PHILOSOPHICAL

TRANSACTIONS

OF THE

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FOR THE YEAR MDCCCXXVII.

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MDCCCXXVII.

ADVERTISEMENT.

The Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions;* which was accordingly done upon the 26th of March, 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they received them, are to be considered in no other light than as a matter of civility, in return for the respect shown to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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ERRATA, PART IV. 1826.

Page 4, line 1, *for* " 9 ten-thousandths," *read* " 9 thousandths."

In Plate VI. page 189, *insert* the letter " N " at the left hand extremity of the horizontal diameter in Fig. 4; and at the upper extremity of the vertical diameter in Fig. 5, *insert* the letter " e."

Directions to the Binder.

Plates I. II. and III. should face page 124, *instead of* 174.

THE Gold and Silver Medals, founded on the Donation of BENJAMIN Count RUMFORD, for the most important discovery, or useful improvement, made and published in any part of Europe during the two preceding years, on Light, or on Heat, were awarded by the Council, held Feb. 8th, 1827, to M. FRESNEL, for his developement of the undulatory theory as applied to the phenomena of polarised light, and for his various important discoveries in Physical Optics.

PHILOSOPHICAL

TRANSACTIONS.

I. *Description of a Percussion Shell, to be fired horizontally from a common gun. By Lieutenant Colonel* MILLER, *late of the Rifle Brigade, and now unattached. Communicated by* R. I. MURCHISON, *Esq. F. R. S.*

Read November 16 and 23, 1826.

BEFORE proceeding to describe this shell, it may be necessary to say a few words on the theory of rifles, with which its construction is intimately connected.

It is a principle now universally admitted, that the irregularity in the flight of shot arises from the inequality in the specific gravities of their sides, and the action of the air upon the inequalities of their surfaces. These imperfections it is impossible to guard against in casting; for although the mould may be constructed with mathematical exactness, yet the metal which is poured into it is known to occupy a larger space when hot than when cold, and is found by experience generally to contract unequally, as the process of cooling proceeds. The same mould will accordingly be found to cast shot of different sizes, and hardly ever a perfect sphere.

In a ball fired from a rifle, these imperfections are corrected by the spiral or rotatory motion of the ball round its axis, by which means any inequalities that may exist in its sides are continually shifting round the axis during the flight, and the course of the projectile is continued straight forward.

The spiral motion itself has hitherto been generally supposed to be communicated to a rifle ball, by the grooves which are made in the barrel of the gun. The following reasons, however, incline me to think that the spiral motion is given to the ball, not only by the direction it receives from the grooves of the barrel at the moment of discharge, but also by the action of the air upon the indentations in the ball itself, which the grooves of the barrel have made, when the ball is pressed against them in loading.

1. The grooves of a rifle run obliquely, making from $\frac{1}{4}$ turn or twist, to $1\frac{3}{4}$ in the length of the barrel. The diameter of the ball being somewhat greater than that of the bore, the ball requires to be driven down with considerable force, following the grooves, and receiving their impression as it descends. The ball therefore is a male, and the barrel a female screw, and unless the ball receive the impression of the grooves, it never acquires the spiral motion. This is ascertained by firing balls into a bank of hard earth, so as to flatten them a little. Unless the indentations made by the grooves of the rifle are distinctly perceptible round the edges of the balls, proving them to have struck end foremost, they never hit the mark with the necessary accuracy. If the balls are too small, and it is attempted to remedy this defect by using very thick patches, however tight they may be rammed down, they never answer the purpose intended.

2. When a rifle is fired at a moderate range, say from 100 to 300 yards, it is found that the accuracy of fire is maintained without any perceptible diminution; from which it may be inferred, that the spiral motion is maintained undiminished also. If, again, a rifle is fired at an elevation producing its greatest range, which is about $37\frac{1}{2}^{\circ}$, its fire will not be so accurate as that of a plain barrel. The reason I conceive to be, that when the ball is at the summit of its flight, where it changes the direction of its course the most rapidly, the grooves have not sufficient length to enable the air to act upon them while the ball is forming the curve, so as to keep the axis and line of flight in one, by which means the position of the ball is suddenly changed, the spiral motion lost, and it continues to fly afterwards with great irregularity.

3. According to the calculation made by Mr. Robins, vol. i. p. 133, ed. 1761, the resistance of the air to a shot passing through it with a velocity of 1550 feet in a second, is equal to 120 times the weight of the shot. Dr. Robison, when treating of projectiles, in his Elements of Natural Philosophy, supposes that this resistance may be increased to 138 times the weight of the shot. But supposing it to be much below what it has been estimated at by either of those gentlemen, it is difficult to believe that a quarter of a revolution in the barrel can communicate to the ball a rotatory motion, which it has to maintain for hundreds of yards against this enormous pressure. And as this pressure falls obliquely on the grooves of the ball, I believe it to be the sustaining cause of the spiral motion.

4. So easy is it to communicate the spiral motion, that a grooved leaden ball will acquire it, in falling perpendicularly

through a roll of paper, or a gun barrel, merely from its own weight pressing against the air, so as to be distinctly visible to the eye. If the bottom of the tube is closed to prevent the air from escaping, it will be easily ascertained when the ball possesses the spiral motion, and when it does not do so, from the peculiar sound it makes against the sides of the tube.

