from one another.

Heat in the discharging circuit when interrupted by air of 1 line of mercury pressure.

Quantity of electricity.	Surface +	Knob +	Surface +	Knob +	Knob −	Surface −
4	·6	10·5	5·3	10·7	6	10·2
5	8·5	16	8·4	15	8	14·3
6	13·5	21·5	14	21	13·6	21·3
7	18·7	29·3	19·7	30	17·5	28·8
Unit of charge ...	1·10	1·87	1·11	1·85	1·07	1·78

The following is the result of all the above observations, showing the relation between the heat developed and the direction of the discharge current:—

Current from the surface to the knob: heat 100.
Current from the knob to the surface: heat 108.

In another series of experiments, the knob in the glass cylinder was 2·7 inches distant from the platinum surface in the glass plate.

Quantity of electricity.	Surface −	Knob −	Surface −	Knob −
5	9·5	6·7	9·2	7
6	15	10·3	15	10
7	17·7	13·7	17	13
8	26·3	18·5	27	18·2
Unit of charge ...	1·18	0·84	1·16	0·83

Here is the mean result:—

Current from the surface to the knob: heat 100.
Current from the knob to the surface: heat 140.

In order to employ stronger charges of the battery (up to a quantity of electricity equal to 16), I replaced the thin platinum wire in the thermometer by a thicker, and found the following to be the mean result of four observations:—

Current from the surface to the knob: heat 100.
Current from the knob to the surface: heat 130.

The knob in the glass cylinder was replaced by a horizontal brass disc, $\frac{1}{12}$ths of an inch in diameter, and 1 line distant from the platinum surface in the glass plate. The results were:—

Current from the surface to the disc: heat 100.
Current from the disc to the surface: heat 122.

I removed the disc so that the brass rod alone remained in the glass cylinder; it was $2\frac{1}{12}$ inches long, and its point was $\frac{5}{6}$ths of an inch distant from the platinum surface. The following were the results:—

Current from the surface to the rod: heat 100.
Current from the rod to the surface: heat 122.

Lastly, the discharge circuit was elongated by a platinum wire, 16 feet long and 0·057 line thick; the knob was again placed in the cylinder at a distance of $\frac{5}{6}$ths of an inch from the platinum surface, and the charge of the battery was increased to a quantity of electricity equal to 16. The mean results were:—

Current from the surface to the knob: heat 100.
Current from the knob to the surface: heat 105.

In all the numerous experiments which were instituted, and in which the discharge current of the battery passed through air rarefied to 1 or 2 lines of pressure between a small and a comparatively great, but arbitrarily-shaped metallic surface, a difference was observed in the heating effect upon the discharging wire according to the direction of the current; this difference, however, although sometimes greater and sometimes less, was always in favour of one and the same direction. From the above experiments, it will be seen that the greatest heat was produced when the direction of the current was the same as that which, in the first-mentioned experiments with a magneto-induction current, produced a very small, or no deflection of the magnetic needle. *When the discharge current of a Leyden battery passes through very rarefied air between a very small and a comparatively large metallic surface, the heat produced in the remaining part of the discharge circuit is greatest when the current passes from the large to the small surface.* The meaning of a very small surface will be hereafter explained.

This remarkable and hitherto unknown change in the action of an electric discharge is easily explained by known facts. I satisfied myself that the battery was equally well discharged whatever might have been the direction of the current. As the discharge circuit had the same constitution in both cases, the difference in the heating power of the current could only be occasioned by the method of discharge. It is well known that a change in the heating effect, occasioned by a change in the method of discharge, can be shown in the clearest possible manner. When the discharge circuit is interrupted by a thin stratum of distilled water placed between metallic plates, and when the strongest charge of the battery which will pass noiselessly through the water is employed, a delicate electric thermometer is necessary in order to detect the small amount of heat which is thereby produced in the discharge wire. If the plates immersed in water, however, are brought inappreciably nearer to one another, the discharge passes through with a spark, and at the same time the heating effect in the discharge wire is so great that the thermometer no longer suffices for its measurement. Again, when the discharge takes place in free air between a knob and a very fine point, whose distance asunder is so regulated that no spark ensues, the heating effect in the wire is very small; whereas it is very great if the fine point is replaced by a blunt one, so as to cause a spark between the two electrodes. In both cases where the spark appeared, the time during which the same quantity of electricity was discharged was greatly diminished; hence, according to a well-known law, the heating effect was greatly increased. Exactly in the same manner as we are able to change the mode of discharging a battery through water and through free air by bringing the electrodes nearer to one another, and by changing the form of one of them, we can, as the new experiments show, attain the same end in very rare air by diminishing the negative electrode. The battery charge can pass through rarefied air, between arbitrarily-shaped electrodes, in two essentially different ways. In the first, which might be called the glimmering manner, a reddish luminous cone of air takes part in the discharge, its summit touches the positive electrode, and its base lies always at some distance from the negative electrode. The stratum of air in contact with a great part of the negative electrode takes part in the discharge in a less degree, and glows with a peculiar blue light. The second manner of discharge is that which I have called discontinuous. A narrow cylinder of air touching both electrodes takes part in the discharge, is rendered white hot, and burst asunder in the same manner as a metallic wire is which connects both electrodes, and whose diameter does not exceed a certain limit dependent upon the charge of the battery. The glimmering discharge takes place so slowly, that the surfaces of both electrodes remain uninjured, and only a small amount of heat is generated in the whole discharge circuit, whereas the discontinuous discharge tears away parts of the electrodes, scatters them red-hot on all sides, and heats the discharge circuit. Whatever may be the distance between the electrodes, the glimmering can be converted into the discontinuous discharge by increasing the density of the discharged quantity of electricity, by the admission of air, or, as our late experiments justify us in concluding, by diminishing the magnitude of the negative electrode. That is to say, if the surface of the negative electrode be very small in comparison to the surface which, with the employed density of the discharged quantity of electricity, would have been covered with a glimmering light, the discharge instead of being glimmering will be discontinuous. The recorded experiments are in complete harmony with this remarkable conclusion. When the direction of the discharge current was from the small platinum surface to the naked knob, the brass rod, and the inferior plate, that is to say, when the negative electrode had a sufficiently large superficial area, of the many partial discharges, which, with decreasing density, constitute the discharge of the battery, a part of the last, owing to their small tension, must have passed as glimmering discharges, and thus have exercised no heating effect. When, on the contrary, the direction of the current was from the knob to the small platinum surface, the negative electrode was limited in magnitude, and a part of the glimmering must have been converted into discontinuous discharges, and thus become effective in heating the circuit. Hence, in accordance with experiment, when the discharge passes from the surface to the knob, the heat in the remaining part of the discharge circuit must be smaller than when the discharge has an opposite direction. It is evident that the number of the glimmering discharges which become effective by reversing the current differs according to the form and distance of the electrodes. Cases may present themselves where the difference in the heating effect is inappreciable; but wherever it does occur, it must be in favour of the stated direction; and this, without exception, I have found experiment to corroborate. The explanation above given is also supported by ocular evidence. If the cylinder is placed in the shade, and observed during the time that the discharge passes from the knob to the surface, a brilliant light is noticed in the space between the electrodes, whilst the knob and rod are but feebly luminous. It may here be remarked, that this brilliant light between the electrodes consists of two dazzling colours; at the negative electrode it is reddish-violet, at the positive greenish-blue. When, on the contrary, the discharge passes from the surface to the knob, a broad reddish light is observable, not only between the electrodes, but throughout the length of the glass cylinder, and stretching over the knob and rod. In the last case, the explanation supposes that a great part of the discharges are glimmering.

In free air, when the discharge spark passes between a very small surface and a knob, no difference between the heating effects in the discharge circuit is observable when the direction of the current is changed. We have yet to examine, therefore, to what degree the rarefaction of the air must be carried in order that this difference may be observable. The glass cylinder was screwed on to the air-pump and kept in communication with the barometer test, the foot and clamp of the cylinder were connected with the discharge circuit of the battery, by means of long copper wires, after the thermometer had also been introduced into it. The knob in the cylinder stood at a distance of $\frac{5}{6}$ths of an inch from the small surface. In the following table the heating effects corresponding to the unit of charge are given, each of which was calculated from four thermometric observations; in the fourth line the heating effect with a positive knob is given, that with a positive surface and equal pressure of air being supposed equal to 100.

Heating effects in the discharge circuit when the latter is interrupted by rarefied air.

Pressure of air in inches.	$\frac{1}{12}$.	$\frac{5}{12}$.	$\frac{5}{6}$.	$1\frac{2}{3}$.	$2\frac{1}{2}$.	$3\frac{1}{3}$.
A. Surface positive.	0·85	1·02	0·93	0·94	1·09	1·18
B. Knob positive...	1·28	1·13	0·96	0·96	1·08	1·19
Ratio of A to B ...	150	111	103	102	100	100

The increase of heat by reversing the discharge in the glass cylinder, and thus making the knob the positive instead of the negative electrode, decreases quickly with increasing pressure of air, and is no longer perceptible at a pressure of $2\frac{1}{2}$ inches. An interesting corroboration of the theorem, that in these experiments two essentially different kinds of electric discharge are involved, is presented by the circumstance, that, in the line A, the greatest heat occurs when the air is most dense ($3\frac{1}{3}$ inches pressure), and, in the line B, when the air is least dense ($1\frac{1}{12}$th of an inch pressure).

In fact, when the greater part of the partial discharges were glimmering, the admission of air must have prevented the glimmering, and thereby rendered these discharges effective in the generation of heat. When, on the contrary, the majority of the partial discharges were discontinuous, the increased quantity of air had the effect of increasing the interval between the several partial discharges, and, consequently, of diminishing the heating effect. Insomuch as, in reference to this heating effect, these opposite actions simultaneously took place in every experiment, it cannot appear surprising that sometimes the one, sometimes the other action was in excess. Thus, in order to explain the observation in the line A with $\frac{5}{12}$ths of an inch pressure, we must assume that by changing the pressure of air from $\frac{1}{12}$ to $\frac{5}{12}$, the effect due to the prevention of the glimmering is in excess, whilst at $\frac{5}{6}$ths of an inch pressure the retardation of the discontinuous discharges is perceptible in the observation. From $3\frac{1}{3}$ inches pressure upwards only one kind of discharge, the discontinuous, appears to exist, whatever may be the condition of the electrodes; and, in fact, at $2\frac{1}{2}$ inches pressure it was no longer possible to detect any difference in the luminous phænomena in the cylinder when the knob was positive and when it was negative. In both cases a bright, broad expanse of light was alone visible in the space between the electrodes.

As might have been predicted, the discharge of the battery, after its passage through air rarefied to $\frac{1}{12}$th of an inch pressure, produced no deflection of the galvanometer needle. When one end of the galvanometer coil was connected with the conductor of an electric machine by means of a wire and the cylinder of rarefied air, the other end being in connexion with the earth, a considerable deflection of the needle was caused by turning the machine; no constant change in this deflection, however, was observable when the position of the cylinder in the circuit was changed, a result which will not surprise when the uncertainty of the experiment is considered.

Experiments on the Induction Apparatus.

The small induction apparatus, described at the commencement, was excited by means of one of Daniell's elements, and the bundle of iron wires half withdrawn from the primary coil. The coil of a delicate galvanometer was introduced into the circuit of the induction current as well as the glass cylinder used with the Leyden battery, containing air rarefied to $\frac{1}{12}$th or $\frac{1}{8}$th of an inch of pressure; lastly, a commutator was introduced so as to change the direction of the current in the glass cylinder without altering anything in the remaining part of the circuit. This position of the commutator was chosen in order to simplify the experiment itself, as well as its description; if the commutator is applied so as to reverse the direction of the induction current throughout the whole of its circuit, or, indeed, that of the voltaic current in the primary coil, an audible change in the play of the tongue connected with the induction apparatus ensues, which has the effect of still further complicating the experiment. Within the cylinder was a brass knob $\frac{3}{8}$ths of an inch thick, which was at first placed at a distance of $\frac{5}{6}$ths of an inch from the platinum surface. When the small surface was the positive electrode, that is to say, when it was connected with what was at first defined to be the positive pole of the induction apparatus, the needle of the galvanometer suffered a *normal* and maximum deflection of 70°; when the knob was the positive electrode, an *abnormal* deflection of 10°. By a *normal* or an *abnormal* deflection is to be understood one which does, or does not, correspond with the direction of the current. On repeating the experiment, a normal deflection of 10° was obtained when the knob was positive. The knob was withdrawn to a distance of $1\frac{5}{12}$ inch from the platinum surface. The knob being negative, I obtained a normal deflection of 40° or 50°; but when the knob was positive, a small abnormal deflection, which afterwards changed to a small normal one. When the knob and surface were $2\frac{2}{3}$ inches asunder, it was necessary to increase the intensity of the induction current, by pushing the bundle of iron wires further into the primary coil, in order that the current might traverse the space between the electrodes. In both positions of the electrodes the deflection was normal, but it was greater with the knob negative than positive. A smaller knob, $\frac{1}{24}$th of an inch in diameter, was placed at a distance of $\frac{17}{18}$ths of an inch from the small surface in the cover. When the knob was negative, the deflection amounted to 60° and was normal; when positive, the deflection was zero at the moment of closing the circuit, but passed into a normal or an abnormal one of 10°. The knob was replaced by a horizontal brass disc, $\frac{11}{12}$ths of an inch in diameter and $1\frac{1}{6}$ inch distant from the platinum surface. The disc being negative, the deflection was normal, and attained the maximum; when positive, it was abnormal, and amounted to 20°. I varied these experiments in numerous ways, but the common result of all was, that when the small platinum surface was positive, the deflection of the needle was always normal, and always occurred immediately after closing the induction circuit; when, on the contrary, that surface was negative, the deflection was sluggish and retrograde. That in the last case the deflection is sometimes abnormal, has been observed also by M. Gaugain, who, had he taken the observation sufficiently into consideration, would not have been led to the erroneous conclusion which he has drawn from his experiments. For there are undoubtedly two oppositely directed induction currents acting in these experiments: the interruption current, in reference to which the poles of the induction coil have been defined; and the contact current, in reference to which the names of the poles of the induction coil must be changed. The action of the two opposite currents can be demonstrated by means of a decomposing apparatus still more successfully than by the galvanometer. Besides the galvanometer and the cylinder of rarefied air, in which a knob was placed $\frac{5}{6}$ths of an inch from the small surface, two platinum points were introduced into the induction circuit; they were about one inch asunder, and stood on a piece of paper moistened with iodide of potassium. As before, the commutator changed the direction of the induction current in the glass cylinder alone. When the small surface in the cylinder was connected with the positive pole of the induction coil, the stain of iodine in seven successive experiments appeared only under that platinum point which was positive in reference to the interruption current; when, on the contrary, the surface was in communication with the negative pole, the stain of iodine in thirteen experiments appeared eight times under the other platinum point alone, and five times under both points. The knob in the cylinder was next placed $1\frac{5}{12}$ inch distant from the small surface: when the latter was positive, the iodine stain appeared five times under the above-defined positive platinum point; when the surface was negative, four times under the other point. *When the whole current of a magneto-induction apparatus is transmitted through very rarefied air, between a very small and a comparatively great surface, the interruption current alone traverses the space between the surfaces when the small one, in reference to that current, is positive; when, on the contrary, the small surface is negative with respect to the interruption current, the contact current is also transmitted.*

It has long been known, that, on account of the small tension possessed by the contact current, the interruption current is the only one which can traverse a space of air. According to Poggendorff*, this small tension of the contact current results from the production of a closed conducting circuit in the neighbourhood of the induction coil when the primary circuit is closed. Thus, although M. Gaugain concluded from his experiments, that, with a certain form of the electrodes, the interruption current did *not* pass through very rarefied air, it follows from the present investigation, that in reality both the contact current and the interruption current are transmitted in this case. Consequently, if the term valve is still applied to Gaugain's apparatus, it must be considered as a valve for the contact current, and not for the interruption current. A glance at the luminous phænomenon furnishes a corroboration of the above theorem, if, indeed, any were required. In order that the light might be more sharply defined, I had the brass knob in the cylinder, as well as the disc, stained black so as not essentially to injure the conduction at their surfaces. When the small surface was positive, the luminous phænomenon was the same as that which takes place between two equal knobs, and which has often been described. A small, well-defined cone of reddish light extended from the small surface to the negative electrode, and ended at some distance therefrom, so that between the base of the cone and the negative electrode a broad dark space remained. The whole surface of the negative electrode, whether its form was that of a knob or a disc, was surrounded by a beautiful blue light which stretched also over the rod. The light was throughout equally bright and quiescent. When, on the contrary, the small surface was negative, the light began immediately to flicker and change its intensity. When the current had been previously regulated so as to produce a very small deflection of the magnetic needle, or none at all, the form of the light was much the same as before. The reddish light of the positive electrode only was less distinctly defined: it extended itself until it reached the negative electrode without leaving a free dark space. The negative electrode itself

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* Phil. Mag. vol. x. p. 124.

was only partially surrounded by a blue light, that is to say, it extended only over the anterior hemisphere of the knob or the anterior surface of the disc. When the current was increased, the blue light was confined to a still smaller space, and was momentarily hidden by the broad reddish sheaf of light which extended between the electrodes. When these luminous phænomena have been once observed in the dark, it is easy to recognize them in daylight by merely shading the cylinder, whatever connexion the electrodes may have with the induction coil.

The experiments with the Leyden battery serve to explain the phænomena of the induction apparatus. It has been shown that the electricity in the battery generates a less amount of heat in the discharge circuit when it is transmitted through rarefied air from the small to the large surface. The same takes place with the interruption current of the induction apparatus. A brass disc was placed in a glass cylinder at a distance of 1 line from the small surface in the cover, and the cylinder, together with the delicate electric thermometer, introduced into the circuit of the induction coil. The heat produced was in general small, but considerably smaller when the direction of the interruption current was from the small to the large surface. When the large disc was negative, the liquid in the thermometer sunk from 1 to 3 lines; when that disc was positive, from 5 to 6 lines. I obtained a constant result by allowing the current passing from the great to the small surface to continue for some time, until the liquid in the thermometer attained its lowest position; the sudden reversion of the current immediately caused the liquid to rise. By simultaneously introducing a galvanometer into the circuit, it was found that the greatest heat was produced when the deflection of the needle was smallest, and the smallest heat when the deflection was greatest. With the Leyden battery no doubt can be entertained that the change in the heating effect is occasioned by a change in the method of discharge, that an increased heating effect is produced by converting the slow glimmering discharge into the much more quickly completed discontinuous discharge. If a similar change in the discharge of the interruption current of the induction apparatus be assumed, the cause of the changed action will be the same, and that of the passage of the contact current will be found. From experiments with the Leyden battery, it is known that the conversion of the glimmering into the discontinuous discharge occasions a less deflection in a galvanometer introduced into the circuit, as well as a diminution in the quantity of any substance decomposed in the circuit. Whilst the glimmering discharge does not injure the electrodes, the discontinuous discharge tears parts of them away, and scatters the incandescent particles in the space between the electrodes. This space will consequently conduct electricity better, and the contact current can pass as a glimmering discharge. That for the transmission of this current a perfect conductor is not necessary, is proved by the fact that the same can be transmitted through a long strip of tolerably moistened paper. These premises being granted, the observed actions of the induction current can be deduced without difficulty. When the whole current of the induction apparatus is passed through well-rarefied air, between a small and a large surface, and the direction of the interruption current is from the small to the large surface, this current alone is transmitted, and that as a glimmering discharge: in the discharge circuit, therefore, only a small amount of heat will be generated, a galvanometer will be strongly and constantly deflected in one direction, and an electrolyzable substance will be decomposed in the ordinary manner, so that a certain constituent of the same will be separated at a certain electrode. If the direction of the current be reversed, so that the interruption current pass through the rarefied air from the great to the small surface, this current will suffer partial discontinuous discharge; hence a greater heat will be produced in the discharge circuit, the deflection in the galvanometer will be less, and a smaller quantity of the electrolyte will be decomposed. These actions will be modified by the glimmering discharge of the contact current, which can now pass in consequence of the greater conductibility of the space, its heating action will be weak, the deflection and decomposition which it causes will both be strong, and in direction opposed to the corresponding actions of the interruption current. The deflection of the needle, therefore, can be still further diminished, or destroyed, or even reversed; the separation of a particular constituent at the former electrode may be prevented, or it may even be disengaged at the opposite electrode; or, lastly, the effects of both currents may remain perceptible: the constituent in question may be separated at both electrodes, and the needle may be deflected sometimes on one side, sometimes on the other. In fact, this changeability in the result takes place, not only in different experiments, but frequently in the same experiment; nor can it surprise one who reflects that it is produced by the changeable successive action of two opposite currents. On the contrary, with the former direction of the induction current, the result, being influenced by one current only, is always essentially the same.

Though not immediately connected with the subject in hand, I may here remind the reader of a paradoxical fact upon which some light may, perhaps, be thrown by the above-proved influence of the interruption current upon the passage of the contact current. When the current of a powerful voltaic battery is allowed to pass in a luminous manner between two wires in free air, the wire which forms the *positive* electrode is more heated than the other, and is the first to become incandescent and to fuse. When, on the contrary, the current of an induction apparatus is allowed to pass with sparks between two equal wire points in air, that wire only becomes incandescent and fuses which forms the negative electrode of the interruption current, and which, as I have long ago shown, can be easily recognized by the covering of blue glimmering light upon it. Hitherto this paradox has not been solved. Is it not possible that the interruption current, part of which apparently passes here as a glimmering discharge, has but little heating effect, but on the other hand increases the conductibility of the space, and facilitates the passage of the contact current, which manifests itself principally by the incandescence of the wire end? It would then be the *positive* electrode of the latter current which produced the greatest heat, so that between the action of a voltaic and that of an induction current no difference would exist. This would be a very simple and acceptable solution of the paradox.

The above research by Prof. Riess was communicated to the Academy of Berlin on the 18th of June, and shortly afterwards, on the 30th of July, M. Gaugain made another communication to the Academy of Paris "On the Electric Conductibility of Air[*]." Having shown in a preceding communication[†], that, within certain limits, the conductibility of air appeared to decrease with its density, he deemed the fact of sufficient theoretical importance to demand re-examination, with a view of more completely substantiating the fact. In his last research, the apparatus described under the name of an electric valve was sometimes employed, and sometimes simple tubes with metallic wires for electrodes. The general result obtained with all the apparatus was the same. When the induced currents of Ruhmkorff's apparatus were passed through air whose density was gradually diminished, the intensity of the electricity, as shown by a galvanometer, began at first to increase (departing from the atmospheric pressure), attained a maximum, afterwards began to decrease, and finally attained a minimum value when the vacuum was as perfect as possible. The density of air corresponding to the maximum intensity of current varied with the magnitude and disposition of the apparatus, with the distance between the electrodes, and with the superficial magnitude of the latter. At one

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[*] *Comptes Rendus,* vol. xli. p. 152.

[†] Ibid. vol. xl. p. 640, and Phil. Mag. vol. x. p. 207.

time this density was as low as 2 millims., at another as high as 50 millims. In general, when the negative electrode presented a large surface, and the thickness of the air traversed by the current was considerable, the period during which the intensities increased was of great extent, and that during which they decreased scarcely appreciable; on the other hand, when the surface of the negative electrode was very small, the period during which the intensities increased embraced only a small number of degrees, that during which they decreased being greatly extended. When the electric valve was employed, the intensity of current, corresponding to the most perfect vacuum an ordinary machine is capable of producing, was sensibly equal to zero; with tubes containing electrodes of metallic wire, the intensity of current corresponding to the most perfect vacuum was always a maximum, but never zero, and in these cases the period during which the intensities diminished was not well defined so long as the distance between the electrodes was small.

The manner in which Prof. Riess would view these results is already known; the following explanation is proposed by M. Gaugain:—"When an electric current traverses a space containing air, the resistance with which it meets ought to vary, as in the case of liquids, with the mean section of the gaseous space which serves to conduct the same. The dimensions of this space cannot, it is true, be immediately ascertained, though it is natural to suppose that they are closely related to those of the sheaf of light which is observable. This hypothesis being admitted, we must conclude that when the negative electrode presents a large surface, the mean section of the current continually increases with the increased rarefaction of the air, for experiment proves that in this case the sheaf of light continually expands. The result is, that the increased section compensates, in a certain measure, for the diminution of conductibility, which is a consequence of the diminution of the elastic force of the air. When, on the contrary, the surface of the negative electrode is very limited, the conducting space formed by the reunion of the elementary currents cannot dilate, nothing compensates for the diminished conductibility, and consequently this diminution manifests itself."

After observing that in the above explanation it is assumed that the same laws which govern the transmission of currents through liquids also apply to gases, M. Gaugain cites an experiment as an additional justification of the assumption. An air-pump receiver was procured, in the upper part of which was a copper rod sliding, air-tight, in a leather box; after exhausting the air under the receiver, the plate of the air-pump was connected with one pole of Ruhmkorff's apparatus, and the rod with the other; the usual sheaf of light was seen between the copper rod and the plate of the air-pump. An insulated thin disc of tin was now placed between the electrodes, at a distance of 5 or 6 centimetres from each of them; the luminous appearance within the receiver was completely changed. When the current traversed the cylinder from top to bottom, a cone of red light descended from the copper rod, and the upper surface of the disc was covered with a blue light; a second cone of red light descended from the lower surface of the disc, and a second covering of blue light surrounded the exhausting tube of the air-pump. Thus the tin disc constituted a double pole, in exactly the same manner as when a metallic disc is placed between the electrodes in a liquid electrolyte, its surfaces become contrary poles, and the resistance in the circuit, instead of being diminished, is increased. This fact being established, the copper rod was next depressed so that its lower extremity approached the tin disc; the latter was then pierced as a sheet of paper would have been, and the current passed unbroken through the tube which was formed; only one cone of red light, only one dark space, and only one blue covering was then seen. M. Gaugain considers this a decisive proof that electricity suffers resistance in its passage from a solid to a gaseous, or from a gaseous to a solid body; "for it is clear," says he, "that the obstacle which compels the current to pierce the tin disc does not proceed from the resistance due to the metal itself, inasmuch as this resistance is much more feeble than that of the air displaced by the tin."

The conductibility of the partial vacuum obtained by means of an air-pump being so small, it was natural to expect that the more perfect one in a barometer tube would be still less conductible. M. Gaugain proved this by connecting the poles of Ruhmkorff's apparatus with the mercury, and with a platinum wire fused into the top of an ordinary barometer tube. He found it impossible to obtain the least appearance of light in the barometer chamber when the distance from the summit of the mercurial column to the platinum wire exceeded 3 or 4 millims., although he employed six of Bunsen's cells.

This result is in opposition to one obtained by M. Masson, and communicated to the Academy on the 7th of February, 1853; but M. Gaugain accounts for this divergence satisfactorily. In fact, the greatest possible care is necessary to expel the air and moisture in the barometer tube; indeed, when all precautions have been taken and a vacuum obtained which will not allow the induced currents to pass, the application of a spirit-lamp to the top of the tube for a few moments is sufficient, although it may have become cool afterwards, to make the vacuum conduct the currents again. It appears that the tubes used by Masson were heated by a lamp after the introduction of the mercury, and hence his results.

XLI. *On the Molecular Constitution of Crystals.*
By JAMES D. DANA.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN a communication to your Journal for August last, by Mr. R. T. Forster, the author makes a statement which I beg the privilege of correcting.

Mr. Forster says, speaking of my views on twin-crystals, published in the American Journal of Science for 1836, that the ideas "were directly borrowed from Sir David Brewster," and that the paper by Sir David, containing his views, "was *quoted by Dana* in the American Journal of Science, and formed the basis of his investigations, without, however, being acknowledged."

At the time of this "quoting by Dana" in 1832, I was still in my teens, and for fourteen years afterwards I had no connexion with the American Journal, and was not in expectation of any such connexion. Instead of Sir David Brewster's paper forming the basis of my investigations, I had not at that time, culpable though it may appear, perused his paper. The conclusions were mainly arrived at during a cruise at sea in the years 1833 and 1834, when I had by me only a copy of Phillips's 'Mineralogy,' from the measurements of crystals in which excellent work, not knowing at that time in my solitude what had been done by others, I devised also the Haüyan method of crystallographic calculation. In my paper, published in 1836, I cite the opinion of Voltz as to the existence of three axes of attraction in molecules, and obviously do not claim originality on that point, the main one in which the theories agree. In a later paper in the American Journal of Science, 1847, vol. iv. p. 169, on "Cohesion," I have alluded to the observations of Sir David Brewster, and made a citation from his paper. The views I have brought forward are different from his in my regarding the axial dimensions of the molecule to be those of the crystal, and not a resultant between the pressure from the combining attraction and the dimensions: and on the subject of twin-crystals, I am not yet aware that anything has been written by Sir David.

I have the highest admiration for the labours of Sir David Brewster, and on the points of difference, while sustaining my own opinions, would not venture to affirm my own correctness, knowing that the subject of molecules is the last that science will clear up. I write, not to make claims of originality, but simply to correct an erroneous statement, and disavow all intentional injustice.

Very respectfully, your obedient Servant,
JAMES D. DANA.

New Haven, Conn.,
September 15, 1855.

XLII. *On certain curious Motions observable at the Surfaces of Wine and other Alcoholic Liquors.* *By* James Thomson, *A.M., C.E., Belfast*[*].

THE phænomena of capillary attraction in liquids are accounted for, according to the generally received theory of Dr. Young, by the existence of forces equivalent to a tension of the surface of the liquid, uniform in all directions, and independent of the form of the surface. The tensile force is not the same in different liquids. Thus it is found to be much less in alcohol than in water. This fact affords an explanation of several very curious motions observable, under various circumstances, at the surfaces of alcoholic liquors. One part of these phænomena is, that if, in the middle of the surface of a glass of water, a small quantity of alcohol or strong spirituous liquor be gently introduced, a rapid rushing of the surface is found to occur outwards from the place where the spirit is introduced. It is made more apparent if fine powder be dusted on the surface of the water. Another part of the phænomena is, that if the sides of the vessel be wet with water above the general level surface of the water, and if the spirit be introduced in sufficient quantity in the middle of the vessel, or if it be introduced near the side, the fluid is even seen to ascend the inside of the glass until it accumulates in some places to such an extent, that its weight preponderates and it falls down again. The manner in which I explain these two parts of the phænomena is, that the more watery portions of the entire surface, having more tension than those which are more alcoholic, drag the latter briskly away, sometimes even so as to form a horizontal ring of liquid high up round the interior of the vessel, and thicker than that by which the interior of the vessel was wet. Then the tendency is for the various parts of this ring or line to run together to those parts which happen to be most watery, and so there is no stable equilibrium, for the parts to which the various portions of the liquid aggregate themselves soon become too heavy to be sustained, and so they fall down.

The same mode of explanation, when carried a step further, shows the reason of the curious motions commonly observed in the film of wine adhering to the inside of a wine-glass, when the glass, having been partially filled with wine, has been shaken so as to wet the inside above the general level of the surface of the liquid; for, to explain these motions, it is only necessary further to bring under consideration that the thin film adhering to the

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[*] Communicated by the Author, having been read at the Meeting of the British Association at Glasgow.

inside of the glass must very quickly become more watery than the rest, on account of the evaporation of the alcohol contained in it being more rapid than the evaporation of the water.

That this part of the explanation is correct, or that these motions of the film in the wine-glass are really due to evaporation, may be shown by a very decisive experiment. If a vial be partly filled with wine and shaken, and then allowed to rest, no motion of the kind described will be found to occur in the thin film wetting the inside, provided that the vial be kept corked. On the cork being removed, however, and the air contained in the vial, and saturated with the vapour of wine, being withdrawn by a tube, so as to be replaced by fresh air capable of producing evaporation, a liquid film is instantly to be seen creeping up the interior of the vial with thick or viscid-looking pendent streams descending from it like a fringe from a curtain. These appearances are quite of the same kind as those met with in the open wine-glass.

Another experiment may be made to show, in a very striking way, the phænomenon of the more watery portion of the surface of a mixed liquid drawing itself away from the more alcoholic portion as follows:—If water be poured to the depth of about a tenth of an inch or less on a flat silver tray or marble slab, previously cleaned from any film which could hinder the water from thoroughly wetting its surface; and if then a little alcohol or wine be laid on the middle of that water, immediately the water will rush away from the middle, leaving a deep hollow there, and indeed leaving the tray bare of all liquid except an exceedingly thin film of the spirit, which continues always thinnest close to the margin of the water, because the water draws out to itself every portion of the spirit which approaches close to its margin.

The experiment alluded to near the commencement of the present paper, in which spirit was to be introduced into the middle of a surface of water previously dusted over with fine powder, may be well conducted as follows:—A tube for supplying the spirit should be provided*, which may be three or four inches long, half an inch or three-quarters in diameter, and terminating at bottom in a small open point, which, if found too wide, may be partially stopped by the insertion of a piece of thick soft thread, such as a strand from the wick of a spirit-lamp. A knot on the thread inside of the tube will serve as a valve to curtail or stop the flow of the spirit when required. The surface of the water should be clean and free from any kind of pellicle,

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* The tube of a small glass syringe as sold by apothecaries will serve the purpose well.

such as is often met with, and is sometimes not easily avoided. It should then be lightly dusted over with some fine powder not apt to be quickly wet: Lycopodium powder will serve the purpose. Then the tube filled with spirit is to be dipped with its open point into the surface of the water, and instantly a nearly circular patch round the point of the tube will be seen occupied with liquid rushing outwards and completely divested of the covering of powder, while on the part outside of that patch there will be seen, by the motions of the powder, one, two, three, or many radial streams flowing outwards from the middle, and other return streams or eddies flowing backwards to the margin of the patch, on arriving at which each particle seems suddenly as if driven outwards with a rapid impulse. The margin of the central patch is usually to be seen formed like as of leaves of a plant growing out all round, and some superimposed on others, and all in rapid motion. The nature and causes of these forms of the margin, and of the eddies outside of the margin, I have not as yet been able satisfactorily to explain.

Another experiment may be made which is quite in accordance with the explanations already given, and which, being due to condensation of alcohol on a surface of water, is interesting when viewed in comparison with that in which the motions were shown to be produced by evaporation:—If a silver spoon, perfectly wetted with water so that a thin film adheres to it, be held over an open cup or vessel containing strong alcohol, the surface of the liquid will become greatly agitated with numerous motions, which are to be attributed to the unequal and varying condensation of the vapour of the alcohol at different parts of the surface of the film, according as the vapour is wafted about in fumes by the air.

While engaged in the investigation of the phænomena which I have now described, my attention has been turned to some other very interesting phænomena previously observed by Mr. Cornelius Varley, and described by him in the fiftieth volume of the Transactions of the Society of Arts. He observed with the aid of the microscope numerous motions of extremely curious and wonderful characters in fluids undergoing evaporation. Although I have not yet had it in my power to examine into all the phænomena he has discovered relative to these motions, yet I think that many of them, or all, are to be explained according to the principles I have now proposed.

I have not had access to the Transactions of the Society of Arts to read Mr. Varley's paper in full, but I quote the following abstract of his results from Queckett's 'Treatise on the Microscope,' 1st ed. p. 413:—"The plan recommended is as follows: take an animalcule-cage of moderate size, and upon the tablet place a drop of turpentine or spirits of wine, &c., then slide over it the thin glass cover, but do not compress the fluid very much; the microscope being placed in the vertical position, and provided with a magnifying power from 40 to 100 diameters, the contents of the cage are to be examined in the same way as if animalcules were contained in it. As the evaporation of either of these fluids takes place, numerous currents and vortices will be seen, especially if a small quantity of finely-powdered coal be ground into them; the particles of coal being very light, are held in suspension whilst the evaporation is going on, and are whirled about by the currents in different directions." The following fluids Mr. Varley has given as the best for the illustration of the currents:—

"1. A drop of spirits of wine, or of naphtha, exhibits two, three, or four vortices or centres of circulation, according to the size of the drop; and if these vortices are viewed laterally, the lines of particles will be seen forming oblique curves from top to bottom of the drop.

"2. Oil of turpentine shows a rapid circulation in two continuous spirals, one to the right, the other to the left, around the drop. These meet in the opposite diameter, from which the particles are carried slowly across the diameter to the place of starting, and this continues while there is fluid enough to let it be seen.

"3. If, however, the drop does not exceed one-tenth of an inch in diameter, it presents the appearance of particles continually rising up in the middle, and radiating in gentle curves to the circumference.

"4. If the liquid be put into a very small vial, similar motions are perceived, the particles when they have reached the side of the vial going down to rise up afterwards in the centre or axis.

"5. If a bubble of air be enclosed in the liquid, motions similar to those described in No. 2 are observed in the part immediately in contact with the bubble.

"6. In a flat drop of new wine laid on the tablet or disc of the aquatic live-box, but not compressed by the cover, the motion was a regular uniform circulation, the particles rising from below at one end of the drop, then passing straight across on the surface, and descending at the other end."

XLIII. *On Pressures of Saturated Vapours.*
By W. J. Macquorn Rankine, *C.E., F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In consequence of some remarks which have been addressed to me respecting my letter on the above subject in the Number of your Magazine for October, I think it desirable to explain, that the formula

$$\log P = A - \frac{B}{\tau} - \frac{C}{\tau^2} - \ldots \qquad (2)$$

is in no way deduced from the formula

$$\log P = a - b \cdot \log \tau - \frac{c}{\tau}, \qquad (3)$$

and that the coefficients A, B, C are not functions of a, b, c. In fact, the formula (2) was *suggested as probable* by the hypothesis of an attraction between the liquid and its vapour, as explained in the Numbers of your Magazine for December 1851 and December 1854; and A, B, C are part of an indefinite series of constants determined empirically, so as to agree with M. Regnault's experiments; while (3) is the result of an independent hypothesis as to a condition of saturated vapour, which may, as I have already stated, prove to be purely ideal, and respecting which, therefore, I refrain from entering into detailed investigations until we possess exact data as to the densities of vapours.

The formula of Professor Holtzmann,

$$\log P = a - \frac{b}{\tau},$$

appears to be nearly the same with that of Roche, and may be held to constitute the *first approximation* towards the law of the maximum pressure of vapours.

I have the honour to be, Gentlemen,
Your most obedient Servant,
W. J. Macquorn Rankine.

Glasgow, October 1, 1855.

P.S. Since the above remarks were written, Sir John Lubbock has done me the favour to send me a table showing the differences between the results of his formula (1) and those of the experiments of M. Regnault on steam; the three constants in the formula having been determined from the same data with those in the formula published by me in 1849, viz. the pressures of steam at 26°·86, 100°, and 220° Centigrade.

These differences are, on the whole, greater than those resulting from my formula (2); but still they are so small as to lead to the following conclusion:—that to Sir John Lubbock belongs the merit of having in 1840 first proposed a formula containing *three* constants only, which is capable of giving the pressures of vapours with a near approximation to accuracy throughout the whole range of existing experiments.

W. J. MACQUORN RANKINE.

Glasgow, October 6, 1855.

XLIV. *On the Magnetic Variation in the Vicinity of the Cape of Good Hope. By* Colonel EDWARD SABINE, *Treas. & V.P.R.S.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN, 13 Ashley Place, Oct. 16, 1855.

IF you should have room for the accompanying communication in your November Number, you will oblige me by its insertion.

I remain,
Your obedient Servant,
EDWARD SABINE.

In a letter which has been printed and circulated by the Board of Trade, addressed to Mr. Hartnup of the Liverpool Observatory, by Captain Klein of the Dutch Bark 'Magdalena,' who had been supplied at Liverpool, in April 1854, with a set of the marine meteorological instruments which had been made under the superintendence of the Kew Committee of the British Association, and verified at the Kew Observatory, it is satisfactory to read the testimony of an obviously experienced navigator in regard to the efficiency of the Kew marine barometer. Captain Klein remarks, "I never observed with such a nice and beautiful working instrument as the Kew model barometer. With the greatest motion of the vessel it makes no greater jump than half a tenth; while at the same time it is so very sensitive, that the series will give a correct idea of the atmospheric tides." Captain Klein's testimony cannot but be very satisfactory to the gentlemen of the Kew Committee, and yet more so to Messrs. Welch and Adie, by whose ingenuity and pains the prevailing faults in the ordinary construction of the marine barometer have been greatly surmounted, whilst the price of the instrument has been reduced to about two-thirds of its usual previous amount.

The object of the Board of Trade in printing and circulating Captain Klein's letter, was doubtless a desire to make his experience of the good performance of these barometers known amongst the owners and masters of our merchant ships, with the view of introducing the use of these instruments more extensively in our mercantile marine,—an object which may be further promoted by making Captain Klein's opinion known to the readers of the Philosophical Magazine.

But the particular object of this communication is not so much to give additional currency to the Captain's commendation of the Kew barometer, as to notice a remark of his which occurs subsequently in the same printed letter, regarding maps of the magnetic variation. The remark is as follows:—"It was surprising to see how incorrect the variation charts are: my results differed often greatly from Raper's approximations, and I should frequently have wavered had I not invariably obtained the same amount as was indicated by the last observations made on land." With respect to one of the authorities to which Captain Klein refers, viz. "Raper's approximations," I have of course nothing to say; I am not myself acquainted with the work; but as being myself the author of a recent Map of the Variation over the North and South Atlantic Oceans, I could not but be solicitous, on reading Captain Klein's remark, to obtain a knowledge of any inaccuracies within the limits of that map which might appear to be substantiated by his observations; and having understood that the observations had been placed in the hands of Dr. Buys Ballot, Director of the Marine Meteorological Department in Holland, I wrote to that gentleman requesting him to communicate to me any particulars contained in them which might appear to be at variance with the variation lines in my map, to which I knew Dr. Buys Ballot had the means of referring.

I may here notice, that as the accuracy of my map rests wholly on the accuracy of the observations which were coordinated in its construction, I can have no motive whatsoever for not being glad to receive corrections, if more accurate observations are produced than those which were at my command when the map was constructed; and that for the express purpose of obtaining such corrections (should any appear to be required) by means of the officers of the Dutch Navy, who have the general reputation of being careful and skilful navigators, I presented several copies of my map a short time after it was published to Chevalier Swart, Hydrographer of the Dutch Navy, for distribution among Dutch officers. The chief peculiarities in this map which might be considered to add to its practical value, were, that it was constructed in all its parts for one and the same epoch, viz. 1840; that no other observations than those of contemporaneous, or very nearly contemporaneous date, were employed in its construction; that all the observations on which it was based were engraved on the face of the map; and that a memoir was attached to it in which these observations were discussed, and their corrections, either for local attraction if made on board ship, or for the difference of epoch if any, were carefully stated; and a table was also given referring to different latitudes and longitudes, by which the variation in any particular locality within the limits of the map might be approximately corrected for any other epoch (not very distant from 1840), conformably to the secular change which had taken place in the previous half century, or more exactly between 1787 and 1840.