5. If it is asserted, that the spiral motion in a rifle ball is sustained *solely* by the impulse it receives from the barrel, I am aware of no proof that can be produced in support of that opinion, either from analogy or experiment. If, on the other hand, we suppose it to be a motion, both *produced* and sustained by the action of the air, it will be in strict accordance with the effect the air is known to produce in all similar cases. Among other familiar instances may be given that of the arrow, the windmill, the shuttlecock, the smoke-jack, a window ventilator, and a grooved paper cylinder drawn through the air. And it may be observed of the arrow, shuttlecock, and cylinder, that each revolves in equal distances of air, whatever their velocity may be, and the spiral motion is maintained undiminished to the end of the flight.

Having been led to infer from the above-mentioned reasons, that the spiral motion is in all cases, both produced and sustained by the action of the air; and believing it to be impossible to reconcile theory and experiment upon any other principle, the next idea that suggested itself, was the possibility of giving the spiral motion to grooved shot, when fired from a plain barrel. The same thought appears to have occurred to Mr. ROBINS sometime before his death, about the year 1745; for, in a memorandum found among his papers, he observes, when speaking of rifles, and the rotatory motion of the ball, " I have made some experiments on *simpler* methods of performing this, and applicable to iron bullets. My success as yet has not been what I could wish, but it has however been sufficient to encourage a farther prosecution, which, if I shall ever pursue farther, I know not." vol. i. p. 317. That eminent mathematician has left us no clue to discover what his plan was; but in his tracts, he dwells so much on the effects which the resistance of the air is capable of producing on the flight of shot, that one can hardly fail to be impressed with the idea, that it must have been in some way or other connected with that medium.

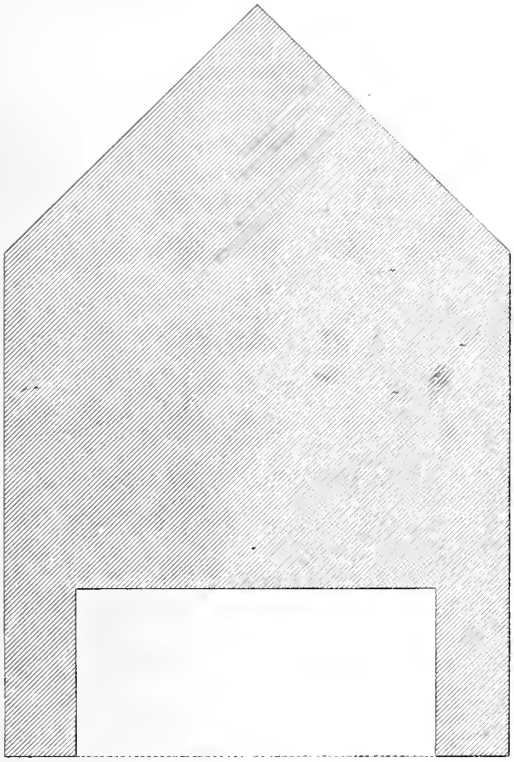
I shall now proceed to give a brief account of the experiments that have been made for the purpose of attaining the object in view. The first difficulty which presented itself, was that of applying the principle to shot of a spherical figure. The utter hopelessness of getting the air to act in the way desired, upon shot of this shape, soon led me to prefer the cylindrical to the spherical form, and it is to the improvement of that figure of shot accordingly, my subsequent endeavours have been directed.

My first experiments were made merely for amusement, for the purpose of shooting seals and sea-birds, in Bantry Bay, during the summer of 1821. Though not very successful, they were repeated from time to time as opportunity offered.

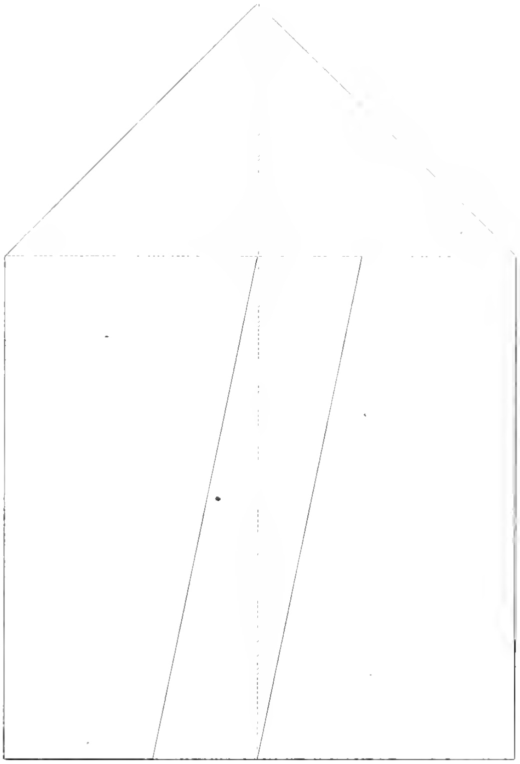
Hemispherical ends to the balls were thought of, and afterwards abandoned, as it was found desirable to dispose of the weight of the ball in such a manner as to give the greatest possible length to the sides. Grooves of various kinds were tried, and narrow ones were found not to answer. They must be of sufficient width to expose their sides, upon which the air presses, to its action throughout their length. For, if the air has not a free passage through them (Plate I. fig. 2) it will act as backwater on a mill-wheel. It is upon this principle that the blades of a windmill will not act if placed against a wall; and that rifles with very narrow grooves are found not to answer. In the beginning of 1822, some further experiments were made at Woolwich. On the first occasion, 10 grooved leaden musket balls were fired from a plain barrel through a target, at the distance of 100 yards. The balls were well finished, but heavier behind than before; and from the marks left by them in the target in passing through it, two of them appeared to have turned in their flight. From the circular holes left in the target by the others, they appeared to have passed through without turning. In the next experiment, balls of a somewhat better construction were employed. Several were fired at 40 yards, into a mass of boards and clay prepared for the purpose; and out of the number so fired, three or four were found in the exact position in which they lodged, all point foremost.

A grooved wooden shot was then fired six times from a 5½ inch howitzer, against a bank, at 50 yards. This experiment distinctly pointed out the necessity of balancing the two extremities, as that which was heaviest showed an evident tendency to drop lower and lower during the flight.

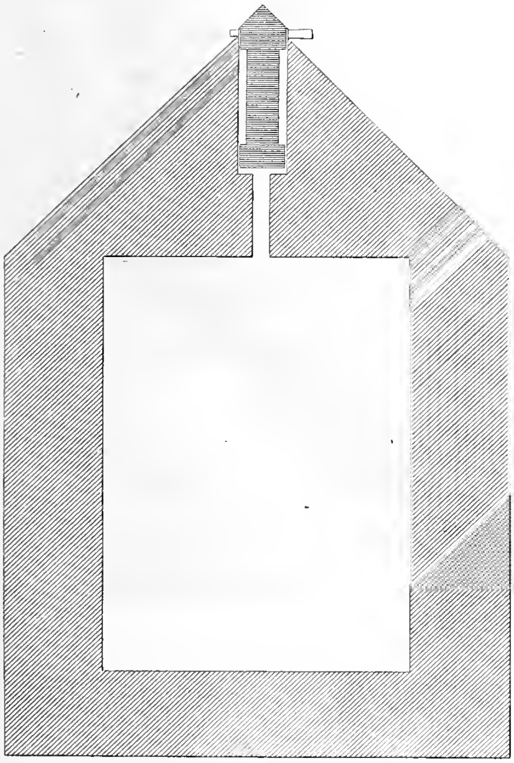
A wooden shot of the same form and proportions was then fired from the same gun, with a 3 lb. iron shot in its centre. This being properly balanced flew very steadily, hit the target, and then split without passing through it, but leaving



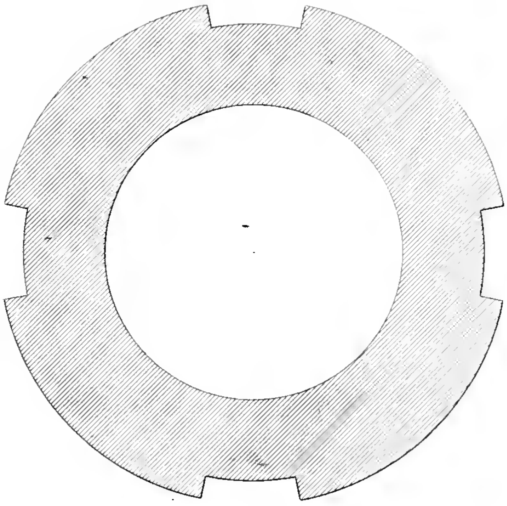
1. Vertical Section of Shot.



2. Obliquity of Grooves.



3. Vertical Section of Shell.



4. Horizontal Section of Shell.

a circular hole in the target, and a deep indentation on the cone, where it came in contact with the target.

During the following summer of 1823, I made several hundreds of experiments with grooved leaden musket-balls, fired from a plain barrel, at all ranges. When constructed with sufficient accuracy, they were found to fire very true, and to strike invariably point foremost. In order to balance them, the content of the cone, that of the cavity, and that of the part surrounding the cavity, must all three be equal, and the depth of the cavity must be equal to two-thirds of the height of the cone (Plate I. fig. 1.) The experiments with respect to twist, were found to correspond with those of rifles; that is, the velocity of the ball was found to diminish, in proportion as the twist was increased. In some of the balls used, the obliquity of the grooves was carried as high as a turn in 6 inches, which was found to diminish the velocity so much, that the person marking the shots repeatedly supposed the ball to have passed, before it reached the target. Numerous experiments were made during the same year, with wooden shot from a 24 pounder at the fort of Kinsale. They were found to range 400 and 500 yards; the largeness of their size rendered their position and flight easily perceptible to the eye; any imperfection in their construction was easily detected; and by firing them into the sea, each shot bore to be discharged a great many times before it became unserviceable. Others were afterwards fired by night with lighted fuses fixed in their sides, in order to mark their revolutions with greater distinctness. It was found that they possessed the spiral motion, and that it was maintained undiminished throughout their flight.