Dr. Buys Ballot has kindly engaged to furnish me with a full account, both of Captain Klein's observations and of others made in the Dutch frigate 'De Ruyter,' in a similar voyage between Batavia and Amsterdam. It appears that a large portion of these observations apply to other parts of the ocean than those comprised in my map; but *the vicinity of the Cape of Good Hope*, which is included in that map, is specially pointed out by Dr. Buys Ballot as a locality in which the Dutch observations are at variance with the maps. Dr. Buys Ballot writes as follows:—"It is true that the corrections for local attraction are not applied (in the Dutch observations) in strictly due manner; but I have persuaded myself that since the local attraction was very small in those two vessels, and did not exceed 4° in any direction, this error could not affect the conclusions in any appreciable manner: therefore I think that really the maps give in the vicinity of the Cape a too great westerly variation."

If I could entertain the slightest doubt about the accuracy of the variation as given in my map for the vicinity of the Cape of Good Hope, I should most readily wait for the particulars which Dr. Buys Ballot is good enough to promise to furnish me; but I think that I am able to show that the variation in that locality as given in the map, whether it be taken for 1840 or for the present time, rests on too solid a foundation to be shaken by observations, however carefully made on shipboard, but which are uncorrected for the ship's attraction.

On reference to the memoir accompanying the map (Phil. Trans. 1849, part 2. art. xii.), it will be seen that the first in importance amongst the authorities for the variation at the Cape of Good Hope are the observations at the magnetic observatory at that station, giving as a mean result 29° 7′. The only doubt that could attach to a determination made with every care and precaution, and with the excellent instrumental means of a fixed magnetic observatory, resting also in this case on years of observation, would be the possibility of the existence of station error, viz. possible local deviation occasioned by rocks of particular character in the vicinity of the observatory affecting the needle; but this is a matter which has been long since set at rest by the strict accordance which has been found between the results obtained at the observatory itself, and at stations distant from it a few miles in different directions. Amongst these may be named the variations most carefully observed by Admiral Du Petit Thouars in False Bay in April 1839 (29° 9′), and by Sir James Clark Ross in Simon's Bay in April 1840 (29° 23′). The general result is also confirmed by nearly cotemporaneous observations (1837 to 1845), made at sea in the vicinity of the Cape by Admirals Vaillant and Du Petit Thouars, and Captains Sir James Clark Ross, Crozier, Moore and Clark, which are detailed in the memoir referred to, and are inserted in the map; the mean of all the sea observations, viz. 28° 51′, being shown in the map to correspond to the intersection of lat. −35° and long. 17° 30′ E., which, with a proper allowance for latitude and longitude, accords almost precisely with 29° 07′ at the observatory in lat. −33° 56′ and long. 18° 29′ E.

So far in regard to the accuracy of the map in relation to the period for which it was constructed, viz. 1840; but it may be asked, may not the magnetic variation in the vicinity of the Cape have altered since 1840 by the operation of secular change, so as to make the determinations on which the map was based inapplicable to the present epoch? It is well known, that generally in the Southern Atlantic the westerly variation increases at a nearly uniform rate of secular change of about 8′ in the year; and it is also known that the same rate of secular increase, or more exactly 7′·56, did obtain at the Cape of Good Hope from the earliest observations of Davis and Keeling in 1605 and 1609, to those of Captain FitzRoy and of Sir James Ross in 1836 and 1840. (Cape Magnetic Observations, vol. i. p. lx.) Had this rate of secular change (which had lasted so long) continued to 1854 (when Captain Klein's observations were made), the variation at the Cape would have been nearer 31° than 29° 07′; that is to say, the westerly variation in my map would have been *in defect* in 1854, instead of *in excess* as Dr. Buys Ballot supposes. The Cape, however, was selected as a station for a magnetic observatory, amongst other reasons for this one in particular, that the form of the variation lines in its vicinity, and their known progress from east to west in the temperate zone of the southern hemisphere, gave reason to believe that the secular increase of the variation which had taken place for so many years was approaching the period of its termination, when it would be succeeded by a nearly stationary variation continuing for several years. The probability of this alteration in the secular change is noticed in the memoir accompanying the map. (Phil. Trans. 1849, p. 200.) That it has been realized, has been shown by the records of the Cape Observatory. Between January 1841 and August 1850, the westerly variation had increased from 29° 7′ only to 29° 14′·6; or at something less than an average annual rate of 1′. (Cape Magnetic Observations, vol. i. p. lxv.) Supposing this rate to have continued to 1854, when Captain Klein observed, the variation at the Cape should have been 29° 18′, which is still 11′ *greater* than that which is shown by the map (instead of *less*).

But we are not without data from the vicinity of the Cape still nearer the epoch at which Captain Klein observed, from which we may obtain a corroboration of the correctness of the variation as given in the map, and its applicability to the present time. Lieutenant Dayman, R.N., formerly of the Magnetic Observatory of Van Diemen's Land, whose skill and accuracy as a nautical surveyor are justly held in the highest estimation, has transmitted to the Admiralty the results of determinations in 1853 at eighteen different stations, partly observed on land, and partly at sea on board H.M. Surveying Ship Hydra, between Capes Hangklip and Aguilhas. By the kindness of the Hydrographer, these were sent to me as soon as they reached the Admiralty. The mean of the eighteen determinations is 29° 24′. It would be difficult to say what result could be more in accordance with the map, allowing for the small difference of latitude and longitude. Therefore, had Captain Klein's observations agreed with the latest observations made on land (*i. e.* at the Cape Observatory and on the coast in its vicinity), they would also have substantially agreed with my map. If they differ from it, they differ also from the observations obtained by many observers of high reputation both on shore and at sea.

In conclusion, I wish to express the satisfaction with which I view the evidence afforded by Captain Klein's communication of the increasing interest which is taken in these questions, since the attention of the mercantile marine of different nations has been called to their importance; and I also see with pleasure the freedom with which Captain Klein's remarks and criticisms are given. We may confidently expect that one result of such increased attention will be improvement in maps of the magnetic variation in localities where they need improvement: and it may not be amiss to notice, that since, by the increased employment of iron in ships, the compass has been rendered much more subject to deviation than it formerly was, maps of the magnetic variation which can be relied upon, both as to epoch and amount of variation, have, besides their primary use in navigation, this additional value, that a single observation taken of the variation with the ship's head on the course which she may happen to be steering, furnishes, by comparison with the map, the deviation of the compass with the ship's head on that particular point in that part of her voyage; and thus such a map facilitates the task, which has now become an essential part of correct navigation, of the continual examination, and correction when required, of the tables of the deviation with the ship's head on different points. The inverse proceeding, or the endeavour to correct maps formed on a sufficiently extended basis of coordinated land and sea observations by observations at sea which have not been corrected for deviation, is hardly to be recommended: and doubtless as variation maps become more deserving in themselves, and their value is better known, will be less likely to be practised.

XLV. *On the Curvature of the Indian Arc; and the great Geological Law, that the various parts of the Solid Crust of the Earth are perpetually undergoing a Change of Level. By* Archdeacon PRATT.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

OF the existence of the great law brought to light by the science of geology, that the various parts of the earth's surface are perpetually undergoing a change of level, there can be now no reasonable doubt. The observations made on landmarks on the coast of the Baltic,—the singular phænomenon of the columns of the temple of Serapis near Naples, although now above the water-level, as they must have been when the temple was built, and yet showing marks of having been below the surface, in the perforations of a kind of sea shell-fish at the present day flourishing in the neighbouring waters,—and the original researches and bold generalizations of Mr. Darwin among the ancient and recent coral formations of the South Seas,—combine to establish the law in the most satisfactory manner.

2. To assign a cause of this law may be more difficult than to recognize its existence. Mr. Babbage, and others after him, have attributed it to the expansion and contraction of masses of rock beneath, caused by accessions and withdrawals of heat through chemical agencies. Obscure as the origin of the law may be, the importance of its discovery cannot be over-estimated, as it supplies so clear an explanation of the wonderful alternations of level which must have taken place to produce the phænomena of fresh- and salt-water formations which geology brings to light.

3. Satisfactorily as a law of nature may have been made out, it can never be out of place to bring forward new illustrations, especially in one of such importance as this law possesses. I shall therefore make no apology for asking you to admit into your Magazine a proof drawn from an entirely new province, that of mathematical physics. The circumstances and arguments on which it is based I will now relate.

4. The mean ellipticity of the earth has been investigated by both the Astronomer Royal and the late M. Bessel with great care, by a comparison of arcs of meridian measured in various parts of the earth; and although the methods were somewhat different, they arrived at the same conclusion, one making the ellipticity $\frac{1}{300{\cdot}7}$, the other $\frac{1}{300{\cdot}8}$, the difference being inappreciable. Now this is the ellipticity to which theory leads us in supposing the earth to be in a fluid state, the density to increase gradually towards the centre by the law $\frac{\sin qr}{r}$ (r being the distance from the centre, and q a constant), the density of the superficial stratum to be that of granite, and the mean density of the earth to be what the late Mr. F. Baily made it. This coincidence has been generally considered to be a conclusive argument in favour of the hypothesis that the earth was once fluid, and acquired its present form (at any rate its present *mean* form) at that time, and from hydrostatic principles.

5. These investigations, however, do not show that the whole meridian through any place is exactly elliptical, nor that the meridians through places in different longitudes are all alike. They show merely that the *mean* curvature of the different parts of the earth is that which corresponds with the fluid condition of its mass, which is, as I have said, a very strong argument in favour of the hypothesis of original fluidity.

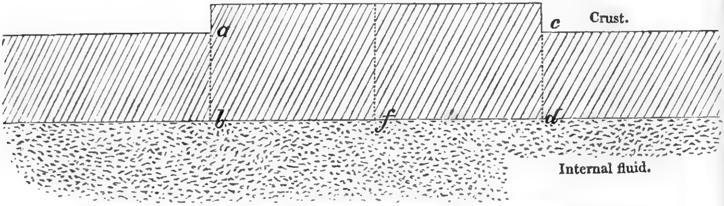
6. That the curvature of every meridian does not equal the mean curvature can, I think, be most satisfactorily proved in the case of the great Indian arc, about 800 miles long, and lying between Kaliana (about 50 miles from the Himalaya Mountains) and Damargida. This arc has been divided into two nearly equal parts, and the lengths and astronomical amplitudes of both have been determined with great precision. The astronomical amplitudes, however, need correction for the attraction of the Himalayas, which are found to have a sensible effect upon the plumb-line. In a paper published in the Transactions of the Royal Society for the present year, I have given a method for calculating the effect of this attraction, and have reduced the formulæ to numbers. On calculating the curvature of the Indian arc from the estimated lengths of its two portions, and the amplitudes corrected for attraction, I find that it is greater than the mean curvature; in fact, that the middle point of the whole 800 miles is raised a few feet higher above the cord joining its extremities, than if it possessed the mean curvature.

7. An hypothesis has been thrown out, since the publication of my result, to show that the effect of attraction from the enormous mass rising above the mean surface in the Himalayas,

would be counteracted by a counter or negative attraction arising from a *deficiency* of matter below*. It is assumed that the interior of the earth is fluid, and that the crust is sufficiently thin to allow the principles of hydrostatics to regulate the external form of the surface, or that this was the state of things when the surface took its present form. It is further assumed that the solid crust of the earth is light compared with the fluid mass on which it floats.

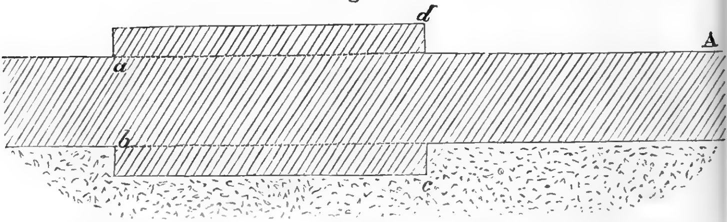
The case is then taken (fig. 1) of a table-land 2 miles high, 100 miles wide, and of indefinite length,—suggested, no doubt, by the plateau from which the Indus and Brahmapootra flow, more than 10,000 feet above the sea-level, and forming the most important part of the attracting mass in my paper. The mean crust is taken to be 10 miles thick, and therefore 12 at the table-land. It is then conceived that the crust is broken through in the dotted lines *ab, cd, ef*, where lies the greatest tendency to crack, and the force of cohesion necessary to prevent the two portions from sinking into the fluid below is estimated. The force is found to be equal to a weight of 20 miles' length of the rock; and the conclusion is justly drawn, that as no such power of cohesion can exist in the crust, the state of things represented in fig. 1 cannot be that of nature.

Fig. 1.



8. The hypothesis, as represented in fig. 2, is then suggested,

Fig. 2.



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* See a paper in the Transactions of the Royal Society for 1855, "On the Computation of the Effect of the Attraction of Mountain Masses," &c., by G. B. Airy, Esq., Astronomer Royal.

that the table-land is supported by the buoyancy of a mass of crust projecting downwards into the heavy fluid. This representing the state of things, a mass *ad* rising above the surface and attracting a distant station A, necessarily implies a corresponding *deficiency* of mass in the locality *bc*, causing an almost equal *negative* attraction at A counteracting the former positive attraction.

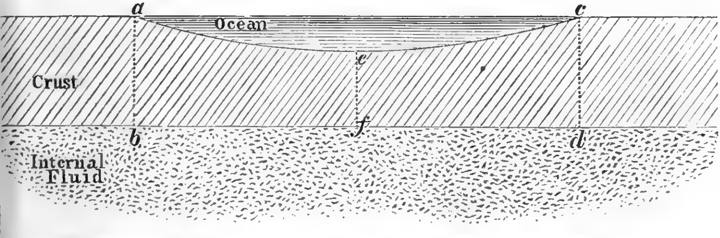
9. There are some difficulties in the way of this ingenious hypothesis which I wish now to state.

1st. On what physical principle can it be assumed that the crust of the earth is lighter than the fluid immediately below it? It might rather be imagined, that by the process of consolidation in using heat it would become contracted and therefore heavier.

2nd. If the result at which Mr. Hopkins has arrived in his calculations on precession be correct, that the solid part of the crust is at least 1000 miles thick, an enormous interval of time must have elapsed since the crust was sufficiently thin for the form of its surface to be affected by the fluid below. During this immense interval, there has been abundance of time for the geological law, of which I have spoken at the beginning of this paper, to have altered the form of the surface, originally given by hydrostatic principles, to such a degree as to have obliterated all traces of it; indeed, the whole table-land and all the mountain range may have been heaved up during this time.

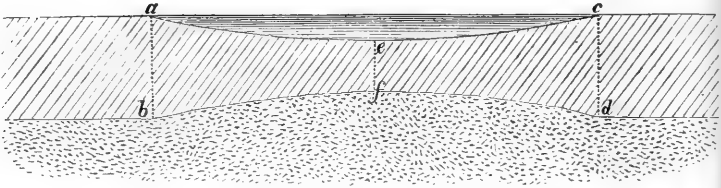
10. But I have another, a third difficulty. If the crust be so thin, or was so when the earth assumed its present contour, what must be the state of the crust beneath deep and wide oceans?

Fig. 3.



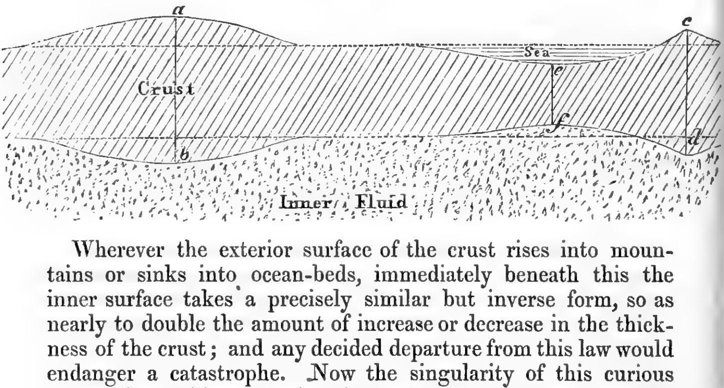
The state of things in fig. 3 could no more exist than that of fig 1. If we take the width of the ocean to be only 100 miles, and average depth 2 miles, and density half that of the crust, the cohesive force necessary to prevent the upward pressure of the internal fluid from breaking up the crust and letting in the ocean, is a force equal to the weight of about 14 miles of rock. This state of things cannot exist, and we must substitute fig. 4, in

Fig. 4.



which there is a hollow in the under part of the crust, allowing the heavy fluid to rise to such a height above its mean level that the deficiency of weight of the ocean may exactly equal the excess of weight of the heavy fluid below. Any departure from this form would endanger a catastrophe. By introducing, then, the principles of floatation to explain the cause of the exterior form of the earth's surface, we come to the following conclusion regarding the inner surface of the crust.

Fig. 5.



Wherever the exterior surface of the crust rises into mountains or sinks into ocean-beds, immediately beneath this the inner surface takes a precisely similar but inverse form, so as nearly to double the amount of increase or decrease in the thickness of the crust; and any decided departure from this law would endanger a catastrophe. Now the singularity of this curious inverse law militates against its reality. But more especially does it seem to contradict all notions of the process of solidification by cooling down. We can conceive no reason why the crust should have thickened so much less at *ef* than at *ab* and *cd*, but quite the contrary. It might have been expected that at *f* the crust would have thickened downwards faster than at *b* and *d*, till a pretty uniform thickness had been attained and kept up.

11. I feel disposed, therefore, to hesitate about the existence of a deficiency of matter below mountains, and rather to suppose that the variations of the present external form of the earth have

not been brought about by hydrostatical principles, although the *mean form* has most probably been so.

12. If this be a correct conclusion, it follows (see art. 6) that the country about the middle of the Indian arc is more elevated by some feet than its extremities. No doubt if this long arc were further subdivided into many portions, and their lengths estimated and their amplitudes corrected for attraction, they would all be found to have different ellipticities, the mean of which would be the ellipticity of the Indian arc. For it is hardly to be supposed, that if the form of the whole arc has been made to deviate from the fluid form by upheaval from below, that the curve of 800 miles has perfectly preserved its symmetry north and south of the middle point. Different portions are no doubt bent differently, and this process of calculation would show it. The calculation which I have made (and for the details of which I must refer to the paper in the Philosophical Transactions) exhibits this principle only for the whole arc, and not for its parts separately.

13. But this, I think, is sufficient to establish my point, and I gather from it the two following conclusions:—

I. The crust of the earth was too thick at the time the surface assumed its present form, and therefore is now too thick, to suffer the fluid below to regulate that form; for it has been proved that a large portion of it, the Indian arc of 800 miles, has not the form which the fluid theory requires.

II. Since the epoch when the crust became too thick for the fluid below to regulate the form of the surface, other forces—of upheaval, or of depression, or both—must have been in action; for the middle of the Indian arc is some feet higher than its two extremities when measured from the fluid level.

The forces thus brought to light by an investigation in mathematical physics must be of the nature of those which exhibit the great geological law I proposed to illustrate.

J. H. Pratt.

Lausanne, October 6, 1855.

XLVI. *Examination of an Organic Matter found in an ancient Egyptian Bottle. By* Philip B. Ayres, *M.D. Lond.*[*]

A SHORT time since I was requested by my friend B. Nightingale, Esq., to submit to analysis a substance he had shaken out of an ancient Egyptian bottle, supposed by him to have contained wine. The bottle was of coarse earthenware, formed of clay mixed with a considerable proportion of sand; it was glo-

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[*] Communicated by the Author.

bular, with a very short neck; a mouth so narrow that the little finger could not be inserted into it; with two ears through which a strap or cord might be passed so as to suspend it; and of the capacity of about a pint.

The substance shaken from this bottle had evidently formed a crust on its inner surface, produced by evaporation of, or subsidence from, its former contents, mixed with sand derived either from the coarse internal surface or from without. As presented to me, it was a mixture of sand with flattened masses of a deep brownish-black colour, earthy fracture, porous structure, and a saline, somewhat styptic taste.

When heated on platinum foil over the spirit-lamp, a fragment burned with a clear flame, without either fusion or intumescence, leaving a charred mass, from which the charcoal was burned off with difficulty.

As the substance was thus proved to be organic, I picked out the masses from the sand; but as much of organic matter still remained mixed with the sand, this was employed for the qualitative, reserving the purer masses for the quantitative analysis.

A. The sand mixed with the organic matter was boiled repeatedly with distilled water, yielding a deep reddish-brown solution, approaching very closely to that of port wine, which after filtration remained perfectly transparent, but of so deep a colour that when in bulk it was black and opake.

The solution reddened litmus distinctly, but not strongly.

Hydrochloric, nitric, and sulphuric acids threw down a light, flocculent, deep reddish-brown precipitate, leaving the supernatant liquid of a pale straw-colour. Solution of caustic potash caused no precipitate or change of colour. Solution of gelatine caused no change. Tincture of galls also produced no change. Lime-water deepened the colour, and produced a copious light-brown flocculent precipitate. Oxalate of ammonia rendered the fluid turbid without deepening its colour. Diacetate of lead caused so copious a brown precipitate as to render the liquid gelatinous. Acetate of lead, a moderately bulky, brown, flocculent precipitate, but not nearly so copious as that with the diacetate. Sulphate of copper, a brown precipitate. Sulphate of alumina and potash also a brown precipitate. Bichloride of tin, a light, flocculent, brown precipitate. A portion of the liquid evaporated to dryness left a black, shining, brittle mass.

B. The residue *A*, insoluble in boiling water, was treated with boiling alcohol, spec. grav. 0·835. The solution was light yellow. It became turbid on the addition of water, and deposited granular, semicrystalline, fatty matter by evaporation, which was solid when cold, but melted on application of heat.

C. The mixture of sand and organic matter insoluble in water and alcohol was next treated with solution of caustic potash, which without the aid of heat dissolved the greater portion of the remaining organic matter, and became deep brownish-black, although perfectly transparent when viewed in thin layers.

This was evidently the insoluble humic acid analogous to that found in vegetable earth. It was precipitated in a flocculent form, of a brown colour, by hydrochloric acid, and the supernatant liquid became light straw-colour. When washed and dried, it was left as a shining, black, friable mass.

D. A portion of the deposit was heated to redness in a test-tube. It swelled up, partially fused, gave off a dense white vapour, which condensed into drops of a light yellow empyreumatic oil, and left a bulky black charcoal. No odour of ammonia could be perceived, nor were any white vapours produced by the introduction of a rod moistened with hydrochloric acid into the tube, thus proving the absence of nitrogen. The odour of the vapour and of the oil was that of burnt vegetable matter.

Quantitative Analysis.

A. 39·7 grains, the whole of the purer organic matter separable from the sand, digested with alcohol, spec. grav. 0·835, yielded a deep brownish-red solution. The whole of the soluble matter was removed by repeated digestion with fresh portions of alcohol until exhausted. The alcoholic solutions were mixed and evaporated to dryness, leaving a brownish residue weighing 15·5 grains. During the evaporation, part of the waxy or resinous matter separated and collected on the surface of the fluid.

a. The alcoholic residue was treated with æther, which dissolved a considerable portion, forming a light reddish solution, which reddened blue litmus-paper strongly.

The ætherial solutions, on evaporation to dryness, left a solid reddish-brown matter weighing 11·4 grains, which approached more in consistence to wax than fat. This residue yielded scarcely a tinge of colour to boiling water, on the surface of which it melted and collected in drops.

b. The undissolved residue (*a*) was treated with cold distilled water, which formed a vinous-red solution, leaving the undissolved matter almost colourless. The solution reddened litmus rather strongly. On evaporation to dryness it left a brownish-black matter, similar in most of its characters to the aqueous extract (*B*), weighing 2·9 grains. Heated to redness, it first fused, then swelled up enormously, leaving a voluminous charcoal, which burned off very slowly and yielded 1·16 grain of ash.

c. The residue of the successive action of æther and water (*b*) was treated with a boiling mixture of equal parts of æther and alcohol, in which it wholly dissolved. The solution was yellowish, became turbid on cooling, and again recovered its transparency when heated. By evaporation it yielded a nearly white, friable, granular powder, which melted on exposure to heat, and burned with a white flame.

B. The residue (*A*) exhausted by alcohol, treated with cold distilled water, yielded a red-brown solution so deep in colour as to be opake when in bulk. It was finally exhausted, first with cold and afterwards with boiling water. The solution, evaporated to perfect dryness, left a black, shining, brittle residue, weighing 11·0 grains.

When heated, it burned with a very feeble flame, semi-fused, swelled moderately, and left a somewhat voluminous iridescent charcoal, which burned off very slowly. 5·5 grains yielded exactly 1·0 grain of ashes.

The ash was partly soluble in distilled water. The soluble portion had a decided alkaline reaction, effervesced with acids, remained clear on the addition of solution of caustic potash, gave a precipitate with nitrate of silver which was almost wholly redissolved by nitric acid in excess; yielded a copious white precipitate with chloride of barium, which did not wholly redissolve in hydrochloric acid, and gave a decided yellow precipitate with bichloride of platinum and alcohol.

The portion of the ashes insoluble in water dissolved completely in nitric acid with effervescence. The solution yielded,—with caustic potash, a white flocculent precipitate; with oxalate of ammonia, an abundant white precipitate; with caustic ammonia, a very slight precipitate, which became abundant on addition of phosphate of soda; and with ferrocyanide of potassium, a deep blue tint, the liquid remaining clear.

The ashes then consisted of carbonate of potash, traces of sulphate of potash, carbonate of lime, traces of carbonate of magnesia and of oxide of iron.

C. After complete exhaustion by alcohol and water, the insoluble residue *B* was treated, first with cold, and afterwards with boiling solution of caustic potash, so long as the liquid was coloured. The cold solution of potash became of so deep a red-brown colour as to be almost opake, even in a single drop.

The mixed liquids were precipitated by dilute hydrochloric acid in the slightest possible excess; the precipitate collected, washed and dried, weighed 5·5 grains. It was black, shining, and resembled perfectly in appearance the dried aqueous extract *B*.

D. After complete exhaustion by alcohol, water, and caustic potash, the residue was light fawn-coloured, approaching to white. When perfectly dried, it weighed 7·7 grains.

After complete incineration the loss was 2·7 grains, which must be considered as insoluble humus, the *terreau charbonneux* of Berzelius.

The ashes partially dissolved in concentrated hydrochloric acid, leaving only some grains of sand and insoluble silica. The liquid was thrown on a filter; the insoluble matter, collected, washed and dried, weighed exactly 1·0 grain; consequently the portion of the ash soluble in hydrochloric acid weighed 4·0 grains. The filtered liquid yielded a copious, white, flocculent precipitate, with solution of caustic potash; a copious, white, but not flocculent precipitate with oxalate of ammonia; a very slight turbidity with chloride of barium; a very slight flocculent precipitate with caustic ammonia; a blue tinge, but scarcely a perceptible precipitate, with ferrocyanide of potassium; and a greenish-blue tinge with ferridcyanide of potassium.

The ash consisted then chiefly of carbonate of lime, with traces of carbonate of magnesia; traces of sulphate of lime and of oxide of iron, silica and sand.

The substances submitted to examination are then,—

Resinous or waxy matter soluble in æther (*Aa*)	28·7
Friable, pulverulent, resinous matter, soluble in a mixture of alcohol and æther (*Ac*).	3·0
Extractive matter soluble in alcohol and water (*Ab*)	4·3
Red colouring matter soluble in water (*B*) . .	22·6
Humic acid (*C*)	13·8
Insoluble organic matter, the *terreau charbonneux* of Berzelius	6·8
Ashes	20·5
	99·7

The results of this analysis lead to the conjecture that the bottle was used for containing red wine; but as modern wines contain mere traces of resinous or waxy matter, it was necessary to refer to the customs of the ancients and their treatment of wines. The ancients, not contented as we are in the present day with wine in its natural condition, were accustomed to flavour their wines with a variety of substances. Dr. Smith* states that they were accustomed to throw into the vats or amphoræ various condiments or seasonings (medicamina, conditura), among the chief of which were sea-water, turpentine, pitch, tar or resin, gypsum, burned marble, calcined shells, aromatic herbs, spices and gums. Another mixture called mulsum was composed of wine or must mixed with honey, in the proportion of four of the former to one of the latter. Honey contains wax, and it is by no means impossible that the residue in the Egyptian bottle was that of some one of these vinous compounds. The large amount

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* Dictionary of Greek and Roman Antiquities.

of waxy or resinous matter evidences that it could not be pure wine, but either one of the compounds alluded to, or mulsum; most probably the former. As the Romans derived many of their customs from the Greeks, so in turn the Greeks in all probability borrowed from the Egyptians, with whom they held frequent intercourse. It has been suggested to me, that the matter contained in the bottle was a kind of varnish employed to fix the fresco-paintings adorning the Egyptian tombs, which might be a solution of some waxy or resinous substance in wine; but I am disinclined to this opinion, on account of the large quantity of red colouring matter extracted by water, as well as the small portion of residue contained in the bottle.

The red colouring matter was very analogous in its characters to that in port wine, but it had evidently undergone gradual and partial change into humic acid and insoluble humus. These together constitute above one-third of the whole organic matter. The large quantity of ashes is partly owing to the admixture of loose sand from the sides of the bottle, and to what must have penetrated from without in so sandy a country as Egypt.

XLVII. *On the Experiment of* Leidenfrost. *Extracted from a Letter from* Professor Buff *to* Professor Tyndall.

My dear Sir,

When I consider how well grounded your views are with regard to the origin of the phænomenon described by Trevelyn, I am surprised to find, with reference to another of the phænomena of heat, namely the experiment of Leidenfrost, that the theory of a repulsive action at a distance by heat seems to find favour in your eyes. At least I must conclude so much from a casual remark in a memoir upon the subject by Mr. A. Church. Mr. Church mentions an experiment, which he witnessed in your lectures, by which you show that water in the spheroidal condition interrupts the passage of an electric current to the red-hot metal underneath*, and observes that you regard this experiment as a proof that the surface of the vessel is separated by an interval from the drop within it.

The same experiment has been often made and observed under various conditions by myself, but I am not able to ascribe to it the same force of proof. I only find that the passage of electricity through water in the spheroidal state is rendered extremely difficult, and solely because the points of mutual contact between water and metal are reduced to a comparatively

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* M. Poggendorff was, I believe, the first to perform this experiment. —J. T.

small number. The current can thereby certainly to all appearance be interrupted; whether this occurs or not, depends, according to my experience, for a given liquid, on the delicacy of the galvanometer, on the strength of the electromotive force, and finally on the size of the drop itself. The spheroidal water behaves towards the hot surface as mercury towards a wire dipped into it, which is not wetted by the liquid metal. In a memoir published several years ago (*Annalen der Chemie und Pharmacie,* vol. lxxvii. p. 1), I have communicated different modes of experiment, which are, moreover, very easily devised, for the purpose of rendering visible the propagation of an electric current through water in the spheroidal condition. The most evident, however, as I learned afterwards, is the electric glow first observed by Mackrell on thin metallic wires, when they are used as electrodes of a very powerful electric circuit and immersed in hot water. In the case of the negative pole, it might be objected that during the continuance of the red heat it is surrounded by a brush (*sprühlicht*); this, however, is not necessarily the case with the positive wire, and when it is absent, the wire exhibits at a red heat the phænomenon of Leidenfrost perfectly. The current, and even the electrolytic decomposition, also continue; for when the positive electrode disperses no brush, the gas developed upon it is always pure oxygen. The continuance of the current in this and similar cases is perhaps not a certain proof of the existence of direct contact; for the layer of gas, or vapour between the water and the red-hot surface, may at this high temperature assume a small degree of conductibility. I am, however, only desirous of showing, that the extinction of the current in other cases cannot be regarded as a proof of the existence of an interval between the water and the surface by which it is sustained.

Indeed, the spheroidal condition of water exhibits in its most essential phænomena sufficient evidence against the idea that insulation and repulsion are necessary conditions of the spheroidal state. Permit me to lay before you briefly those proofs which I regard as the most important.

In a silver spoon coated with soot, water asserts the spheroidal form at ordinary temperatures, exactly as when the spoon is raised to a high temperature.

The drop which rolls upon the hot surface of a liquid, for example a drop of æther upon water, bends the surface of the liquid, as a mercury surface is bent by the pressure of a rod of glass.

Large drops assume an oscillatory motion, which often continue with remarkable regularity, and cause the formation of star-shaped figures: without a partial contact of the water with the surface beneath, the production of these figures would be an impossibility.

The greatest drops are not obtained upon such surfaces as radiate heat best, but on those which possess the highest capacity of conduction; surfaces of silver are therefore preferable to all others for these experiments.

The phænomenon is produced with volatile liquids only; the higher the boiling-point of the liquid, the more strongly heated must be the surface on which it rests, in order to hold it in the spheroidal state. The experiment therefore succeeds better with æther than with water when bad conductors are used as vessels, such, for example, as porcelain and glass. The lowest temperature at which the heated surface is not wetted by the liquid poured upon it, must always be higher than the boiling-point of the liquid. For example, in a silver basin at 75° C. æther, at 137° C. alcohol, and at 144° C. water first assume the spheroidal form.

In making these observations, I will not assert that Leidenfrost's drop cannot be separated from the heated surface which supports it. I believe, on the contrary, that even drops of considerable magnitude may be torn from the surface by the force of the developed vapour. I only affirm that such insulation is not the necessary condition for the production of the phænomenon. This is caused, in my opinion, by the alteration produced by heat in the relative attractions exerted by the surface of the vessel upon the liquid, and by the liquid particles upon each other.

The surface of a vessel will, it is known, be wet by a liquid poured into it when the attraction of the matter of the vessel for the liquid is greater than that of the particles of the liquid for each other. In the reverse case the liquid asserts the spheroidal form. The mutual attraction of the particles of the body is without doubt diminished at a high temperature. If a silver basin be heated with the water within it, the surface continues wet even when the liquid boils. In fact, in this case the temperature of the silver basin is only a little higher than that of the water. The mutual attraction of the silver and the particles of water has therefore been diminished almost in the same proportion as the attraction of the water particles for each other; the relation of the attractions remains constant. If the silver basin be heated beyond the temperature of boiling water, its adhesion for water must diminish in a greater degree than the cohesion of the particles of the liquid which still remains at its boiling-point. Hence sooner or later a temperature will be attained, at which the cohesion overpowers the adhesion. The wetting of the surface then ceases, and the spheroidal condition sets in, quite in harmony with the ordinary laws of capillarity.

Perhaps, my dear Sir, you will be inclined to agree with this manner of viewing the subject; if so, I entertain the hope that you will excuse this somewhat elaborate statement of the question. With great esteem, I have the pleasure to subscribe myself,

Yours most sincerely,
H. Buff.

Giessen, Sept. 29, 1855.

With reference to the subject of Prof. Buff's interesting letter, I may be permitted to say that I have never published anything on the experiment of Leidenfrost, though I have taken some pains to instruct myself as to the cause of this singular phænomenon. At an early period of railway travelling, Mr. George Stephenson conceived the idea of making his locomotives rest on steam springs. The steam within the boiler formed an elastic cushion for the support of the boiler. My belief is, that the spring suggested by the great engineer is precisely that which supports the drop in the experiment of Leidenfrost; the liquid, I believe, rests upon a cushion of its own vapour. Some of my reasons for agreeing with those who hold this belief are as follows:—1. The incessant motion of the drop necessitates a continuous expenditure of force, and this force is only attainable by the production of vapour underneath the drop, which, as is well known, speedily diminishes in bulk in consequence of the evaporation. 2. In a deep concave vessel, the vapour escapes laterally from the liquid as it encounters the least resistance in this direction; but if the liquid be suffered to spread itself over a comparatively flat surface, or better still, if the surface be rendered a little convex towards the centre of the drop, the vapour finds the resistance in a lateral direction greater than that in a vertical direction, and the steam actually breaks through the thin liquid layer above it and escapes at the centre. The existence of the vapour beneath the drop is thus easily reduced to demonstration. 3. If an interval really exists between the drop and the surface, it occurred to me that if this interval could be made manifest to the eye it would tend to settle the question. With this view I made the following experiment:—A silver basin was inverted, and the polished convex surface slightly dinted so that a drop might rest upon it. A fine platinum wire was stretched vertically behind the basin and united with the poles of a small voltaic battery; the wire was in this way rendered vividly luminous. A drop of deeply black ink was placed upon the surface of the basin; bringing the eye on a level with the bottom of the drop, and looking towards the platinum wire, the latter was seen through the space between the drop and the bottom of the basin*. In this way the under surface of a drop,

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* A little spirit mixed with the ink renders the experiment more easy.

half an inch or more in diameter, may be examined, and it will be found that the liquid is at no point in contact with the metal. While, therefore, I agree with M. Buff in rejecting the explanation which attributes the phænomenon to a propping-up of the drop by the calorific rays, I think the facts justify the opinion that an interval exists between the liquid and the metal. This interval, as proved by Poggendorff, is sufficient to interrupt an ordinary voltaic current; but being filled with vapour at a high temperature, it is quite conceivable, as surmised by M. Buff, that a small portion of a strong current may pass through it. M. Buff's observation, that the phænomenon is not exhibited except by those liquids which are capable of furnishing the necessary vapour, is also confirmatory of the above view: and the fact that good conductors, which quickly supply the heat necessary for evaporation, show the phænomenon better than good radiators which do not furnish this supply, appears to be equally corroborative of the opinion which I have ventured to espouse.

XLVIII. *On the Hypothesis of Molecular Vortices, or Centrifugal Theory of Elasticity, and its Connexion with the Theory of Heat.* *By* WILLIAM JOHN MACQUORN RANKINE, *C.E., F.R.SS.L. & E., F.R.S.S.A. &c.*[*]

SECTION FIRST.—*Relations between Heat and Expansive Pressure.*

(1.) IN February 1850, I laid before the Royal Society of Edinburgh a paper, in which the laws of the pressure and expansion of gases and vapours were deduced from the supposition, that that part of the elasticity of bodies which depends upon heat, arises from the centrifugal force of the revolutions of the particles of elastic atmospheres surrounding nuclei or atomic centres. A summary of the results of this supposition, which I called the Hypothesis of Molecular Vortices, was printed in the Transactions of this Society, vol. xx., as an introduction to a series of papers on the Mechanical Action of Heat; and the original paper has since appeared in detail in the Philosophical Magazine for December 1851.

In that paper, *the bounding surfaces of atoms* were defined to

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[*] Communicated by the Author, having been read to the Royal Society of Edinburgh, December 15, 1851.

[The following paper is republished by permission of the Council of the Royal Society of Edinburgh. It has received no alterations except some verbal corrections, some additions to the forms of notation, and the omission of some unnecessary repetitions. A few notes have been added, which are distinguished by dates.]

be imaginary surfaces, situated between and enveloping the atomic nuclei, and symmetrically placed with respect to them, and having this property—that at these surfaces the attractive and repulsive actions of the atomic nuclei and atmospheres upon each particle of atomic atmosphere balance each other. The pressure of the atomic atmospheres at those imaginary boundaries is the part of the total expansive pressure of the body which varies with heat; the effect of the centrifugal force of molecular vortices being to increase it.

In the subsequent investigation it was assumed, that, owing to the symmetrical action of the particles of gases in all directions, and the small amount of those attractive and repulsive forces which interfere with the elasticity of their atmospheres, no appreciable error would arise from treating the boundary of the atmosphere of a single atom, in calculation, as if it were spherical; an assumption which very much simplified the analysis.

An effect, however, of this assumption was, to make it doubtful whether the conclusions deduced from the hypothesis were applicable to any substances except those nearly in the state of perfect gas. I have, therefore, in the present paper investigated the subject anew, without making any assumption as to the arrangement of the atomic centres, or the form of the boundaries of their atmospheres. The equations deduced from the hypothesis, between expansive pressure and heat, are therefore applicable to all substances in all conditions; and it will be seen that they are identical with those in the original paper; showing that the assumption, that the atomic atmospheres might be treated in calculation as if spherical, did not give rise to any error.

By the aid of certain transformations in those equations, I have been enabled, in investigating the principles of the mutual transformation of heat and expansive power, to deduce Joule's *law* of the equivalence of heat and mechanical power directly from them, instead of taking it (as I did in my previous papers) as a consequence of the principle of *vis viva*. Carnot's *law* is also deduced directly from the hypothesis, as in one of the previous papers.

(2.) *Classification of Elastic Pressures.*—The pressures considered in the present paper are those only which depend on the volume occupied by a given weight of the substance; not those which resist change of figure in solids and viscous liquids. Certain mathematical relations exist between those two classes of pressures, but they do not affect the present investigation.

To illustrate this symbolically, let V represent the volume occupied by unity of weight of the substance, so that $\frac{1}{V}$ is the mean density; Q the quantity of heat in unity of weight, that is to say, the *vis viva* of the molecular revolutions, which, according to the hypothesis, give rise to the expansive pressure depending on heat; and let P denote the total expansive pressure. Then

$$\mathrm{P} = \mathrm{F}(\mathrm{V}, \mathrm{Q}) + f(\mathrm{V}). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

In this equation, F(V, Q) is the pressure of the atomic atmospheres at the surfaces called their boundaries, which varies with the centrifugal force of the molecular vortices as well as with the mean density; and $f(\mathrm{V})$ is a portion of pressure due to the mutual attractions and repulsions of distinct atoms, and varying with the number of atoms in a given volume only. If the above equation be differentiated with respect to the hyperbolic logarithm of the density, we obtain the coefficient of elasticity of volume

$$\frac{1}{\mathfrak{v}} = -\frac{d\mathrm{P}}{\frac{d\mathrm{V}}{\mathrm{V}}} = -\frac{d}{\frac{d\mathrm{V}}{\mathrm{V}}}\mathrm{F}(\mathrm{V}, \mathrm{Q}) - \frac{d}{\frac{d\mathrm{V}}{\mathrm{V}}}f(\mathrm{V}), \quad . \quad . \quad (1\mathrm{A})$$

where $\mathfrak{v}$ denotes the cubic compressibility.

The latter portion of this coefficient, $-\frac{d}{\frac{d\mathrm{V}}{\mathrm{V}}}f(\mathrm{V})$, consists of two parts, one of which gives rise to rigidity, or elasticity of figure, as well as to elasticity of volume, while the other gives rise to elasticity of volume only. The ratio of each of those parts to their sum must be a function of the heat, the former part being greater, and the latter less, as the atomic atmosphere is more concentrated round the nucleus; that is to say, as the heat is less; but their sum, so far as elasticity of volume is concerned, is a function of the density only.

That is to say, let the total coefficient of elasticity of volume be denoted thus,

$$\frac{1}{\mathfrak{v}} = \mathrm{J} + \phi(\mathrm{C}_1, \mathrm{C}_2, \mathrm{C}_3), \quad . \quad . \quad . \quad . \quad . \quad (1\mathrm{B})$$

C_1, C_2, C_3 being coefficients of rigidity round the three axes of elasticity, and J a coefficient of fluid elasticity; then

$$\left.\begin{aligned} \mathrm{J} &= -\frac{d}{\frac{d\mathrm{V}}{\mathrm{V}}}\mathrm{F}(\mathrm{V}, \mathrm{Q}) - \psi(\mathrm{V}, \mathrm{Q}) \cdot \frac{d}{\frac{d\mathrm{V}}{\mathrm{V}}}f(\mathrm{V}) \\ \phi(\mathrm{C}_1, \mathrm{C}_2, \mathrm{C}_3) &= -\Big(1 - \psi(\mathrm{V}, \mathrm{Q})\Big) \cdot \frac{d}{\frac{d\mathrm{V}}{\mathrm{V}}}f(\mathrm{V}) \end{aligned}\right\} \quad . \quad . \quad (1\mathrm{C})$$

For the present, we have to take into consideration that portion only of the expansive pressure which depends on density and heat jointly, and is the means of mutually converting heat and expansive power; that is to say, the pressure at the boundaries of the atomic atmospheres, which I shall denote by

$$p=\mathrm{F}(\mathrm{V},\mathrm{Q}).$$

Pressures, throughout this paper, are supposed to be measured by units of weight upon unity of area; densities, by the weight of unity of volume*.

(3.) *Determination of the External Pressure of an Atomic Atmosphere.*—Let a body be composed of equal and similar atomic nuclei, arranged in any symmetrical manner, and enveloped by an atmosphere, the parts of which are subject to attractive and repulsive forces exercised by each other and by the nuclei. Let it further be supposed, that this atmosphere at each point has an elastic pressure proportional to the density at that point, multiplied by a specific coefficient depending on the nature of the substance, which I shall denote by h. (This coefficient was denoted by b in previous papers.)

Let ρ and p' denote the density and pressure of the atomic atmosphere at any point; then

$$p'=h\rho.$$

Let

$$-g\frac{d\Phi}{dx},\quad -g\frac{d\Phi}{dy},\quad -g\frac{d\Phi}{dz}$$

be the accelerative forces operating on a particle of atomic atmosphere, in virtue of the molecular attractions and repulsions, which I have made explicitly negative, attractions being supposed to predominate. The property of the surfaces called the boundaries of the atoms is this,—

$$\left(\frac{d\Phi}{dx}\right)_1=0,\quad \left(\frac{d\Phi}{dy}\right)_1=0,\quad \left(\frac{d\Phi}{dz}\right)_1=0,$$

the suffix $_1$ being used to distinguish the value of quantities at those surfaces. Hence Φ_1 is a maximum or minimum. Those surfaces are symmetrical in form round each nucleus, and equidistant between pairs of adjacent nuclei. Their equation is

$$\Phi-\Phi_1=0.$$

Let M denote the total weight of an atom, μ that of its atmospheric part, and $\mathrm{M}-\mu$ that of its nucleus; then

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* Sept. 1855.—There is reason to believe that in many substances the elasticity of figure depends on more than three independent coefficients; but as the present paper relates to elasticity of volume only, the above equations are nevertheless sufficient to illustrate the classification of elastic pressures.

MV is the volume of the atom,

$\frac{\mu}{\mathrm{MV}}$ the mean density of the atmospheric part, measured by weight, the nucleus being supposed to be of insensible magnitude,

and we have the following equations:—

$$\left.\begin{aligned} \mathrm{MV} &= \iiint_{(1)} dx\,dy\,dz \\ \mu &= \frac{\mu}{\mathrm{MV}}\iiint_{(1)} dx\,dy\,dz = \iiint_{(1)} \rho\,dx\,dy\,dz \end{aligned}\right\} \quad . \quad . \quad (2)$$

The suffix $(_1)$ denoting that the integration is to be extended to all points within the surface

$$(\Phi - \Phi_1 = 0).$$

According to the hypothesis now under consideration, *heat* consists in a revolving motion of the particles of the atomic atmosphere communicated to them by the nuclei. Let v be the common mean velocity possessed by the nucleus of an atom and the atmospheric particles, when the distribution of this motion has been equalized. I use the term *mean* velocity to denote that the velocity of each particle may undergo small periodic changes, which it is unnecessary to consider in this investigation.

Then the quantity of heat in unity of weight is

$$\mathrm{Q} = \frac{v^2}{2g},$$

being equal to the mechanical power of unity of weight falling through the height $\frac{v^2}{2g}$. The quantity of heat in one atom is of course MQ, and in the atmospheric part of an atom μQ.

I shall leave the form of the paths described by the atmospheric particles indeterminate, except that they must be closed curves of permanent figure, and included within the surface $(\Phi - \Phi_1 = 0)$. Let the nucleus be taken as the origin of coordinates, and let α, β, γ be the direction-cosines of the motion of the particles at any point (x, y, z). Then the equations of a permanent condition of motion at that point are

$$\left.\begin{aligned} -\frac{1}{\rho}\cdot\frac{dp'}{dx} - \frac{d\Phi}{dx} - 2\mathrm{Q}\left(\alpha\frac{d}{dx} + \beta\frac{d}{dy} + \gamma\frac{d}{dz}\right)\alpha = 0 \\ -\frac{1}{\rho}\cdot\frac{dp'}{dy} - \frac{d\Phi}{dy} - 2\mathrm{Q}\left(\alpha\frac{d}{dx} + \beta\frac{d}{dy} + \gamma\frac{d}{dz}\right)\beta = 0 \\ -\frac{1}{\rho}\cdot\frac{dp'}{dz} - \frac{d\Phi}{dz} - 2\mathrm{Q}\left(\alpha\frac{d}{dx} + \beta\frac{d}{dy} + \gamma\frac{d}{dz}\right)\gamma = 0 \end{aligned}\right\}. \quad (3)$$

Let r be the length, and α', β', γ' the direction-cosines of the radius of curvature of the path of the particles through (x, y, z); then the above equations obviously become

$$\left.\begin{aligned}-\frac{1}{\rho}\frac{dp'}{dx}-\frac{d\Phi}{dx}-2Q\frac{\alpha'}{r}&=0\\ -\frac{1}{\rho}\frac{dp'}{dy}-\frac{d\Phi}{dy}-2Q\frac{\beta'}{r}&=0\\ -\frac{1}{\rho}\frac{dp'}{dz}-\frac{d\Phi}{dz}-2Q\frac{\gamma'}{r}&=0\end{aligned}\right\} \quad \text{(3 A)}$$

If these equations are integrable,

$$\frac{\alpha'}{r}dx+\frac{\beta'}{r}dy+\frac{\gamma'}{r}dz$$

must be an exact differential. Let $-\phi$ be its primitive function, the negative sign being used, because α', β', γ' must be generally negative. Then the integral of the equations (3) is

$$\log_e\rho=\frac{1}{h}\int\frac{dp'}{\rho}=\frac{1}{h}(2Q\phi-\Phi)+\text{ constant};$$

or taking ρ_1 to denote the pressure at the bounding surface of the atom,

$$\rho=\rho_1 e^{\frac{2Q}{h}(\phi-\phi_1)-\frac{1}{h}(\Phi-\Phi_1)} \quad \text{(4)}$$

Our present object is to determine the superficial-atomic density ρ_1, and thence the pressure $p=h\rho_1$, in terms of the mean density $\frac{1}{V}$ and heat Q. For this purpose we must introduce the above value of ρ into equation (2), giving

$$\mu=\rho_1\cdot\iiint_{(1)} e^{\frac{2Q}{h}(\phi-\phi_1)-\frac{1}{h}(\Phi-\Phi_1)}dx\,dy\,dz,$$

whence

$$p=h\rho_1=h\mu\div\iiint_{(1)} e^{\frac{2Q}{h}(\phi-\phi_1)-\frac{1}{h}(\Phi-\Phi_1)}dx\,dy\,dz. \quad (5)$$

Let the volume of the atom be conceived to be divided into layers, in each of which ϕ has a constant value. Then we may make the following transformations :—

$$\left.\begin{aligned}\iiint dx\,dy\,dz&=k\text{MV}\int e^{k(\phi-\phi_1)}\frac{\psi}{\psi_1}d\phi\\ \iiint e^{-\frac{1}{h}(\Phi-\Phi_1)}dx\,dy\,dz&=k\text{MV}\int e^{k(\phi-\phi_1)}\frac{\omega}{\omega^1}d\phi\end{aligned}\right\} \quad (6)$$

k being a specific constant, and ψ and ω functions of ϕ, and of the nature and density of the substance.

The lower limit of integration of ϕ must be made $-\infty$, that it may include orbits of indefinitely small magnitude described round the atomic centre.

The nature of the function ψ is limited by the following condition:—

$$1=k\int_{-\infty}^{\phi_1} e^{k(\phi-\phi_1)}\frac{\psi}{\psi_1}\,d\phi. \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

Let

$$\frac{2Q}{hk}+1=\theta.$$

Then these transformations give the following result for the pressure at the bounding surface of an atom:—

$$\left.\begin{aligned} p=h\rho_1&=\frac{h\mu}{MV}\div\int_{-\infty}^{k\phi_1} e^{\theta k(\phi-\phi_1)}\frac{\omega}{\omega_1}k d\phi \\ &=\frac{h\mu}{MV}\cdot\frac{\theta\omega_1}{\omega_1-\frac{\omega'_1}{\theta}+\frac{\omega''_1}{\theta^2}-\frac{\omega'''_1}{\theta^3}+\&c.} \end{aligned}\right\}, \quad . \quad . \quad (8)$$

ω'_1, &c. being the successive differential coefficients of ω with respect to $k\phi$, when $\phi=\phi_1$.

(4.) The following transformation will be found useful in the sequel.

Let λ be the indefinite value of $\log_e V$, and λ_1 its actual value in the case under consideration. Let G be the same function of λ which ω is of $k\phi$, and let G′, G″, &c. be its successive differential coefficients with respect to λ.

Let

$$H_1=\int_{-\infty}^{\lambda_1} e^{\theta(\lambda-\lambda_1)}G d\lambda=\frac{G_1}{\theta}-\frac{G'_1}{\theta^2}+\frac{G''_1}{\theta^3}-\&c.$$

Then

$$p=\frac{h\mu G_1}{MVH_1}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

The function H has the following properties, which will be afterwards referred to:—

$$\left.\begin{aligned} &\frac{dH_1}{d\lambda_1}+\theta H_1-G_1=0 \\ &\int_{-\infty}^{\lambda_1} H d\lambda=-\frac{dH_1}{d\theta} \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

(5.) *Case of a Perfect Gas.*—As a substance is rarefied, it gradually approaches a condition in which the pressure, under like circumstances as to heat, varies proportionally to the density. This is because the effect of the molecular attractions and repulsions on the pressure diminishes with the density, so that

Φ, ω, and G approximate to constant quantities. In the limiting or perfectly gaseous condition, therefore,

$$\mathrm{H}_1=\frac{\mathrm{G}_1}{\theta},$$

and

$$p=\frac{h\mu\theta}{\mathrm{MV}}=\frac{h\mu}{\mathrm{MV}}\left(\frac{2\mathrm{Q}}{hk}+1\right). \quad . \quad . \quad . \quad . \quad . \qquad (11)$$

(6.) *Equilibrium of Heat: Nature of Temperature and Real Specific Heat.*—When the atmospheres of atoms of two different substances are in contact at their common bounding surface, it is necessary to a permanent condition that the pressure in passing that surface should vary continuously.

Let (a) and (b) be taken as characteristics, to distinguish the specific quantities peculiar to the two media respectively. Let dm denote the volume of an indefinitely thin layer close to the bounding surface. Then the following equations must be fulfilled to ensure a permanent condition:—

$$p(a)=p(b)\,;\ \frac{dp'}{dm}(a)=\frac{dp'}{dm}(b) \text{ when } p'=p. \quad . \qquad (12)$$

By making the proper substitutions in equation (4), it appears that

$$p'=pe^{k\vartheta(\phi-\phi_1)}\frac{\psi_1}{\omega_1}\cdot\frac{\omega}{\psi}.$$

Hence

$$\frac{dp'}{dm}(p'=p)=p\left(\theta\frac{d(k\phi)}{dm}+\frac{d\cdot\frac{\omega}{\psi}}{dm}\right).$$

Now p is the same for both media: $\frac{\psi_1}{\omega_1}\cdot\frac{\omega}{\psi}=e^{-\frac{1}{h}(\Phi-\Phi_1)}$ is either a maximum or a minimum, so that its differential is null; and dm is a continuous function of $k\phi$, so that $\frac{d(k\phi)}{dm}(a)=\frac{d(k\phi)}{dm}(b)$. There remains only the function of heat,

$$\theta=\frac{2\mathrm{Q}}{hk}+1.$$

Therefore the condition of a permanent state of molecular motion, that is to say, the condition of *equilibrium of heat*, is that this function shall be the same for the two substances; or that

$$\frac{2\mathrm{Q}_a}{h_a k_a}=\frac{2\mathrm{Q}_b}{h_b k_b}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (13)$$

Hence *temperature* depends on the above function only; for the definition of temperature is, that bodies at the same temperature are in a permanent condition as to heat, so far as their mutual action is concerned.

The ratio of the *real specific heat* of (*a*) to that of (*b*) is obviously

$$h_a k_a : h_b k_b. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (14)$$

(7.) *Measure of Temperature and Specific Heat.*—The function θ is proportional to the pressure of a perfect gas at a constant density. That pressure, therefore, is the most convenient *measure* of temperature.

Let τ denote *absolute* temperature, as measured by the pressure of a perfect gas at constant density, and reckoned from a certain *absolute zero*, 274°·6 Centigrade, or 494°·28 Fahrenheit below the temperature of melting ice. Let κ be a constant which depends on the length of a degree on the thermometric scale, and is the same for all substances in nature.

Then

$$\left.\begin{array}{l} \tau = \kappa\theta = \dfrac{2\kappa Q}{hk} + \kappa \\ Q = (\tau - \kappa)\,\dfrac{hk}{2\kappa} \end{array}\right\}; \quad . \quad . \quad . \quad . \quad . \quad . \qquad (15)$$

and the real specific heat of the substance, that is to say, the depth of fall, under the influence of gravity, which is equivalent to a rise of one degree of temperature in the body, is represented by

$$\mathfrak{k} = \frac{hk}{2\kappa}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (16)$$

The pressure of a perfect gas is represented in terms of temperature by

$$p = \frac{h\mu\tau}{MV\kappa}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (17)$$

It may also be expressed thus: let τ_0 denote the absolute temperature of melting ice in degrees of the scale employed, and V_0 the volume of unity of weight of the substance in the theoretical state of perfect gas, at the temperature of melting ice and pressure unity; then

$$p = \frac{V_0}{V}\frac{\tau}{\tau_0}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (18)$$

On comparing this with equation (17), we see that

$$\left.\begin{array}{l} \dfrac{h\mu}{\kappa M} = \dfrac{V_0}{\tau_0}; \\ \kappa = \dfrac{h\mu\tau_0}{MV_0}; \ \dfrac{h\mu}{MV_0} = \dfrac{\kappa}{\tau_0}. \end{array}\right\}. \quad . \quad . \quad . \qquad (19)$$

Now h is the specific elasticity of the atomic atmosphere of the substance, $\frac{\mu}{\mathrm{MV}_0}$ is the mean specific gravity of that atmosphere when the body is in the theoretical state of perfect gas, and κ and τ_0 are the same for all substances in nature. Therefore, *for every substance in nature, the mean specific gravity of the atomic atmosphere in the theoretical state of perfect gas is inversely proportional to the specific elasticity of that atmosphere.*

Real specific heat may also be thus expressed:—

$$\text{ꝁ}=\frac{\mathrm{V}_0}{\tau_0}\cdot\frac{k\mathrm{M}}{2\mu}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (20)$$

in which $\frac{\mathrm{V}_0}{\tau_0}$ corresponds to $\frac{1}{\mathrm{C}n\mathrm{M}}$ in my former papers, and $\frac{k\mathrm{M}}{2\mu}$ to $\frac{3k\mathrm{M}}{2\mu}$ or $\frac{1}{\mathrm{N}}$; so that

$$\frac{h\mu}{\kappa\mathrm{M}}=\frac{\mathrm{V}_0}{\tau_0}=\mathrm{N}\text{ꝁ}. \quad . \quad . \quad . \quad . \quad . \quad (20\,\mathrm{A})$$

The factor N appears to depend on the chemical constitution of the substance*.

(8.) *Total Pressure of Substances in general, expressed in terms of temperature.*

In equation (9) let $\frac{\tau}{\kappa}$ be put for θ; then

$$\mathrm{P}=p+f(\mathrm{V})=f(\mathrm{V})+\frac{h\mu\tau}{\kappa\mathrm{MV}}\mathrm{G}_1\div\left\{\mathrm{G}_1-\frac{\kappa\mathrm{G}_1'}{\tau}+\frac{\kappa^2\mathrm{G}''_1}{\tau^2}-\&\mathrm{c}.\right\}$$

$$=f(\mathrm{V})+\frac{\mathrm{V}_0}{\tau_0}\frac{\tau}{\mathrm{V}}\left\{1-\frac{\mathrm{A}_1}{\tau}-\frac{\mathrm{A}_2}{\tau^2}-\frac{\mathrm{A}_3}{\tau^3}-\&\mathrm{c}.\right\}, \quad . \quad . \quad (21)$$

where

$$\mathrm{A}_1=\frac{-\kappa\mathrm{G}'}{\mathrm{G}_1};\quad \mathrm{A}_2=-\frac{\kappa^2}{\mathrm{G}_1^2}(\mathrm{G}_1'^2-\mathrm{G}_1''),$$

$$\mathrm{A}_3=-\frac{\kappa^3}{\mathrm{G}_1^3}(\mathrm{G}_1'^3-2\mathrm{G}_1'\mathrm{G}_1''+\mathrm{G}_1''');\ \&\mathrm{c}.$$

This formula is identical with that which I employed in my former paper, to represent the pressure of an imperfect gas, and which I found to agree with M. Regnault's experiments, when the coefficients A and the function $f(\mathrm{V})$ had been calculated empirically†.

[To be continued.]

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* The values of N for atmospheric air, oxygen, hydrogen, and carbonic oxide, differ from 0·41 by quantities which are within the limits of errors of observation. (1853.) See also Phil. Mag. June 1853, and Trans. Roy. Soc. Edinb. vol. xx. p. 589.

† For further applications of equation (21), see the paper of Messrs. Joule and Thomson in the Phil. Trans. for 1854. (Sept. 1855.)

XLIX. *On the Preparation of Aluminium from Kryolite.* *By* ALLAN DICK, *Esq.*[*]

IN the last Number of this Journal there was a translation of a paper of H. Rose of Berlin, describing a method of preparing aluminium from kryolite. Previously, at the suggestion of Dr. Percy, I had made some experiments on the same subject in the metallurgical laboratory of the School of Mines; and as the results obtained agree closely with those of Rose, it may be interesting to give a short account of them now, though no detailed description was published at the time, a small piece of metal prepared from kryolite having simply been shown at the weekly meeting of the Royal Institution (March 30, 1855), accompanied with a few words of explanation by Faraday.

Shortly after the publication of M. Deville's process for preparing the metal from chloride of aluminium, along with Mr. Smith I tried to make a specimen of the metal; but we found it a much more difficult thing to do than Deville's paper had led us to anticipate, and had to remain contented with a much smaller piece than we had hoped to obtain. It is, however, undoubtedly only a matter of time, skill and expense, to join successful practice to the details of M. Deville.

Whilst making these experiments, Dr. Percy had often requested us to try whether kryolite could not be used instead of the chlorides, but some time elapsed before we obtained a specimen of the mineral.

The first experiments were made in glass tubes sealed at one end, into which alternate layers of finely-pounded kryolite and sodium cut into small pieces were introduced, and covered in some instances by a layer of kryolite, in others by one of chloride of sodium. The tube was then heated over an air-gas blowpipe for a few minutes, till decomposition had taken place and the product was melted. When cold, on breaking the tube it was found that the mass was full of small globules of aluminium; but owing to the specific gravity of the metal and flux being nearly alike, the globules had not collected into one button at the bottom: to effect this, long-continued heat would be required, which cannot be given in glass tubes, owing to the powerful action of the melted fluoride upon them. To obviate this difficulty, a platinum crucible was lined with magnesia, by ramming the same hard in, and subsequently cutting out all but a lining. Into this, alternate layers of kryolite and sodium were placed, with a thickish layer of kryolite on the top. The crucible was covered with a tight-fitting lid, and heated to redness for about half an hour over the air-blowpipe. When cold, it was placed in

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[*] Communicated by the Author.

water, and after soaking for some time, the contents dug out, gently crushed in a mortar, and washed by decantation. Two or three globules of aluminium, tolerably large considering the scale of the experiment, were obtained along with a great number of very small ones. The larger ones were melted together under chloride of potassium.

Some experiments made in iron crucibles were not attended with the same results as those of Rose. No globules of any considerable size remained in the melted fluorides; the metal seemed to alloy with the sides of the crucibles, which acquired a colour like zinc. It is possible that this difference may have arisen from using a higher temperature than Rose, as these experiments were made in the furnace, not over the air-blowpipe.

Porcelain and clay crucibles were also tried, but laid aside after a few experiments owing to the action of the fluorides upon them, which in most cases was sufficient to perforate them completely.

These remarks, it is hoped, may tend to draw the attention of experimenters still further to kryolite as a source of this interesting metal; although the experiments which led to them were carried only a short way towards the perfection which practice would probably confer.

Metallurgical Laboratory, School of Mines,
October 26, 1855.

L. *On the Potash and Soda Felspars of the Dublin Mountains.*
By Francis M. Jennings, *M.R.I.A., F.G.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

The paper of the Rev. Prof. Galbraith in the August Number of your Journal, relative to the composition of the felspars of the Dublin Mountains, shows that he entirely mistakes the object of my remarks, which were to this effect: that when an inference as to the composition of the felspar of a granite mountain is to be made, the specimen analysed should partake as closely as possible of the general lithological character of the mountain. Now the specimen selected by Mr. W. Wilson of the Geological Survey, and carefully analysed by Mr. Dugald Campbell, is very similar in its composition to those examined by Prof. England and Mr. Foster (see Proc. Royal Irish Acad. Feb. 28, 1853), whilst the analyses of the Rev. Prof. Galbraith are very different; he, therefore, is not in a position to charge others with want of care in the analyses, nor is he correct in saying, that to estimate the quantities of potash or soda in felspar it is necessary to determine the quantities of silica, &c.

	Prof. Galbraith (Phil. Mag. Jan. 1855), his own selection.	Prof. Galbraith, off same specimen as Mr. Campbell's.
KO . . .	10·68	4·65
NaO . . .	3·26	3·39

	Mr. Campbell, same specimen as Prof. Galbraith's.	Mr. Foster.	Prof. England.
KO . . .	3·02	4·15	5·72
NaO . . .	3·98	4·34	6·89

The difference in Prof. Galbraith's and Mr. Campbell's analyses is not owing, I presume, to any difference in the constitution of the felspars, but *most probably* to the Professor's not having taken out all the mica.

These analyses leave the question of the quantity of potash and soda in the felspar of the Three Rock Mountain thus: three analyses by three different men give the soda in excess; two by Prof. Galbraith give the potash in excess, that one his own selection giving the smallest quantity of soda. With this evidence before us, is it not rather premature to argue that this mountain contains in its felspar a greater quantity of potash than soda? And it shows the danger of making inferences as to the composition of minerals constituting a large portion of a mountain from one analysis, as done by Prof. Galbraith. How many it requires to be able to judge I shall not say, but I think those given, unless all selected specially as being like in general character, cannot have much bearing on the matter, and prove how much more is to be done before drawing conclusions from one analysis, and that one different from all the others. Further discussion, I think, must be useless, until Prof. Galbraith exhibits portions of the specimens which he has analysed; and my calling for their public exhibition is, I doubt not, the reason why my entering into this discussion is looked on by Prof. Galbraith as an "interference."

I am, Gentlemen,
Yours,
Francis M. Jennings.

LI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 309.]

April 19, 1855.—The Lord Wrottesley, President, in the Chair.

The following communication was read:—

"Reply of the President and Council of the Royal Society to an application from the Lords of the Committee of Privy Council for Trade, on the subject of Marine Meteorological Observations."

[This Letter was communicated to the Society in pursuance of a resolution of the Council. The Secretary explained that it had been drawn up by the Treasurer, Colonel Sabine, and submitted, before final adoption by the Council, to several Fellows of the Society specially conversant with the subjects to which it refers.]

Royal Society, Somerset House,
February 22, 1855.

Sir,—In the month of June last, the Lords of the Committee of the Privy Council for Trade caused a letter to be addressed to the President and Council of the Royal Society, acquainting them that their Lordships were about to submit to Parliament an estimate for an Office for the Discussion of the Observations on Meteorology, to be made at sea in all parts of the globe, in conformity with the recommendation of a conference held at Brussels in 1853; and that they were about to construct a set of forms for the use of that Office, in which they proposed to publish from time to time and to circulate such statistical results obtained by means of the observations referred to, as might be considered most desirable by men learned in the science of Meteorology, in addition to such other information as might be required for the purposes of Navigation.

Before doing so, however, their Lordships were desirous of having the opinion of the Royal Society, as to what were the great desiderata in Meteorological science; and as to the forms which may be best calculated to exhibit the great atmospheric laws which it may be most desirable to develope.

Their Lordships further state, that as it may possibly happen that observations on land upon an extended scale may hereafter be made and discussed in the same Office, it is desirable that the reply of the Royal Society should keep in view, and provide for such a contingency.

Deeply impressed with a sense of the magnitude and importance of the work which has been thus undertaken by Her Majesty's Government and confided to the Board of Trade, and fully appreciating the honour of being consulted, and the responsibility of the reply which they are called upon to make;—considering also that by including the contingency of *land* observations, the inquiry is, in fact, co-extensive with the requirements of Meteorology over all accessible parts of the earth's surface,—the President and Council of the Royal Society deemed it advisable, before making their reply, to obtain the opinion of those amongst their foreign members who are known as distinguished cultivators of Meteorological science, as well as of others in foreign countries, who either hold offices connected with the advancement of Meteorology, or have otherwise devoted themselves to this branch of science.

A circular was accordingly addressed to several gentlemen whose names were transmitted to the Board of Trade in June last, containing a copy of the communication from the Board of Trade, and a request to be favoured with any suggestions which might aid Her Majesty's Government in an undertaking which was obviously one of general concernment.

Replies in some degree of detail have been received from five of these gentlemen[*], copies of which are herewith transmitted.

The President and Council are glad to avail themselves of this opportunity of expressing their acknowledgements to these gentlemen, and more particularly to Professor Dove, Director of the Meteorological Establishments and Institutions in Prussia, whose zeal for the advancement of Meteorology induced him to repair personally to England, and to join himself to the Committee by whom the present reply has been prepared. Those who are most familiar with the labours and writings of this eminent meteorologist will best be able to appreciate the value of his co-operation.

The President and Council have considered it as the most convenient course to divide their reply under the different heads into which the subject naturally branches. But before they proceed to treat of these, they wish to remark generally, that one of the chief impediments to the advancement of Meteorology consists in the very slow progress which is made in the transmission from one country to another of the observations and discussions on which, under the fostering aid of different Governments, so much labour is bestowed in Europe and America; and they would therefore recommend that such steps as may appear desirable should be taken by Her Majesty's Government, to promote and facilitate the mutual interchange of Meteorological publications emanating from the Governments of different countries.

Barometer.

It is known that considerable differences, apparently of a permanent character, are found to exist in the mean barometric pressure in different places; and that the periodical variations in the pressure in different months and seasons at the same place, are very different in different parts of the globe, both as respects period and amount; insomuch that in extreme cases, the variations have even opposite features in regard to period, in places situated in the same hemisphere and at equal distances from the equator.

For the purpose of extending our knowledge of the facts of these departures from the state of equilibrium, and of more fully investigating the causes thereof, it is desirable to obtain, by means of barometric observations strictly comparable with each other, and extending over all parts of the globe accessible by land or sea, *tables*, showing the mean barometric pressure *in the year*, *in each month of the year*, and *in the four meteorological seasons*,—on land, at all stations of observation,—and at sea, corresponding to the middle points of spaces bounded by geographical latitudes and longitudes, not far distant from each other.

The manner of forming such tables from the marine observations which are now proposed to be made, by collecting together observations of the same month in separate ledgers, each of which should correspond to a *geographical space* comprised between specified

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[*] Dr. Erman of Berlin; Dr. Heis of Münster; Prof. Kreil of Vienna; Lieut. Maury of Washington; and M. Quetelet of Brussels.

meridians and parallels, and to a *particular month*, is too obvious to require to be further dwelt upon. The distances apart of the meridians and parallels will require to be varied in different parts of the globe, so that the magnitudes of the spaces which they enclose, and for each of which a table will be formed, may be more circumscribed, when the rapidity of the variation of the particular phenomenon to be elucidated is greatest in regard to geographical space. Their magnitude will also necessarily vary with the number of observations which it may be possible to collect in each space, inasmuch as it is well known that there are extensive portions of the ocean which are scarcely ever traversed by ships, whilst other portions may be viewed as the highways of a constant traffic.

The strict comparability of observations made in different ships may perhaps be best assured, by limiting the examination of the instruments to comparisons which it is proposed to make at the Kew Observatory, before and after their employment in particular ships. From the nature of their construction, the barometers with which Her Majesty's navy and the mercantile marine are to be supplied are not very liable to derangement, except from such accidents as would destroy them altogether. Under present arrangements they will all be carefully compared at Kew before they are sent to the Admiralty or to the Board of Trade; and similar arrangements may easily be made by which they may be returned to Kew for re-examination at the expiration of each tour of service. The comparison of barometers when embarked and in use, with standards, or supposed standards, at ports which the vessels may visit, entails many inconveniences, and is in many respects a far less satisfactory method. The limitation here recommended is not, however, to be understood as applicable in the case of other establishments than Kew, where a special provision may be made for an equally careful and correct examination.

At land stations, in addition to proper measures to assure the correctness of the barometer and consequent comparability of the observations, care should be taken to ascertain by the best possible means (independently of the barometer itself), the height of the station above the level of the sea at some stated locality. For this purpose the extension of levels for the construction of railroads will often afford facilities.

It may be desirable to indicate some of the localities where the data, which tables such as those which have been spoken of would exhibit, are required for the solution of problems of immediate interest.

1°. It is known, that, over the Atlantic Ocean, a low mean annual pressure exists near the equator, and a high pressure at the north and south borders of the torrid zone (23° to 30° north and south latitudes); and it is probable that from similar causes similar phenomena exist over the corresponding latitudes in the Pacific Ocean: the few observations which we possess are in accord with this supposition; but the extent of space covered by the Pacific is large and the observations are few; they may be expected to be greatly increased by the means now contemplated. But it is particularly over the Indian Ocean, both at the equator and at the borders of the torrid zone, that the phenomena of the barometric pressure, not only annual but also monthly, require elucidation by observations. The Trade-winds, which would prevail generally round the globe if it were wholly covered by a surface of water, are interrupted by the large continental spaces in Asia and Australia, and give place to the phenomena of monsoons, which are the indirect results of the heating action of the sun's rays on those continental spaces. These are the causes of that displacement of the trade-winds, and substitution of a current flowing in another direction, which occasion the atmospheric phenomena over the Indian Ocean, and on the north and south sides of that Ocean, to be different from those in corresponding localities over, and on either side of the equator in the Atlantic Ocean, and (probably generally also) in the Pacific Ocean.

It is important alike to navigation and to general science to know the limits where the phenomena of the trade-winds give place to those of the monsoons; and whether any and what variations take place in those limits in different parts of the year. *The barometric variations are intimately connected with the causes of these variations, and require to be known for their more perfect elucidation.*

The importance, indeed, of a full and complete knowledge of the variations which take place in the limits of the trade-winds generally in both hemispheres, at different seasons of the year, has long been recognized. On this account, although the present section is headed "Barometer," it may be well to remark here, that it is desirable that the forms supplied to ships should contain headings, calling forth a special record of the latitude and longitude where the trade-wind is first met with, and where it is first found to fail.

2°. The great extent of continental space in Northern Asia causes, by reason of the great heat of the summer and the ascending current produced thereby, a remarkable diminution of atmospheric pressure in the summer months, extending in the north to the Polar Sea, and on the European side as far as Moscow. Towards the east it is known to include the coasts of China and Japan, but the extent of this great diminution of summer pressure beyond the coasts thus named is not known. A determination of the monthly variation of the pressure over the adjacent parts of the Pacific Ocean is therefore a desideratum; and for the same object it is desirable to have a more accurate knowledge than we now possess of the prevailing direction of the wind in different seasons in the vicinity of the coasts of China and Japan.

3°. With reference to regions or districts of increased or diminished *mean annual* pressure, it is known that in certain districts in the temperate and polar zones, such as in the vicinity of Cape Horn extending into the antarctic polar Ocean, and in the vicinity of Iceland, the mean annual barometric pressure is *considerably* less than the average pressure on the surface of the globe generally; and that anomalous differences, also of considerable amount, exist in the mean annual pressure in different parts of the arctic ocean. These all require special attention, with a view to obtain a more perfect knowledge of the facts, in regard to their amount, geographical extension, and variation with the change of seasons, as well as to the elucidation of their causes.

Dry Air and Aqueous Vapour.

The apparently anomalous variations which have been noticed to exist in the mean annual barometric pressure, and in its distribution in the different seasons and months of the year, are also found to exist in each of the two constituent pressures which conjointly constitute the barometric pressure. In order to study the problems connected with these departures from a state of equilibrium under their most simple forms,—and generally for the true understanding of almost all the great laws of atmospheric change,—it is necessary to have a separate knowledge of the two constituents (viz. the pressures of the dry air and of the aqueous vapour) which we are accustomed to measure together by the barometer. This separate knowledge is obtained by means of the hygrometer, which determines the elasticity of the vapour, and leads to the determination of that of the dry air, by enabling us to deduct the elasticity of the vapour from that of the whole barometric pressure. It is therefore extremely desirable that tables, similar to those recommended under the preceding head of the barometer, should be formed at every land station, and over the ocean at the centres of geographical spaces bounded by certain values of latitude and longitude, for the *annual*, *monthly*, and *season* pressures,—1. Of the aqueous vapour; and 2. Of the dry air; each considered separately. Each of the said geographical spaces will require its appropriate ledger for each of the twelve months.

It may be desirable to notice one or two of the problems connected with extensive and important atmospherical laws which may be materially assisted by such tables.

1°. By the operation of causes which are too well known to require explanation here, the dry air should always have a minimum pressure in the hottest months of the year. But we know that there are places where the contrary prevails, namely, that the pressure of the dry air is greater in summer than in winter. We also know that when comparison is made between places in the same latitude, and having the same, or very nearly the same, differences of temperature in summer and in winter, the differences between the summer and winter pressures of the dry air are found to be subject to many remarkable anomalies. The variations in the pressure of the dry air do not therefore, as might be at first imagined, depend altogether on the differences between the summer and winter temperatures at the places where the variations themselves occur. The increased pressure in the hottest months appears rather to point to the existence of an overflow of air in the higher regions of the atmosphere from *lateral sources*; the statical pressure at the base of the column being increased by the augmentation of the superincumbent mass of air arising from an influx in the upper portion. Such lateral sources may well be supposed to be due to *excessive ascensional currents* caused by *excessive summer heats* in certain places of the globe (as, for example, in Central Asia). Now the lateral overflow from such sources, traversing in the shape of currents the higher regions of the atmosphere, and encountering the well-known general current flowing from the equator towards the pole, has been recently assigned with considerable probability (derived from its correspondence with many otherwise anomalous phenomena already known, and which all receive an explanation from such supposition) to be the original source or primary cause of the *rotating storms* or *cyclones*, so well known in the West Indies and in China under the names of hurricanes and typhoons. A single illustration may be desirable. Let it be supposed that such an excessive ascensional current exists over the greatly heated parts of Asia and Africa in the northern tropical zone,—giving rise, in the continuation of the same zone over the Atlantic Ocean, to a lateral current in the upper regions; this would then be a current prevailing in those regions from east to west: and it would encounter over the Atlantic Ocean the well-known upper current proceeding from the equator towards the pole, which is a current from the south-west. An easterly current impinging on a south-west current may give rise, by well-known laws, to a rotatory motion in the atmosphere, of which the direction may be the same as that which characterises the cyclones of the northern hemisphere. To test the accuracy of this explanation, we desire to be acquainted with the variations which the *mean pressure of the dry air undergoes in the different seasons* in the part of the globe, where, according to this explanation, considerable variations having particular characters ought to be found.

2°. We have named one of the explanations which have been recently offered of the primary cause of the northern cyclones. Another mode of explanation has been proposed, by assuming the condensation of large quantities of vapour, and the consequent influx of air to supply the place. In such case the phenomena are to be tested in considerable measure by the variations which the *other constituent* of the barometric pressure, namely, the *aqueous vapour*, undergoes.

3°. The surface of sea in the southern hemisphere *much* exceeds that in the northern hemisphere. It is therefore probable that at the season when the sun is over the southern hemisphere, evaporation over the whole surface of the globe is more considerable than in the opposite season when the sun is over the northern hemisphere. Supposing the pressure of the dry air to be a constant, the difference of evaporation in the two seasons may thus produce for the whole globe an *annual barometric variation,* the aggregate barometric pressure over the *whole* surface being highest during the northern winter. The separation of the barometric pressure into its two constituent pressures would give direct and conclusive evidence of the cause to which such a barometric variation should be ascribed. It would also follow, that evaporation being greatest in the south, and condensation greatest in the north, the water which proceeds from south to north in a state of vapour, would have to return to the south in a liquid state, and might possibly exert some discernible influence on the currents of the ocean. The tests by which the truth of the suppositions thus advanced may be determined, are the variations of the meteorological elements in different seasons and months, determined by methods and instruments strictly comparable with each other, and arranged in such tables as have been suggested. A still more direct test would indeed be furnished by the fact (if it could be ascertained), that the quantity of rain which falls in the northern is greater than that which falls in the southern hemisphere; and by examining its distribution into the different months and seasons of its occurrence. Data for such conclusions are as yet very insufficient; they should always, however, form a part of the record at all land stations where registers are kept.

In order that all observations of the elasticity of the aqueous vapour may be strictly comparable, it is desirable that all should be computed by the same tables; those founded upon the experiments of MM. Regnault and Magnus may be most suitably recommended for this purpose, not only on their general merits, but also as being likely to be most generally adopted by observers in other countries.

Temperature of the Air.

Tables of the mean temperature of the air in the year, and in the different months and seasons of the year, at above 1000 stations on the globe, have recently been computed by Professor Dove, and published under the auspices of the Royal Academy of Sciences at Berlin. This work,—which is a true model of the method in which a great body of meteorological facts, collected by different observers and at different times, should be brought together and coordinated,—has conducted, as is well known, to conclusions of very considerable importance in their bearing on climatology, and on the general laws of the distribution of heat on the surface of the globe. These tables have, however, been formed exclusively from observations made *on land*. For the completion of this great work of physical geography, there is yet wanting a similar investigation for the *oceanic* portion; and this we may hopefully anticipate as likely to be now accomplished by means of the marine observations about to be undertaken. In the case of the temperature of the air, as in that of the atmospheric pressure previously adverted to, the centres of geographical spaces bounded by certain latitudes and longitudes will form points of concentration for observations, which may be made within those spaces, not only by the same but also by different ships; provided that the system be steadily maintained of employing only instruments which shall have been examined, and their intercomparability ascertained, by a competent and responsible Authority;—and provided that no observations be used but those in which careful attention shall have been given to the precautions which it will be necessary to adopt, for the purpose of obtaining the correct knowledge of the temperature of the external air, amidst the many disturbing influences from heat and moisture so difficult to escape on board ship. In this respect additional precautions must be used if *night observations* are to be required, since the ordinary difficulties are necessarily much enhanced by the employment of artificial light. Amongst the instructions which will be required, perhaps there will be none which will need to be more carefully drawn, than those for obtaining the correct temperature of the external air under the continually varying circumstances that present themselves on board ship.

In regard to *land stations*, Professor Dove's tables have shown that data are still pressingly required from the British North American possessions intermediate between the stations of the Arctic Expeditions and those of the United States; and that the deficiency extends across the whole North American Continent in those latitudes from the Atlantic to the Pacific. Professor Dove has also indicated as desiderata, observations at the British Military stations in the Mediterranean (Gibraltar, Malta and Corfu), and around the Coasts of Australia and New Zealand: also that *hourly* observations, continued for at least one year, are particularly required at some one station in the West Indies, to supply the diurnal corrections for existing observations.

Whilst the study of the distribution of heat at the surface of the globe has thus been making progress, in respect to the *mean annual temperature* in different places, and to its *periodical variations* in different parts of the year at the same place, the attention of physical geographers has recently been directed (and with great promise of important results to the material interests of men as well as to general science) to the causes of those fluctuations in the temperature, or departures from its mean or normal state at the same place and at the same period of the year, which have received the name of "non-periodic variations." It is known that these frequently affect extensive portions of the globe at the same time; and are generally, if not always, accompanied by a fluctuation of an opposite character, prevailing at the same time in some adjoining but distant region; so that by the comparison of synchronous observations a progression is traceable, from a locality of maximum increased heat in one region, to one of maximum diminished heat in another region. For the elucidation of the non-periodic variations even *monthly* means are insufficient; and the necessity has been felt of computing the mean temperatures for periods of much shorter duration. The Meteorological Institutions of those of the European States which have taken the foremost part in the prosecution of meteorology, have in consequence adopted *five-day means*, as the most suitable intermediate gradation between daily and monthly means; and as an evidence of the conviction which is entertained of the value of the conclusions to which this investigation is likely to lead, it has been considered worth while to undertake the prodigious labour of calculating the five-day means of the most reliable existing observations during a century past. This work is already far advanced; and it cannot be too strongly recommended, that at all fixed stations, where observations shall hereafter be made with sufficient care to be worth recording, five-day means may invariably be added to the daily, monthly, and annual means into which the observations are usually collected. The five-day means should always commence with January 1, for the purpose of preserving the uniformity at different stations, which is essential for comparison: in leap years, the period which includes the 29th of February will be of six days.

In treating climatology as a *science*, it is desirable that some correct and convenient mode should be adopted, for computing and expressing the *comparative variability* to which the temperature in different parts of the globe, and in different parts of the year in the same place, is subject from non-periodic causes. The *probable variability*, computed on the same principle as the *probable error* of each of a number of independent observations, has recently been suggested as furnishing an index "of the probable daily non-periodic variation" at the different seasons of the year; and its use in this respect has been exemplified by calculations of the "index" from the five-day means of twelve years of observations at Toronto, in Canada (Phil. Trans. 1853, Art. V.). An index of this description is of course of absolute and general application; supplying the means of comparing the probable variability of the temperature in different seasons at *different places* (where the same method of computation is adopted) as well as at the *same place*. It is desirable that this (or some preferable method if such can be devised for obtaining the same object) should be adopted by those who may desire to make their observations practically useful for sanitary or agricultural purposes, or for any of the great variety of objects for which climatic peculiarities are required to be known. Having these three data, viz. the mean annual temperature,—its periodical changes in respect to days, months, and seasons,—and the measure of its liability to non-periodic (or what would commonly be called, irregular) variations,—we may consider that we possess as complete a representation of the climate of any particular place (so far as temperature is concerned), as the present state of our knowledge permits.