Having, as I conceived, conclusively ascertained by experiment, that the spiral motion may be communicated to a grooved cylindrical ball fired from a plain barrel, it occurred to me, that by the help of this principle, shells might be so constructed as to explode by percussion. The plan I have adopted for this purpose will easily be understood by an examination of Plate I. fig. 3. A round peg is placed in the apex of the cone, working in a cylinder, and a pellet of percussion powder is placed under the peg, and over the vent which communicates with the cavity of the shell. By this means, as soon as the point of the peg strikes against any hard substance, it slides in and ignites the percussion powder, which instantly communicates with the bursting charge. The cross pin is employed merely by way of precaution, and is removed as soon as the shell is put into the muzzle of the gun, which is then rammed home with a hollow-headed ramrod.

The first four shells of this construction which were tried, were made partly of iron, but principally of wood, and fired from a 24 pounder against one of the bastions of Kinsale fort, at somewhat more than 100 yards. All of them exploded upon coming in contact with the fort, the splinters flying to a great height in the air. One exploded against a rock at the distance of about 200 yards, and several, fired at a longer range, missed the object. Experiments were afterwards made with iron shells, fired from a 6 pounder, and the first were by no means successful. The shells were cast a great deal too thin, so that a considerable proportion of them exploded in the gun, and we did not succeed once in hitting a target at 240 yards. It may here be noticed as a circumstance well known, that shells are apt to explode in firing, either from being too thin, or from a portion of sand remaining in the chamber after casting, which produces ignition from the concussion of the powder. Notwithstanding all the care that can be taken, this sometimes occurs in action; but as the propelling charge is much greater than the bursting one, the splinters are carried forward, without injury to the gunners.

The second day's experiments, made from the same gun, were more successful. The shells weighed 7 lbs. 2 oz., and the bursting charge was five oz. Eight were fired at a wooden target 8 inches thick, placed immediately in front of a soft slate rock. Several were fired through the target at 60 yards. Seven out of the eight succeeded. The eighth burst in firing. On examining the result, it was found that the explosions had produced a much greater effect upon the rock than upon the target. The target was then placed in an open situation near the sea, and two shells fired through it. It was then observed that the explosions fell behind it, and the splinters were carried several hundred yards into the sea. One shell fired into the sea with 14 oz. of powder, and at an elevation of 6°, appeared to range about 1100 yards.

In the course of the third day's experiments, several still exploded in firing. One however succeeded very well against a thicker target, and two, fired across the bay, exploded against the old castle, one at 800, and the other at 850 yards.

The next experiments were carried on at Leith Fort in the end of 1823. The shells used weighed 10 lbs., the bursting charge 7 oz., and were fired from a 9 pounder, with a charge of 1lb. Two casks filled with sand and stones, and about 4 feet each in diameter, were placed on the sea shore at low-water mark, and a target 4 feet square, and 15 inches thick, made of old ship timber, was placed between them, and backed with sand and stones. Four shells were fired at this, at 180 yards; two of them missed, and the third exploded in one of the casks, the splinters passing out at the other side, and carrying part of the contents of the cask along with them. The fourth struck the target in the centre, and blew it to pieces, leaving an opening in it upwards of 2 feet in diameter. Several of the planks of which it was made were cut asunder, and one of them was blown backwards 12 or 15 feet.

In the next day's experiments, a target was placed at the same range, but being made of decayed timber, and backed with sand only, the effect was by no means so perceptible as on the day preceding. Several exploded between the target and the sand, without doing much execution. Four passed over the target, and ricochéd along the water for several hundred yards. It was observed that they kept circling to the right, in consequence of the grooves running in that direction. One was found in the sand under the target, cone foremost, but without the peg; and another exploded about 20 yards from the gun. From this circumstance, the necessity of adopting some contrivance for keeping the pegs firm in their position became apparent, and also of making them air-tight, in order that the compressed air might not produce ignition. The range was this day tried against that of a round shot fired from the same gun, and the two were reckoned by the artillery officers present to be as nearly as possible equal, 1½ lb. of good powder giving the same range both with shot and shell, as 3 lbs. of that used by the Ordnance.

In the third day's experiments, the shells used were half an inch longer in the sides, and the twist reduced from 48 to 55 inches. The weight of each was 10½ lbs. including the bursting charge of 7½ oz., and they were fired with a charge of 1½ lb. The first was fired into the sea at 15° elevation, and as far as could be judged from the distance of the ships at anchor in the roads, ranged about 2200 yards. The next was fired at the Martello tower in the sea, at 6° elevation, and at a range of 1200 yards. It ricochéd from the water at about 1000 yards, and exploded against the tower. A two-feet thick target was then placed on the shore at about 110 yards, and 5 shells were fired at it. Three of them burst in it, and it was found that they had penetrated about 16 inches before the explosion took effect, and the splinters, after tearing away the back planks, passed into the sea like grape. Their range was then tried on Leith Sands, from a brass field piece. An empty shell weighing 9½ lbs, with a charge of 1½ lb. at 5° elevation, went 1330 yards: another weighing 10½ lbs. went only 1200 yards. It may be observed, that the cone of the former was sharper than that of the latter. Another weighing 10½ lbs. at 11° elevation, went 1820 yards. No wadding was used in any of the experiments either at Leith or Kinsale.