It is obvious that much of what has been said under this article is more applicable to land than to sea observations; but the letter of the Board of Trade, to which this is a reply, requests that both should be contemplated.

Temperature of the Sea, and Investigations regarding Currents.

It is unnecessary to dwell on the practical importance to *navigation* of a correct knowledge of the currents of the ocean; their direction, extent, velocity, and the temperature of the surface-water relatively to the ordinary ocean temperature in the same latitude; together with the variations in all these respects which currents experience in different parts of the year, and in different parts of their course. As the information on these points, which may be expected to follow from the measures adopted by the Board of Trade, must necessarily depend in great degree on the *intelligence*, as well as the *interest* taken in them by the observers, it is desirable that the instructions to be supplied with the meteorological instruments should contain a brief summary of what is already known in regard to the principal oceanic currents; accompanied by charts on which their supposed limits in different seasons, and the variations in those limits which may have been observed in particular years, may be indicated, with notices of the particularities of the temperature of the surface-water by which the presence of the current may be recognized. Forms will also be required for use in such localities, in which the surface temperatures may be recorded at hourly or half-hourly intervals, with the corresponding geographical positions of the ship, as they may be best inferred from observation and reckoning. For such localities also it will be necessary that the tables, into which the observations of different ships at different seasons are collected, should have their bounding lines of latitude and longitude brought nearer together than may be required for the ocean at large.

In looking forward to the results which are likely to be obtained by the contemplated marine observations, it is reasonable that those which may bear practically on the interests of navigation should occupy the first place; but, on the other hand, it would not be easy to over-estimate the advantages to physical geography, of general tables of the surface temperature of the ocean in the different months of the year, exhibiting, as they would do, its normal and its abnormal states, the mean temperature of the different parallels, and the deviations therefrom, whether permanent, periodical, or occasional. The knowledge which such tables would convey is essentially required for the study of climatology *as a science*.

The degree in which climatic variations extending over large portions of the earth's surface may be influenced by the variable phenomena of oceanic currents in different years, may perhaps be illustrated by circumstances of known occurrence in the vicinity of our own coasts. The admirable researches of Major Rennell have shown that in ordinary years, the warm water of the great current known by the name of the Gulf-stream is not found to the east of the meridian of the Azores; the sea being of ordinary ocean temperature for its latitude at all seasons and in every direction, in the great space comprised between the Azores, and the coasts of Europe and North Africa: but Major Rennell has also shown that on two occasions, viz. in 1776 and in 1821–1822, the warm water by which the Gulf-stream is characterised throughout its whole course (*being several degrees* above the ordinary ocean temperature in the same latitude), was found to extend across this great expanse of ocean, and in 1776 (in particular) was traced (by Dr. Franklin) quite home to the coast of Europe. The presence of a body of unusually heated water, extending for several hundred miles both in latitude and in longitude, and continuing for several weeks, at a season of the year when the prevailing winds blow from that quarter on the coasts of England and France, can scarcely be imagined to be without a considerable influence on the relations of temperature and moisture in those countries. In accordance with this supposition, we find in the Meteorological Journals of the more recent period (which are more easily accessible), that the state of the weather in November and December 1821 and January 1822 was so unusual in the southern parts of Great Britain and in France, as to have excited general observation; we find it characterised as "most extraordinarily hot, damp, stormy, and oppressive," that "the gales from the W. and S.W. were almost without intermission," "the fall of rain was excessive" and "the barometer lower than it had ever been known for 35 years before."

There can be little doubt that Major Rennell was right in ascribing the unusual extension of the Gulf-stream in particular years to its greater initial velocity, occasioned by a more than ordinary difference in the levels of the Gulf of Mexico and of the Atlantic in the preceding summer. An unusual height of the Gulf of Mexico at the head of the stream, or an unusual velocity of the stream at its outset in the Strait of Florida, are facts which may admit of being recognized by properly directed attention; and as these must precede, by many weeks, the arrival of the warm water of the stream at above 3000 miles' distance from its outset, and the climatic effects thence resulting, it might be possible to anticipate the occurrence of such unusual seasons upon our coasts.

Much, indeed, may undoubtedly be done towards the increase of our partial acquaintance with the phænomena of the Gulf-stream, and of its counter currents, by the collection and coordination of observations made by casual passages of ships in different years and different seasons across different parts of its course; but for that full and complete knowledge of all its particulars, which should meet the maritime and scientific requirements of the period in which we live, we must await the disposition of Government to accede to the recommendation, so frequently made to them by the most eminent hydrographical authorities, of a specific survey of the stream by vessels employed for that special service. What has been recently accomplished by the Government of the United States in this respect, shows both the importance of the inquiry and the great extent of the research; and lends great weight to the proposition, which has been made to Her Majesty's Government on the part of the United States, for a joint survey of the whole stream by vessels of the two countries. The establishment of an office under the Board of Trade specially charged with the reduction and coordination of such data may materially facilitate such an undertaking.

Storms or Gales.

It is much to be desired both for the purposes of navigation and for those of general science, that the captains of Her Majesty's ships and masters of merchant vessels should be correctly and thoroughly instructed in the methods of distinguishing *in all cases* between the rotatory storms or gales, which are properly called *Cyclones*, and gales of a more ordinary character, but which are frequently accompanied by a veering of the wind, which under certain circumstances might easily be confounded with the phenomena of Cyclones, though due to a very different cause. It is recommended therefore that the instructions, proposed to be given to ships supplied with meteorological instruments, should contain clear and simple directions for distinguishing *in all cases* and *under all circumstances* between these two kinds of storms; and that the forms to be issued for recording the meteorological phenomena during great atmospheric disturbances should comprehend a notice of all the particulars which are required for forming a correct judgment in this respect.

Thunder-storms.

It is known that in the high latitudes of the northern and southern hemispheres thunder-storms are almost wholly unknown; and it is believed that they are of very rare occurrence over the ocean in the middle latitudes when distant from continents. By a suitable classification and arrangement of the documents which will be henceforward received by the Board of Trade, statistical tables may in process of time be formed, showing the comparative frequency of these phenomena in different parts of the ocean, and in different months of the year.

It is known that there are localities on the globe where, during certain months of the year, thunder-storms may be considered as a periodical phenomenon of daily occurrence. In the Port Royal Mountains in Jamaica, for example, thunder-storms are said to take place *daily* about the hour of noon from the middle of November to the middle of April. It is much to be desired that a full and precise account of such thunder-storms, and of the circumstances in which they appear to originate, should be obtained.

In recording the phenomena of thunder and lightning it is desirable to state the duration of the interval between the flashes of lightning and the thunder which follows. This may be done by means of a seconds-hand watch, by which the time of the apparition of the flash, and of the commencement (and of the conclusion also) of the thunder may be noted. The interval between the flash, and the commencement of the thunder, has been known to vary in different cases, from less than a single second to between 40 and 50 seconds, and even on very rare occasions to exceed 50 seconds. The two forms of ordinary lightning, viz. zigzag (or forked) lightning, and sheet lightning, should always be distinguished apart; and particular attention should be given both to the observation and to the record, in the rare cases when zigzag lightning either bifurcates, or returns upwards. A special notice should not fail to be made when thunder and lightning, or either separately, occur in a perfectly cloudless sky. When globular lightning (balls of fire) is seen, a particular record should be made of all the attendant circumstances. These phenomena are known to be of the nature of lightning, from the injury they have occasioned in ships and buildings that have been struck by them; but they differ from ordinary lightning not only by their globular shape, but by the length of time they continue visible, and by their slow motion. They are said to occur sometimes without the usual accompaniments of a storm, and even with a perfectly serene sky. Conductors are now so universally employed in ships, that it may seem almost superfluous to remark that, should a ship be struck by lightning, the most circumstantial account will be desirable of the course which the lightning took, and of the injuries it occasioned; or to remind the seaman that it is always prudent, after such an accident has befallen a ship, to distrust her compasses until it has been ascertained that their direction has not been altered. Accidents occurring *on land* from lightning will, of course, receive the fullest attention from meteorologists who may be within convenient distance of the spot.

Auroras and Falling Stars.

Auroras are of such rare occurrence in seas frequented by ships engaged in commerce, that it may seem superfluous to give any particular directions for their observation *at sea*; and land observatories are already abundantly furnished with such. It is, of course, desirable that the meteorological reports received from ships should always contain a notice of the time and place where Auroras may be seen, and of any remarkable features that may attract attention.

The letter from Professor Heis, which is one of the foreign communications annexed, indicates the principal points to be attended to in the instructions which it may be desirable to draw up for the observation of "Falling Stars." For directions concerning Halos and Parhelia, a paper by Monsieur Bravais in the 'Annuaire Météorologique de la France' for 1851, contains suggestions which will be found of much value.

Charts of the Magnetic Variation.

Although the variation of the compass does not belong in strictness to the domain of meteorology, it has been included, with great propriety, amongst the subjects treated of by the Brussels Conference, and should not therefore be omitted here. It is scarcely necessary to remark, that whatever may have been the practice in times past, when the phenomena of the earth's magnetism were less understood than at present, it should in future be regarded as indispensable, that variation-charts should always be constructed for a *particular epoch,* and that *all parts* of the chart should show *the variation corresponding to the epoch for which it is constructed.* Such charts should also have, either engraved on the face or attached in some convenient manner, a table, showing the approximate annual rate of the secular change of the variation in the different latitudes and longitudes comprised: so that, by means of this table, the variation taken from the chart for any particular latitude and longitude may be corrected to the year for which it is required, if that should happen to be different from the epoch for which the chart is constructed.

A valuable service would be rendered to this very important branch of hydrography, if, under the authority of the new department of the Board of Trade, variation-charts for the North and South Atlantic Oceans, for the North and South Pacific Oceans, for the Indian Ocean, and for any other localities in which the requirements of navigation might call for them, were published at *stated intervals*, corrected for the secular change that had taken place since the preceding publication. Materials would be furnished for this purpose by the observations which are now intended to be made, supposing them to be collected and suitably arranged with proper references to date and to geographical position, and to the original reports in which the results and the data on which they were founded were communicated. By means of these observations the tables of approximate correction for secular change might also be altered from time to time as occasion should require, since the rate of secular change itself is not constant.

All observed variations, communicated or employed as data upon which variation-charts may be either constructed or corrected, should be accompanied by other observational data (the nature of which ought now to be well understood), for correcting the observed variation for the error of the compass occasioned by the ship's iron. It is also strongly recommended that no observations be received as data for the formation or correction of variation-charts, but such as are accompanied by a detailed statement of the principal elements both of observation and of calculation. Proper forms should be supplied for this purpose; or, what is still better, books of blank forms may be supplied, in which the observations themselves may be entered, and the calculation performed by which the results are obtained. Such books of blank forms would be found extremely useful both for the variation of the needle, and for the chronometrical longitude (as well as for lunar observations, if the practice of lunar observations be not, as there is too much reason to fear it is, almost wholly discontinued). By preparing and issuing books of blank forms suitable for these purposes, and by requesting their return in accompaniment with the other reports to be transmitted to the Board of Trade at the conclusion of a voyage, the groundwork would be laid for the attainment of greatly improved habits of accuracy in practical navigation in the British mercantile marine.

The President and Council are aware that they have not exhausted the subject of this reply in what they have thus directed me to address to you; but they think that perhaps they have noticed as many points as may be desirable for *present* attention; and they desire me to add, that they will be at all times ready to resume the consideration if required, and to supply any further suggestions which may appear likely to be useful.

I have the honour to be, Sir,
Your obedient Servant,
W. SHARPEY,
Sec. R.S.

The Secretary of the Lords of the Committee of Privy Council for Trade.

June 21.—The Lord Wrottesley, President, in the Chair.

The following communications were read:—

"On a supposed Aërolite or Meteorite found in the Trunk of an old Willow Tree in the Battersea Fields." By Sir Roderick Impey Murchison, F.R.S.

In bringing this notice before the Royal Society, it is unnecessary to recite, however briefly, the history of the fall of aërolites or meteorites, as recorded for upwards of three thousand years, though I may be pardoned for reminding my Associates, that the phenomenon was repudiated by the most learned academies of Europe up to the close of the last century. The merit of having first endeavoured to demonstrate the true character of these extraneous bodies is mainly due to the German Chladni (1794), but his efforts were at first viewed with incredulity. According to Vauquelin and other men of eminence who have reasoned on the phenomena, it was in 1802 only that meteorites obtained a due degree of consideration and something like a definite place in science through the studies of Howard, as shown in his memoir published in the Philosophical Transactions.

Vauquelin, Klaproth, and other distinguished chemists, including Berzelius and Rammelsberg, have successively analysed these bodies, and the result of their labours, as ably brought together in the work of the last-mentioned author, is, that whilst they have a great general resemblance and are distinguishable on the whole by their composition from any bodies found in the crust of the earth, each of their component substances is individually found in our planet. They are also peculiarly marked by the small number of minerals which have collectively been detected in any one of them; nickel and cobalt, in certain relations to iron, being the chief characteristics of the metallic meteorites.

Of the various theories propounded to account for the origin of these singular bodies, it would indeed ill become a geologist like myself to speak; and referring in the sequel to some of the various works in which the subject has been brought within formula, I will at once detail the facts connected with the discovery of this metalliferous body in the heart of a tree, as now placed before the Members of our Society, feeling assured that, whatever be their ultimate decision, my contemporaries will approve of the efforts that have been made to account for this singular and mysterious phenomenon.

On the 2nd of June, a timber merchant, residing at North Brixton, named Clement Poole, brought the specimen now exhibited to the Museum of Practical Geology, when it occurred to Mr. Trenham Reeks, our Curator, that it might be a meteorite, and on inspecting its position in the mass of wood, and having heard all the evidence connected with it, I was disposed to form the same conclusion. On submitting a small portion of the metallic part to a qualitative test in the metallurgical laboratory of our establishment, the presence of nickel, cobalt and manganese was detected in the iron included in the mass, and as the surface was scorified, indented, uneven, and partially coated with a peculiar substance, the surmise as to the meteoric nature of the imbedded material seemed to be rendered much more probable. Again, in looking at the wood which immediately surrounded that portion of the mass which remained, as it is now, firmly inserted in the tree, a blackened substance was observed to be interpolated between the supposed meteorite and the surrounding sound wood. On the outside of this substance (which had somewhat a charred aspect) we observed a true bark, which follows the sinuosities of the wood wherever the latter appears to have been influenced by the intrusion of the foreign mineral matter. [The specimen is represented in the annexed wood-cut.]

Seeing thus enough to satisfy our conjecture, if sanctioned by other evidence, I desired Mr. Poole to bring all the fragments of the wood he had not destroyed which surrounded this body. On placing the ends of some of these (also now exhibited) on the parts from which they had been sawed off, they indicated that the space between the mineral substance and the surrounding sound wood widened upwards; the decayed wood passing into brown earthy matter with an opening or cavity into which rootlets extended. On interrogating Mr. Poole, who cut down the tree and superintended the breaking up of its timber, I learnt from him all requisite particulars respecting its dimensions, the position of the ferruginous mass, the quantity of wood above and below it, a description of the place where the stool of the tree was still to be seen, and of the parties who, living on the spot, were acquainted with every circumstance which could throw light on the case.

At this period of the inquiry, the Museum in Jermyn Street was visited by Dr. Shepard, Professor in the University College, Amherst, United States, whose researches on meteorites are widely known, and who has furnished an able classification of them by which they are divided into the two great classes of stony and metallic. Having carefully examined the specimen, Dr. Shepard expressed his decided belief that it was a true meteorite, and the next day wrote to me the following account of it; at the same time referring me most obligingly to a series of interesting publications on the subject as printed in America and Europe*:—

"Concerning the highly interesting mineral mass, lately found enclosed in the trunk of a tree, and of which you have done me the honour to ask my opinion, I beg leave to observe, that I have no hesitation in pronouncing it to be a true meteoric stone.

"Aside from the difficulty of otherwise accounting for it, under the circumstances in which it is found, the mass presents those peculiar traits that are regarded as characteristic of meteorites. It has, for example, a fused, vitrified black coating, which is quite continuous over a considerable part of the mass, and contains several grains and imbedded nodular and vein-like portions of metallic iron, in which I understand nickel and cobalt have been detected.

"The general character of the body of the stone is indeed peculiar; and as a whole, unlike any one I have yet seen; it being principally made up of a dull greyish yellow, peridotic mineral, which I have nowhere met with among these productions, except in the Hommoney Creek meteoric iron mass, and which exists in it only in a very limited quantity. It is singular to remark also, that the stone under notice strikingly resembles in size, shape and surface, the iron above alluded to.

"The absence of the black, slaggy coating on one of the broad surfaces of the stone, may arise from its having been broken away, by the violence to which it must have been subjected in entering the tree; for it appears to have buried itself completely at its contact, an operation which would probably have been impossible, in the case of a stone, but for its wedge-shaped configuration, and the coincidence of one of its edges with the vertical fibres of the wood."

In reply to a question I subsequently put to Dr. Shepard as to

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* Dr. Shepard's numerous memoirs on meteorites are all to be found in the volumes of the American Journal of Science and Art, and in the same work the reader will find not only the general classification of these bodies by this author, who possesses a collection from 103 localities, but also essays on the same subject by his countrymen Dr. Troost, Professor Silliman, jun., and Dr. Clark.

In our own country, Mr. Brayley published some years ago a comprehensive view of this subject in the Philosophical Magazine, and recently Mr. Greg has in the same publication put together all the previous and additional materials, with tables showing the geographical distribution of meteorites. Among the well-recorded examples of the fall of metalliferous meteorites, no one is more remarkable than that which happened in the year 1851, about sixteen leagues S.E. of Barcelona in Spain. In describing that phenomenon, Dr. Joaquim Balcells, Professor of Natural Sciences at Barcelona, has illustrated the subject with much erudition, whilst his theoretical views are ingenious in his endeavour to explain how meteorites are derived from the moon.

whether he knew of any examples of meteorites having struck trees in America, he replied as follows:—

"I think you will find in the volume I left with Mr. Reeks at the Museum, an account of the fall of Little Piney, Missouri, February 13th, 1839; in which it is stated that the stone struck a tree and was shattered to fragments, it being one of a brittle character. In the interior of the Cabarras county, N. Carolina, a stone (October 31, 1849) I know struck a tree, and I found it was difficult, indeed impossible, to separate completely the adhering woody fibres from the rough hard crust of the meteorite. The stone in this case is a peculiarly tough one, having a decidedly trappean character, rendering it as nearly infragile as cast iron."

Aware that some time must elapse before the precise analysis, which I wished to be made in the laboratory of Dr. Percy, could be completed, and that the last meeting of the Royal Society was to be held this evening, I announced the notice I am now communicating. At the same time I resolved to visit the locality where the tree stood, and to obtain on the spot all the details required. Having done so, accompanied by Mr. Robert Brown, Sir Philip Grey Egerton, Professor J. Nicol, and Mr. Trenham Reeks, the information ultimately obtained was as follows:—

The man who helped to cut down the tree confirmed in every respect the evidence of Mr. Poole as to its position, height, and dimensions, and pointed out to us the stump or stool we were in search of, which is to be seen at nearly 200 yards to the east of the St. George's Chapel, Lower Road, Battersea Fields, and at the eastern end of a nursery garden, between the railway and the road, occupied by Mr. Henry Shailer.

The tree was a large willow, probably about sixty years of age, which stood immediately to the east of the old parsonage house recently pulled down. Its stem measured about 10 feet in circumference at 3 feet above the ground, and had a length of between 9 and 10 feet; from its summit three main branches extended, one of which, pointing to the S.W. or W.S.W., had been for many years blighted, and was rotten to near its junction with the top of the main trunk; a portion of this blighted main branch is exhibited. The other two main branches, which rose to a height of 50 or 60 feet, were quite sound; a part of one of these offsets is also exhibited.

The stool of the tree was visibly perfect and without a flaw, and at the wish of Mr. R. Brown, a section of it has been obtained since our visit, which is also here, and the rings of which seem to confirm the supposition as to the age of the tree.

Mr. Poole having conveyed the tree to Brixton, cut the trunk into two nearly equal parts, intending to make cricket-bats out of each. In doing so, he perceived that the upper portion of the lower of the two segments was in a shaky or imperfect condition, and hence he resolved to saw off the upper part of it, intending thereby to obtain wood large enough for the "pods" of his cricket-bats, but not such entire bats as he was making out of the upper segment.

In dividing the tree, the saw was stopped at about 8 inches from the surface on one side (or the breadth of a large saw) by a very hard, impenetrable substance, which was supposed to be a nail, and hence Mr. Poole resolved to break up the portion of the wood he had previously condemned as of inferior quality, and hewing it down from the sides he uncovered, to his astonishment, the great lump of metalliferous matter, as now seen. Attaching little value to it, much of the surrounding wood was thrown away or used up before the specimen was brought to Jermyn Street; but enough has been obtained to throw light on the probable or possible origin of the included mass.

On interrogating Henry Shailer, a market gardener, who has long lived on the spot and managed the ground where the tree grew, when it was part of the garden of the former clergyman (Mr. Weddell), I learnt from him that he had known the spot for sixty years, that in his days of boyhood it was a fellmonger's yard, before it was attached to the garden. He had observed that the tree was blighted in one of its main branches for many years, and had always supposed that it was struck by lightning in one of two storms, the first of which happened about 1838 or 1839, the other about nine years ago.

So far the evidence obtained might be supposed to favour the theory that this ferruginous mass[*] had been discharged near to the blighted branch, and had penetrated downwards into the tree, to the position in which we now see it, charring and warping the wood immediately around it in its downward progress; whilst in the sixteen years which have elapsed, the wood renovating itself, produced the appearance which has so much interested the eminent botanists who have examined it, viz. Mr. R. Brown, Dr. Lindley, Professor Henfrey, Dr. J. Hooker, and Mr. Bennett.

On the other hand, I must now point out some features of this extraordinary case which check the belief in the included mass being a meteorite.

We found lying near the root of the tree two fragments, one of which is similar to the substance included in the tree, while the other is decidedly an iron slag. On bringing these fragments, weighing several pounds, to Jermyn Street, and on breaking one of them, it was found, like the supposed meteorite, to contain certain small portions of metallic iron, in which both nickel and cobalt were also present; and hence the scepticism which had prevailed from the beginning of the inquiry in the minds of some of my friends, was worked up into a definite shape.

The occurrence of stones enclosed in wood is not a novel phenomenon. Mr. Robert Brown has called my attention to two cases as recorded in the following works:—

"De Lapide in trunco Betulæ reperto. G.F. Richter in Acta Phys. Med. Acad. Nat. Curios. volume 3, page 66[†]."

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[*] The ferruginous mass is, it is supposed, about thirty pounds in weight; but as one of its extremities is still imbedded in the wood, the precise weight cannot be stated.

[†] "Lapis prædurus subalbicans et manifeste siliceus pruni ferme aut juglandis

"Descriptio Saxi in Quercu inventi. Kellander, Acta Literaria et Scientiæ Sueciæ." 1739, pp. 502, 503.

Since the Battersea phenomenon was announced, Professor Henslow, to whom I had applied, wrote to me saying, that he possessed a remarkable example of a stone which was found imbedded in the heart of a tree, in sawing it up in Plymouth Dockyard; and he has obligingly sent up the specimen, which is now also exhibited. In this case, judging from the mineral character of the rock, and its being slightly magnetic, Professor Henslow supposed that it was perhaps a volcanic bomb. On referring it to Dr. Shepard, that gentleman entertains the opinion that it is also a meteorite, and states that it resembles certain meteoric stones with which he is acquainted; suspicions of which had also been entertained by Professor Henslow, From the examination of a minute fragment which I detached from this stone, it appears to be composed of a base of felspathic matter, with minute crystals of felspar and of magnetic iron pyrites. Externally it has a trachytic aspect, though, when fractured, it more resembles, in the opinion of Mr. Warington Smyth, a pale Cornish elvan or porphyry than any other British rock with which it can be compared. Whatever may have been the origin of this stone, which is of the size of a child's head, it is essentially different from the metalliferous mass from Battersea, to which attention has been specially invited, and its position in the heart of an oak is equally remarkable. Like the Battersea specimen, the segment of wood from Plymouth Dockyard is characterised by an interior bark which folds round the sinuosities of the included stone.

In respect to the envelopment of manufactured materials in trees, my friend, Mr. H. Brooke, the distinguished mineralogist, tells me that he perfectly remembers the case of an iron chain which had been enclosed in the heart of a tree, the wood of which was sound around the whole of the included metallic body. This specimen was to be seen some years ago in the British Museum. Again, he informs me that at Stoke Newington he recollects to have seen a tree, the trunk of which had grown over and completely enclosed a scythe, except on the sides where its ends protruded*.

Whatever may have been the origin of the metalliferous mass from Battersea, its discovery has at all events served to develope certain peculiarities in the growth of plants which appear to be of high interest to the eminent botanists who have examined the parts of this tree which surrounded the supposed meteorite. Unwilling to endeavour to anticipate the final decision as to the origin of the body in question, I may be permitted to feel a satisfaction that its discoverer brought it to the Establishment of which I am the Director, and which numbers among its officers a Fellow of this Society, who is so

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minoris magnitudine. * * * * Nidus ad figuram lapidis non plane accommodatus, sed quadrangulus, et hinc illinc in mediocres rimas desinens, corticeque imprimis notabili, non multum ab exteriori cute diverso, maximam partem vestitus."

* Many other examples of extraneous bodies found enclosed in the heart of trees have been brought to my notice since this account was written. The most curious of these is perhaps that of an image of the Virgin, which having been placed in a niche had become imbedded by the growth of the tree around it.

well calculated, by his analytical researches, to settle the question on a permanent basis. Should the metallurgical analyses now under the conduct of Dr. Percy lead to the inevitable conclusion that the composition of this body is different from that of well-authenticated meteorites, and is similar to that of undoubted iron slags, we shall then have obtained proofs of the great circumspection required before we assign a meteoric origin to some of these crystalline iron masses, which though not seen to fall, have, from their containing nickel, cobalt and other elements, been supposed to be formed by causes extraneous to our planet.

Postscript, 30th June 1855.—The following are the analyses above referred to, which have been given to me by Dr. Percy since the preceding notice was read:—

"*The slag-like matter* (1) attached to the metal in the tree, as well as the similar matter (2) with adherent metal which was found by Mr. Reeks in the vicinity of the tree, has been analysed. The results are as follow:—

	No. 1.	No. 2.
Silica	58·70	63·52
Protoxide of iron	35·46	32·30
Lime	0·30	0·59
Magnesia	0·74	0·21
Protoxide of manganese	trace	trace
Alumina	3·40	2·85
Phosphoric acid	0·43	0·57
Sulphur as sulphide	trace	trace
	99·03	100·04

"No. 1. was analysed by Mr. Spiller, and No. 2. by Mr. A. Dick, chemists who have been incessantly engaged at the Museum during the last two years and a half in the analyses of the iron ores of this country, and whose great experience renders their results worthy of entire confidence. Cobalt and nickel were not sought for in either case, but the metallic iron enveloped in both specimens contained a minute quantity of cobalt and nickel. Another piece of slag-like matter, which was found on the ground near the tree, and which from its external characters I have no hesitation in pronouncing to be a slag, was examined for cobalt and nickel, and gave unequivocal evidence of the former in minute quantity, though not satisfactorily of the latter.

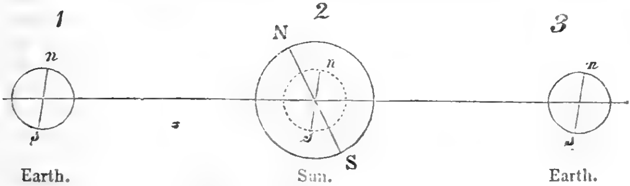
"The metal previously mentioned is malleable iron. That which was detached from the slag-like matter, found outside the tree, was filed and polished, and then treated with dilute sulphuric acid. After this treatment, the surface presented small, confused, irregularly-defined crystalline plates, and was identical in appearance with the surface of a piece of malleable iron similarly treated after fusion in a crucible."

Extract of a Letter from Professor Langberg of Christiania to Colonel Sabine, dated June 10, 1855.

"Of all the important results from the discussions of the British Colonial Observatory, the discovery of the *direct* action of the sun on the magnetism of the earth is certainly a fact of the highest interest, in opening quite a new field for investigation; and few modern discoveries in this branch of science have interested me more than yours of the annual variation of the diurnal variation of declination. It seems that M. Secchi of Rome has nearly touched at the same discovery, and I am indeed glad that the enormous quantity of calculations, which you are superintending, did not prevent you from publishing your results before the ripening fruit was plucked by another. The first beautiful result of this annual variation is the explanation of the fact, which you have deduced from the observations at St. Helena and the Cape of Good Hope, that the horary variation of declination does *not* vanish in passing from the northern to the southern magnetic hemisphere, but only changes signs at the equinoxes. I think every physicist will agree with you, that no thermic hypothesis will be able to explain this annual variation, but as you say (Toronto, ii. p. xix), it is 'obviously connected with, and dependent on, the earth's position in its orbit relatively to the sun, around which it revolves, as the diurnal variation is connected with the rotation of the earth on its axis.' But you have given no hint how this different position in its orbit can affect the magnetic condition of the earth, except so far as you suppose that the excentricity of the orbit is the reason that the total magnetic force is about $\frac{2}{1000}$ greater at the perihelion than at the aphelion (page xciii); but even granted that this variation is the effect of the excentricity, it cannot be the cause of the annual variation of the declination, as this is of contrary signs in the two semiannual periods.

"I have thought that this annual variation might possibly be explained by the following considerations, which I (although with extreme diffidence) shall venture to lay before you.

"As the recent magnetic observations have proved without doubt the *direct* magnetic action of the sun, or that the sun itself is a magnet, the sun must accordingly have magnetic polarity or magnetic poles. Now in our ignorance of the causes of the magnetic condition of the heavenly bodies, I think it reasonable to connect it in some way with their rotation on their axes, and to assume that generally their magnetic axis will nearly coincide with the axis of rotation; at all events, if these do not coincide, but include a small acute angle, the sum of the magnetic influences on a distant magnetic body during a whole rotation, will be nearly the same as if the magnetic poles were placed in the axis of rotation. If we suppose a magnet E revolving about another S, the magnetic axis of E remaining parallel with itself, but not parallel with the axis of S (as the earth around the sun), then the magnetic induction of S on E will depend on the relative position of both magnetic axes. Moreover, if we only regard the mean of the magnetic induction of the sun on the earth during several rotations of both on their axes, we may approximately assume that the magnetic axis of both coincides with the axis of rotation, and compare their relative position during a whole revolution or annual period, with the magnetic variation in question. Now, if a plane is laid through the sun's axis parallel with the axis of the earth, this plane intersects the ecliptic in two points, whose longitudes are 286° and 106°, and has an inclination of 83°·2, both axes forming an angle of 25°·8. Accordingly, the northerly prolongation of both axes will *converge* (as in 1 and 2) when the longitude is 286°, or about seventeen days after the summer solstice, and the southerly prolongation (as in 2 and 3) when the longitude of the earth is 106°, or sixteen days after the winter solstice. *About sixteen days after the equinoxes* the axes are in the position 2, and the radius vector forms the greatest angle with the above-named plane, viz. 83°·2. It is therefore evident, that the greatest magnetic induction takes place at the two solstices, but of opposite character, as the north poles converge in one, and the south poles in the other semiannual period: the change takes place *sixteen days after the equinoxes,* exactly as you have found by observation, and I regard this circumstance as very important evidence for my hypothesis, although you have shown that it could also be accounted for by the fact, that all magnetic induction takes some time ere it attains its maximum.



"If the sun's magnetic axis does not coincide with the axis of rotation, then I suppose we shall find, by more minute examination of the observations, that there exists also a small magnetic variation corresponding to the sun's geocentric rotation, or 27·68 days.

"There seems also to be strong reason to suppose that Hansteen's discovery about the annual periodic frequency of the aurora borealis, which has a marked maximum at the equinoxes, and even a more marked maximum at the solstices, is connected with the same cause."

LII. *Intelligence and Miscellaneous Articles.*

CONTRIBUTIONS TOWARDS THE KNOWLEDGE OF FLUORESCENT BODIES. BY G. WERTHER.

IN the following experiments a ray of light was thrown, by means of a heliostat, upon a prism provided with a lens, and the portion of the spectrum from the red to the violet was shut off. The substances to be examined should in this arrangement be held only in the violet and opake portion of the spectrum. It appeared, and indeed this is characteristic of fluorescent bodies, that all the violet was extinguished, and instead of this a yellowish or bluish-green light of greater or less intensity made its appearance. In the other parts of the spectrum these bodies exhibited the usual prismatic colours. The greater or less extent of the fluorescent part of the spectrum was always compared with some sulphate of quinine spread upon paper which was stretched over a glass plate. Some of the substances which are fluorescent in the solid form were also dissolved and laid upon paper. Protocyanide of potassium and platinum laid on to a sufficient thickness, appeared, with regard to the extent and distinctness of its projection of Fraunhofer's lines, as good as sulphate of quinine. Acetate of uranium and soda did not show fluorescence until its concentrated solution had been applied several times. Its solution also did not exhibit the epipolization (the bluish lustre) presented by solution of sulphate of quinine. Solution of protocyanide of potassium and platinum also does not exhibit this property when greatly diluted. The salts examined were,—

	Behaviour	
Compounds.	In the violet.	Behind the violet.
$U^2O^3, PO^5, 5HO$ $U^2O^3, AsO^5, 5HO$	.........Strongly yellowish-green.	Fluorescent greenish-yell.
$(2U^2O^3+HO)PO^5+8HO$...	Bluish-green.	Fluorescent greenish-yell.
$(2U^2O^3+HO)PO^5+6HO$...	Very slight greenish-yellow.	Slight yellowish-green.
$(2U^2O^3+HO)PO^5+3HO$...	No result.	
$(2U^2O^3+HO)PO^5$............	Strongly yellowish-green.	Slight greenish-yellow.
$(2U^2O^3+HO)AsO^5+8HO$ $(2U^2O^3+HO)AsO^5+6HO$	No result.	Slight greenish-yellow.
$(CaO+2U^2O^3)PO^5+8HO$ The same, as lime-uranite.	Strong sea-green.	Strong yellowish-green.
$(CuO+2U^2O^3)PO^5+8HO$ The same, as chalcolite.	No result.	
$(CuO+2U^2O^3)AsO^5+8HO$.	No result.	
$KO\bar{A}+2(U^2O^3\bar{A})+2HO$ ...	No result.	Slight yellowish-green.
$NaO\bar{A}+2(U^2O^3\bar{A})$	Bluish-green.	Strong yellowish-green.
$(U^2O^3)^2, PO^5+(U^2O^3)^3, PO^5$ $2(2U^2O^3, PO^5)+NaO, U^2O^3+3HO$ $(NaO+2U^2O^3), AsO^5+5HO$	No result.	
U^2O^3, NO^5+6HO............	Strong yellowish-green.	Strong greenish-yellow.

—*Journ. für Prakt. Chemie*, vol. lxv. p. 349.

ON THE ACETATES OF MANGANESE, CADMIUM, AND NICKEL.

BY M. C. VON HAUER.

The acetate of manganese assumes the form of rose-coloured rhomboidal lamellæ, generally with truncated edges; it contains four atoms of water, and is probably isomorphous with the acetate of magnesia. The acetate of cadmium is so difficult to obtain, that several chemists have denied its existence. It crystallizes in rather large prisms, with strongly brilliant planes, which M. Zepharovich has determined to belong to the augitic (hemiprismatic) system. The acetate of cadmium contains three atoms of water, and is identical in chemical character with the acetate of zinc. It is only obtained by leaving the acid solution to evaporate in absolute repose.

Acetate of nickel contains four atoms of water. Its crystallographic description has been recently given in M. Schabus's memoir, which obtained the prize offered by the Imperial Academy of Vienna. —*Proceedings of the Imperial Geological Institute of Vienna, March* 6, 1855.

METEOROLOGICAL OBSERVATIONS FOR SEPT. 1855.

Chiswick.—September 1. Clear and fine: cloudy. 2. Cloudy. 3. Clear: slight shower: fine. 4. Cloudy. 5. Fine: cloudy: clear and cold at night. 6. Clear: cloudy. 7. Very clear: cloudy: fine. 8. Dense fog: clear and very fine. 9. Very fine. 10. Foggy: fine: very clear. 11. Very fine. 12. Slight fog: very fine: clear: rain at night. 13. Rain. 14. Constant rain. 15. Uniformly overcast: fine. 16. Overcast: clear and fine. 17. Rain: hazy: overcast. 18. Hazy: overcast. 19. Slight haze: fine. 20—22. Foggy in the mornings: very fine. 23. Fine: cloudy: very clear. 24. Overcast. 25. Fine: cloudy: clear: frosty at night. 26. Slight haze: very dry air: frosty at night. 27. Foggy: very fine: cloudy. 28, 29. Cloudy: rain. 30. Hazy: cloudy: rain at night.

Mean temperature of the month .... 56°·11
Mean temperature of Sept. 1854 .... 56 ·93
Mean temperature of Sept. for the last twenty-nine years ... 56 ·98
Average amount of rain in Sept. .... 2·503 inches.

Boston.—Sept. 1. Cloudy: rain P.M. 2—6. Cloudy. 7, 8. Fine. 9, 10. Cloudy. 11, 12. Fine. 13. Fine: rain A.M. 14—18. Cloudy. 19, 20. Fine. 21, 22. Cloudy. 23. Fine. 24. Fine: rain A.M. 25. Cloudy. 26, 27. Fine. 28. Fine: rain P.M. 29. Cloudy: rain A.M. 30. Cloudy.

Sandwick Manse, Orkney.—Sept. 1, 2. Bright A.M.: cloudy P.M. 3. Cloudy A.M. and P.M. 4. Bright A.M.: clear P.M. 5. Showers A.M.: clear P.M. 6. Drizzle A.M. and P.M. 7. Cloudy A.M.: showers P.M. 8. Clear A.M.: showers P.M. 9. Bright A.M.: showers P.M. 10. Cloudy A.M.: showers, aurora P.M. 11. Showers A.M.: cloudy, aurora P.M. 12. Rain A.M.: clear, aurora P.M. 13. Cloudy A.M.: clear P.M. 14. Cloudy, hoar-frost A.M.: clear, aurora P.M. 15. Bright, hoar-frost A.M.: cloudy P.M. 16. Showers A.M.: clear P.M. 17, 18. Showers A.M. and P.M. 19. Cloudy A.M.: drops P.M. 20. Cloudy A.M. and P.M. 21. Showers A.M.: drizzle P.M. 22. Damp A.M.: drizzle P.M. 23. Cloudy A.M.: clear, fine P.M. 24. Clear A.M.: fine, cloudy P.M. 25. Cloudy A.M.: fine, drops P.M. 26. Drops A.M.: fine, clear P.M. 27. Drops A.M.: cloudy P.M. 28. Drops A.M. and P.M. 29. Rain A.M.: fog P.M. 30. Fog A.M. and P.M.

Mean temperature of Sept. for twenty-eight previous years ... 52°·42
Mean temperature of this month .... 52 ·74
Mean temperature of Sept. 1854 .... 55 ·07
Average quantity of rain in Sept. for fifteen previous years ... 2·81 inches.

Meteorological Observations made by Mr. Thompson *at the Garden of the Horticultural Society at* CHISWICK, *near London*; *by* Mr. Veall, *at* BOSTON; *and by the* Rev. C. Clouston, *at Sandwick Manse*, ORKNEY.

Days of Month.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick.		Boston. 8½ a.m.	Orkney, Sandwick.		Chiswick.		Boston. 8½ a.m.	Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
1855. Sept.	Max.	Min.		9¼ a.m.	8½ p.m.	Max.	Min.		9¼ a.m.	8½ p.m.						
1.	30·346	30·282	29·83	30·34	30·50	68	45	62	55½	53	nw.	n.	nw.	……	……	·04
2.	30·315	30·146	29·88	30·42	30·34	67	45	60	56½	55	n.	nne.	w.	……	·04	……
☾ 3.	30·131	30·058	29·75	30·27	30·17	71	50	60	57	54	ne.	nne.	w.	·02	……	……
4.	29·997	29·962	29·60	30·06	30·16	68	44	61·5	55	46	nw.	nne.	nw.	·01	……	……
5.	30·018	29·987	29·60	30·23	30·29	64	42	59	48	44	ne.	n.	nw.	……	……	·07
6.	30·274	30·226	29·35	30·36	30·37	62	36	52·5	51	51	ne.	nnw.	w.	……	……	……
7.	30·445	30·385	30·05	30·22	30·30	66	28	50·5	53	54	ne.	nnw.	ssw.	……	……	·02
8.	30·429	30·199	29·90	29·85	29·90	74	33	53·5	56	52½	sw.	w.	w.	……	……	·05
9.	30·164	30·100	29·65	29·96	30·05	73	33	60	54	52	w.	n.	wnw.	……	……	·05
10.	30·122	30·050	29·69	29·98	29·86	70	36	57	58	53½	e.	nw.	ssw.	……	……	·03
● 11.	30·055	30·051	29·61	29·79	29·94	72	37	55	56	53	ne.	nw.	w.	……	……	·15
12.	30·161	30·059	29·71	29·91	30·04	73	39	54	54	49	w.	nw.	nnw.	·02	……	·21
13.	29·976	29·919	29·54	30·15	30·15	69	50	59	50½	44	sw.	s.	ne.	·10	·13	·08
14.	30·007	29·926	29·55	30·06	30·02	54	45	57	49½	42	ne.	nne.	ese.	·32	……	……
15.	30·093	30·090	29·68	29·93	29·82	64	45	52	52½	50	sw.	nw.	sse.	……	……	……
16.	30·133	29·954	29·47	29·72	29·78	68	54	62	52½	51½	w.	wsw.	w.	·04	……	·21
17.	30·026	29·944	29·58	29·71	29·70	62	55	56	54½	52½	e.	wsw.	w.	·06	……	·11
18.	29·952	29·941	29·46	29·60	29·61	69	54	61	55½	51½	sw.	s.	w.	……	……	·15
☽ 19.	30·042	29·965	29·50	29·78	29·70	68	47	61	52½	53	ne.	n.	s.	……	……	·18
20.	30·130	30·112	29·58	29·73	29·77	75	44	58	56	59	sw.	sw.	ssw.	……	……	·07
21.	30·256	30·242	29·72	29·82	30·02	74	45	59	59½	55	sw.	w.	calm	……	……	·05
22.	30·274	30·237	29·80	30·12	30·32	75	41	56	59½	54	e.	n.	wnw.	……	……	·05
23.	30·250	30·229	29·77	30·46	30·55	76	55	60	53	48	e.	nne.	nnw.	……	……	……
24.	30·436	30·413	29·96	30·55	30·46	65	42	57·5	52	49	ne.	nne.	sse.	……	·03	……
○ 25.	30·430	30·305	29·96	30·27	30·14	65	27	58	52½	55½	e.	sse.	sse.	……	……	……
26.	30·247	30·091	29·70	29·99	29·98	61	26	50	53½	51	e.	sse.	wsw.	……	……	……
27.	29·972	29·752	29·50	29·82	29·58	74	51	56	54	51½	sw.	sse.	se.	……	……	……
28.	29·669	29·446	29·18	29·32	29·45	72	56	60	57½	52½	sw.	sse.	nne.	·32	……	……
29.	30·024	29·538	29·13	29·60	29·72	73	55	61·5	54	55½	se.	s.	ese.	·08	·10	·21
30.	29·505	29·423	29·04	29·75	29·70	68	47	63	56	53	se.	e.	se.	·10	……	·01
Mean.	30·128	30·031	29·29	29·992	30·013	68·66	43·56	57·7	54·30	51·18				1·15	0·30	1·74

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[FOURTH SERIES.]

DECEMBER 1855.

LIII. *On the Transmission of Heat in Metals.*
By G. WIEDEMANN*.

1. *Conductibility of Zinc.*

AN investigation undertaken by myself and Dr. Franz showed that the values of the relative conductibility of different metals for heat and electricity were very nearly equal†.

With the same apparatus which served for the earlier experiments, I have recently undertaken a determination of the conductibility of zinc for heat.

A zinc wire, 4·4 millims. thick, was used; its surface was very carefully polished, but not silvered. The wire was investigated in air. Its temperatures were taken at points, which followed each other at distances of 2 inches from the warmest part of the wire (the zero point) to the coldest, and in degrees of the galvanometer described in the above paper, were as follows:—

	I.		II.		III.	
x.	*t*.	*q*.	*t*.	*q*.	*t*.	*q*.
0	205		160·5		129·5	
2	115·5	2·361	90·5	2·355	75	2·319
4	67·7	2·301	51·5	2·340	44·25	2·317
6	40·25	2·303	30·5	2·311	27·6	
8	25		19	2·290	—	
10	—		13			
	2·39	2·322		2·324		2·318

The numbers in the column x are the distances from the zero to the several points on the bar at which the temperatures were

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* From Poggendorff's *Annalen*, vol. xcv. p. 337.

† Poggendorff's *Annalen*, vol. lxxxix. p. 497; and Phil. Mag. vol. vii. p. 33.

observed; under t are found the corresponding deflections of the galvanometer when the thermo-electric element used in measuring the temperatures was applied to those parts of the bar; under q the quotients are given which are obtained by dividing the t standing near the corresponding q, into the sum of the temperatures standing above and below it.

If the highest temperature observed in each series be assumed equal to 100, the other temperatures will be as follows:—

	I.	II.	III.	Mean.
x.	t.	t.	t.	
0	100	100	100	100
2	56·3	56·7	57·9	57
4	33·0	32·1	34·2	33·1
6	19·6	19·0	21·3	20·0
8	12·2	11·8	—	12·0
10	—	7·2	—	7·2

From the mean value of the quotients $q=2·321$, the relative conductibility of the zinc bar may be estimated at 20·3, if the conductibility of an equally thick silver bar be placed at 100. If the deflections of the galvanometer given in the first table be reduced to actual excess of temperature of the several points of the bar above the surrounding air, the conductibility of zinc against that of silver at 100 will be—

19·0.

Becquerel found that the conductibility of zinc for electricity was 24 (silver =100). Zinc takes, therefore, pretty much the same place in the list of metals, whether considered as a conductor of heat or electricity.

2. *Passage of Heat from one metal to another.*

In considering the relative conductibilities of the different metals for heat, the question suggests itself, what takes place on the passage of heat from one metal to another.

Besides the old experiments of Fourier*, made with the contact thermometer, from which, however, no certain conclusions can be drawn, this question has been treated by M. Despretz†, and not long since, briefly, by M. Angström‡.

M. Despretz pressed together the sections of a copper and a tin bar, and heated the free end of the copper bar. The temperatures were observed by means of thermometers sunk in different parts of the bars, and from these Despretz calculated the temperatures at the point of contact, and found that the temperature of the copper bar there was 1°·47 C. higher than that of

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* Poggendorff's *Annalen*, vol. xiii. p. 327.

† Ibid. vol. xlvi. p. 484.

‡ Ibid. vol. lxxxviii. p. 165.

the tin bar. If a piece of paper were interposed between them, the difference amounted to 5°·5 C. Hence Despretz concludes that heat suffers a resistance in its passage from one metal to another. Besides the numbers here mentioned, Despretz gives none by which the entire process of the experiment can be realized.

Angström has also instituted only isolated experiments. He heated a bar composed of lead, copper and tin, at the one or the other end, and observed the temperatures at different places by means of thermometers sunk in the bar. Angström found that the ratio of the temperatures of two parts of the bar, on opposite sides of a place of contact of two of its metallic components, was greater when the heat passed from the worse to the better conductor than in the reverse case. In any case this last method of experiment is far too complicated to allow any direct conclusions to be drawn as to the different facilities with which heat passes in either the one or the other direction. The bars being comparatively short, and hence the temperatures observed at points equidistant from each other not forming a geometrical series, the mathematical consideration of the question is by no means simple.

The experiments of Gore also give no certain results as to the passage of heat from one metal to another.

The following experiments are intended to settle the point in question, and to examine whether heat really experiences any resistance in its passage.

For this purpose the same apparatus was used which had been constructed for the determination of the relative conductibility of the metals for heat. Two metal bars, 5 millims. thick, whose conductibility had been already determined, were cut in two, their sections accurately fitted upon each other, and carefully joined by means of an extremely thin layer of solder. The bars thus joined were placed in the apparatus and heated. The experiments were made in air, and the temperatures read off by the application of a thermo-electric element at points 2 inches asunder, commencing at the colder and proceeding to the warmer parts of the bar. In a second series of observations, the temperatures were read off at points midway between the points at first observed, so that at last the temperature of the bars was obtained from inch to inch.

Of numbers of experiments made in this manner I will select only a few. In the following Table, the numbers under x are the distances of the several points of the bars commencing at the place of junction; those under t are the corresponding temperatures represented in degrees of the galvanometer. The points of the warmer bar, mentioned first in the table, are marked with +, and those of the colder with −.

x.	Copper-German silver. *t.*	Copper-Iron. *t.*	Silver-Iron. *t.*	Copper-Copper. *t.*	Brass-Silver. *t.*
+4	130·75	155	154·5	146	—
+3	117	140·5	140·75	124	138
+2	108·5	129·0	131·5	110·7	102
+1	103	118·2	124·75	97	71·1
0				87·5	
−1	52·75	71·5	78·25	77·5	39·5
−2	31·75	52·5	55·6	67·0	35·0
−3	19·75	37·5	40·5	59·5	31
−4	11·75	27·5	28·8	49·2	27
−5	8·25	20	20	—	23·8
−6	—	15	15·1	—	—
−7	—	11·5	10·75	—	—

At the place of junction a slight displacement of the thermo-electric element (with the exception of the case of the two last bars) causes a considerable deflection in the galvanometer.

In all cases the temperatures of the colder bars form almost a geometrical series, and the bars can hence be considered infinitely long.

If heat passes from one metallic rod into another equally thick but infinitely long, the two rods must show a change of temperature at their junction if the heat experience a resistance in its passage*.

If the temperature y_1 at the place of contact of the colder bar be calculated by multiplying the value under −1 with the mean ratio of the geometrical series which the temperatures of the colder bars form; if, further, this value be put equal 100, and the other temperatures of the bars calculated accordingly, we can, by an accurate delineation of the curve of heat corresponding to the warmer bar, find the value y of the temperature of the warmer bar at the place of contact.

The mean of the present and other series of experiments is found to be,—

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* *Vide* Poisson's *Théorie mathématique de la Chaleur.* If y and y_1 be the temperatures of the warmer and colder bars at their place of contact, and if r be a constant dependent upon the resistance to passage, and different for different metals, the quantity of heat, w, which passes in a unit of time through the point of contact is

$$w=\frac{1}{r}(y-y_1).$$

If the difference of temperature $y-y_1=0$, the constant of the resistance of passage r must also be equal to 0, for otherwise the quantity of heat passing through the point of contact would be $w=0$.

	y_1.	y.
Silver-Iron	100	111·5
Copper-German silver .	100	115
Copper-Iron	100	107·5
Copper-Copper . . .	100	100
Brass-Silver	100	99

According to this, a difference of temperature and a resistance of passage are only observed when heat passes from a better to a worse conducting metal. No such difference was observed in the passage of heat from a worse to a better conductor, nor in the passage through the place of contact of two metal rods of the same material.

These contradictions suggested some deficiency in the method of observation.

If the thermo-electric element be applied to equally warm points of heated bars of different conductibilities, the heat of the point touched communicates itself to the element. Without regarding the difference which results from the difference in the specific heats of both bars, more heat will pass to the part cooled by contact with the thermo-electric element in the case of the better than of the worse conducting bar. The first, therefore, will appear comparatively warmer. This error may cause the differences at the places of contact noted in the above Table, which are the more prominent the greater the difference between the conductibilities of the two metals. The warmer the place of junction is, the more these differences are seen, on which account they scarcely appear in the passage of heat from a worse to a better conducting metal, where the latter ought, properly, to appear warmer than the former.

This error can, however, exercise no disturbing influence on the relative conductibilities of the bars themselves. For on the supposition that the conductibilities of the bars do not vary at different temperatures, the changes of temperature at the several points of the same bar, caused by the application of the thermo-electric element, must be proportional to the temperatures themselves.

Hence the relative proportion of the temperatures observed at different parts of the bar remains unchanged, and this is alone decisive in calculating the relative conductibilities.

It was necessary, in obtaining reliable results as to the passage of heat, to make the loss of heat of the bars on the application of the thermo-electric element as small as possible. For this purpose the following arrangement of the experiments was made.

Round bars of different metals, of 13·2 millims. diameter and 157 millims. long, were made, and accurately ground plane at one end. A bar of iron and a bar of copper of the same diameter, but 666 millims. long, and a bar of bismuth equally thick but 265 millims. long, were ground plane at one end. These bars were then placed in a wooden frame with their polished ends free, &c., and the arrangement made, that to the plane end of each of the longer bars the plane end of one of the shorter was firmly screwed. At a distance of 2 millims. from the place of contact, and thence at distances of 21·4 millims., holes of 0·9 millim. wide and 8 millims. deep were bored. These holes were filled with oil. The end of each shorter bar was surrounded by a tin case, through which steam was passed for a long time. A screen protected the rest of the apparatus from the rays of heat. The whole apparatus was placed in a tin vessel surrounded by water, so that the loss of heat during the experiment should be as uniform as possible. After the apparatus had been constantly heated for two or three hours, and the heat in the joined bars had become constant, the temperatures were determined by sinking the thermo-electric element in the holes. The thermo-electric element was needle-formed, and consisted of two wires of iron and german silver, 0·3 millim. thick., placed together, but only soldered just at the end where they dipped in the holes. At the other ends copper wires were soldered, which communicated with the spirals of the galvanometer described in an earlier treatise*. The whole of the thermo-electric element, with the exception of the points dipping in the holes, was fastened in a glass tube with sealing-wax, which was cemented in a vessel of water in order to keep the posterior ends of the element at a constant temperature.

The following are some of the results obtained with the apparatus. Under *x* the series of holes is given, beginning with that nearest the place of junction. The holes corresponding to the warmer bars, which are first in the list, are marked with +, and the colder with −. Under *t* stand the corresponding temperatures. Each series contains the mean of several successive observations.

	Copper-Bismuth.	Zinc-Bismuth.	Copper-Iron.		Tin-Iron.		Iron-Iron.	Iron-Copper.
			I.	II.	I.	II.		
x.	*t.*	*t.*	*t.*	*t.*	*t.*	*t.*	*t.*	*t.*
+4	268·7	274·7	246·5	255·5	184·5	196·6	193	185
+3	261·2	252·5	235·7	243·7	155·5	161	158	—
+2	256	236·7	226	234	130	133·6	132	111·3
+1	252	226·7	218	226	107	110	108	81·5
−1	236·0	212	214·5	221	102·5	105	103·2	78·7
−2	120	108	178	181·2	84·5	86·8	85	74·7
−3	58·7	57·5	149·5	153	72	73·8	70·2	71
−4	32·2	33·5	126	130	61	63·5	59	67·5
−5	—	21·2	106	—	51·2	53·1	—	—

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* Poggendorff's *Annalen*, vol. lxxxix. p. 504.

If from these numbers the temperatures y and y_1 of the bars at their points of contact be calculated, we shall get—

	y.	y_1.	$y-y_1$.
Copper-Bismuth . .	252	252	0
Zinc-Bismuth . . .	226	226	0
Copper-Iron I. . .	217·7	217·5	0·2
Copper-Iron II. . .	225·5	225	0·5
Tin-Iron I. . . .	105	104·5	0·5
Tin-Iron II. . . .	108	107·1	0·9
Iron-Iron	105·5	105·1	0·4
Iron-Copper . . .	79·2	79·2	0

The difference of temperature of two bars in contact is hence very small. It is not greater in the case of two bars of different metals, whether the heat pass from the better to the worse conductor or the reverse, than in the case of two bars of the same metal.

Hence a resistance to the passage of heat through the place of contact of two bars cannot be proved within the limits of these observations*.

If the two bars are not perfectly in contact, a decrease of temperature at their place of contact is observed. The same happens when the bars are separated by a thin layer of some bad conductor.

Experiments made in this manner gave the following series of observations:—

	I.	II.	III.	IV.
	Zinc-Iron not fitting well on each other.	Iron-Copper not fitting well on each other.	Copper-Iron separated by a thin sheet of paper.	Zinc-Bismuth separated by a thin sheet of paper.
x.	t.	t.	t.	t.
+4	174·5	155·5	262	267
+3	153·5	125	253·7	245
+2	133·7	97	246	230
+1	125	71·7	239	220
−1	120	65·5	189·2	197
−2	99·7	61·5	157·2	100
−3	87·7	58	132·5	52
−4	71·5	55·1	113	29
−5	61	—	95·2	18

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* For an accurate calculation of the relative conductibilities of different metals the numbers in question are not fitted, since the quotient q of the sums of the temperatures of two points divided by the temperature of the middle point differs too little from 2. Hence a small error of observation changes materially the calculated result.

By calculating the temperatures y and y_1 of the warmer and colder bars corresponding to the places of contact, we have—

	y.	y_1.	$y-y_1$.
Zinc-Iron (not well fitted on each other)	123·5	122·2	1·3
Iron-Copper do.	68·4	66·1	2·3
Copper-Iron (separated by paper)	238·5	192·5	46
Zinc-Bismuth do.	219	211	8

These differences do not necessitate the existence of a resistance of passage. When the conductibility of the united bars at their place of junction is lessened by imperfect contact, or by the insertion of a piece of paper, a considerable decrease of temperature takes place at those points, by which a difference between the temperatures of the two bars at the place of contact could be easily explained.

LIV. *On the Principle of Isorrhopic Axes in Statics.*

By W. J. Macquorn Rankine, *C.E., F.R.SS.L. & E.*[*]

THEOREM. *Every equilibrated system of forces applied to a connected system of points, is capable of resolution into three rectangular independently equilibrated systems of parallel forces applied to the same points.*

Demonstration. Assume any set of rectangular axes, to which reduce the forces and the positions of their points of application; and let X, Y, Z be the components of the force applied to any point (x, y, z).

Let

$$\Sigma.\mathrm{X}x=\mathrm{A};\quad \Sigma.\mathrm{Y}y=\mathrm{B};\quad \Sigma.\mathrm{Z}z=\mathrm{C};$$

$$\Sigma.\mathrm{Y}z=\Sigma.\mathrm{Z}y=\mathrm{L};\quad \Sigma.\mathrm{Z}x=\Sigma.\mathrm{X}z=\mathrm{M};\quad \Sigma.\mathrm{X}y=\Sigma.\mathrm{Y}x=\mathrm{N}.$$

Conceive the surface of the second order whose equation is

$$\mathrm{A}x^2+\mathrm{B}y^2+\mathrm{C}z^2+2\mathrm{L}yz+2\mathrm{M}zx+2\mathrm{N}xy=\text{constant}.$$

Then if the forces and the positions of their points of application be reduced anew to the principal axes of that surface, we shall have

$$\mathrm{L}=0,\quad \mathrm{M}=0,\quad \mathrm{N}=0;$$

and consequently each of the three systems of component forces parallel to those three principal axes will be *in equilibrio*, independently of the other two systems. Q. E. D.

The above theorem, so far as I am aware, is new.

Glasgow, Oct. 26, 1855.

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[*] Communicated by the Author.

LV. *On the Composition and Phosphorescence of Plate-sulphate of Potash. By* Dr. FREDERICK PENNY, *Professor of Chemistry, Andersonian University, Glasgow*[*].

THE subject of the present notice is a technical product from kelp. It is called "Plate-sulphate of Potash," and is so named from the circumstance of its being crystallized in thick plates or slabs, consisting of the aggregrated layers of successive crops of crystals.

It may be regarded as one of the chemical products peculiar to Glasgow, being manufactured only at one or two other places, and even there to a limited extent.

There are several points in the chemical history of this salt possessing a high degree of scientific interest. The most important are, the peculiarity of the process by which it is obtained in thick slabs or plates, the brilliant phosphorescence of its crystals at the time of their formation, and its remarkable composition. To these at least I shall limit my remarks in the present notice.

I. *Formation of the Plate-sulphate.*

In working kelp upon the large scale, several saline products are obtained, and among these there are two salts, containing sulphate of potash as the predominating ingredient. One is powdery or granular, and is distinguished as *soft* or *granulated* sulphate of potash, and the other is the *plate*-sulphate.

Soft sulphate is obtained by simply evaporating or concentrating the kelp-leys, when it falls as a heavy granular precipitate. It is a very impure substance, being largely intermixed with sulphate of soda, common salt, carbonate of soda, sulphocyanide of potassium, iodide of sodium, and with insoluble matter and water. The amount of sulphate of potash existing in soft sulphate is very variable. That from cut-weed kelp seldom contains more than 32 per cent. Good drift-weed kelp yields a sulphate not usually under 44 per cent., and occasionally as high as 65 per cent. It is used in making plate-sulphate, and for agricultural manures, particularly when of low strength.

Plate-sulphate is made in two ways: either from the impure soft sulphate, or directly from the liquors that result by steeping kelp in hot water. The process is called "plating," and the mode of working is peculiar. In the ordinary process of crystallizing salts by solution in water, each crop of crystals, when fully formed, is removed from the crystallizing vat or vessel after the mother-liquor has been run off; a second or third crop being obtained by evaporating the mother-liquor, or by dissolving in

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[*] Communicated by the Author.

it, when heated, a further quantity of the salt. In plating sulphate of potash, a different mode of proceeding is adopted. The soft sulphate is dissolved either in weak ley from kelp, or in boiling water, until the liquor is about 44° of Twaddell. This is evaporated to 48° or 50°, and run into coolers to crystallize. When the crystals have fully formed and the mother-liquor has completely cooled, the latter is siphoned off, and being saturated with soft sulphate as before, it is again transferred to the coolers, and a second crop of crystals deposited on the first. These operations are repeated several times, and in this way from four to six, or even seven, successive and cohering crops are obtained in the form of a hard thick cake. The number of crops deposited depends on the success of the process, the richness of the soft sulphate, and the thickness of cake required.

In plating directly from kelp, the leys, or liquors obtained from steeping the kelp in water, are boiled down till the specific gravity of the liquor indicates that sulphate of potash is beginning to fall. The fires are then drawn, and after a brief reposure, to allow the floating impurities to subside, the liquor is run into the cooler and crystallized. In a few days a crop of plate-sulphate is found adhering to the sides. The mother-liquor is siphoned off, and a fresh quantity of concentrated kelp-ley being run into the cooler, a second crop of crystals is deposited on the surface of the first. A succession of crops is thus accumulated until the plate or cake is considered of sufficient thickness.

From this brief description of the process of plating, it is manifest that the salt, whether made from kelp-leys or soft sulphate, must be more or less charged with impurities. The appearance of the cakes, indeed, shows that it is far from being a chemically pure substance; and on examining it, the presence of minute quantities of carbonate of soda, common salt, and insoluble matter, may be readily detected. The large and well-defined crystals studded over the surface of the cakes are, however, nearly pure; and when carefully selected, contain very little foreign matter.

II. *Its Phosphorescence.*

The brilliant and sparkling light emitted from this salt at the time of crystallizing has been frequently observed. It may be witnessed by simply looking into a vat of liquor in process of cooling and crystallizing, and particularly when the temperature of the liquor is about 100° F. It is not a continuous luminosity or phosphorescence of the crystalline mass, but an emission of vivid sparks or scintillations as the crystals separate from the liquor, analogous to the phosphorescence of arsenious acid in the act of crystallizing from the hydrochloric acid.

The luminosity of salts in the act of crystallizing, and especially of sulphate of potash, both pure and mixed with various salts of soda, has been made the subject of repeated experimental investigation by several chemists. The extended and interesting researches by Rose* must be generally known. In Gmelin's 'Chemistry' we have a condensed but faithful account of the results observed by different experimenters. The subject is still involved in considerable obscurity.

I regret that I am as yet unable to give a satisfactory explanation of the phænomena in regard to the salt now under consideration, having reluctantly been obliged, by an enforced relaxation from all such work, to discontinue for a time the experiments undertaken to elucidate the subject. I may mention, however, the principal phænomena that have been observed in the trials already made.

1st. The phosphorescence is not visible so long as the temperature of the crystallizing liquor is much above 100° F.; but as the temperature falls below this, the scintillations become vivid and frequent.

2nd. When a rod of glass, of wood, or of metal, is drawn across the surface of the crystalline mass below the liquor, a line of brilliant light is produced in the track of the rod.

3rd. On breaking the layer of crystals that frequently forms on the surface of the liquor, vivid sparks are emitted as the detached portions sink to the bottom of the vessel.

4th. On plunging a mass of the crystals into hot liquor and immediately withdrawing it, a few scintillations are visible; but if the crystals are kept in the hot liquor for a few minutes, and then plunged into nearly cold liquor, the flashes are very brilliant, and diffused over the entire surface of the mass.

5th. The most beautiful effect, however, is produced when a quantity of moderately warm liquor is dashed upon a crop of crystals in a vat, from which the mother-liquor has some hours previously been drawn off. The crystals become instantaneously illuminated with myriads of beautiful and vivid coruscations. A second quantity of the same heated liquor thrown upon the mass gives a much less brilliant effect, and a third quantity scarcely any flashes. But if a bucket of perfectly cold liquor is dashed upon the crystals immediately after the hot liquor has ceased to give scintillations, the result is even more splendid and striking than in the last experiment.

When the heated liquor is dashed upon crystals of chloride of potassium, or upon wood, or iron, or ice, no visible effect is produced.

6th. In recrystallizing the salt, no phosphorescence is visible.

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* Poggendorff's *Annalen*, vol. lii.

The results of these few experiments, though affording very little insight into the cause of the phænomena, plainly indicate that the emission of light takes place within a limited range of temperature, and that actual contact of the crystals with the crystallizing liquor is essential. Agitation of the liquor with a rod produces no effect.

III. *Composition.*

I have already stated that the composition of this salt is remarkable. It has hitherto been viewed and described as a peculiar form or variety of neutral sulphate of potash, and as such it is regarded both by chemists and manufacturers. The results of my analyses do not confirm this view of its composition. They show unequivocally, that, instead of being a mere crystalline modification of the neutral sulphate, it is a double salt, consisting of sulphate of potash and sulphate of soda. I have analysed bold and well-defined crystals, carefully selected from numerous specimens obtained from different makers, and I find that they are perfectly uniform in composition. I have also analysed crystals of the same salt, procured by recrystallizations of the commercial salt.

In these analyses it never gave more than 42·5 per cent. of potash; and parties acquainted with this article commercially, know that in the bulk it never contains more than from 75 to 78 per cent. of sulphate of potash,—equal to about 41·5 per cent. of potash, whereas sulphate of potash contains 54 per cent.

It is not, I conceive, either desirable or necessary to give the details of the methods of analysis, or particulars of the several results. It will be sufficient to state, that the sulphuric acid was determined, as usual, in the state of sulphate of baryta, the potash by means of bichloride of platinum, and the soda indirectly by deduction. The only precaution requiring notice is, that the precipitated sulphate of baryta must be well washed *after ignition*, to remove the potash which is carried down with the first precipitate.

The mean of twelve analyses, the results of which accord very closely, gave the following proportion of potash, sulphuric acid and soda, in 100 parts:—

	Experiment.	Theory.
Potash	42·22	42·47
Sulphuric acid . .	48·24	48·19
Soda	9·54	9·34
	100·00	100·00

A carefully selected specimen gave 42·46 per cent. of potash.

These proportions correspond to the formula $3KO\ SO^3, NaO\ SO^3$, as shown in the third column of the above statement.

The presence of soda in the salt was easily established by decomposing a known quantity with nitrate of baryta, and crystallizing the solution filtered from the precipitate. Crystals of nitrate of soda mixed with those of nitre were abundantly deposited. In another experiment, a known quantity of the salt dissolved in water was decomposed by chloride of barium, and the solution, separated from the precipitate by filtering, was evaporated to dryness; and the mixed chlorides, after treatment with bichloride of platinum, were tested with the delicate and beautiful process for the detection of soda proposed by Dr. Andrews at the Meeting of the British Association in Belfast*.

It will be at once remarked, that the composition of this salt does not correspond to that of any other double salt of potash. The atomic proportions of its ingredients are at variance with our ordinary notions of the constitution of these compounds. I hope, however, to be able to explain the anomaly by the examination of other compounds of sulphate of potash, and of chromate of potash, which I have in process of preparation.

In Gmelin's 'Chemistry†,' it is incidentally mentioned that 1 part by weight of dry carbonate of soda, and 2 parts of bichromate of potash, give crystals having the same form as sulphate of potash, and containing $3KO\ CrO^3, NaO\ CrO^3$, which corresponds exactly with plate-sulphate of potash. It is also stated that crystals containing *about* $2KO\ SO^3, NaO\ SO^3$, may be prepared by fusing equal atomic proportions of the two sulphates, and crystallizing the mixture from water. Rose made several analyses of the salt, obtained by crystallizing together the sulphates of potash and soda; but the discrepancies in the results led him to doubt the definite composition of the crystals.

Dr. Gladstone‡ has made a series of experiments with a view to produce a definite double sulphate of potash and soda. He crystallized various mixtures of the two sulphates, and obtained a salt to which he ascribed the formula $5KO\ SO^3, NaO\ SO^3$. Such a compound would contain 46·44 per cent. of potash, and 47·43 of sulphuric acid.

In several of its physical characters, the plate-salt differs essentially from neutral sulphate of potash. It is, for example, more readily fusible, and it may be melted without much difficulty in a platina crucible with the ordinary gas-blowpipe; whereas sulphate of potash is only slightly softened at the edges, even when the blast from the bellows is urged to the fullest extent; it requires the full bright red heat of a furnace for perfect fusion.

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* Chemical Gazette, vol. x.

† Vol. i. p. 108.

‡ Quarterly Journal, vol. vi.

The density of the double sulphate is also greater than that of sulphate of potash. The specific gravity of its crystals, finely pulverized, taken in oil of turpentine at 59° F., was found to be 2·6682. After being fused, its specific gravity was 2·6708. The specific gravity of sulphate of potash, taken in the same way and at the same time, was in the crystallized state 2·6438; and in the vitreous or fused state, 2·6567. Dr. Lyon Playfair gives 2·644 as the specific gravity of sulphate of potash.

There is also a marked difference in the solubility of this salt in cold and boiling water, as compared with that of sulphate of potash. 100 parts of a saturated solution of $KO\ SO^3$, at 217° contain 22·5 parts of salt, or 100 parts of water at that temperature dissolve 29 parts; 100 parts of the solution of plate-salt at 217°·5 contain 28·9 parts, or 100 water will dissolve nearly 40·8 parts.

It is, I think, more than probable that certain specimens which have been figured as sulphate of potash, were crystals of this double salt. I have crystallized sulphate of potash under various circumstances, but have never obtained crystals in the forms figured by Phillips*, Brooke†, and others, unless a salt of soda has been present in the solution, causing, as I believe, the formation of the double salt. Both upon the large and small scale, sulphate of potash was deposited either in rhomboidal prisms or in hexahedral prisms, with bipyramidal summits, or in bipyramidal dodecahedrons.

In conclusion, I may mention that I have examined the successive crops of crystals obtained by recrystallizing the plate-sulphate. Several pounds of the salt were dissolved in water, and treated as usual, the crystallizing operations being continued to the 11th crop. The results were as follows:—

1*st Crop*. Sulphate of potash with a little plate-salt.
2*nd Crop*. Plate-salt, with short prisms of sulphate of soda.
3*rd Crop*. Chiefly sulphate of soda; crystals large and bold.
4*th Crop*. Layer of plate-salt with sulphate of soda above.
5*th*, 6*th* and 7*th Crops*. Sulphate of soda.
8*th*, 9*th* and 10*th Crops*. Almost wholly plate-salt.
11*th Crop*. Sulphate of soda.

It is therefore quite manifest, that, with care, this salt can be produced at pleasure by recrystallization from the commercial salt, or by crystallizing a mixture of sulphate of potash and sulphate of soda in equal atomic proportions.

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* Ann. Phil. 1822.

† Ibid. 1824.

LVI. *On the Theory of Diamagnetism. Letter from* Professor WEBER *to* Prof. TYNDALL.

MY DEAR SIR, Göttingen, Sept. 25, 1855.

ACCEPT my best thanks for your kind communication of the 3rd of September; I am gratified to learn that the apparatus executed by M. Leyser in Leipzig for the demonstration of diamagnetic polarity has so completely fulfilled your expectations. This intelligence is all the more agreeable to me, inasmuch as before the apparatus was sent away, it was not in my power to go to Leipzig and test the instrument myself.

It gave me great pleasure to learn that Mr. Faraday and M. De la Rive have had an opportunity of witnessing the experiments, and of convincing themselves as to the facts of the case.

It was also of peculiar interest to me to learn that you had succeeded in establishing the polarity of the self-same heavy glass with which Faraday first discovered diamagnetism. This is the best proof that these experiments do not depend upon the conductive power of bismuth for electricity.

I have read with great interest your memoir "On the Diamagnetic Force," &c. contained in the Philosophical Transactions, vol. cxlv. It has been your care to separate the *fact* of diamagnetic polarity from the *theory*, and to place the former beyond the region of doubt. Allow me, with reference to this subject, to direct your attention to a passage at page 39 of your memoir, which you adduce as a conclusion from my theory; the passage runs as follows:—

"The magnetism of two iron particles in the line of magnetization is increased by their reciprocal action; but, on the contrary, the diamagnetism of two bismuth particles lying in this direction is diminished by their reciprocal action."

This proposition is by no means a necessary assumption of my theory, but is rather a direct consequence of diamagnetic polarity, if the facts be such as both you and I affirm them to be. What, therefore, you have adduced against the above conclusion must be regarded as an argument against diamagnetic polarity itself. The *diamagnetic reciprocal action* of the bismuth particles in the line of magnetization is necessarily opposed to the *action of the exciting magnetic force*. The latter must be enfeebled, because the diamagnetic is opposed to the *magnetic reciprocal action* of iron particles which lie in the line of magnetization, through which latter it is known the action of the exciting magnetic force is increased. Hence also the *modification* produced in bismuth by magnetic excitement, whatever it may be, must be weakened, because the force of excitation is diminished.

(I believe, however, that this argument against diamagnetic polarity may also be surmounted. The phænomenon which you have observed must be referred to other circumstances, also connected with the compression of the bismuth. For the diamagnetic reciprocal action is, as I have shown, much too weak to produce an effect which could be compared in point of magnitude with the reciprocal action produced in the case of iron.)

I take this opportunity of adding a few remarks for the purpose of setting my theory of diamagnetic polarity in a more correct light.

My theory assumes:—1, that the fact of diamagnetic polarity is granted; 2, that in regard to magnetic phænomena, Poisson's theory of two magnetic fluids, and Ampère's theory of molecular currents, are equally admissible. Whoever denies the first fact, or rejects the theory of Ampère, cannot, I am ready to confess, accept my theory.

But supposing that you do not reject Ampère's theory of permanent molecular currents, but are disposed to enter upon the inner connexion and true significance of the theory, you will easily recognize that it is by no means *an arbitrary assumption of mine*, that in bismuth molecular currents are excited, when the exciting magnetic force is augmented or diminished; but that the excitation of such molecular currents is *a necessary conclusion from the theory of Ampère*, which conclusion Ampère himself could not make, because the laws of voltaic induction, discovered by Faraday, were unknown to him. In all cases where molecular currents *exist*, by increase or diminution of the magnetic exciting force *molecular currents must be excited*, which either add their action to, or subtract it from, the action of those already present.

Finally, permit me to make a few remarks on the following words of your memoir:—

"To carry out the assumption here made, M. Weber is obliged to suppose that the molecules of diamagnetic bodies are surrounded by channels, in which the induced currents, once excited, continue to flow without resistance."

The assumption of channels which surround the molecules, and in which the electric fluids move without resistance, is an assumption contained in the theory of Ampère, and is by no means added by me for the purpose of explaining diamagnetic polarity. *A permanent molecular current without such a channel involves a manifest contradiction*, according to the law of Ohm.

I may further observe, that I do not wonder that you regard a theory which is built upon the assumption of such channels, as "so extremely artificial that you imagine the general conviction of its truth cannot be very strong." In a certain sense I quite agree with you, but I only wish to convince you that this objection applies really to the theory of Ampère, and only applies to mine in so far as it is built upon the former. (You may perhaps find less ground for objecting to the speciality of such an assumption, if you separate the simple fundamental conception, which recommends itself particularly by a certain analogy of the molecules to the heavenly bodies in space, from those additions which Ampère was forced to make, in order to apply the mathematical methods at his command, and to make the subject one of strict calculation. He was necessitated to reduce the case to that of *linear* currents, which necessarily demand channel-shaped bounds, if every possibility of a lateral outspreading is to be avoided.)

To place my theory of diamagnetic polarity in a truer light, I am anxious also to convince you that this theory is by no means based upon new assumptions (hypotheses); but that it only rests upon such conclusions as may be drawn from the theory of Ampère, when the laws of voltaic induction discovered by Faraday, and the laws of electric currents by Ohm, are suitably connected with it. I affirm, that, even if Faraday had not discovered diamagnetism, by the combination of Ampère's theory with Faraday's laws of voltaic induction, and Ohm's laws of the electric current, as shown in my memoir, the said discovery might possibly have been made.

In respect, however, to the artificiality of the theory of Ampère, I hope that mathematical methods may be found whereby the limitation before mentioned to the case of linear currents may be set aside, and with it the objection against channel-form beds. All our molecular theories are still very artificial. I for my part find less to object to in this respect in the theory of Ampère than in other artificialities of our molecular theories; and for this reason, that in Ampère's case the nature of the artificiality is placed clearly in view, and hence also a way opened towards its removal.

To Mr. Faraday I beg of you to present my sincerest respect.

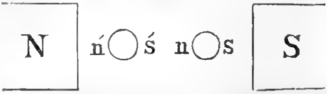
Believe me, dear Sir,

Most sincerely yours,

WILHELM WEBER.

Professor Tyndall.

The foregoing letter possesses more than a private interest, and I have therefore laid it before the readers of the Philosophical Magazine. On one point in it only would I ask permission to make a remark, and that is the proposition, that the diminution of the excitement of a row of bismuth particles in the line of magnetization by their reciprocal action is *"a direct consequence of diamagnetic polarity."* M. Weber (I believe) founds this proposition on the following considerations:—Let a series of bismuth particles lie in the axial line between the magnetic poles N and S: the polarity excited in these particles by the direct action of the poles will be that shown in the figure, being the reverse of that of iron particles under the same circumstances. But as the end *n* of the right-hand particle tends to excite a magnetism like its own in the end *s*′ of the left-hand particle, and *vice versâ*, this action is opposed to that of the magnet, and hence the magnetism of such a row of particles is enfeebled by their reciprocal action.



Now it appears to me that there is more assumed in this ingenious argument than experiment at present can bear out. There are no experimental grounds for the assumption, that what we call the north pole of a bismuth particle exerts upon a second bismuth particle precisely the same action that the north pole of an iron particle would exert. Magnetized iron repels bismuth; but whatever the *fact* may be, the *conclusion* is scarcely warranted, that *therefore* magnetized bismuth will repel bismuth. Supposing it were asserted that magnetized iron attracts iron and repels bismuth, while magnetized bismuth attracts bismuth and repels iron, would there be anything essentially impossible, self-contradictory, or absurd involved in the assertion? I think not. And yet if even the possible correctness of such an assertion be granted, the proposition above referred to becomes untenable. It will be observed that it is against a conclusion rather than a fact that I contend. With regard to the fact, I should be sorry to express a positive opinion; for this is a subject on which I am at present seeking instruction, which may lead me either to M. Weber's view or the opposite. Be that as it may, the result cannot materially affect the respect I entertain for every opinion emanating from my distinguished correspondent on this and all other scientific subjects.

J. T.

LVII. *On the Hypothesis of Molecular Vortices, or Centrifugal Theory of Elasticity, and its Connexion with the Theory of Heat.* By WILLIAM JOHN MACQUORN RANKINE, *C.E., F.R.SS.L. & E., F.R.S.S.A. &c.*

[Concluded from p. 363.]

SECTION SECOND.—*Relations between Heat and Expansive Power.*

(9.) *VARIATIONS of Sensible and Latent Heat: Fundamental Equation of the Theory.*—If the forms, positions, and magnitudes of the paths described by the revolving particles of the atomic atmospheres be changed, whether by a variation of mean density, or by a variation of temperature, an increase or diminution of the *vis viva* of their motion, that is to say, of the heat of the body, will take place in virtue of that change of the paths of motion; an increase when they are contracted, and a diminution when they are dilated.

Let $\delta . Q$ represent, when positive, the indefinitely small quantity of heat which must be communicated to unity of weight of a substance, and when negative, that which must be abstracted from it, in order to produce the indefinitely small variation of temperature $\delta\tau$ simultaneously with the indefinitely small variation of volume δV. Let $\delta . Q$ be divided into two parts,

$$\delta Q + \delta Q' = \delta . Q,$$

of which δQ, being directly employed in varying the *velocity* of the particles, is the variation of the *actual* or *sensible* heat possessed by the body; while $\delta Q'$, being employed in varying their *orbits*, represents the amount of the mutual transformation of heat with expansive power and molecular action, or the variation of what is called the *latent* heat; that is to say, of a molecular condition constituting a source of power, out of which heat may be developed.

The variation of sensible heat has evidently this value,

$$\delta Q = \mathfrak{k}\delta\tau. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (22)$$

Let $\delta x, \delta y, \delta z$ be the displacements of the orbit of the particles of atomic atmosphere at the point (x, y, z). A molecule $\rho dx\,dy\,dz$ is acted upon by the accelerative forces (see equation 3A),

$$-2Q\frac{d\phi}{dx}; \quad -2Q\frac{d\phi}{dy}; \quad -2Q\frac{d\phi}{dz};$$

parallel to the three axes respectively.

The sum of the actions of those forces on the molecule $\rho dx\,dy\,dz$ during the change of temperature and volume, is

$$-2\mathrm{Q}\left(\frac{d\phi}{dx}\,\delta x+\frac{d\phi}{dy}\,\delta y+\frac{d\phi}{dz}\,\delta z\right)\rho dx\,dy\,dz$$
$$=-2\mathrm{Q}\delta\phi\rho dx\;dy\;dz.$$

The sum of such actions upon all the particles in unity of weight is equal in amount, and opposite in sign, to the variation of latent heat; that is to say,

$$\delta\mathrm{Q}'=\frac{2\mathrm{Q}}{\mathrm{M}}\iiint_{(1)}\rho\delta\phi dx\;dy\;dz. \quad . \quad . \quad . \quad . \quad (23)$$

To determine the value of the variation $\delta\phi$, let it be divided into two parts, thus:—

$$\delta\phi=\delta\phi_1+\delta\Delta\phi,$$

where $\Delta\phi=\phi-\phi_1$.