Some further experiments were made at Woolwich in April 1826, on the hull of a 28 gun ship. Part of the shells used were of the same construction as those last tried at Leith, the angle formed by the sides of the cone being 112°. In others it was raised to a right angle, which increased the length of the shell about half an inch. Their weight was about 11lbs. including the bursting charge of 8½ oz. A pasteboard wad, ⅓ of an inch thick, was put over the powder, and made to fit the gun with great exactness, so as to prevent the escape of any part of the charge by the windage. The ship was moored in the river with her broadside to the gun, which was a brass 9 pounder, placed on the shore, at a range of 330 yards. Ten shells were fired from this position, with a charge of 1½lb. Two struck the vessel without exploding; one of them passed through both its sides; the other was found on board, without the peg, and with a piece of wood jammed into the place the peg had occupied. This circumstance leaves no doubt that the peg had, in this case, come out in firing, and renders it probable that the same accident had occurred to the other also. One exploded in firing, and another went over the vessel; two struck the water, and afterwards lodged in the ship without exploding; the remaining four exploded *upon striking her.*

In the second day's experiments, the pegs were fixed more securely by having worsted twisted round them, so as to require a blow equal to a weight of 30 lbs. falling from a height of 3 inches, to produce ignition, and a slip of paper was pasted over each, so as effectually to prevent them from slipping out when the shell was discharged from the gun. The charge was this day increased to 1¾lb. Thirteen rounds were fired at 330 yards: the gun was then retired to 450 yards, and 8 rounds more fired. Out of the 21 fired, 7 were cast so large that they stuck in the gun, and could not be rammed home. From these little could be expected, although two of them succeeded. One exploded in firing, and another on striking the water about 200 yards from the gun. One struck the water very near the ship, and exploded upon passing through the ship's side, a little above the water mark. Six struck the vessel, all of which exploded; the others either went too high or too low, and the whole of those that went too low, rose again from the water and lodged in the ship. Upon examining the vessel, it was found that the shells had passed through her side before the explosion took effect, and that the splinters had done more than usual execution. They had gone through a cast iron caboose, torn up part of the deck, thrown down great part of a wooden partition that ran along between decks, and some of them had lodged in the opposite side of the ship. One shell cut a strong chain cable in two, which was put round the vessel for the purpose of raising her, in the event of her being sunk, some of the splinters flying backwards on explosion, while the remainder passed through her side. Another fired from the farthest range, passed through the mainmast close to the deck, and set it on fire towards the centre, so as to render it necessary to cut part of it away before the fire could be extinguished.

In the third and last day's experiments, 10 of the longer shells were fired empty, at a very large target placed against the practice bank, from a brass gun, at a range of 800 yards, with a charge of 1¾ lb. of powder. The two first were fired at 3° elevation, and passed a great way over the bank. The remaining 8 were fired at 2¾° elevation; two of those also went over, 1 grazed short, 2 passed through the target, one of them after grazing 2 yards short, and the other 3 struck the bank not far from the target; the range therefore of the shells here used, when filled and fired at 3° elevation, may be about 1300 yards. One dug out of the bank appeared to have turned; two others, on being dug out, were found cone foremost, and another that passed through the target, left a hole perfectly circular, evidently showing that it had passed through horizontally. Four of the shorter shells were fired at 600 yards, but were found very inferior to the longer ones, both in range and accuracy of fire.

Having detailed the experiments that have been made with percussion shells, as accurately as I can, and as fully as seems to be necessary, I shall now conclude with some observations connected with the subject.

So far as range and efficiency are concerned, the experiments have perhaps been as successful as could have been expected, in so novel an invention. With respect to accuracy of fire, I am fully sensible that much still remains to be done; and to those who ask why this most important object was not more completely attained before the discovery was submitted to the public, I beg leave to answer, that no invention in gunnery, so far as I am aware, either in former or more recent times, has ever been brought to perfection without the help of long continued and laborious experiments, which from their nature are so expensive, that they cannot be expected to be prosecuted at the cost of any individual. In the present instance, only one hundred and four shells have been fired altogether, eighty-five of which were filled with powder, and out of these, thirty-nine exploded upon striking the objects fired at. In the experiments made at Woolwich, on the Pheasant sloop of war, in the river, which are certainly the most important that have yet been made, only eleven succeeded out of thirty-one; no great number certainly, but at the same time enough to have destroyed the vessel, had they been heavy metal. The fire at present is sufficiently accurate for the range at which naval actions are generally fought, but the object in view is to make them available to the full extent of their range, and I shall accordingly point out the means by which I conceive this object may be very much facilitated. Considerable improvement may, I think, yet be made in the mode of casting them; for although the gentlemen of the Carron Company have bestowed great pains on those that were cast by them, it seldom happens in matters of this kind, that the most simple process is discovered in the first instance. They may also be turned in a lathe, by means of machinery, which their shape will allow them to be with great facility, and thus rendered perfectly cylindrical, and of the same size. In the course of the experiments that have been made, the shell has also been greatly improved by an addition to the length originally given to it. This might, *a priori*, have been expected, as its weight is increased, without increasing the resistance of the air; and by this alteration its range is found to be increased also. The accuracy of fire is also found to be greater, as the angle of departure is diminished. Hence, greater accuracy of fire may be expected from heavy guns than from light ones, the sides of the shells in the former being much longer than in the latter, while the windage in both is equal. The following construction of a shell for a 9 pounder, I consider as the best, so far as the experiments made will allow me to determine. In these proportions, the length of the sides is increased half an inch beyond that of any yet used, and the twist is reduced from 55 to 72 inches.