First. With respect to $\delta\phi_1$, it is obvious, that because, according to equations (6, 7),

$$\mathrm{MV}=k\mathrm{MV}\int_{-\infty}^{\phi^1}e^{k\Delta\varphi}\,\frac{\psi}{\psi_1}\,d\phi,$$

we must have

$$\delta\mathrm{V}=k\mathrm{V}\delta\phi_1 \text{ and } \delta\phi_1=\frac{\delta\mathrm{V}}{k\mathrm{V}}.$$

Hence the first part of the integral (23) is

$$\frac{2\mathrm{Q}}{\mathrm{M}}\,\delta\phi_1\iiint_{(1)}\rho dx\,dy\,dz=\frac{2\mu\mathrm{Q}}{\mathrm{M}k\mathrm{V}}\,.\,\delta\mathrm{V}$$
$$=\frac{h\mu}{\kappa\mathrm{M}}(\tau-\kappa)\frac{\delta\mathrm{V}}{\mathrm{V}}=\mathrm{N}\mathfrak{k}(\tau-\kappa)\frac{\delta\mathrm{V}}{\mathrm{V}}. \quad . \quad (23\mathrm{A})$$

To determine the second part of the integral, we have the condition, that the quantity of atomic atmosphere enclosed within each surface at which $\Delta\phi$ has some given value is invariable; that is to say,

$$\left(\delta\Delta\phi\frac{d}{d\Delta\phi}+\delta\mathrm{V}\frac{d}{d\mathrm{V}}+\delta\tau\,\frac{d}{d\tau}\right)\left(k\rho_1\mathrm{MV}\int_{-\infty}^{\varphi}\epsilon^{k\theta\Delta\varphi}\,\frac{\omega}{\omega_1}\,d\phi\right)=0.$$

Hence

$$\delta\Delta\phi=\frac{-\left(\delta\mathrm{V}\frac{d}{d\mathrm{V}}+\delta\tau\,\frac{d}{d\tau}\right)\left(k\rho_1\mathrm{MV}\int_{-\infty}^{\varphi}e^{k\theta\Delta\varphi}\,\frac{\omega}{\omega_1}\,d\phi\right)}{k\rho_1\mathrm{MV}e^{k\theta\Delta\varphi}\,\frac{\omega}{\omega_1}}.$$

The value of the second part of the integral (23) is now found to be

$$\frac{2\mathrm{Q}}{\mathrm{M}}\iiint_{(1)}\rho\delta\Delta\phi dx\,dy\,dz=\frac{2\mathrm{Q}}{\mathrm{M}}k\rho_1\mathrm{MV}\int_{-\infty}^{\varphi_1}e^{k\theta\Delta\varphi}\,\frac{\omega}{\omega_1}\delta\Delta\phi d\varphi$$
$$=-\frac{2\mathrm{Q}}{k\mathrm{M}}\left(\delta\mathrm{V}\,\frac{d}{d\mathrm{V}}+\delta\tau\frac{d}{d\tau}\right)\left\{\rho_1\mathrm{MV}\int_{-\infty}^{k\varphi_1}\int_{-\infty}^{k\varphi}e^{k\theta\Delta\varphi}\,\frac{\omega}{\omega_1}k^2d\phi^2\right\}.$$

In the double integral, let $\lambda = \log_e V$ be put for $k\phi$, G for ω, and H for the single integral, as in equation (9). Then the double integral becomes

$$\frac{1}{G_1}\int_{-\infty}^{\lambda_1} H\,d\lambda = -\frac{1}{G_1}\frac{dH_1}{d\theta} \text{ by equation (10)}$$

$$= -\frac{\kappa}{G_1}\frac{dH_1}{d\tau}.$$

Also because $\rho_1 MV = \frac{\mu G_1}{H_1}$ by equation (9), and $\frac{2Q}{kM} = \frac{h}{\kappa}(\tau-\kappa)$, the second part of the integral (23) is found to be

$$\frac{h\mu}{M}(\tau-\kappa)\left(\delta\tau\frac{d}{d\tau}+\delta V\frac{d}{dV}\right)\frac{dH_1}{H_1 d\tau}. \quad . \quad . \quad (23\text{ B})$$

Hence, adding together (23 A) and (23 B), we find for the total variation of latent heat,

$$\delta Q' = \frac{h\mu}{M}(\tau-\kappa)\left\{\delta\tau.\frac{d^2\log_e H_1}{d\tau^2}+\delta V.\left(\frac{1}{\kappa V}+\frac{d^2\log_e H_1}{d\tau dV}\right)\right\}. \quad (24)$$

To express this in terms of quantities which may be known directly by experiment, we have by equations (10) and (9),

$$\frac{dH_1}{H_1 d\lambda}+\theta-\frac{G_1}{H_1}=0,$$

that is to say,

$$\frac{d\log_e H_1}{dV} = \frac{G_1}{H_1 V}-\frac{\tau}{\kappa V} = \frac{M}{h\mu}p-\frac{\tau}{\kappa V};$$

and therefore

$$\log_e H_1 = \frac{M}{h\mu}\int p\,dV - \frac{\tau}{\kappa}\log_e V + f(\tau) + \text{ constant.}$$

$f(\tau)$ is easily found to be $= -\log_e \tau$ for a perfect gas, and being independent of the density, is the same for all substances in all conditions; hence we find (the integrals being so taken that for a perfect gas they shall $=0$),

$$* \ \frac{d\log_e H_1}{d\tau} = \int\left(\frac{M}{h\mu}\frac{dp}{d\tau}-\frac{1}{\kappa V}\right)dV - \frac{1}{\tau}$$

$$\frac{d^2\log_e H_1}{d\tau^2} = \frac{M}{h\mu}\int\frac{d^2p}{d\tau^2}dV + \frac{1}{\tau^2}$$

$$\frac{d^2\log_e H_1}{d\tau dV} = \frac{M}{h\mu}\frac{dp}{d\tau}-\frac{1}{\kappa V};$$

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* This coefficient corresponds to $-\frac{U}{\kappa}$ in the notation of my previous paper on the Mechanical Action of Heat.

and therefore

$$\delta Q' = (\tau - \kappa)\left\{ \delta\tau \,.\left(\frac{h\mu}{M\tau^2} + \int \frac{d^2p}{d\tau^2}\,dV \right) + \delta V \,.\, \frac{dp}{d\tau} \right\} \quad . \quad (25)$$

is the variation of latent heat, expressed in terms of the pressure, volume, and temperature; to which, if the variation of sensible heat, $\delta Q = \mathfrak{k}\delta\tau$, be added, the complete variation of heat, $\delta Q + \delta Q' = \delta \,.\, Q$, in unity of weight of the substance, corresponding to the variations δV and $\delta\tau$ of volume and temperature, will be ascertained.

The only specific coefficient in the above formula has the following value:—

$$\frac{h\mu}{M} = N\mathfrak{k}\kappa. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (25\ A)$$

It is obvious that equation (25), with its consequences, is applicable to any mixture of atoms of different substances *in equilibrio* of pressure and temperature; for in that case τ, $\frac{dp}{d\tau}$, and $\frac{d^2p}{d\tau^2}$ are the same for each substance. We have only to substitute for $\frac{h\mu}{M}$ the following expression,—

$$n_1 \frac{h_1\mu_1}{M_1} + n_2 \frac{h_2\mu_2}{M_2} + \&c. = \kappa \,.\, \Sigma(nN\mathfrak{k}), \quad . \quad . \quad (25\ B)$$

where n_1, n_2, &c. are the proportions of the different ingredients in unity of weight of the mixture, so that $n_1 + n_2 +$ &c. $= 1$.

Equation (25) agrees exactly with equation (6) in the first section of my original paper on the Theory of the Mechanical Action of Heat. It is the fundamental equation of that theory, and I shall now proceed to deduce the more important consequences from it.

(10.) *Equivalence of Heat and Expansive Power.* Joule's *Law.*—From the variation of the heat communicated to the body, let us subtract the variation of the expansive power given out by it, or

$$P\delta V = \{p + f(V)\}\delta V.$$

The result is the variation of the total power exercised upon, or communicated to, unity of weight of the substance, supposing that there is no chemical, electrical, magnetic, or other action except heat and pressure; and its value is

$$\begin{aligned} \delta\Psi = \delta Q + \delta Q' - P\delta V = \delta\tau \,.\left\{ \mathfrak{k} + \frac{h\mu}{M}\left(\frac{1}{\tau} - \frac{\kappa}{\tau^2}\right) + (\tau - \kappa)\int \frac{d^2p}{d\tau^2}\,dV \right\} \\ + \delta V \,.\left\{ (\tau - \kappa)\frac{dp}{d\tau} - p - f(V) \right\}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (26) \end{aligned}$$

This expression is obviously *an exact differential,* and its integral is the following function of the volume and temperature:—

$$\Psi=\mathfrak{k}(\tau-\kappa)+\frac{h\mu}{\mathrm{M}}\left(\log_e\tau+\frac{\kappa}{\tau}\right)+\int\left\{(\tau-\kappa)\frac{dp}{d\tau}-p\right\}d\mathrm{V}$$

$$-\int f(\mathrm{V})d\mathrm{V}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (27)$$

Accordingly, the total amount of power which must be exercised upon unity of weight of a substance, to make it pass from the absolute temperature τ_0 and volume V_0 to the absolute temperature τ_1 and volume V_1, is

$$\Psi(\mathrm{V}_1,\tau_1)-\Psi(\mathrm{V}_0,\tau_0).$$

This quantity consists partly of expansive or compressive power, and partly of heat, in proportions depending on the mode in which the intermediate changes of temperature and volume take place; but the total amount is independent of these changes.

Hence, *if a body be made to pass through a variety of changes of temperature and volume, and at length be brought back to its primitive volume and temperature, the algebraical sum of the portions of power applied to and evolved from the body, whether in the form of expansion and compression, or in that of heat, is equal to zero.*

This is one form of the law proved experimentally by Mr. Joule, of the equivalence of heat and mechanical power. In my original paper on the Mechanical Action of Heat, I used this law as an axiom, to assist in the investigation of the Equation of Latent Heat. I have now deduced it from the hypothesis on which my researches are based; not in order to prove the law, but to verify the correctness of the mode of investigation which I have followed.

Equations (26) and (27), like equation (23), are made applicable to unity of weight of a mixture, by putting $\Sigma n\mathfrak{k}$ for $\mathfrak{k}$, and $\Sigma n\frac{h\mu}{\mathrm{M}}$ for $\frac{h\mu}{\mathrm{M}}$.

The train of reasoning in this article is the converse of that followed by Professor William Thomson of Glasgow, in article 20 of his paper on the Dynamical Theory of Heat, where he proves from Joule's law, that the quantity corresponding to $\delta\Psi$ is an exact differential.

(11.) *Mutual Conversion of Heat and Expansive Power.* Carnot's *Law of the Action of Expansive Machines.*—If a body be made to pass from the volume V_0 and absolute temperature τ_0 to the volume V_1 and absolute temperature τ_1, and be then brought back to the original volume and temperature, the total power exerted (Ψ) will have, in those two operations, equal arithmetical values, of opposite signs. Each of the quantities Ψ

consists partly of heat and partly of expansive power, the proportion depending on the mode of intermediate variation of the volume and temperature, which is arbitrary. If the mode of variation be different in the two operations, the effect of the double operation will be to transform a portion of heat into expansive power, or *vice versâ.*

Let (a) denote the first operation, (b) the *reverse* of the second. Then

$$\Psi_b=\Psi_a.$$

The terms of Ψ which involve functions of τ only, or of V only, are not affected by the mode of intermediate variation of those quantities. The term on which the mutual conversion of heat and expansive power depends is therefore

$$\int\left\{(\tau-\kappa)\frac{dp}{d\tau}-p\right\}d\mathrm{V}(b)=\int\left\{(\tau-\kappa)\frac{dp}{d\tau}-p\right\}d\mathrm{V}(a),$$

or

$$\int\left(\frac{d\mathrm{Q}'}{d\mathrm{V}}-p\right)d\mathrm{V}(b)=\int\left(\frac{d\mathrm{Q}'}{d\mathrm{V}}-p\right)d\mathrm{V}(a).$$

Hence

$$\int\frac{d\mathrm{Q}'}{d\mathrm{V}}d\mathrm{V}(a)-\int\frac{d\mathrm{Q}'}{d\mathrm{V}}d\mathrm{V}(b)=\int p\,d\mathrm{V}(a)-\int p\,d\mathrm{V}(b);$$

which last quantity is the amount of the *heat transformed into expansive power,* or the total latent heat of expansion in the *double operation.*

Let

$$\int\frac{dp}{d\tau}d\mathrm{V}=\int\frac{1}{\tau-\kappa}\cdot\frac{d\mathrm{Q}'}{d\mathrm{V}}d\mathrm{V}=\mathrm{F}.$$

Then, because

$$\frac{d\mathrm{Q}'}{d\mathrm{V}}d\mathrm{V}=(\tau-\kappa)d\mathrm{F},$$

we have

$$\int_{\mathrm{V}_0}^{\mathrm{V}_1}p\,d\mathrm{V}(a)-\int_{\mathrm{V}_0}^{\mathrm{V}_1}p\,d\mathrm{V}(b)=\int_{\mathrm{F}_0}^{\mathrm{F}_1}(\tau-\kappa)d\mathrm{F}(a)-\int_{\mathrm{F}_0}^{\mathrm{F}_1}(\tau-\kappa)d\mathrm{F}(b)$$

$$=\int_{\mathrm{F}_0}^{\mathrm{F}_1}{}_a-\tau_b)d\mathrm{F}=\int_{\mathrm{V}_0}^{\mathrm{V}_1}\frac{\tau_a-\tau_b}{\tau_u-\kappa}\frac{d\mathrm{Q}'_a}{d\mathrm{V}}d\mathrm{V}, \quad . \quad . \quad . \quad . \quad (28)$$

in which τ_u and τ_b are the pair of absolute temperatures, in the two operations respectively, corresponding to *equal values of* F*.

This equation gives a relation between the heat transformed into expansive power by a given pair of operations on a body, the latent heat of expansion in the first operation, and the mode

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* F is what has since been called a "Heat-potential," and with the addition of the term $\int\left(\frac{\mathfrak{k}}{\tau-\kappa}+\frac{h\mu}{\mathrm{M}\tau^2}\right)d\tau$, a "Thermo-dynamic Function." (1855.)

of variation of temperature in the two operations. It shows that the proportion of the original latent heat of expansion finally transformed into expansive power, is a function of the temperatures alone, and is therefore independent of the nature of the body employed.

Equation (28) includes Carnot's law as a particular case. Let the limits of variation of temperature and volume be made indefinitely small. Then

$$dp\,d\mathrm{V} = \frac{d\tau}{\tau-\kappa}\cdot\frac{d\mathrm{Q}'}{d\mathrm{V}}\,d\mathrm{V}\,;$$

and dividing by $d\tau\, d\mathrm{V}$,

$$\frac{dp}{d\tau} = \frac{1}{\tau-\kappa}\cdot\frac{d\mathrm{Q}'}{d\mathrm{V}}.$$

This differential equation is also an immediate consequence of equation (25).

If $\frac{\mu}{\mathrm{J}}$ be put for $\frac{1}{\tau-\kappa}$, and JM for $\frac{d\mathrm{Q}'}{d\mathrm{V}}$, it becomes identical with the equation by which Professor William Thomson expresses Carnot's law, as deduced by him and by M. Clausius from the principle, that *it is impossible to transfer heat from a colder to a hotter body, without expenditure of mechanical power.*

The investigation which I have now given is identical in principle with that in the fifth section of my paper on the Mechanical Action of Heat, but the result is expressed in a more comprehensive form.

Equation (28), like (25), (26) and (27), is applicable to a mixture composed of any number of different substances, in any proportions, provided the temperature, the pressure, and the coefficients $\frac{dp}{d\tau}$, $\frac{d^2p}{d\tau^2}$ are the same throughout the mass.

(12.) *Apparent Specific Heat.*—The general value of apparent specific heat of unity of weight is

$$\mathrm{K} = \frac{d\mathrm{Q}}{d\tau} + \frac{d\mathrm{Q}'}{d\tau} + \frac{d\mathrm{Q}'}{d\mathrm{V}}\cdot\frac{d\mathrm{V}}{d\tau} = \mathfrak{k} + (\tau-\kappa)$$
$$\left\{\frac{h\mu}{\mathrm{M}\tau^2} + \int\frac{d^2p}{d\tau^2}\,d\mathrm{V} + \frac{d\mathrm{V}}{d\tau}\cdot\frac{dp}{d\tau}\right\}. \quad . \quad . \quad (29)$$

agreeing with equation (13) of my previous paper.

The value in each particular case depends on the mode of variation of volume with temperature. Specific heat at constant volume is

$$\mathrm{K}_{\mathrm{V}} = \mathfrak{k} + (\tau-\kappa)\left(\frac{h\mu}{\mathrm{M}\tau^2} + \int\frac{d^2p}{d\tau^2}\,d\mathrm{V}\right). \quad . \quad . \quad (30)$$

When the pressure is constant, we must have

$$\frac{d\mathrm{P}}{d\mathrm{V}}\,d\mathrm{V} + \frac{dp}{d\tau}\,d\tau = 0\,;$$

and, consequently,

$$\frac{dV}{d\tau} = -\frac{\frac{dp}{d\tau}}{\frac{dP}{dV}};$$

therefore specific heat at constant pressure is

$$K_P = K_V + (\tau - \kappa)\frac{\left(\frac{dp}{d\tau}\right)^2}{-\frac{dP}{dV}}. \quad . \quad . \quad . \quad . \quad (31)$$

This agrees with equation (16) of Professor Thomson's paper, if $\frac{J}{\mu}$ in his notation $= \tau - \kappa$.

If the body be a perfect gas, then

$$\left.\begin{aligned} K &= \frac{V_0}{\tau_0}\left(\frac{kM}{2\mu} + \frac{\kappa}{\tau} - \frac{\kappa^2}{\tau^2} + \frac{\tau-\kappa}{V}\cdot\frac{dV}{d\tau}\right) = \mathfrak{k}\left\{1 + N(\tau-\kappa)\left(\frac{\kappa}{\tau^2} + \frac{dV}{Vd\tau}\right)\right\} \\ K_V &= \frac{V_0}{\tau_0}\left(\frac{kM}{2\mu} + \frac{\kappa}{\tau} - \frac{\kappa^2}{\tau^2}\right) = \mathfrak{k}\left\{1 + N\left(\frac{\kappa}{\tau} - \frac{\kappa^2}{\tau^2}\right)\right\} \\ K_P &= K_V + \frac{V_0}{\tau_0}\left(1 - \frac{\kappa}{\tau}\right) = \frac{V_0}{\tau_0}\left(\frac{kM}{2\mu} + 1 - \frac{\kappa^2}{\tau^2}\right) = \mathfrak{k}\left\{1 + N\left(1 - \frac{\kappa^2}{\tau^2}\right)\right\} \end{aligned}\right\}. (33)$$

The fact that the specific heats of all nearly perfect gases for unity of weight are inversely proportional to their specific gravities, shows that $\frac{kM}{2\mu}$ is the same for each of them*.

(13.) *Velocity of Sound in Fluids.*—Let a denote the velocity of sound in a fluid, and $d\,.\,P$ the total differential of the pressure. Then

$$a = \sqrt{\left(g\cdot\frac{d\,.\,P}{d\,.\,\frac{1}{V}}\right)} = \sqrt{\left\{gV^2\left(-\frac{dP}{dV} - \frac{dP}{d\tau}\cdot\frac{d\tau}{dV}\right)\right\}}. \quad . \quad (33)$$

If it were possible to maintain the temperature of each particle of the fluid invariable during the passage of sound, this velocity would be simply

$$\sqrt{\left(g\cdot\frac{dP}{d\,.\,\frac{1}{V}}\right)}.$$

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* The fact, recently ascertained by M. Regnault, that the specific heat of atmospheric air at constant pressure is sensibly constant throughout a great range of temperature, proves that at ordinary temperatures $\frac{\kappa^2}{\tau^2}$ is an inappreciably small fraction. (December 1853.) The specific heat of atmospheric air was accurately predicted by means of these equations, three years before M. Regnault ascertained it experimentally.

But we have reason to believe that there is not time, during the passage of sound, for an appreciable transfer of heat from atom to atom; so that for each particle

$$dQ + dQ' = 0; \text{ or, } K = 0 \text{ in equation (29).}$$

To fulfil this condition, we must have

$$\frac{d\tau}{dV} = -\frac{\tau - \kappa}{K_V} \cdot \frac{dp}{d\tau}.$$

Consequently,

$$a = \sqrt{\left\{gV^2\left(-\frac{dP}{dV} + \frac{\tau - \kappa}{K_V} \cdot \left(\frac{dp}{d\tau}\right)^2\right)\right\}};$$

or by equation (31),

$$a = \sqrt{\left(g\frac{dP}{d\cdot\frac{1}{V}} \cdot \frac{K_P}{K_V}\right)}. \qquad (34)$$

That is to say, *the action of heat increases the velocity of sound in a fluid, beyond what it would be if heat did not act, in the ratio of the square root of the specific heat at constant pressure to the square root of the specific heat at constant volume.*

This is Laplace's law of the propagation of sound; which is here shown to be applicable, not only to perfect gases, but to all fluids whatsoever*.

General Note, September 1855.

All that is said in articles (9.) and (10.) of mixtures of atoms of different substances *in equilibrio* of pressure and temperature, is applicable also to mixtures of portions of the same substance in two different conditions, *e. g.* the liquid and the vapourous; and thus from equation (25) it is found, that the latent heat of evaporation of unity of weight of a given fluid is represented by

$$\delta Q' = (\tau - \kappa) \cdot \frac{dp}{d\tau} \delta V,$$

where p is the pressure of the vapour in contact with its liquid at the absolute temperature τ, and δV is the increase of volume undergone by unity of weight of the fluid in the act of evaporating. If for $\tau - \kappa$ be put $\frac{J}{\mu}$, as before, this equation is transformed into that deduced by Messrs. Clausius and Thomson from Carnot's principle.

The recent experiments of Mr. Joule and Professor William

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* For experimental verifications of this law, see the Philosophical Magazine for June 1853, and the Transactions of the Royal Society of Edinburgh, vol. xx. pp. 588, 589. (1853.)

Thomson (Phil. Trans. 1854), have shown that the quantity denoted by κ is practically inappreciable; so that both this quantity, and the terms containing h as a factor, may be neglected in equation (25) and those which follow it, until there is experimental evidence of their having sensible values.

In the preceding paper, the laws of the expansive action of heat are deduced from a mechanical hypothesis, called that of Molecular Vortices. Those laws are capable of being expressed and proved independently of any hypothesis (see Phil. Trans. 1854, part 1, and a paper "On the Science of Energetics," Edinb. Phil. Journ. July 1855); but it is nevertheless considered, that a molecular hypothesis which has already led to the anticipation of some laws subsequently confirmed by experiment, may possibly lead hereafter to the anticipation of more such laws, and may at all events be regarded as interesting in a mathematical point of view; although its objective reality, like that of other molecular hypotheses, be incapable of absolute proof.

LVIII. *On the Felspars occurring in the Granites of the Dublin and Wicklow Mountains. By the* Rev. JOSEPH A. GALBRAITH, *Erasmus Smith's Professor of Natural and Experimental Philosophy in the University of Dublin.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN the last Number of your Journal, Mr. Jennings observes that I am incorrect in stating that it is necessary to determine the quantities of silex, alumina, &c. in a rock or mineral in order to obtain the alkalies which it contains. I beg leave to say that I never made such a statement; I certainly did observe, in reference to a single and incomplete analysis which he furnished in order to settle an important question, that where accuracy was an object, it was most desirable to have a complete analysis, in which each constituent was weighed, and this for the obvious reason, that if all the parts, reduced to per-centages, on being added together yielded one hundred, the utmost confidence might be placed in a result which thus checked itself: in the justice of this statement I am sure that every chemist who reads your Journal will acquiesce.

With regard to the mineral constitution of the granite rocks of the Dublin and Wicklow chain, the question at issue is that which was raised about three years ago by Sir Robert Kane, when he stated to the Royal Irish Academy that the soda in these granites was so preponderant over the potash, as to lead to the conclusion that the potash should be referred to the mica which the granite contained, and that the felspar was an albitic or soda felspar. Considerable doubts having been expressed as to the accuracy of this statement, I undertook to search this mountain chain for felspar crystals, and having selected seven specimens from widely distant localities, I submitted them to analysis, and sent the results to your Journal, in the January Number of which for the present year they were published; the average ratio of the potash to the soda in these felspars was 4·5:1, a result which differed so widely from Sir Robert Kane's statement, that I expressed my inability to conceive how the felspar which entered into the constitution of the rock could be destitute of potash. I have preserved the specimens, and shall have great pleasure in allowing Mr. Jennings to examine them at any time he chooses.

With a view to the direct determination of this question, Professor Haughton undertook the analysis of a series of *rock specimens* selected from different localities in the range, and arrived at a result totally at variance with Sir Robert Kane's assertion; the average result of his analyses, as published in the July Number of your Journal, gives for the ratio of the potash to the soda in the rock specimens taken from the main chain, 4·822 : 2·967, or in round numbers 5 : 3. Professor Haughton's analyses prove that the felspar, as found in large crystals, differs from that which enters into the constitution of the rock in this respect, namely, that while potash predominates in both cases over the soda, the ratio in the latter case is less than in the former.

The single analysis furnished by Mr. Jennings, although it does not fall in perfectly with Professor Haughton's results, must be pronounced to be equally inconsistent with Sir Robert Kane's statement, which requires us to believe that the felspar of the rock is destitute of potash. This is a complete statement of the question so far as facts have come to light; and I am inclined to think that if Sir Robert Kane still remains silent, the scientific public, unable any longer to withhold their judgement, will pronounce this question to be settled against him. Why he should maintain this reserve on a purely scientific question I am at a loss to understand, as he has had ample time to make good the promise which he volunteered to the Royal Irish Academy in February 1853. I find, moreover, on referring to public documents, that he has made considerable progress in this investigation. In the Report of the Museum of Irish Industry, made 26th January, 1854, he says, "The chemical officers of the Museum have been occupied with a very extensive series of analyses (still in progress) of the felspathic and other alkaliferous rocks of the mountain ranges south of Dublin; of this investigation, twenty-four detailed rock analyses have been already completed." In a return to an order of the House of Commons on a motion made by Mr. George Alexander Hamilton, bearing date 3rd May, 1854, I find the following statement:—"Considerable progress has been made with a very extended investigation into the composition of the felspathic and hornblendic rocks of the county Wicklow; the analytical portions of this work, perhaps the most laborious and extensive of its kind ever undertaken, will include between 400 and 500 quantitative analyses of rock specimens." In the Report to the Board of Trade, 16th January of the present year, it is again represented that "in the chemical department of the museum the examination of the felspathic rocks, &c. has made considerable progress."

When we consider that this investigation was undertaken by Sir Robert Kane in a public capacity, and in conjunction with the officers of the Geological Survey, it must be allowed that the public have a right to ask for information on the subject; and when we consider that Sir Robert Kane publicly pledged himself to produce his analyses, we must conclude that he is bound as a man of science to redeem his pledge and substantiate his statement.

I remain, Gentlemen,
Your obedient Servant,
Joseph A. Galbraith.

Trinity College, Dublin,
November 14, 1855.

LIX. *On Reciprocal Molecular Induction.*

To Prof. W. Thomson, F.R.S.

Royal Institution, Nov. 26, 1855.

My dear Sir,

The communication from Professor Weber which appears in the present Number of the Philosophical Magazine, has reminded me, almost too late, of your own interesting letter on the same subject published in the April Number of this Journal. A desire to finish all I have to say upon this question at present induces me to make the following remarks, which, had it not been for the circumstance just alluded to, might have been indefinitely deferred.

With reference to the mutual action of a row of bismuth particles, you say that "it is perfectly easy to demonstrate that it *must* be such as to impair the 'diamagnetization' when the line of the row is parallel to the lines of force" (the 'must,' you will remember, is put in italics by yourself). From this you infer, that in a uniform field of force a bar of bismuth would set its length along the lines of force. Further on it is stated that this action is "demonstrated with as much certainty as the parallelogram of forces;" and you conclude your letter by observing that "the assertions which I [yourself] have made are demonstrable in every case without special experiment, ... and are absolutely incontrovertible, as well as incapable of verification, by experiment or observation on diamagnetics."

Most of what I have to say upon this subject condenses itself into one question.

Supposing a cylinder of bismuth to be placed within a helix, and surrounded by an electric current of sufficient intensity; can you say, *with certainty*, what the action of either end of that cylinder would be on an external fragment of bismuth presented to it?

If you can, I, for my part, shall rejoice to learn the process by which such certainty is attained: but if you cannot, it will, I think, be evident to you that the verb "*must*" is logically "defective."

We *know* that magnetized iron attracts iron: we *know* that magnetized iron repels bismuth: this, so far as I can see, is your only experimental ground for *assuming* that magnetized *bismuth* repels bismuth, and yet you affirm that an action deduced from this assumption "is demonstrated with as much certainty as the parallelogram of forces." Do I not state the question fairly? I can, at all events, answer for my earnest wish to do so.

It is needless to remind one so well acquainted with the mental experience of the scientific inquirer, that the very letters which you attach to your sketch, page 291, may tempt us to an act of abstraction—a forgetfulness of a possible physical difference between the *n* of iron and the *n* of bismuth—which may lead us very wide of the truth. The very term 'pole' often pledges us to a theoretic conception without our being conscious of it. You are also well aware of the danger of shutting the door against experimental inquiry on an unpromising subject; and when you apparently do this in your concluding paragraph, I simply accept it as a strong way of expressing your personal conviction, that the action referred to is too feeble to be rendered sensible by experiment.

Believe me, dear Sir,
Most truly yours,
JOHN TYNDALL.

LX. *On the supposed Influence of the Hot-blast in augmenting the quantity of Phosphorus in Pig-iron.* *By* DAVID S. PRICE, *Ph.D., F.C.S.; and* E. CHAMBERS NICHOLSON*.

THE employment of the hot-blast in the smelting of iron is admitted to occasion the production of pig-iron of inferior quality, that is to say, contaminated with larger amounts of foreign elements than that smelted with cold-blast.

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* Communicated by the Authors. This paper was read in part at the British Association, Liverpool, 1854.

In the present communication it is not our intention to enter upon the discussion of this subject generally, as we intend reserving this for a future communication, but to limit our remarks to the consideration of the supposed influence of hot-blast in augmenting the quantity of phosphorus, an element of almost constant occurrence in pig-iron, and to the presence of which in bar-iron the peculiar property of the metal known as *cold shortness* is attributed.

In a paper published in the Quarterly Journal of the Chemical Society*, and also in one read before the British Association in 1849†, by Mr. Wrightson, several analyses of cast iron, the produce of Staffordshire furnaces worked by hot, warm, and cold blast, are given to prove that a larger amount of phosphoric acid is reduced when hot-blast is employed.

The increase in the per-centage of phosphorus in the iron smelted with hot- over that with cold-blast is exhibited in the following series of Mr. Wrightson's results:—

	1.	2.	3.	4.	5.	6.	7.	8.
Hot-blast	0·51	0·55	0·50	0·71	0·54	......	0·07	0·40
Cold-blast	0·47	0·41	0·31	0·20	0·21	0·36	0·03	0·36
Increase of P in hot-blast......	0·04	0·14	0·19	0·51	0·33	......	0·04	0·04

The ores from which the iron was smelted were also analysed, and found to contain the following per-centages of phosphoric acid:—

Binds.	Blue flats.	Penny earth.	Gubbin.	White iron stone.	White free.	Black free.
traces.	traces.	1·00	0·32	0·95	0·90	trace.

The increase in the amount of phosphorus indicated in the above table as occurring in hot-blast iron, does not, in the absence of a knowledge of the relative quantities of the respective ores employed, which vary as much as 1 per cent. in the quantity of phosphoric acid which they contain, and also of an examination of the blast-furnace slag produced, appear to afford sufficient proof in support of this opinion.

Karsten ‡, in speaking of bog-iron ore, says that, when smelted, the whole of the phosphate of iron is reduced to the state of phosphide and absorbed by the iron. Berthier §, from experiments conducted on a small scale, does not coincide with this view, and considers that the difficulty of detecting small quantities of phosphoric acid must account for its not appearing

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* Vol. i. p. 330. † Chem. Gaz. vol. vii. p. 478.

‡ *Handbuch der Eisenhüttenkunde*, vol. ii. § 368. Berlin, 1841.

§ *Traité des Essais par la Voie sèche*, vol. ii. p. 262.

amongst the constituents in the various analyses of blast-furnace slags which he has described in his work.

From experiments made by assaying pure hæmatite with variable per-centages of phosphate of lime* and suitable proportions of flux and charcoal, we have, by analysing the buttons of metal and slags obtained, taking care to select those only in which the operation was perfect, that is to say, in which a colourless slag and graphitous button were produced, also corroborated Berthier's results, as will be apparent from the following table:—

Pure peroxide of iron.	Phosphate of lime.	Flux.	Coke.	Per-centage of phosphorus in button.	
				Calculated.	Found.
1. 10·0 grms.	0·25 grms.	10·0 grms.	2·25 grms.	0·60	0·56
2. 10·0 ...	0·75 ...	10·0 ...	2·25 ...	1·83	1·60
3. 10·0 ...	2·50 ...	10·0 ...	2·50 ...	6·20	2·60
4. 10·0 ...	5·00 ...	10·0 ...	2·50 ...	12·60	6·00

In the assay of ores, and also of scoriæ from forge- and mill-furnaces, which contain a large amount of phosphoric acid, results widely different to the foregoing were arrived at. The following, selected from a great number of experiments, are marked illustrations of this:—

		Lime.	Coke.	Flux.	Per-centage of phosphorus in button.	
					Calculated.	Found.
	grms.	grms.	grms.	grms.		
5. Welch black-band ordinary quality—calcined	10·0	3·5	1·75	......	0·82	0·81
6. Argillaceous iron ore—calcined, very rich in phosphoric acid	10·0	1·5	1·20	......	6·60	6·41
7. Brown hæmatite, also rich in phosphoric acid	10·0	......	......	10·0	6·90	6·70
8. Scoria from puddling-furnace	10·0	......	2·0	10·0	13·6	12·5
9. Scoria from balling-furnace...	10·0	......	2·0	10·0	2·27	2·25

The time occupied in performing the assays, and the furnace conditions under which they were conducted, were about the same in all cases. Where flux was employed, it consisted of two parts of clay-shale and one part of lime.

It is necessary to state, that the phosphoric acid in the ores above cited was in combination with lime. In the cinders it existed as phosphate of iron.

In experiments Nos. 1 and 2, it will be seen that the quantity of phosphorus found agrees pretty well with the theoretical

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* The amount of phosphoric acid in the phosphate of lime was previously determined.

amount, whilst in 3 and 4 it falls considerably short. That this is owing to the length of time during which the reduction process is carried on we have no doubt, as we have frequently repeated the assays with the same proportions, and have found the amount of phosphorus in the button to vary considerably, never having succeeded in obtaining more than four-fifths of the total quantity. If, however, the cementation were prolonged for a sufficient length of time, it is very probable that the whole of the phosphoric acid would be reduced.

We have undertaken many experiments upon the large scale with the view of deciding this point. For this purpose we have determined the amount of phosphorus in iron that had been smelted from argillaceous ores by cold-blast, and by a blast heated to 600° F.

The following are the results:—

	I.	II.	III.	IV.
Hot-blast	0·74	0·68	0·71	0·58
	V.	VI.	VII.	VIII.*
Cold-blast	0·81	0·62	0·68	0·63

The iron in both cases was what is known as good No. 2 foundry pig.

In two instances it will be seen that the per-centage of phosphorus is higher in the cold-blast iron than in the hot; but the difference in these and in the other two is so slight, that it may fairly be attributed to the variations in the composition of the ore.

The slags produced simultaneously with four of the above irons were examined for phosphoric acid by the usual methods, as well as by molybdate of ammonia; and it was only by the latter reagent that we were able to find minute traces. We append the analyses of the slags:—

	I.	II.	V.	VI.
Silicic acid	39·95	40·20	41·64	42·94
Alumina	17·41	16·45	13·20	16·29
Lime	29·64	30·00	35·91	31·10
Magnesia	6·47	7·29	4·21	4·16
Protoxide of iron . . .	0·24	0·57	0·11	0·34
Protoxide of manganese .	0·91	0·84	0·74	0·51
Sulphide of calcium . .	3·60	2·71	2·19	2·16
Alkalies	1·46	1·30	1·70	1·87
Phosphoric acid . . .	trace	trace	trace	trace
Loss	0·32	0·64	0·30	0·63
	100·00	100·00	100·00	100·00

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* These correspond with the per-centage amounts of phosphorus calculated from the analyses of the ores, that the pig-iron should contain if all the phosphoric acid were reduced.

As far as our experience enables us to judge, we incline to the opinion that when the process of reduction is complete, or nearly so, that is to say, when no oxide of iron or small quantities only pass off with the slag, that then the whole of the phosphoric acid is reduced, and the phosphorus absorbed by the iron independently of the temperature of the blast. The analyses above given prove such to be the case with the iron smelted from the ores ordinarily employed in this country, in which the amount of phosphoric acid seldom exceeds 1 per cent.

With ores and scoriæ containing large amounts of phosphoric acid, we have also had opportunities of proving that when smelted with hot-blast, all the phosphoric acid is reduced, and the phosphorus absorbed by the iron; this we have found to be the case with ores containing from 2 to 3 per cent. of phosphoric acid, and with scoriæ with as much as from 8 to 10 per cent; but we have not had an opportunity of examining a product smelted with cold-blast from similar materials.

The following exhibits the per-centage of phosphorus, (IX.) in gray pig-iron smelted with hot-blast from pisolitic iron ore, (X.) of gray pig-iron smelted with hot-blast from puddling furnace scoriæ and clay-shale:—

IX.	X.
2·56	6·94

The slags respectively made with these products had the following composition:—

	IX.	X.
Silicic acid	45·64	41·11
Alumina	10·84	9·46
Lime	35·01	37·90
Magnesia	3·16	2·11
Protoxide of iron	0·71	0·39
Protoxide of manganese	trace	1·61
Sulphide of calcium	3·30	6·41
Alkalies	0·82	0·71
Phosphoric acid	trace	trace
Loss	0·52	0·30
	100·00	100·00

Phosphoric acid is present in blast-furnace slags when white iron is being smelted, that is, when the slag contains appreciable quantities of protoxide of iron, as will be seen by the following analyses.

	XI.	XII.
Silicic acid	41·11	37·84
Alumina	13·45	13·20
Lime	29·82	20·68
Magnesia	4·75	2·93
Protoxide of iron	6·44	20·83
Protoxide of manganese	0·66	0·80
Alkalies	1·84	1·08
Sulphide of calcium	1·34	0·87
Phosphoric acid	0·15	1·77
Loss	0·44	0·05
	100·00	100·00

No. XI. is that of a slag resembling black bottle-glass in appearance, and from its liquidity when melted is termed by the workmen a *scouring slag*. It was from argillaceous ore.

No. XII. from pisolitic ore when the working of the furnace was much deranged. This slag was exceedingly heavy, of a pitch-black colour, with the surface of the blocks in the *tap wagons* of the dull, dark red, bronze-like aspect characteristic of very bad furnace slags.

From these results we must regard the ore as being melted up (not smelted) and flowing away with the slags, although in very different degrees in the two examples given.

The analyses of crystalline slags by Percy and D. Forbes*, in all of which phosphoric acid was sought, lead to the same inference, phosphoric acid having been discovered and estimated in only one instance, and that in a slag similar to XI. It contained FeO 4·94, and PO^5 0·19.

In conclusion we will briefly recapitulate the results of our experiments.

1st. That in assaying ores, all the phosphorus of the phosphates will be found in the button.

2nd. That when the ordinary iron ores, such as the argillaceous ores, black bands, hæmatites, &c. are smelted, the iron produced, if it be gray, will contain all the phosphorus of the ore, whether the furnace be driven with hot or cold blast.

Lastly. That the slag may contain phosphoric acid in determinable quantity when white iron is being smelted.

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* British Association Report, 1846; and Chem. Gaz. vol. v. p. 29.

LXI. *Some Considerations respecting the Lunar Origin of Aërolites.* *By* R. P. Greg, *Esq.**

Dr. Lawrence Smith of Louisville University, U.S., has just published in the American Journal of Science, Second Series, vol. xix. May 1855, an interesting memoir on Meteorites, which deserves attention even from those who may not agree with the theory of their *lunar* origin. He directs attention to the physical, chemical as well as mineralogical, characteristics of aërolites, pointing out the volcanic and igneous nature which some of them possess. He agrees with me in the importance of separating these bodies from shooting stars and periodic luminous meteors, a circumstance which no astronomer except Olmsted has noticed or valued. He rejects their atmospheric origin, and considers them as certainly belonging to, or as having proceeded from, a larger whole, and not to have resulted from the condensation of so many independent cosmical particles. He then comes to the *lunar* theory, and after giving its history and naming the principal advocates of it†, lays down the following propositions:—

"1st. That all meteoric masses have a community of origin.

"2nd. At one period they formed parts of some large body.

"3rd. They have all been subject to a more or less prolonged igneous action corresponding to that of terrestrial volcanoes.

"4th. That their source must be deficient in oxygen.

"5th. That their average specific gravity is about that of the moon.

"From what has been said under the head of common characters of meteorites, it would appear far more singular that these bodies should have been formed separately from each other, than that they should have at one time or another constituted parts of the same body; and from the character of their formation, that body should have been of great dimensions. Let us suppose all the known meteorites assembled in one mass, and regarded by the philosopher, mindful of our knowledge of chemical and physical laws. Would it be considered more rational to view them as the great representatives of some one body that had been broken into fragments, or as small specks of some vast body in space that at one period or another has cast them forth? The latter, it seems to me, is the only opinion that can be entertained in reviewing the facts of the case.

"As regards the igneous character of the minerals composing meteorites, nothing remains to be added to what has already

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* Communicated by the Author.

† It was proposed by an Italian philosopher, Terzago, in 1660, and has been at different times, and for various reasons, supported by Olbers, Biot, Brandes, Poisson, Quetelet, Arago, Benzenburg and Laplace.

been said; in fact no mineralogist can dispute the great resemblance of these minerals to those of terrestrial volcanoes, they having only sufficient difference in association, to establish that, although igneous, they are extra-terrestrial. The source must also be deficient in oxygen, either in a gaseous condition or combined as in water: the reasons for so thinking have been clearly stated as dependent upon the existence of *metallic iron* in meteorites; a metal so oxidizable, that in its terrestrial associations it is almost always found combined with oxygen, and never in its metallic state.

"What then is that body which is to claim common parentage of these celestial messengers that visit us from time to time? Are we to look at them as fragments of some shattered planet whose great representatives are the thirty-three asteroids between Mars and Jupiter, and that they are 'minute outriders of the asteroids' (to use the language of Mr. R. P. Greg, in a late communication to the British Association) which have been ultimately drawn from their path by the attraction of the earth? For more reasons than one this view is not tenable; many of our most distinguished astronomers do not regard the asteroids as fragments of a shattered planet; and it is hard to believe if they were, and the meteorites the smaller fragments, that these latter should resemble each other so closely in their composition; a circumstance that would not be realized if our earth was shattered into a million of masses large and small.

"If then we leave the asteroids and look to the other planets, we find nothing in their constitution, or the circumstances attending them, to lead to any rational supposition as to their being the original habitation of the class of bodies in question. This leaves us then but the *moon* to look to as the parent of meteorites, and the more I contemplate that body, the stronger does the conviction grow, that to it all these bodies originally belonged."

Dr. Smith then notices the similarity existing between the respective densities of the moon and aërolites, but does not lay great weight on that point; though he thinks their chemical composition a strong ground in favour of their lunar origin. He goes on to say,—

"Laplace's view of the matter was connected with present volcanic action in the moon, but there is every reason to believe that all such action has long since ceased in the moon. This, however, does not invalidate this theory in the least, for the force of projection and modified attraction to which the detached masses were subjected, only gave them new and independent orbits around the earth, that may endure for a great length of time before coming in contact with the earth.

"The various astronomers cited concur in the opinion, that a body projected from the moon with the velocity of about 8000 feet per second, would go beyond the mutual point of attraction between the earth and moon, and already having an orbital velocity, may become a satellite of the earth with a modified orbit.

"The important question then for consideration is, the force requisite to produce this velocity. The force exercised in terrestrial volcanoes varies. According to Dr. Peters, who made observations on Ætna, the velocity of some of the stones was 1250 feet a second, and observations made on the peak of Teneriffe gave 3000 feet a second. Assuming, however, the former velocity to be the maximum of terrestrial volcanic effects, the velocity with which the bodies started (stones with a specific gravity of about 3·00) must have exceeded 2000 feet a second to permit of an absorbed velocity of 1250 feet through the denser portions of our atmosphere. Now suppose the force of the extinct volcanoes of the moon to have equalled that of Ætna, the force would have been more than sufficient to have projected masses of matter at a velocity exceeding 8000 feet a second; for the resistance to be overcome by the projectile force, is the attractive force of the moon, which is from five to six times less than that of the earth, so that the same projectile force in the two bodies would produce vastly greater velocities on the moon than on the earth, discarding of course atmospheric resistance, of which there is none in the moon*."

In the following, I think Dr. Smith, as he does in several of his arguments, rather begs the question in his anxiety to make the moon's position and physical powers favour as much as possible the production and discharge of aërolites to the earth. It would appear to me more reasonable and consistent with our ideas of terrestrial volcanoes to suppose, that the total absence of water and atmosphere in the moon is favourable to the notion, that volcanic action there is of a less violent and explosive character than on the earth. Dr. Smith says, however,—

"But doubtless, were the truth of the matter known, the projectile force of lunar volcanoes far exceeded that of any terrestrial volcanoes extinct or recent, and this we infer from the enormous craters of elevation to be seen upon its surface, and their great elevation above the general surface of the moon, with their borders thousands of feet above their centre; all of which

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* The editors of the American Journal here insert the following in a foot-note:—

"It would require at the moon the same force to produce an *initial* velocity of 8000 feet a second as at the earth; and the difference of rate at the end of the first second would be slight (discarding from consideration the atmosphere).—Eds."

point to the immense internal force required to elevate the melted lava that must have at one time poured from their sides. I know that Prof. Dana, in a learned paper on the subject of lunar volcanoes (Am. J. Sci. [2] ii. 375), argues that the great breadth of the craters is no evidence of great projectile force, the pits being regarded as boiling craters where force for lofty projection could not accumulate. Although his hypothesis is ingeniously sustained, still, until stronger proof is urged, we are justified, I think, in assuming the contrary to be true, for we must not measure the convulsive throes of nature at all periods by what our limited experience has enabled us to witness.

"As regards the existence of volcanic action in the moon without air or water, I have nothing at present to do, particularly as those who have studied volcanic action concede that neither of these agents is absolutely required to produce it; moreover, the surface of the moon is the strongest evidence we have in favour of its occurring under those circumstances."

Doubtless volcanic action has been highly developed at the surface of the moon, but in the absence of all water, we may conclude that lava floods have rather been emitted from her volcanoes, than discharges of stones and ashes.

It is still considered by some astronomers that the moon is not altogether without an atmosphere; though that be of small extent, it may nevertheless be rich in oxygen. It would be difficult to suppose that the extensive volcanic action which has evidently taken place in the moon, could have been exerted or maintained without the presence of oxygen; and if we admit that aërolites come from that body, we must necessarily concede there the existence of oxygen, since most aërolites contain a considerable quantity of silica, magnesia and alumina.

After all, the scarcity of oxygen where meteorites originate, a fact in itself highly probable, as based on the non-oxidation of the iron, nickel and phosphorus, if true, does not *prove* they proceed from the moon, but merely that they come from some place deficient in oxygen.

If meteoric masses of native iron really come from the moon, their non-oxidation might arise more from the absence of aqueous vapour than from an absence or deficiency of oxygen. While fairly admitting that some aërolites have proceeded from lunar volcanoes, because such have all the characters of erupted volcanic rocks, as those of Juvenas, Weston and Bishopville, it by no means follows, as Dr. Smith would argue, that *all* meteoric masses, even iron ones, also come from the same source or place; for in many aërolites and meteoric irons there is little if anything of a volcanic character.

We cannot reasonably suppose that lunar volcanoes have ejected enormous masses of iron, whether in a pure or oxidized state, when iron occurs in such small quantity and so rarely as the product of terrestrial volcanoes, and then most frequently deposited by sublimation. The density of the moon, as given by Dr. Smith, is only 3·6, while that of the earth is 5·6; this renders it still more improbable that substances of *greater* density, as the metals, are more abundantly ejected from the volcanoes in the moon, than from those of the earth, the latter body having the greater average density. I would also observe, that the metal *nickel*, present in almost all known aërolites and iron masses, has never yet been observed as a direct product of our volcanoes; this is not, however, an argument of much moment one way or the other, especially as that metal, in the form of red nickel, is known to occur in grauwacke at Reichelsdorf in Hessia*.

The argument, however, against the first proposition of Dr. Smith, that "all meteoric masses have a community of origin," (and militating therefore against his conclusion that that common origin is the moon's volcanoes,) which may be most forcibly illustrated, is where we consider the case of an iron mass weighing from 10 to 20 tons, as those from Durango and Rio de la Plata; for it is at once evident that no ordinary initial volcanic force could ever project such ponderous masses beyond the point or limit of the mutual attractions of the moon and earth. The calculations which have been made respecting the velocity of stones projected from Ætna or Teneriffe, are based on the supposition that such stones are of moderate size, having a density of only 3·0, or nearly three times less than that of iron.

I believe I am speaking within bounds when I state, that no stone weighing more than 100 lbs. has ever been ejected from the above-named volcanoes by a force, which, if exerted at the moon's surface, would allow of its reaching the desired point of neutralized attractions.

Dr. Daubeny states that the stones which overwhelmed Pompeii did not weigh more than 8 lbs.; and I myself can bear witness that the largest erupted blocks which crop out from under the lava of Mount Somma, and much nearer the central cone therefore than Pompeii, seldom exceeded 50 lbs. in weight. It can assuredly then only be stones of very moderate size, say of some 5 or 10 lbs., which could in any case, reasonably and practically speaking, reach or pass that limit where the *superior* attraction of the moon herself is lost.

It surely would make a material difference in our calcula-

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* I have a fine specimen in my cabinet of minerals from that locality, the matrix very much resembling some meteoric stones.

tions, whether a mass projected from a volcano in the moon weighed 30 lbs. or 30,000 lbs.; but such a difference Dr. Smith seems entirely to have overlooked; it is sufficiently great, however, in the present state of our knowledge of this subject, to over-rule the possibility that the larger iron meteoric masses can have a *lunar origin.*

Dr. Smith gives us the result of some interesting experiments, to prove the fallacy of judging of the actual size of meteors by their apparent size; I shall again quote his own words:—

"In my experiments, three solid bodies in a state of vigorous incandescence were used: 1st, charcoal points transmitting electricity; 2ndly, lime heated by the oxy-hydrogen blow-pipe; 3rdly, steel in a state of incandescence in a stream of oxygen gas. They were observed on a clear night at different distances, and the body of light (without the bordering rays) compared with the disk of the moon, then nearly full, and 45° above the horizon. Without going into details of the experiment the results will be tabulated.

	Actual diam. as seen at 10 in.	Apparent diam. at 200 yards.	Apparent diam. at $\frac{1}{4}$ mile.	Apparent diam. at $\frac{1}{2}$ mile.
Carbon points	$\frac{3}{10}$ of an inch,	$\frac{1}{2}$ the diam. moon's disc,	3 diam. do.	$3\frac{1}{2}$ diam. do.
Lime light . .	$\frac{4}{10}$ ...	$\frac{1}{3}$ ...	2 ...	2 ...
Incandes. steel	$\frac{2}{10}$ ...	$\frac{1}{4}$ ...	1 ...	1 ...

"If then the apparent diameter of a luminous meteor at a given distance is to be accepted as a guide for calculating the real size of these bodies, the

Charcoal points would be	80	feet in diam. instead of	$\frac{3}{10}$ of an in.
Lime	50	...	$\frac{4}{10}$ ...
The steel globule ...	25	...	$\frac{2}{10}$ ...

"I need not here enter into any explanation of these deceptive appearances, for they are well-known facts, and were tried in the present form only to give precision to the criticism on the supposed size of these bodies."

Dr. Smith is evidently anxious to reduce to a minimum the size of the lunar aërolites, and proceeds a little further on to say:—

"This then will conclude what I have to say in contradiction to the supposition of large solid cosmical bodies passing through the atmosphere, and dropping small portions of their mass. The contradiction is seen to be based; first, upon the fact that no meteorite is known of any very great size, none larger than the granite balls to be found at the Dardanelles along side of the pieces of ordnance from which they are discharged; secondly, on the fallacy of estimating the actual size of these bodies from their apparent size; and lastly from its being opposed to all the laws of chance, that these bodies should have been passing through an atmosphere for ages and none have yet encountered the body of the earth."

It is not strictly true that no meteorite is known of any great size, *i. e.* not larger than the well-known cannon-balls of granite at the Dardanelles, for one or two of the larger meteoric iron masses have been described by travellers as being 7 feet in length and weighing 15 tons. It would be improbable that such a mass could be projected beyond the mouth of either a lunar or terrestrial volcano, much less reach a height of several thousand feet.

The following calculations will show that a mass of iron, having a spherical form, and weighing 20,000 lbs., could not reasonably have a greater velocity than 372 feet in a second if projected from a lunar volcano. The calculations are based on the following premises.

A stone having 5·6 inches in diameter, with a density of 3·0, and weighing 10 lbs., is assumed to be projected from a lunar volcano at the rate of 9000 feet in a second, *i. e.* with a velocity more than sufficient, according to Dr. Smith and others, to allow it to pass the limits of mutual attraction between the moon and the earth.

Taking the sp. gr. of iron 8·0, and bearing in mind that the areas are as the squares and the masses as the cubes of the diameters, we arrive at the following results. A mass of iron (globular) to weigh 20,000 lbs., sp. gr. 8, will be a little over 50·9 inches in diameter; found thus:—

$$\sqrt[3]{\left(\frac{20{,}000\text{ lbs}\times 16\text{ oz.}\times 1728}{8000\text{ sp. gr.}\times {\cdot}5236}\right)} = \sqrt[3]{132009}=50{\cdot}9\text{ in. nearly.}$$

A similar mass of stone to be the same weight, and sp. gr. 3·0, must be 70·6 inches in diameter; found thus:—

$$\sqrt[3]{\left(\frac{132009\times 8}{3}\right)} = \sqrt[3]{352024}=70{\cdot}6.$$

Now if a piece of stone 5·6 inches in diameter, weighing 10 lbs., be projected with a velocity of 9000 feet per second, a mass 70·6 inches diameter, and weighing 20,000 lbs., could only be projected with a velocity of 715 feet per second, because the weights would increase so much faster than the sectional area.

$$\begin{array}{lccccccc} & \text{Lbs.} & & \text{Diam.} & & \text{Velocity.} & & \\ \text{Thus as} & \dfrac{20{,}000}{10} & : & \dfrac{(70{\cdot}6)^2}{(5{\cdot}62)^2} & :: & 9000; & & \\ \text{or as} & 2000 & : & \dfrac{4984{\cdot}36}{31{\cdot}36} & :: & 900 & : & 715\text{ feet.} \end{array}$$

The mass of iron has a less sectional area than the stone because of its greater specific gravity, viz. in the proportion of $(50{\cdot}9)^2$ to $(70{\cdot}6)^2$; it would therefore only be projected with a velocity of 372 feet per second; or as

$$498436 : 259081 :: 715 : 372.$$

That is, a velocity more than 20 times too small to allow of the larger known meteoric masses to reach the earth, if projected from a lunar volcano.

The exclusion from a lunar origin of the larger meteoric masses, especially iron ones, though not perhaps altogether subversive of the lunar theory generally, is yet injurious to its stability.

The physical constitution and internal appearance of some aërolites also, as those of Barbotan, Weston, Juvenas, and Bishopville, are entirely opposed to the idea either of an atmospheric origin, or of any consolidation of homologous, or nebulous particles existing in interplanetary space. They are evidently *parts*, as Dr. Lawrence Smith likewise justly insists on, of some larger whole, and are not unfrequently true igneous, if not volcanic rocks. Physically speaking, there is little choice left to us but to consider some of them certainly as having true geological and mineralogical characteristics; either proceeding from volcanoes in the moon, or portions of a broken satellite or planetary body: there may indeed be difficulties and objections to either supposition.

In a previous Number of this Journal, I endeavoured to bring forward some arguments and facts in favour of the latter idea, and I barely alluded to the lunar theory, but since the publication of Dr. Lawrence Smith's elaborate essay in its favour, I have now ventured to state more fully some reasons, not without their weight, against a theory in many respects so plausible.

LXII. *On a New Ore of Silver.* *By* H. J. Brooke, F.R.S..*

I RECEIVED some years since from Mexico a specimen of an ore of silver, said to be carbonate. It occurs in small, compact, irregular-shaped, earthy-looking masses, imbedded in carbonate of lime and quartz accompanied by crystallized blue carbonate of copper. Its colour is dull, dark gray; it is entirely devoid of lustre, and its hardness appears to differ in different parts of the specimen.

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* Communicated by the Author.

It was examined in a very cursory manner by the late Richard Phillips; and the portion he examined being found to effervesce with acid and to contain silver, he was led to regard it as a carbonate, and particularly as it so much resembled the carbonate of silver described by Selb.

A recent examination of it, however, by Mr. Richard Smith, in the metallurgical laboratory at the Museum of Practical Geology in Jermyn Street, shows it to be a very different compound, and one new to mineralogy; and there can be no doubt that the carbonic acid which deceived Mr. Phillips was derived from the intermixed carbonates of lime and of copper.

The analysis of two small portions of the earthy part of the substance separated from the matrix gave the following results per cent.:—

	I.		II.
	a.	*b.*	
Silver	16·09		17·18
Antimony . . .	7·82	7·50	7·28
Sulphur	1·41		1·84
Selenium . . .	2·81		3·58
Chloride of silver .	1·26		2·67
Oxide of copper .	10·46		8·61
Silica	45·56		41·81
Alumina	2·06		4·04
Peroxide of iron .	2·21		
Lime	1·72		2·83
Carbonic acid . .	2·92	3·04	
Combined water .	2·31		
Hygroscopic water .	·99		
	97·61		

The whole of the copper contained in the mineral is dissolved out by acetic acid; from this we may infer that it is not present in the form of sulphide or selenide. The acetic acid solution was found to contain lime, but did not give any precipitate with the addition of hydrochloric acid, nitrate of silver, or chloride of barium.

LXIII. *Notices respecting New Books.*

Hughes's *Reading Lessons. First Book.* London: Longman, Brown, Green, and Longmans. 1855.

THOSE who are practically engaged in the education of youth will be best able to appreciate the value of such reading lessons as Mr. Hughes intends his to be, as well as the difficulties which have to be surmounted in realizing his ideal. In order to form a fair estimate of the success of his undertaking, and to make these difficulties understood, it will be well to consider briefly what is required in such reading lessons.

In order to suit all tastes, the subjects should embrace the several departments of literature, science, and art. The writers should be men of known ability, accustomed to original research in their several departments, so that what they write may be quickened by that peculiar freshness and vigour which a direct acquaintance with their subjects alone can give. Each lesson should be a creation, and not a compilation. Each author should strive by all possible means to arouse and maintain the interest of his reader; for this purpose he should avoid too much detail, and should be less anxious to increase the number of his facts, than to surround each fact with its own atmosphere of interest. All looseness of expression and insufficiency of explanation should be carefully avoided, their consequences upon boys being extremely disheartening and injurious. The strictest accuracy in facts and expressions should be preserved, and all definitions should be as clear, decided, and concise as possible.

The difficulty of Mr. Hughes's task will now be evident. To find a number of men to co-operate with him in the fulfilment of the above demands is not easy, and it will not, therefore, be surprising to find that the success which has crowned his efforts, though considerable, is not complete. As far as this first book is concerned, however, we shall see that its success is certainly sufficient to encourage him to persevere in his praiseworthy task; and we sincerely hope, that whilst preserving the many good qualities of the first, his forthcoming books will be free from its defects.

This is not the place to speak much of the departments of literature and art. We shall merely remark, that the lessons on Mental Culture, by Mr. Edward Purcell, contain many sound thoughts and much good practical advice; they will be read by serious youths of more advanced age with profit and interest. The Biographies, which form an admirable feature in a boy's reading book, are on the whole well written. We know nothing more calculated to rouse a boy's faculties into activity, and induce him to be diligent and persevering, than well-written lives of men who have demonstrated by their works what diligence and perseverance can accomplish. In the forthcoming books we hope to see this feature more fully developed. The selections of poetry are not as well chosen as we could wish, neither can we say that the lessons on Music and Fine Arts are likely to prove quite successful. The chapters on English Literature by Mr. George L. Craik are well written, and will find many readers. Believing, as we do, that these lessons may do something towards cultivating a love for English literature in boys, we shall not stop to inquire whether the rival claims of English authors are there quite justly stated; for it is far more important to develope in boys the *power* of judging for themselves, than to furnish them with the *opinions* of other judges. Everybody knows the attractions which natural history possesses for young people, and we doubt not that

Mr. Patterson will be fully appreciated by them. Want of space will not allow us to do more than notice, that the departments of Ethnology and of Animal and Vegetable Physiology are in the hands of the well-known writers Latham, Mann, and Lankester. We hasten to consider more carefully those parts of the book with whose subjects we are more immediately connected.

Mr. Hughes himself is the writer of the lessons on Physical Geography, and, as might be expected from the originator of the whole scheme, has performed his task well. We wish we could say the same of Mr. J. Beete Jukes, the writer of the pages on Geology, or rather Mineralogy. From a director of the Geological Survey of Ireland one naturally expects to have interesting articles, or at least, articles free from gross inaccuracies; but justice, and the very fact of his high position, compel us to say that his productions are neither interesting nor free from the most egregious blunders; indeed the book would be considerably improved if his part of it could be completely suppressed. We are quite aware that a sentence so severe as this should not be lightly pronounced, nor should it be unaccompanied by proofs; no other consideration except this would induce us to give greater prominence to errors which we feel sure a careful perusal would have prevented. The articles are uninteresting on account of their sterility, and of the almost entire absence of that freshness and life which we believe to be indispensable. The uninteresting descriptions of minerals, unenlivened, as they to a great extent are, by interesting associations, are far more calculated to check than to awaken in the reader any love for the science.

But the facts brought forward are themselves often inaccurate, and the author's ignorance or unpardonable carelessness will unfortunately either confuse or misinform the attentive reader. For instance, on page 72 the reader is informed by Mr. Jukes, that "common salt is formed by the combination of soda with hydrochloric acid," and on page 217, by Prof. Frankland, that it is formed by the combination of sodium and chlorine. Which is he to believe? Mr. Jukes calls fluor-spar a fluorate of lime. No such compound as the latter is known to chemists,—fluor-spar is fluoride of calcium. Iodine and bromine are placed among the seven substances which, according to Mr. Jukes, "when pure, are always found in the state of gas or aëriform fluid;" now every chemical book we have seen tells us that iodine is a dark-coloured *solid* having a metallic lustre, and that bromine is a *liquid* of a very deep brownish-red colour, and almost three times heavier than water. Again, we are told that platinum "is the heaviest of all metals except gold." Now the specific gravity of platinum, although it varies a little with its structure, is about 21·5, whereas that of gold is only 19·5. More blunders might be exposed, but the above surely warrant our severe sentence. With the hope, therefore, that Mr. Jukes will either be more cautious, or decline writing altogether in such books, we pass on with pleasure to Professor Frankland's lessons on Chemistry.

These lessons have many good qualities, and from the spirited and pleasant manner in which they are written, they will no doubt, as they deserve, have many admirers. The facts are here accurate, and the manner in which they are introduced, as well as the peculiar treatment of the whole subject, leave no doubt upon the mind that the author is perfectly at home, and speaks from his own experience. It is only as critics that we feel it to be our duty to draw attention to a slight absence of strictness and philosophic accuracy in some of the theoretical questions introduced; before doing so, however, let us admit that these defects diminish but little the value of the lessons; indeed, their general excellence *suggests*, in some measure, that closer examination which has led to the detection of what we deem to be faults. For instance, great and, we think, undue stress is laid upon the elective character of the chemical force, by which it is thought to be distinguished from all other natural forces. "If," says the author, "we place a piece of silver and a piece of iron in a tumbler of water, we notice after the lapse of a few days that the iron has lost its lustre and become covered with rust, whilst the brilliant surface of the silver remains unchanged. Here one of the constituents of the atmosphere, oxygen, which was dissolved in the water, finding itself in the presence of silver and iron, made a selection between the two metals, and preferring the iron, united exclusively with that metal." This is quite true, but it can scarcely be considered characteristic of the chemical force; for Professor Frankland knows quite well, that if a magnet were presented to the same two pieces of metal, it would manifest precisely the same preference, and make exactly the same selection. Again, the term *elasticity* is used in a rather obscure manner. Let us suppose that the young reader has turned to the very useful appendix at the end of the book, and found elasticity defined, very properly, as "the property which some bodies have of recovering their form and dimension after the force which changed their shape and appearance is withdrawn," and then imagine his confusion when he reads the following sentences:—"*Heat* is the principle of *elasticity*."—"Just as cohesion modifies chemical affinity, &c., so *heat* or *elasticity* produces analogous results by the formation of volatile compounds, and their expulsion in the gaseous form." Or lastly, "the mode of decomposition being here determined by the *elasticity* of the carbonate of ammonia, which is expelled from the mixture in the form of white vapours." It may be objected, that in chemistry the term elasticity has a different meaning from the one above given; but if so, why has not Mr. Connon noticed it in his appendix to the article on chemistry? The fact is, however, that the term has, or ought to have, but one meaning, and in the lessons on chemistry other words would certainly have conveyed the author's meaning more distinctly. These defects, however, are not serious ones, and we are of opinion that the lessons on chemistry are amongst the most successful of the series: we heartily recommend them to the attention of all young readers.

Mr. Purcell's lessons on Mechanics are also well and clearly written.

Professor Tyndall's, on Natural Philosophy, are in every respect admirable. The style is attractive, lucid, and vigorous. The collision of bodies, and the laws of falling bodies, are generally somewhat difficult questions for boys, but they are here made so remarkably clear and simple that no boy of ordinary intelligence can fail to understand them. All we will add is, that if the boy not only reads, but studies these lessons, and then tries to solve the problems proposed therein, his success will be certain; and it will bring with it so much pleasure and encouragement, that he cannot fail speedily to overcome far greater difficulties.

The last lessons we shall notice are those by the Rev. Robert Main, on Astronomy, which may also be pronounced successful. The explanations are usually clear, though there are some obscure sentences. For instance, in the following there is surely some misprint:—"The real motion of the earth from west to east produces an exactly equal apparent motion of the sun in the same direction." In the following there must be some omission: "Since the ecliptic and equator are both *great circles*, that is, since both pass through the visible sphere of the heavens, their planes will cut each other in a line, which is a diameter of the sphere." We disagree with the introductory sentence, too, inasmuch as we cannot understand "why the grandeur of its objects" can make astronomy "the noblest of all physical sciences;" neither are we aware that astronomers are more "completely separated from the petty cares and concerns of our present existence" than other physicists. All this is merely accredited nonsense, usually uttered thoughtlessly, and not worthy of being uttered at all. It forms no part of Mr. Main's *lessons*, however, which are really good.

In conclusion, we wish Mr. Hughes every success; his first book is a good one, and we hope his future books will be better. We have given unusual space to our review because we deem his project an important one, and because it is almost the first of its kind in which the principle has been recognized, that only the ablest men can expound the elements of a science successfully, and that the task is in every respect worthy of their valuable time.

A Treatise on Conic Sections. By the Rev. GEORGE SALMON, *A.M.* Third Edition. London: Longman, Brown, Green, and Longmans. 1855.

It is a source of considerable satisfaction to find, amongst the crowd of very imperfect educational books which are daily issued from our press, a treatise so truly valuable as the present; and it is also cheering to learn that the public has so far recognized its merits as to demand a third edition. The book is now sufficiently well known, otherwise its title might mislead many; for although it is true that the greater part of it is devoted to an examination of the properties of the conic sections, yet this being done from an analytical point of view chiefly, it was necessary to prepare the reader by a similar investigation of the properties of the line and circle; so that, in fact, the whole constitutes a very efficient treatise on the elements of analytic geometry, such as may with advantage be placed in the hands of every student who has mastered the elements of Euclid, plane trigonometry, and algebra. We venture to assert, that amongst students already acquainted with its merits, this is one of their favourite text-books; for the treatment throughout is admirably clear, strict, and elegant,—in fact, such as can be achieved only by one who, besides that perfect mastery of the subject which can only be acquired by original research, possesses also that unacquirable talent of lucid exposition, and is guided by that knowledge of the difficulties usually encountered by students, which experience only can give.

This third edition is revised and enlarged, and contains many improvements upon the former two, with respect to its type, arrangement, and its well-chosen and numerous selection of examples. The student will do well to work all these conscientiously, and to pay particular attention to those interesting chapters on "abridged notation." The last chapter contains a short but clear account of the most important modern geometric methods, amongst which is that beautiful method of reciprocal polars first introduced by Poncelet, an acquaintance with which may be said to form an epoch in the history of every young mathematician.

The importance of these methods may be estimated from the fact, that in the hands of a Steiner, with scarcely any help from algebra, they have become the most powerful instruments of discovery, and have given him a wonderful insight into the nature and properties of curves; an insight which is, perhaps, more thorough and direct than any attainable by one who is accustomed to call in the more mechanical aid of algebraic calculation.

We do not wish here to revive the old and useless discussion on the comparative merits of the algebraic and geometric methods; both have undoubtedly their advantages, and both are indispensable. In the greater part of the present treatise, however, the former of these methods is adopted; and in drawing attention to the importance of the last chapter, we would merely remark, that if his object be to obtain a thorough knowledge of the properties of conics, the student will do well to combine the two methods to a greater extent than is here done; for the fact cannot be disputed, that the very facility with which results can be obtained algebraically, may indirectly prevent that intimate acquaintance with the properties of curves which a rigid geometrical investigation always secures.

LXIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Concluded from p. 389.]

June 21.—The Lord Wrottesley, President, in the Chair.

THE following communications were read:—

"On the Existence of a Magnetic Medium. By T. A. Hirst, Esq.

In a note on the above subject, communicated to the Royal Society on March 16, 1855, Professor Williamson objects to a certain conclusion deduced by Professor Tyndall in a letter addressed by the latter to Mr. Faraday and recently published in the Philosophical Magazine. Professor Tyndall's conclusion was that, according to the hypothesis of the existence of a magnetic medium in space and of the identity of magnetism and diamagnetism, a compressed diamagnetic cube ought to be less repelled when the magnetic force acts on the line of compression than when it acts at right angles to that line; a result which his own experiments have contradicted. Against the legitimacy of this conclusion Professor Williamson urges that "Dr. Tyndall seems to have assumed, that on the compression of an aggregate of particles of a diamagnetic substance, the medium is not displaced by the particles in their change of position." We shall be better able to estimate the value of this objection by recalling the steps of Professor Tyndall's argument.

A magnetic cube was taken which had *already* been compressed: its deportment before a magnet was experimentally examined, and deductions drawn concerning the changes that would occur in that deportment by merely conceiving the magnetic capacity of the material particles to be diminished, without in any way altering the distances between those particles, and consequently without in any way displacing the magnetic medium in the interstices of the body.

Instead of the assumption attributed to Dr. Tyndall, he might, with greater justice, be accused of having disregarded the possible presence of the medium within the body; but in his own defence he may with perfect justice reply, that in Mr. Faraday's experiments, which originally gave rise to the discussion, no such interpenetration of two media existed.

Admitting, however, that the interstices of a body are occupied by the medium, it may be interesting to inquire whether, from an argument similar to Professor Tyndall's, the same decided conclusion could, with equal accuracy, be deduced. To answer the inquiry, it must be remembered that the force of the argument in question depends essentially upon the justness of the supposition that a diamagnetic cube may, theoretically, be produced from a magnetic one by conceiving the magnetic capacity of the particles of the latter to be sufficiently diminished. It is evident that the total attraction of the cube by a magnet will be equal to the sum of the attractions of the material particles, and of the medium contained in its interstices. If this sum be greater than the attracting force upon the quantity of medium which the cube *and its contents* displace, the substance is called magnetic, for it will be drawn towards the magnet; if less, it is called diamagnetic, for it will be repelled from the magnet. But in our present knowledge of the properties of the medium there is nothing incompatible with the supposition that the density of the internal medium may so far exceed that of the external, that the attraction of the former by the magnet is *itself* greater than the attraction of the medium displaced by the cube and its contents. If so, however, no conceivable diminution of the magnetic capacity of the material particles could possibly render such a cube diamagnetic.

This is sufficient to show that, admitting the presence of the medium within the cube, the method of argument adopted by Professor Tyndall would not be strictly applicable, unless the density of the internal medium were subjected to limits which the advocates of its existence might possibly be unwilling to grant.

But it may be asked, if, whilst admitting that the medium may exist in the interstices of the body, it be granted that a diamagnetic may be produced from a magnetic cube in the manner assumed by Professor Tyndall, does it still follow, necessarily, that attraction is always greatest—repulsion least—when the force acts in the line of compression? In other words, can a conclusion contradictory to experimental facts be *then* legitimately deduced?

In attempting a reply to this question, it will, perhaps, be best to employ the following symbols. Let W represent the attracting force of the magnet upon the medium displaced by the cube and its contents. The value of W will, of course, be unaltered, no matter whether the force acts in, or at right angles to the line of compression. When the force acts in the line of compression, let P_1 represent the attracting force upon the particles, W_1 the attracting force upon the internal medium, and let F_1 be proportional to the resultant attraction of the cubical mass towards the magnet. Let P_2, W_2 and F_2 have similar significations when the force acts at right angles to the line of compression. Then we may put

$$F_1=P_1+M_1-W,$$
$$F_2=P_2+M_2-W.$$

Now, in a compressed magnetic cube, experiment proves that

$$F_1>F_2,$$

or

$$P_1+M_1>P_2+M_2,$$

i. e.

$$P_1-P_2>-(M_1-M_2).$$

As long as we are ignorant of the properties of the medium within the body, we will, for the sake of completeness, consider the following three distinct cases.

I. The attracting force upon the medium within the cube is the same when the force acts in either the one or the other of the two directions, with respect to the line of compression. Here

$$M_1=M_2,$$

hence

$$P_1>P_2.$$

II. In whichever of the two directions of the force the attraction of the particles may be greatest, the attraction of the internal medium is also greatest in the same direction. Here, according as P_1 is greater or less than P_2, M_1 is greater or less than M_2; hence, inasmuch as

$$P_1+M_1>P_2+M_2,$$
$$P_1>P_2 \text{ and } M_1>M_2.$$

III. In whichever of the two directions of the force the attraction of the particles may be greatest, the attraction of the internal medium is greatest in the direction perpendicular to the same. Here, according as P_1 is greater or less than P_2, M_1 is less or greater than M_2, so that the two hypotheses

$$(1) \quad P_1 > P_2 \text{ and } M_1 < M_2, \text{ and}$$
$$(2) \quad P_1 < P_2 \text{ and } M_1 > M_2$$

are both compatible with the sole condition,

$$P_1 + M_1 > P_2 + M_2.$$

In order to test the applicability of Professor Tyndall's method of argument in each of these cases, let us conceive, with him, that the magnetic capacity of the particles is so far diminished that their attractions P_1 and P_2 are reduced to p_1 and p_2. This may evidently be done by making $p_1 = aP_1$ and $p_2 = aP_2$, where a represents a positive fraction whose magnitude can be diminished indefinitely. If, under this supposition, the resultant attractions F_1 and F_2 become f_1 and f_2, then it can easily be proved that, however small the value of a may be, we shall always have

$$f_1 > f_2$$

in the cases I., II. and III. (1). That is to say, with the distributions of the internal medium assumed in these cases, the resultant attraction of the cubical mass will always be greatest, or repulsion least, when the force acts in the line of compression, no matter how diamagnetic the cube may have become by diminishing the magnetic capacity of its particles. It may just as easily be proved, however, that in case III. (2) a value of a may be chosen sufficiently small to make

$$f_1 < f_2,$$

that is to say, with the distribution and properties of the internal medium here supposed, it is quite possible so far to diminish the magnetic capacity of the particles as to obtain a cube which will be attracted least, or repelled most strongly when the force acts in the line of compression. This conclusion involves nothing contradictory to experimental facts, whereas the former one does.

I will not here enter into the question of the relative probability of these three cases, supposing the medium to exist. My sole object has been to show that, although the method of argument adopted by Professor Tyndall is strictly applicable in a great number of cases, even when the medium is supposed to fill the interstices of the body, yet it is possible to attribute properties to this medium of such a nature as to avoid the conclusion, contradictory to fact, which he has deduced. This may be done in two ways. *First*, the density of the internal medium may be such as to render it impossible to produce a diamagnetic from a magnetic cube in the manner assumed, *i. e.* by diminishing the magnetic capacity of the material particles. *Secondly*, granting that a diamagnetic cube may be so produced, the distribution and properties of the internal medium may still be such as to cause the cube to be attracted least, or repelled most strongly when the force acts on the line of compression, and thus, if the substance be diamagnetic, to cause it to agree, in its deportment, with experimental results. On the other hand, if these hypothetical properties of the internal medium be discarded as artificial or inadmissible, then at present I see no way of escaping the conclusion of Professor Tyndall's argument.

With regard to the explanation given by Professor Williamson, it will be observed that he pursues a path quite different from that of Professor Tyndall, when he considers the effects produced by compressing a number of particles surrounded by a magnetic medium. This compression, he states, may alter the attraction of the mass by a magnet in two ways;—"first, by altering the density of the matter; secondly, by altering the density of the medium." By the term '*density of matter*' is usually understood the relation which exists between the quantity of matter which a body contains, and the volume of the space enclosed by its external surface. But in the present case, where a comparison is instituted between the matter of the body and the medium which is supposed to fill all its pores, we must, I imagine, understand by the term 'density of matter,' the relation which exists between the sum of the masses of the particles, and the sum of their volumes; but if so, then, the particles themselves being incompressible, it is clear that compression could not alter the 'density of matter.'

As to the second effect of compression, viz. an alteration of the density of the medium, it may be quite conceivable, although I do not find that Professor Williamson has any where taken it into consideration. The effects of compression may, therefore, be more correctly described as either—*first*, a diminution of the interstices of a body, without altering the density of the medium which fills them; or *secondly*, a diminution of the interstices, accompanied by an alteration of the density of the medium within them.

Let us assume, as Professor Williamson has virtually done, that the first of these effects takes place; then, if we admit that "in a cubical mass of carbonate of iron the material particles are more magnetic than the medium which they displace, and the force with which it is attracted is proportional to this excess," we can by no means admit that, because "it becomes more magnetic by compression, we must conclude that the loss of magnetic medium *from its interstices* is more than supplied by the magnetic matter which takes its place;" for, according to what has already been advanced, the excess of the attraction of the material particles above that of the medium they displace is the same after, as it was before compression; inasmuch as compression merely changes the relative *situations* of the particles, by bringing them closer together, but does not in the least alter their volume, and consequently does not in the least alter the quantity of medium they displace.

With respect to carbonate of lime, Professor Williamson's conclusion is, of course, untenable, because it is based upon the foregoing one. He says, "when these particles are brought closer together by pressure, with diminution of the intervening spaces occupied by the medium, the mass becomes more diamagnetic, because a certain quantity of the magnetic medium is thus replaced by the less magnetic matter." It is, however, manifest that exactly the same quantity of magnetic medium is displaced by the less magnetic matter after, as there was before compression. Why, then, should diamagnetic action be increased by compression?

Lastly, with respect to the crystals of carbonate of iron and carbonate of lime, Professor Williamson's explanation, although ingenious, is liable to the same objections as those already mentioned. It cannot, in fact, be said that the functions of matter predominate most strongly over those of the medium they displace in any one direction, merely because the particles may be closer together in that direction; for, as long as each particle is surrounded by the medium, the predominance of that function of the particles with which we are concerned, *i. e.* their attraction, over that of the medium they displace will be the same, whatever may be the distances of the particles asunder.

From the foregoing remarks, therefore, it is manifest that, if Professor Tyndall has not yet succeeded in demonstrating that the hypothesis of the existence of a magnetic medium and of the identity of magnetism and diamagnetism is *necessarily* at variance with experimental facts, neither has Professor Williamson succeeded in proving that this hypothesis is in accordance with those facts. The question of the existence of a magnetic medium is still an open one, and will continue to be so until the many important principles which it involves, but which have not been introduced into the present discussion, have been further elucidated by new investigations and new thoughts.

"Anatomical Notices." By Professor Andrew Retzius, of Stockholm. Extracted from a Letter to Dr. Sharpey, dated 10 May, 1855. (Translation.)

"1. *On the Form of the Cranium in the Embryo.*

"So far as I am aware, due attention has not hitherto been given to the different forms presented by the cranium in its earlier stages of growth. In the skeletons of early human embryos to be seen in most museums, the imperfectly ossified cranium is for the most part shrunk up and disfigured. To obtain a correct view of the form of the cranial cavity, I first remove the skin, fascia and muscles; I then, by injecting water through the vertebral canal, thoroughly wash out the soft brain and spinal cord; and lastly, fill the cerebro-spinal cavity with quicksilver or with melted tallow, taking care not to distend it over-much. The opening in the vertebral canal is to be stopped with a little plug of wood, and the preparation allowed to dry.

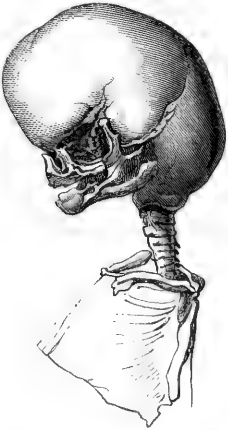
"In the skeleton of a human embryo of the fourth month, prepared in this way, the occipital bone was found to have the form of a funnel, the narrow part of which passed into the vertebral canal, as represented in the accompanying figure 1.

"It thus appears that the human occipital bone, in its early condition, approaches in form to the vertebral canal, and in this respect also it resembles the occipital bone in several quadrupeds, which so obviously represents the first cephalic vertebra.

"For the sake of comparison, I divided the skull of another embryo of the same age into two halves by a vertical median section, washed out the brain and examined the preparation while it lay immersed in weak spirit of wine in a shallow glass capsule. The occipital bone had the same funnel-shape as in the former case. As development advances, the funnel-like form is gradually lost, whilst, on the other hand, the bone appears more deepened or tubular the earlier the embryo to which it belongs. The same is true of quadrupeds.

"In the beautiful figure of the embryo-skull, given in Kölliker's 'Microscopische Anatomie,' B. II. taf. 3. fig. 2, the downward prolongation of the occipital bone appears much less, but whether less than it ought to be I cannot venture to say, as I happen to have no specimen of that age (one month older than the one I have given) with which to compare the figure.

Fig. 1.

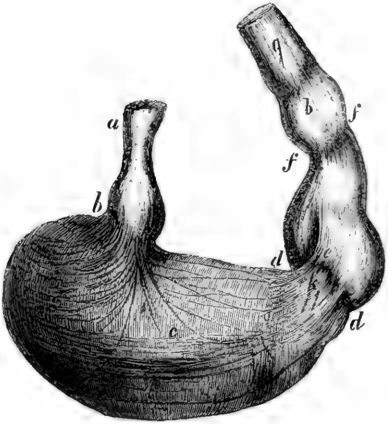


"The early form of the occipital bone I have here described becomes easily intelligible when we remember that the shape of the skull is regulated by that of the brain; and that as the latter is at first greatly elongated, and its ganglia imperfectly covered, the cranium must then also have a correspondingly elongated form.

"2. *On the Antrum Pylori.*

"I have now for some time directed my attention to the determination of the true form of the stomach, and have become more and more convinced that the antrum pylori of the older anatomists ('Pförtnerhöhle' of the Germans) is really a special compartment of the general cavity. I have had occasion to make numerous examinations of the stomach in the bodies of middle-aged women who died in the hospital, and found the form to be nearly as represented in figure 2, where *d d*, *f f* indicate the antrum pylori. This part is also distinguished by greater thickness of its muscular coat, more strongly developed glands, and the presence of the well-known plicæ fimbriatæ. The commencement of the duodenum also forms a special rounded cavity, which I should propose to name the *antrum duodeni*, and which is characterized internally by the absence of valvulæ conniventes, and by the dense array of Brunner's glands beneath its mucous membrane. This part constitutes what has been called the fourth stomach of the Porpoise, and some other Cetaceans. I may observe that the so-called ligaments of the pylorus are connected with the formation of the antrum pylori.

Fig. 2.



" 3. *On the Fascia Superficialis.*

" The accounts usually given of the fascia superficialis are for the most part very imperfect. As far as I can judge, this fascia is in many parts of the body a constant membrane, and really appertains to the skin, as may be particularly well seen in the integuments of the back. The cutaneous muscle of quadrupeds, in most cases, probably arises out of this fascia; the muscular fibres being deposited, as I conceive, in the midst of its substance, and finally becoming covered by it as their perimysium. In this way too, I imagine the platysma myoides and epicranius to be formed."

" On the Effect of Local Attraction upon the Plumb-line at stations on the English Arc of the Meridian, between Dunnose and Burleigh Moor; and a Method of computing its Amount." By the Venerable Archdeacon Pratt.

The author states that in a former communication he had pointed out a method for calculating the deflection of the plumb-line at stations on the Indian arc, caused by the attraction of the Himalayas and of the vast regions beyond, with a view to the correction of the astronomical amplitudes of the measured subdivisions of the arc before they are applied to the determination of the ellipticity of the earth.

The same subject is taken up in the present paper, but in reference to the English arc between Dunnose and Burleigh Moor; and a different method of calculating the attraction is given.

The paper consists of three parts. In the first, the ellipticity of the English arc is calculated without taking account of attraction. The arc is divided into five parts, and the lengths and amplitudes assigned to them in Mudge's Trigonometrical Survey of England, vols. ii. and iii., are made the basis of the calculation. These portions of the arc are compared two and two, and ten values of the ellipticity thence deduced; the mean of which is $-\frac{1}{43\cdot 8687}$. The ten values, of which this is the mean, differ considerably among each other, indicating that there is some disturbing cause, like local attraction, affecting the plumb-line, and therefore the apparent latitudes. The variations of the observed amplitudes are then discussed; and the necessity of calculating the local attraction pointed out.

In the second part a formula is obtained for calculating the attraction. The method is different from that given by the author in his first communication. The curvature of the earth is neglected, as this would have no sensible effect on the results in the British Isles. The attracting mass is divided into a number of smaller masses standing on rectangular bases at the sea-level, and the height of each is taken equal to the average height of the surface above the sea-level. The dimensions of the bases may differ from each other, and are determined by the contour of the surface in such a way that the average height in each mass may not depart materially from the height of any part of it. The investigation leads to the following Rule for determining the horizontal attraction deflection of the plumb-line caused by any one of these Tabular Masses (as the author calls them):—

Rule.—*Take the origin of coordinates at the station where the plumb-line is. Let the plane of* xy *be horizontal, and the axis of* x *in the vertical plane in which the amount of deflection is to be found.*

Write down the coordinates XY xy *of the furthest and nearest angles of the Tabular Mass from the origin;* Y *is always to be considered* $+^{\text{ve}}$, *and* y $+^{\text{ve}}$ *or* $-^{\text{ve}}$ *accordingly.*

Form four ratios, by first dividing each ordinate by the abscissa not belonging to it, and then by dividing each ordinate by its own abscissa, viz.

$$\frac{Y}{x},\ \frac{y}{X},\ \frac{Y}{X},\ \frac{y}{x}.$$

Look in a Table of Tangents for the four angles of which the tangents equal the above ratios.