Length - - -	6,24 inches.
Diameter - -	4,16
Length of sides - - -	4,16
Height of cone - -	2,08
Depth of grooves - -	0,2
Width of ditto round the circumference	0,8
Length of peg - -	1,4
Diameter of ditto - -	0,4
Diameter of vent - -	0,15
Thickness of sides - -	0,85
Thickness of bottom - -	0,74
Diameter of chamber - -	2,46
Height of ditto - -	3,42
Windage - - -	0,04

In all the experiments already made, it has been observed that the line of fire is generally good, but that the shells which have missed the object, went almost invariably either too high or too low, which is exactly the result we might from theory expect, when their ends are not perfectly balanced. The method of proving whether their ends be exactly balanced, is by floating them in mercury, their specific gravity when filled being something less than half that of mercury. When properly balanced, they float horizontally, and the balance is not perfect until that is effected.

It is conceived that shells of this description might be used against towns and stockades, for battering in breach, and also in the field; but it is evidently in naval warfare that they would be most efficient, for the burning of shipping. That for a 24 pounder will weigh 30lbs. and contain 2¼ lbs. of powder, or what is reckoned better, powder and combustible matter in equal parts. The composition of portfire is recommended, as it burns with great intensity, and is not easily extinguished. It may be ground to powder, and mixed with the bursting charge. If a shell of this size exploded on board a ship of war, it would be difficult to extinguish it under any circumstances, and if it passed through near the water-mark, almost impossible. Under these circumstances, ships lying low in the water, might possibly have an advantage over larger ones, from being less exposed to the fire of an enemy.

If ever the weapon should be used in war, it is only to be hoped that it may have a fair and impartial trial; and if it stand the test, the consequences may be considerable.

II. *On the relative powers of various metallic substances as conductors of electricity. By Mr.* William Snow Harris, *of Plymouth, Surgeon. Communicated by* J. Knowles, *Esq. F.R.S. November* 14, 1826.

Read December 14, 1826.

The relation between metallic bodies, as conductors of electricity, has engaged the attention of those whose talents have, at various periods, enriched that branch of science; I enter therefore upon a further investigation of this interesting subject with much diffidence; but having, by an easy method, obtained a series of results, apparently calculated to advance our knowledge of it, I am led to hope that a short account of my inquiries may be honoured by the notice of the Royal Society.

It has been long since observed by one of the most active contributors* to the success of modern science, that the heat evolved by a metallic body, whilst transmitting an electrical charge, is in some inverse ratio to its conducting power—a principle generally admitted, not only as a reasonable deduction, but also as being established by a great variety of facts; I have therefore sought to measure the relative degree of heat, so evolved, by various metallic substances in a gazeous medium such as air, and thus to discover their precise relations as conductors of electricity.

I employed for this purpose a very simple instrument, (represented by fig. 1. in the annexed plate,) which may be

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* Mr. Children's Experiments with a large Galvanic Battery.

considered as little more than an air thermometer, the metal to be examined being drawn into a wire and passed air tight through the bulb. There is a glass tube, whose interior diameter is regular, and somewhat less than $\frac{1}{10}$th of an inch; one of its extremities is bent upwards and outwards for about two inches, and is united by welding to a short glass cup; this last contains a small quantity of coloured spirit. The opposite leg of the tube is sustained by a graduated scale, fixed upon a convenient base. The point at which the coloured spirit rests in this leg is marked zero. Upon the cup is screwed a glass ball of 3 inches diameter, having the metallic wire to be examined passed air tight across its centre. To effect this, there are two flanches of brass carefully cemented about the holes in its sides, each flanch has a small projecting shoulder, which receives the wire, and upon which is finally screwed a small brass ball; this ball has a flattened part to bear against a similar part of the flanch, and thus by a thin collar of leather, the whole is rendered air tight. The wire is secured firmly in its situation by means of a small peg of wood, and the holes in the balls are sufficiently deep to allow both the extremity of the wire and the peg to project a little, for the convenience of removal; thus the substitution of one wire for another is very simple and expeditious. Besides these flanches and balls, the bulb is also furnished with a sort of valve, attached in a similar way to its upper part, which being rendered air tight by a screw, can be occasionally opened so as to form a communication with the external air, and thus the coloured spirit may at all times be adjusted to zero. Fig. 2. Under these circumstances when an electrical explosion of sufficient force is passed through the wire in the bulb, the relative degree of heat it evolves is made evident by the ascent of the fluid along the graduated scale.

I submitted to experiment, by means of this instrument, the following metallic substances; silver, copper, gold, zinc, platinum, iron, tin, lead, alloys of gold and copper, of silver and copper, of silver and gold, brass, alloys of tin and lead, of tin and zinc, and an alloy of tin and copper. The above-named metals were carefully drawn through successive holes in a plate of steel until their diameters were the same, and in order to insure the transmission of an equal and similar explosion through each metal, I adopted the following method; two brass balls of the same dimensions were fixed at a given distance from each other, as in Lane's well known discharging electrometer; one of these balls, being insulated, was placed in immediate connection with the positive side of the battery, whilst the other was connected with the negative side, the metal to be examined forming part of the circuit. This last connection was effected by means of two fixed copper wires, inserted into the balls on each side of the glass bulb, and made perfect at the points of junction. When therefore the charge, accumulating in the battery, acquired a sufficient intensity to pass the given interval, the discharge took place through the wire in the bulb, which thus became immediately the subject of experiment.

The battery consisted of five jars, each containing five square feet of coated surface. They were placed on a metallic base communicating with the negative conductor, and were charged by means of long copper rods projecting immediately from the bottom of each jar.

The electrical machine employed to charge this battery consisted of a circular plate of glass, three feet in diameter, mounted between two horizontal supports of mahogany, the rubbers being insulated on glass pillars at each side of the plate, and joined together behind it by means of a curvilinear tube of brass, which formed the *negative* conductor; whilst the *prime* conductor projected vertically from the front of the frame.

The following table exhibits the results deduced from an extensive series of experiments on the different metallic substances above-named, in which the effect of the explosion is placed opposite the corresponding metal.

	Metals.	Effects.		Metals.	Effects.
	Copper	6	Alloys.	Copper 1 part, silver 1 part	6
	Silver	6		Copper 1 part, silver 3 parts	6
	Gold	9		Copper 3 parts, silver 1 part	6
	Zinc	18	Alloys.	Gold 1 part, silver 1 part	20
	Platinum	30		Gold 1 part, silver 3 parts	15
	Iron	30		Gold 3 parts, silver 1 part	25
	Tin	36	Alloys.	Tin 1 part, lead 1 part	54
	Lead	72		Tin 3 parts, lead 1 part	45
	Brass	18		Tin 1 part, lead 3 parts	63
Alloys.	Gold 1 part, copper 1 part	20	Alls.	Tin 1 part, zinc 1 part	27
	Gold 3 parts, copper 1 part	25		Tin 3 parts, zinc 1 part	32
	Gold 1 part, copper 3 parts	15		Copper 8 parts, tin 1 part	18

There are some interesting circumstances observable by reference to this table. If we consider the heat to be in the inverse ratio to the conducting power, it appears; 1st. That the heats evolved from silver and copper are alike, as also those of iron and platinum, and likewise zinc and brass; whilst the heats evolved from lead and tin, compared with each other are as 2 : 1; the same may be said of zinc and gold, or brass and gold.

2ndly. Considering silver and copper as the *best* conductors, (being the least heated by the explosion) then the conducting power of

Gold to copper or silver is as -	2 : 3
Zinc or brass to copper or silver -	1 : 3
Platinum or iron to copper or silver -	1 : 5
Tin to copper or silver - -	1 : 6
Lead to copper or silver - -	1 : 12

3rdly. It may be observed that the conducting power of metals, when alloyed, is variously affected: thus the conducting power of gold and copper, or gold and silver, when alloyed together, is *less* than either of these metals in a separate state; and the difference in the conducting power increases with the quantity of the inferior conductor alloyed. Thus, gold one part, with copper three parts, had its temperature raised to 15° of the scale; gold and copper in equal parts to 20°; gold three parts, with copper one part, to 25°: the same may be said of gold and silver; whilst an alloy of copper and silver, in similar proportions, does not vary from either of the metals separately.

Tin and lead, alloyed, appears to give a conducting power formed by that of each metal taken singly, and varying, as above, with the quantity of the inferior conductor; thus, an alloy of lead and tin in equal parts, gives an effect equal to one-half the effect on tin, added to one-half the effect on lead, and so on; the same may be said of zinc and tin.

4thly. It is observable that a very small quantity of alloy may influence materially the conducting power; thus copper alloyed with only one-eighth part of its weight of tin, becomes heated by an electrical explosion as much as iron. In accordance with this fact, it was found that wires drawn from some foreign gold coins, said to be very pure, were much worse conductors of electricity, than when drawn from the same previously refined.

I did not find the conducting power to be influenced by any new disposition or arrangement of the quantity of metal; thus, whether the metallic wire was perfectly cylindrical, flattened into a ribbon, or separated into four smaller wires, the effect produced was in each case alike.