Form four more angles by adding (subtracting if they be negative) half of each of these angles just found to (or from) 45°.

From the sum of the log-tangents of the first two of these angles subtract the sum of the log-tangents of the second two.

This result, multiplied by H *feet and by* $\frac{1''}{369}$, *will give the required deflection of the plumb-line in seconds of a degree*—H being the height of the Tabular Mass above the sea-level, and its density being taken equal to half the mean density of the earth, which is that of granite.

The only restriction to be attended to in the application of this Rule is, that the ratio of the height of the attracted station above the sea to each of the horizontal coordinates of the nearest angle of the Tabular Mass, must be so small that its square may be neglected.

If any part of the attracting mass is nearer to the station than this, it must be divided into vertical prisms and the attraction of each found; for which the author gives a formula in a note.

In the third part the Rule is applied, for the purpose of illustration, to obtain an approximate value of the deflection of the plumb-line at Burleigh Moor, the north station of the arc under consideration, situated on the north coast of Yorkshire. The deflection is found to be 3''·644 to the south. The data upon which this calculation is based are gathered by the author from the Map which accompanies General Mudge's account of the English Survey, and the heights marked down on that map.

The deflection at several other stations is deduced from this result of calculation, by using the amplitudes given in Mudge's work, and also in Captain Yolland's 'Astronomical Observations made with Airy's Zenith Sector,' published in 1852, and by supposing the curvature of the meridian to be the mean curvature of the whole globe as laid down by Mr. Airy in his article on the Figure of the Earth. Thus the deflection at Black Down on the Dorsetshire coast (one of the places mentioned in Captain Yolland's volume), the author finds to be 5''·886 to the north, a quantity which tallies well with the deflection assigned to Burleigh Moor on the Yorkshire coast, if the relative heights of the two coasts are compared. This affords a satisfactory evidence of the correctness of the principles laid down in the paper; and, as the author thinks, makes the subject well worthy the attention of those who are interested in the English Survey, and who have it in their power to furnish the most accurate data for the application of the Rule he lays down. The subject is also no less important to the mathematician in his investigation of the figure of the earth.

In a Postscript the author makes the following remarks upon the Astronomer Royal's method of accounting for the large amount of deflection on the Indian arc deduced by the author in his former communication:—

"Since the above was written, I have had the opportunity of seeing a notice of the communication of the Astronomer Royal on the

Density of Table-lands supposed to be supported by a dense fluid or semi-fluid mass; and the use he makes of his suggestions to remove the discrepancy, pointed out in my first communication, between the values of the deflection of the plumb-line in India, as determined by calculating the attraction of the Himalayas, and as indicated by the results of the Great Trigonometrical Survey. The following difficulties occur to me in the way of this highly ingenious and philosophical method of removing the discrepancy:—

"1. It assumes that the hard crust of the earth is sensibly lighter than the fluid or semi-fluid mass, imagined to be a few miles below the surface. But I know of no law, except the unique law of water and ice, which would lead us to suppose that the fluid mass in consolidating would expand and become lighter. One would rather expect it to become denser, by loss of heat and mutual approximation of its particles.

"2. There is, moreover, every reason to suppose, that the crust of the earth has long been so thick, that the position of its parts relatively to a mean level cannot be any longer subject to the laws of floatation. If the elevations and depressions of the earth's surface have always remained exactly what they were at the time when the laws of floatation ceased to have an uncontrolled effect, then the same reasoning would no doubt apply in our case, as if they still had their full sway. But geology shows that other laws are in constant operation (arising most probably, as Mr. Babbage has suggested, from the expansion and contraction of the solid materials of the crust), which change the relative levels of the various parts of the earth's surface, quite irrespectively of the laws of floatation. If Mr. Hopkins's estimate of the thickness of the crust be correct, viz. at least 1000 miles, these laws of change in the surface must have been in operation for such an enormous interval of time, as quite to obliterate any traces of the form of surface which the simple principles of hydrostatics would occasion. Indeed, it seems to me highly probable that the elevation of the Himalayas and the vast regions beyond may have arisen altogether from the slow upheaving force arising from this cause.

"I am inclined to think that the only explanation of the discrepancy between my calculations and the results of the Indian Survey, is to be found in the greater curvature of the Indian Arc."

"Contributions to the History of Aniline, Azobenzole and Benzidine." By A. W. Hofmann, Ph.D., F.R.S.

The transformation of nitrobenzole into aniline by the action of sulphuretted hydrogen is attended with such difficulties and requires especially so much time, that chemists hitherto have generally preferred to prepare this base from indigo. Lately a new modification of Zinin's process has been adopted by M. A. Béchamp*, which consists in submitting nitrobenzole to the reducing action of acetate of protoxide of iron. This process—M. Béchamp simply uses a mixture of iron and acetic acid—is applicable to all nitro-compounds

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* Chem. Gaz., March 1, 1855, p. 81.

and has been extensively tried with the most perfect success in the laboratory of the Royal College of Chemistry. The facility of the process, its rapidity, and the large amount of product it yields in most cases, cannot fail materially to assist the study of the volatile organic bases.

During these experiments several observations were made, which I beg leave to bring under the notice of the Society.

When employing about double the amount of iron which is recommended by M. Béchamp (2·5 instead of 1·2 of iron to 1 part of nitrobenzole), Mr. Alfred Noble found that the latter portion of the distillate solidified partly in the receiver and partly in the condenser. Washed with hydrochloric acid from adhering aniline, and once or twice recrystallized from boiling alcohol, the solid matter was obtained in fine crystals of a yellowish-red colour, and fusing below the boiling-point of water. These crystals possessed all the properties of azobenzole, which was moreover identified by combustion.

0·260 gramme of substance gave 0·755 grm. of carbonic acid and 0·134 grm. of water.

The well-established formula of azobenzole, $C_{12}H_5N$, requires the following values:—

	Theory.		
	Equivalents.	Per-centage.	Experiment.
Carbon	12	79·12	79·12
Hydrogen	5	5·49	5·76
Nitrogen......	1	15·39	
		100·00	

The azobenzole obtained in this manner is so readily purified that this process is greatly preferable to the action of alcoholic potassa upon nitrobenzole, since the latter process simultaneously produces several substances which can be separated only with difficulty from the azobenzole.

A portion of the azobenzole obtained in the above process was converted by means of sulphuretted hydrogen into benzidine. Of the beautifully crystallized compound, a platinum salt was made which was analysed by Mr. Noble.

0·268 grm. of the salt left 0·088 grm. =32·88 per cent. of platinum. The formula $C_{12}H_6N, HCl; Pt Cl_2$ requires 33·09 per cent. of platinum.

Benzidine exhibits a very interesting deportment with nitrous acid; gently warmed in the gas of this acid, obtained by treatment of starch with nitric acid, it gives rise to a powerful reaction. The substance assumes an orange-red colour, and exhibits, after treatment with water, when recrystallized from alcohol, all the properties of azobenzole.

The reproduction of this body from benzidine was moreover fixed in the following numbers:—

0·215 grm. of substance gave 0·623 grm. of carbonic acid, and 0·112 grm. of water.

	Theory. ($C_{12}H_5N$).	Experiment.
Carbon	79·12	79·02
Hydrogen	5·49	5·78
Nitrogen	15·39	
	100·00	

The simplest formulæ of azobenzole and benzidine only differ by one equivalent of hydrogen,—

Azobenzole......	$C_{12}H_5N$
Benzidine	$C_{12}H_6N$,

a relation which sufficiently explains the transformation and reproduction of azobenzole.

"On the Formation and some of the Properties of Cymidine, the Organic Base of the Cymole Series." By the Rev. John Barlow, F.R.S., Sec. R. Inst.

The object of this memoir is to detail the process by which an organic base, provisionally named *Cymidine*, was obtained from the hydrocarbon, cymole, and to describe some of its properties, and certain phænomena attending its production.

The substitution-product, nitrocymole, was procured by acting on cymole by strong nitric acid, both liquids being kept at the temperature $-17\frac{7}{9}°$ Cent. (0° Fahr.). From nitrocymol, cymidine was obtained by Béchamp's modification of Zinin's process, and results of analyses, made by combustion of the platinum salt, and likewise by a silver determination of the hydrochlorate, were found to coincide with the formula $C_{20}H_{15}N$. In the formation of cymidine a neutral oil occurred, having the same boiling-point with cymole. From this hydrocarbon a substitution compound was derived, apparently isomeric with, but possessing a less specific gravity than nitrocymole. This nitro-compound was also subjected to the process of reduction already described, and a basic substance was formed from it, which was identified by a platinum determination with cymidine. Some qualitative experiments, made with cymidine, were also described, and the memoir concluded with the following synoptical table of the homologues of the benzole series.

Hydrocarbons.		Nitro-substances.		Bases.	
Benzole	$C_{12}H_6$	Nitrobenzole	$C_{12}H_5NO_4$	Aniline	$C_{12}H_7N$
Toluole	$C_{14}H_8$	Nitrotoluole	$C_{14}H_7NO_4$	Toluidine	$C_{14}H_9N$
Xylole	$C_{16}H_{10}$	Nitroxylole	$C_{16}H_9NO_4$	Xylidine	$C_{16}H_{11}N$
Cumole	$C_{18}H_{12}$	Nitrocumole	$C_{18}H_{11}NO_4$	Cumidine	$C_{18}H_{13}N$
Cymole	$C_{20}H_{14}$	Nitrocymole	$C_{20}H_{13}NO_4$	Cymidine	$C_{20}H_{15}N$.

Letter from Dr. Herapath to Professor Stokes, "On the Compounds of Iodine and Strychnine."

June 20, 1855.

My dear Sir,—Will you do me the favour to announce to the Royal Society, that I have been engaged, during some months past, in investigating the optical and chemical properties of some crystalline compounds of iodine and strychnine which appear to be strongly indicative and peculiar? One of these bodies, from the analysis hitherto made, would seem to have a formula not very different from the following, viz. $C^{42} H^{22} N^2 O^4 + I^3$, and crystallizes in hexagonal prisms, passing by the ordinary replacement planes to the acute rhombohedron and other forms, all apparently derived from the rhombohedric system; some of these crystalline forms are very strange and unusual. This substance possesses "double absorption" in a very evident degree, and when examined by vertically plane polarized light, the hexahedral prisms are all obstructive of the polarized beam when the length of the prisms lies parallel to the plane of the incident ray; in this position they appear dark sienna-brown in colour; when the long axis of the prisms lies perpendicular to the plane of primitive polarization the crystals transmit a lemon-yellow tint, passing through greenish yellow to sherry-brown.

The other substance appears to be the sulphate of iodo-strychnine and has a decidedly metallic green reflexion, crystallizes in stellate aggregations of prisms, brilliantly green by reflected light, but having a deep blood-colour by transmission; these also possess double absorption and are very peculiar, as a slight increase in thickness renders them wholly impervious to light.

When these matters have been more carefully worked out, I hope to have the pleasure of communicating the results to the Society: in the mean time the present notice will be sufficient for the object in view.

I remain, my dear Sir,
Yours very truly,
W. Bird Herapath.

Professor Stokes, F.R.S.

"On the Constitution and Properties of Ozone." By Thomas Andrews, M.D., F.R.S.

The conflicting views which have so long existed as to the true constitution of ozone, induced the author to undertake a careful investigation of the subject, particularly as he had reason to doubt the accuracy of the only quantitative experiments which have yet been made to elucidate this difficult question. According to the experiments referred to, two substances have been confounded under the name of ozone, one a compound body having the formula HO_3, the other an allotropic variety of oxygen. To ascertain whether, in conformity with this statement, ozone obtained in the electrolysis of water contains hydrogen as a constituent, the author made two series of experiments. In the first series, he followed nearly the same method of investigation by which its compound nature is supposed to have been established, but modified so as to avoid a source of error, which, if neglected, vitiates altogether the results. Electrolytic oxygen, unless very great precautions be taken, is always accompanied by a small but appreciable quantity of carbonic acid, which is liable to be partially absorbed by the potassa set free when a neutral solution of iodide of potassium is decomposed by ozone. By adding a little hydrochloric acid to the solution of iodide of potassium before the commencement of each experiment, this error may be avoided.

The method of performing the experiment was to conduct a stream of electrolytic oxygen through a compound apparatus previously weighed, which contained on one side an acid solution of iodide of potassium, and on the other sulphuric acid; the former to decompose the ozone, the latter to prevent the escape of moisture. The increase in weight of this apparatus gave the entire weight of the ozone; the iodine set free, when reduced to its equivalent in oxygen, the weight of the active oxygen. The precautions to be taken in conducting this experiment are fully described in the communication.

The following are the numerical results of five experiments performed according to the above method:—

Volume of electrolytic oxygen.	Increase in weight of compound apparatus.	Active oxygen deduced from iodine set free.
litres.	grm.	grm.
10·20	0·0379	0·0386
2·72	0·0107	0·0100
2·86	0·0154	0·0138
6·45	0·0288	0·0281
6·80	0·0251	0·0273
Total	0·1179	0·1178

The agreement in these numbers proves that the active oxygen is exactly equal to the entire weight of the ozone, and is therefore identical with it.

In the next series of experiments the author shows that no water is produced in the decomposition of electrolytic ozone by heat. Large quantities of electrolytic oxygen, containing from 38 to 27 milligrammes of ozone, were decomposed by heat, but no water was obtained in a weighed absorption apparatus, in which the gas was exposed, not only to the action of sulphuric acid, but was also passed through a tube containing anhydrous phosphoric acid.

Having confirmed by new experiments the fact that ozone is formed by the action of the electrical spark on pure and dry oxygen, the author proceeds to institute a comparison between the properties of ozone derived from different sources. These he finds to be in every respect the same. Thus ozone, however prepared, is destroyed, or rather converted into ordinary oxygen, by exposure to a temperature of about 237° C., and catalytically, by being passed over peroxide of manganese, no water being formed in either case; it is not absorbed by water, but when sufficiently diluted with other gases, is destroyed by agitation with a large quantity of water; it is also, contrary to the common statements, destroyed by being agitated with lime-water and baryta-water, provided a sufficient quantity of those solutions be used; it has always the same peculiar odour; it bleaches without producing previously an acid reaction; it oxidizes in all cases the same bodies, &c.

From the whole investigation the author draws the conclusion, "that ozone, from whatever source derived, is one and the same substance, and is not a compound body, but oxygen in an altered or allotropic condition."

LXV. *Intelligence and Miscellaneous Articles.*

ON THE HEAT PRODUCED BY THE INFLUENCE OF THE MAGNET UPON BODIES IN MOTION. BY M. LÉON FOUCAULT.

IN 1824, Arago observed the remarkable fact of the attraction of the magnetic needle by conducting bodies in motion. The phænomenon appeared very singular, and remained without explanation until Faraday announced the important discovery of currents of induction. It was then proved, that in Arago's experiments the motion gave rise to currents, which, by reacting upon the magnet, tended to associate it with the moveable body and draw it in the same direction. It may be said, in general terms, that the magnet and the conducting body tend towards a state of relative repose by a mutual influence.

If, notwithstanding this influence, the motion continue, a certain amount of force must be constantly furnished, the moveable part be, as it were, pressed by a break, and this force necessarily produces a dynamic effect, which, according to the new doctrines, I have thought must be found in heat.

We arrive at the same consequence by taking into consideration the currents of induction which succeed one another in the interior of the body in motion; but an idea of the quantity of heat produced would only be acquired with great difficulty by this mode of regarding the affair, whilst by considering this heat as due to a transformation of force, it appeared certain to me that a sensible elevation of temperature would be easily produced in a decisive experiment. Having ready to my hand all the elements necessary for a prompt verification, I proceeded to its execution in the following manner.

Between the poles of a strong electro-magnet I partially introduced the solid of revolution belonging to the apparatus which I have called a *gyroscope,* and which was previously employed in experiments of a very different nature. This solid is a ring of bronze connected by a toothed pinion with an apparatus of wheels, by the action of which, when turned by the hand, it may revolve with a rapidity of 150 or 200 turns in a second. To render the action of the magnet more effective, two pieces of soft iron added to the helices prolonged the magnetic poles, and concentrated them in the vicinity of the revolving body.

When the apparatus is going with the greatest rapidity, the current of six Bunsen's couples, passed into the electro-magnet, stops the movement in a few seconds, as though an invisible break had been applied to the moving body; this is Arago's experiment, as developed by Faraday. But if the handle be then pushed on, so as to restore to the apparatus the movement which it has lost, the resistance experienced requires the application of a certain amount of force, the equivalent of which reappears and accumulates in heat in the interior of the revolving body.

By means of a thermometer inserted in the mass we may follow the gradual elevation of temperature. Having, for example, taken the apparatus at the surrounding temperature of 60°·8 F., I saw the thermometer rise successively to 68°, 77°, 86°, and 93°·2 F.; but the phænomenon had previously become sufficiently developed to ender the employment of the thermometer unnecessary, as the heat produced had become sensible by the hand.

If the experiment appears worthy of interest, it would be easy to arrange an apparatus to reproduce and exaggerate this phænomenon. There is no doubt that by means of a machine properly constructed, and composed only of permanent magnets, high temperatures might be produced, so as to place before the eyes of the public assembled in lecture rooms a curious example of the conversion of force into heat.—*Comptes Rendus*, Sept. 17, 1855, p. 450.

METEOROLOGICAL BSERVATIONS FOR OCTOBER 1855.

Chiswick.—October 1. Fine: showery: fine. 2. Foggy: cloudy: foggy: rain. 3. Dense fog: rain. 4. Densely clouded: heavy rain. 5. Fine: heavy rain. 6. Very fine: cloudy: rain. 7. Fine: foggy at night. 8. Rain: very fine. 9. Foggy: very fine. 10. Fine. 11. Rain. 12. Cloudy and boisterous: rain. 13. Very fine. 14. Foggy: cloudy. 15. Dense fog: fine. 16. Very fine: rain at night. 17. Rain, with fog. 18. Overcast: fine. 19. Light clouds and fine. 20. Fine: cloudy: fine. 21. Cloudy. 22. Foggy: very fine: cloudy. 23. Foggy: uniformly overcast. 24. Fine: clear: fine. 25. Cloudy and boisterous. 26. Very boisterous: overcast: fine. 27. Exceedingly fine. 28. Very clear: fine. 29. Rain: drizzly. 30. Constant heavy rain. 31. Rain: cloudy: rain at night.

Mean temperature of the month	50°·28
Mean temperature of Oct. 1854	48 ·20
Mean temperature of Oct. for the last twenty-nine years	50 ·00
Average amount of rain in Oct.	2·686 inches.

Boston.—Oct. 1. Cloudy. 2. Cloudy: rain A.M. 3. Cloudy. 4. Rain A.M. and P.M.: heavy thunder-storm. 5. Fine: rain P.M. 6. Fine. 7. Cloudy: rain A.M. 8. Foggy. 9. Cloudy: rain P.M. 10. Fine. 11. Rain A.M. and P.M. 12. Cloudy. 13. Fine. 14. Cloudy. 15. Foggy: rain P.M. 16. Fine. 17. Cloudy: rain A.M. 18. Fine. 19. Cloudy: rain P.M. 20. Fine. 21. Cloudy: rain P.M. 22. Cloudy. 23. Cloudy: rain A.M. and P.M. 24. Fine. 25. Cloudy. 26, 27. Rain A.M. and P.M. 28. Fine. 29. Foggy: rain AM. and P.M. 30, 31. Rain A.M. and P.M.

Sandwick Manse, Orkney.—Oct. 1. Fog A.M.: clear P.M. 2. Drizzle A.M.: damp P.M. 3: Showers A.M.: rain P.M. 4. Rain A.M. and P.M. 5. Clear A.M. and P.M. 6. Bright A.M.: cloudy P.M. 7. Rain A.M. and P.M. 8. Hazy A.M.: drizzle P.M. 9. Cloudy A.M.: sleet-showers, aurora P.M. 10. Cloudy A.M.: dry; rain P.M. 11. Showers A.M. and P.M. 12. Showers A.M.: clear P.M. 13. Fine A.M.: clear, fine, aurora P.M. 14. Fine, hoar-frost A.M.: clear, fine, aurora P.M. 15. Fine, frost A.M.: clear, fine P.M. 16. Sleet-showers A.M.: clear P.M. 17. Showers A.M.: rain P.M. 18. Showers A.M.: sleet-showers, aurora P.M. 19. Drops A.M.: clear, fine P.M. 20. Rain A.M.: drizzle P.M. 21. Showers A.M.: clear P.M. 22. Cloudy A.M. and P.M. 23. Rain A.M.: sleet-showers P.M. 24. Hail-showers A.M.: sleet-showers P.M. 25, 26. Rain A.M.: showers P.M. 27. Snow-showers A.M.: clear, aurora P.M. 28. Clear, frost A.M. and P.M. 29. Cloudy A.M.: hazy P.M. 30. Bright A.M.: clear P.M. 31. Bright A.M.: cloudy P.M.

Mean temperature of Oct. for twenty-eight previous years	47°·64
Mean temperature of this month	45 ·72
Mean temperature of Oct. 1854	46 ·39
Average quantity of rain in Oct. for fifteen previous years	5·08 inches.

by Mr. Veall, *at* BOSTON; *and by the* Rev. C. Clouston, *at Sandwick Manse*, ORKNEY.

Days of Month.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick.		Boston. 8½ a.m.	Orkney, Sandwick.		Chiswick.		Boston. 8½ a.m.	Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
1855. Oct.	Max.	Min.		9¼ a.m.	8½ p.m.	Max.	Min.		9¼ a.m.	8½ p.m.						
1.	29·529	29·496	29·00	29·62	29·57	64	40	59	54	50½	sw.	ssw.	ese.	·02	....	·01
☾ 2.	29·641	29·564	29·10	29·63	29·77	66	40	57	50½	44½	w.	n.	e.	·12	·03	·01
3.	29·707	29·517	29·25	29·71	29·56	62	50	58	46	52	s.	se.	ne.	·36	....	·03
4.	29·369	29·284	28·86	29·34	29·08	68	47	59	50	53	sw.	s.	se.	·70	·25	·22
5.	29·351	29·331	28·86	29·09	29·10	56	50	54	55	49	sw.	ssw.	se.	·82	·28	·30
6.	29·351	29·282	28·92	29·16	29·28	65	50	52·5	51	53½	s.	ssw.	ese.	·11	·10	....
7.	29·370	29·250	28·76	29·28	29·36	64	39	57·5	54½	53½	s.	sw.	e.	·03	·20	·47
8.	29·546	29·467	29·00	29·48	29·53	67	36	50	53½	52½	sw.	calm	nne.	·09	....	·45
9.	29·512	29·488	29·04	29·58	29·61	62	41	49·5	46½	41	w.	w.	nnw.	....	....	·12
10.	29·786	29·678	29·26	29·56	29·28	58	39	41	43½	46	nw.	nw.	ssw.	·02	·17	·08
● 11.	29·697	29·573	29·14	29·20	29·17	61	52	52	46½	47	sw.	w.	w.	·25	·20	·30
12.	29·644	29·590	29·14	29·24	29·27	62	37	52	47	42	sw.	w.	calm	·33	·12	·16
13.	29·649	29·585	29·20	29·37	29·39	60	39	45	47	38½	w.	w.	calm	·01	....	·07
14.	29·502	29·448	29·10	29·36	29·36	58	31	46·5	41	38½	nw.	calm	calm	·01	....	....
15.	29·597	29·412	29·05	29·35	29·37	59	32	39·5	40	43½	s.	calm	sw.	·09	....	....
16.	29·733	29·703	29·32	29·44	29·70	58	38	39·5	45	39½	sw.	sw.	n.	·56	·15	·15
17.	29·841	29·705	29·40	29·56	29·38	60	38	48	44	50	ne.	e.	w.	·10	·05	....
☽ 18.	30·012	29·863	29·45	29·34	29·58	64	41	42·5	51	46	w.	wsw.	wnw.	....	·04	·19
19.	30·134	30·045	29·60	29·67	29·87	63	39	49	46	41	sw.	w.	s.	....	....	·06
20.	30·177	30·123	29·68	29·50	29·56	61	50	50	48	52½	sw.	sw.	w.	....	·02	·44
21.	30·169	30·088	29·50	29·50	29·83	62	45	59	53	48	sw.	sw.	w.	·01	....	·33
22.	30·188	30·045	29·74	29·86	29·63	66	46	48	50½	50	sw.	w.	s.	....	·02	·04
23.	29·966	29·841	29·40	29·22	29·33	61	40	55·5	52	41	sw.	ssw.	ssw.	....	·03	·14
24.	30·052	29·901	29·40	29·17	29·51	55	33	46	38	40	w.	wsw.	nw.	....	·02	·47
○ 25.	29·961	29·545	29·47	29·03	28·69	55	50	46	42	49	sw.	ssw.	ssw.	·38	....	·42
26.	29·202	29·121	28·64	28·52	28·93	56	31	50	42	40	sw.	ssw.	nw.	·28	·32	·49
27.	29·639	29·344	29·00	29·30	29·74	54	26	43	37	36½	sw.	nne.	n.	....	·12	·33
28.	29·714	29·685	29·40	29·78	29·70	53	40	42	33½	33½	n.	nw.	se.	·03	·03	....
29.	29·523	29·210	29·22	29·59	29·81	52	46	42	44	44	ne.	nw.	e.	·63	....	....
30.	29·217	29·083	28·80	29·92	29·86	53	46	50	44	43	ne.	n.	nne.	·98	·80	....
31.	29·528	29·300	29·05	29·87	29 89	47	34	44	42½	37½	n.	n.	n.	·22	·75	....
Mean.	29·687	29·567	29·18	29·427	29·732	59·74	40·83	49·2	46·40	45·04				6·15	3·70	5·28

INDEX TO VOL. X.

END OF THE TENTH VOLUME.

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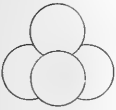
1



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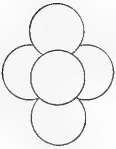

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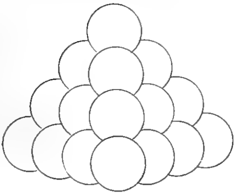

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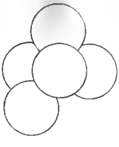

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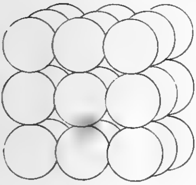

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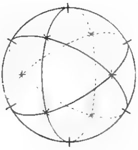

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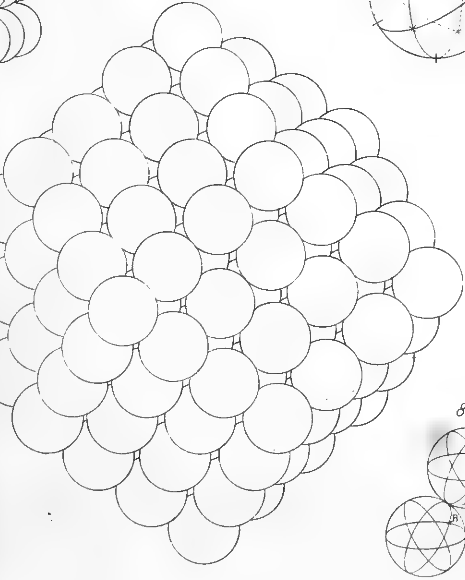

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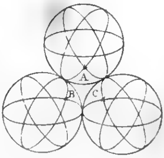

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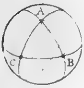

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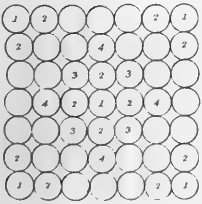

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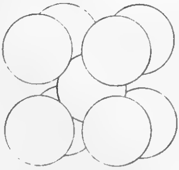

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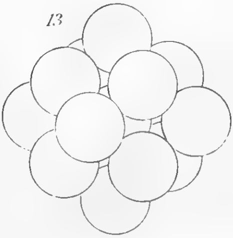
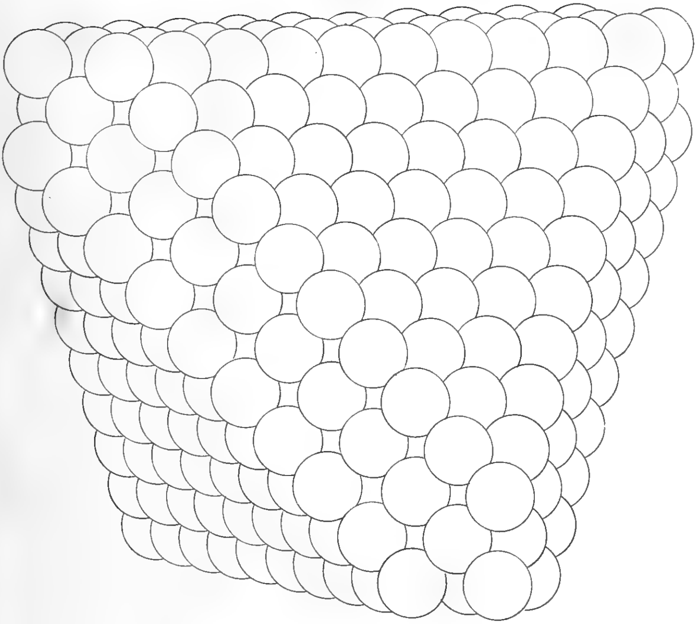


J. Basire sc.

10



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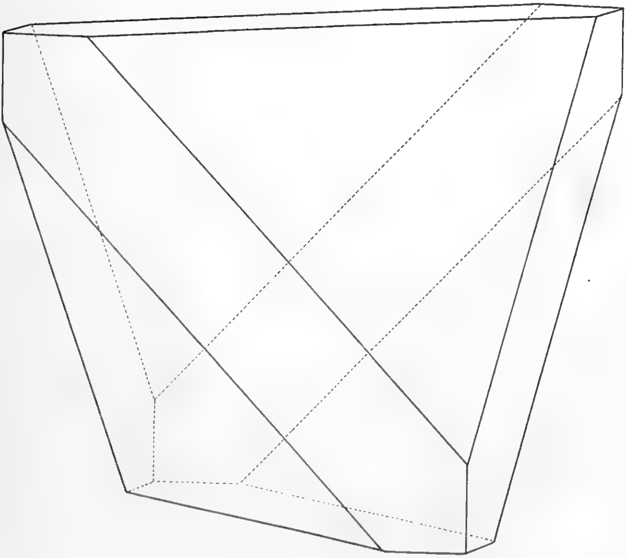
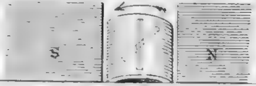


J. Basire, sc.

Normal
Paramagnetic Bar.
Helix excited first.

1



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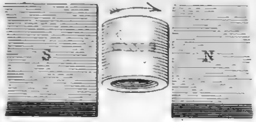

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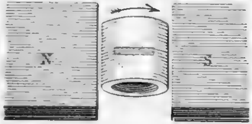

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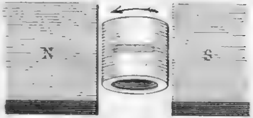
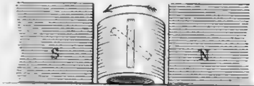


Normal
Diamagnetic Bar.
Magnet excited first.

9



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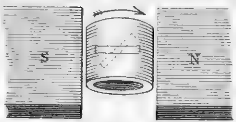

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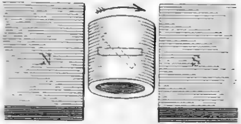

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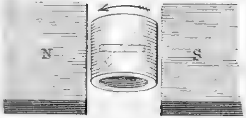
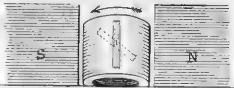


Abnormal
Paramagnetic Bar.
Magnet excited first.

17



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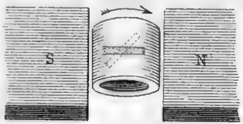

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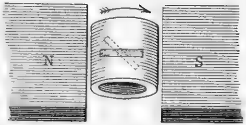

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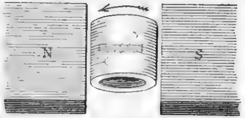
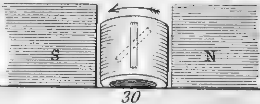


Abnormal
Diamagnetic Bar.
Helix excited first.

25



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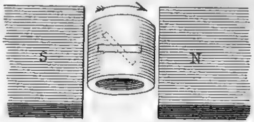

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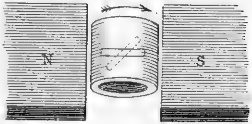

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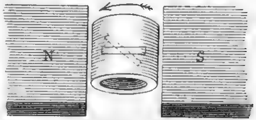
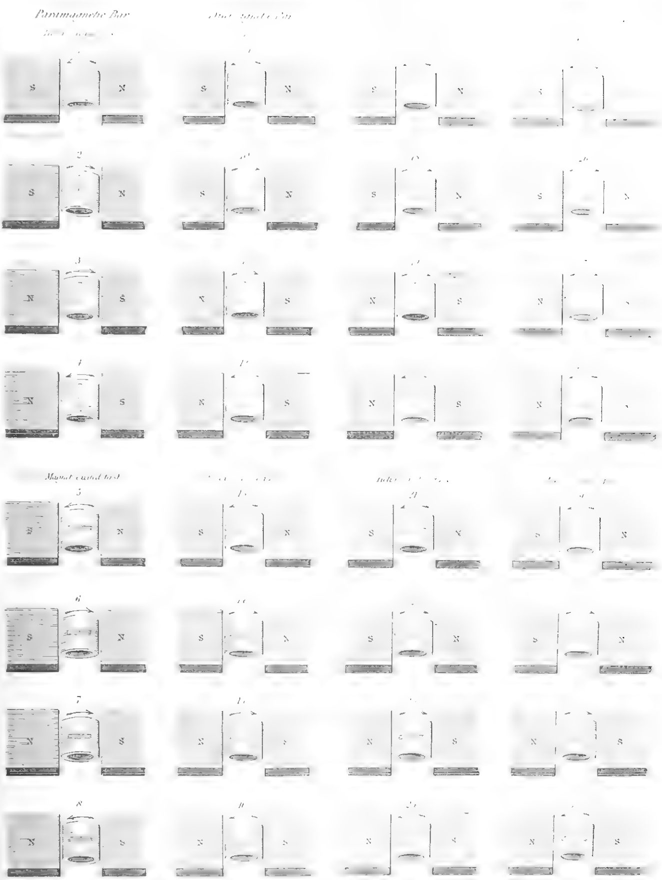



Paramagnetic Bar
Magnet coated test
1
2
3
4
5
6
7
8
S
N

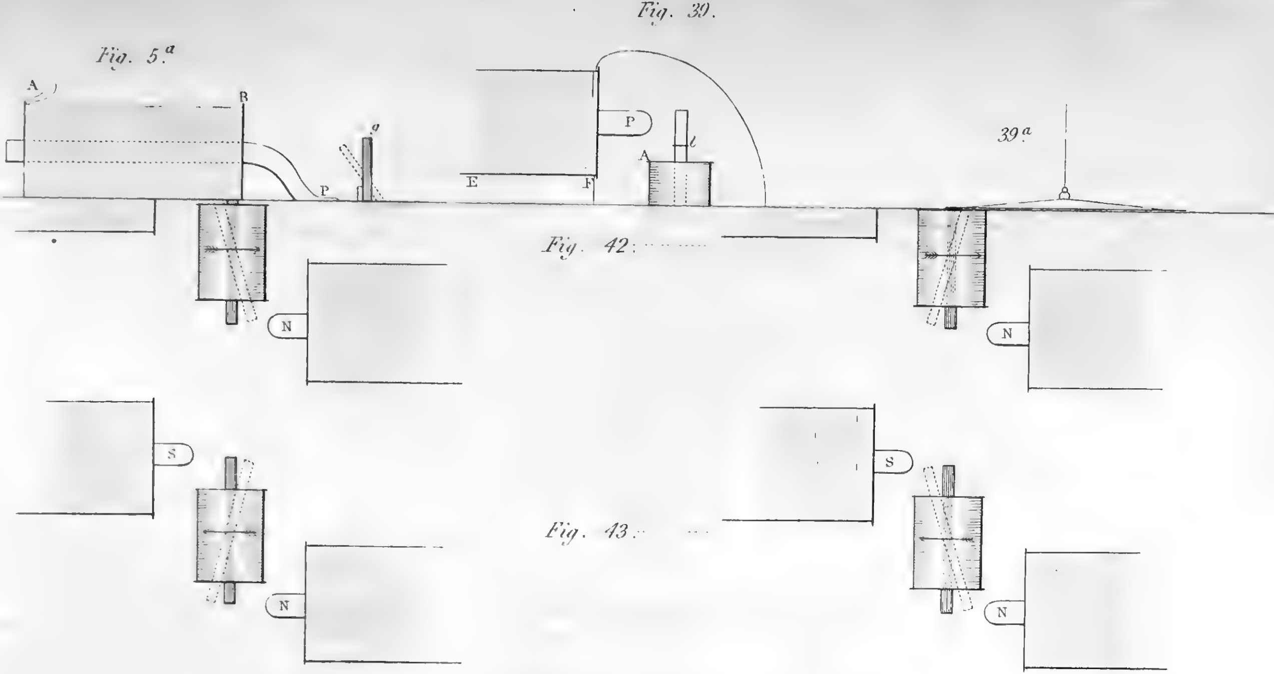


Fig. 5.^{a}

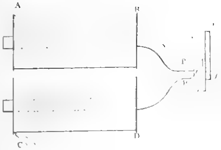
Fig. 39.

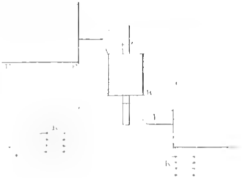
39.^{a}

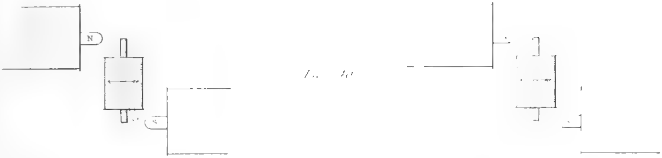
Fig. 42:

Fig. 43:

Fig 5^a







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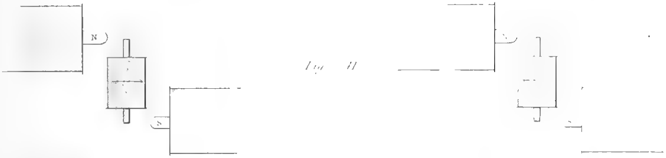

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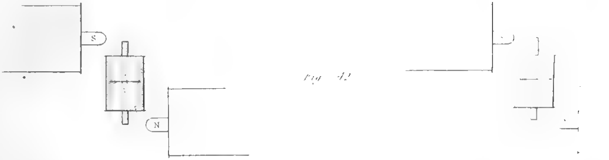
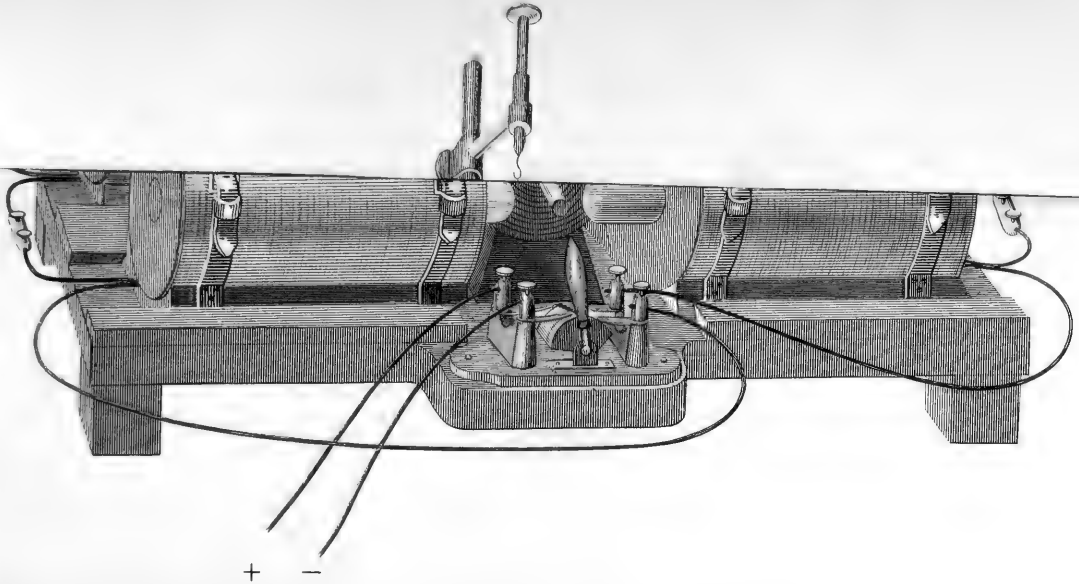


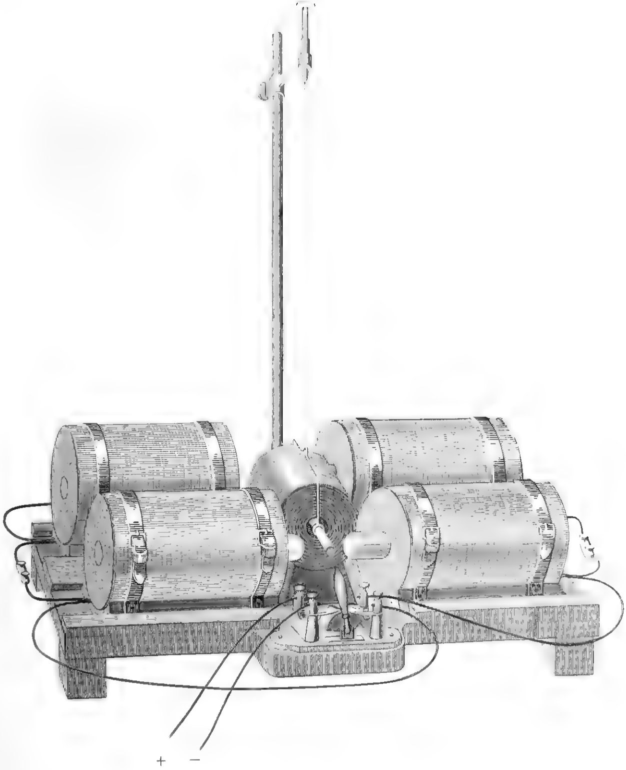
Fig 42

Fig 43



Becker, del.

J. Basire, sc.



C. Baker, del.