The influence of a small portion of alloy on the conducting power renders it necessary to have the metals pure, and I have reason to believe that the specimens, which were in these instances made the subjects of experiment, were as nearly so as possible.

The alloys were prepared by fusing the metals together with a common blow-pipe on a charcoal support, having previously weighed the relative proportions; after which the small button of metal was again weighed and drawn into wire. I am not aware that this method of forming alloys with small quantities of metal is liable to any material error.

The wires operated on in the course of this investigation varied from the $\frac{1}{40}$th to the $\frac{1}{80}$th of an inch in diameter, below which it was not found desirable to reduce them.

EXPLANATION OF PLATE II.

abcd The glass bulb.

ab The wire.

d The brass caps and screws which unite the ball to the glass cup.

e The glass cup.

c A small valve.

ab The flanches and balls.

gfe The glass tube.

gf The scale.

s The stand.

P*a*. *b*N The connecting wires.

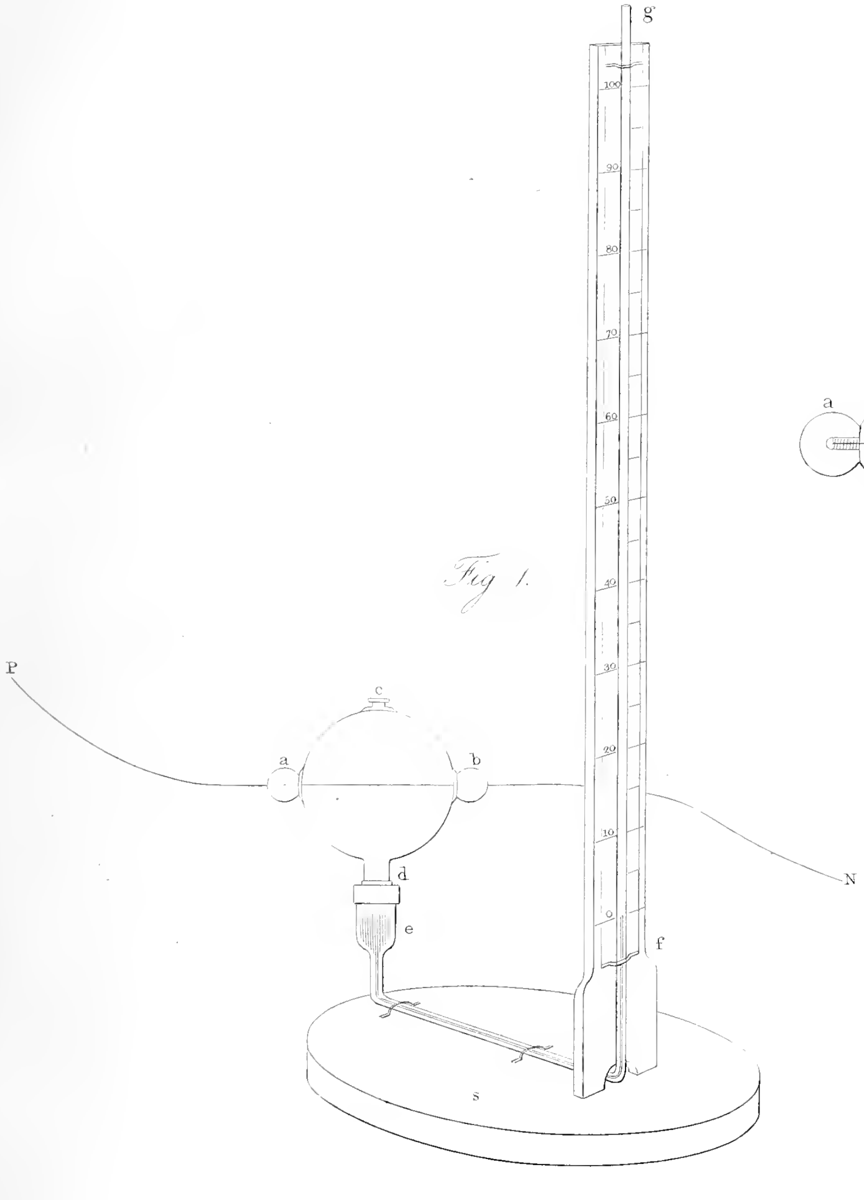

Fig 1.

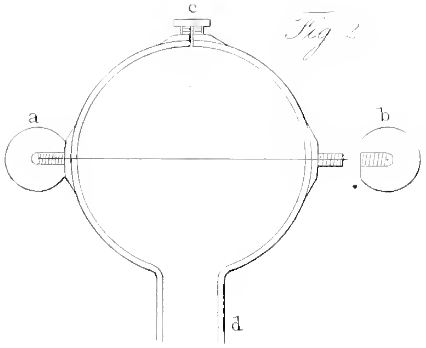

Fig 2.

III. *On the expediency of assigning specific names to all such functions of simple elements as represent definite physical properties; with the suggestion of a new term in mechanics; illustrated by an investigation of the machine moved by recoil, and also by some observations on the Steam Engine.* By DAVIES GILBERT, *Esq. M. P. V. P. R. S. &c.*

Read January 25, 1827.

THE expediency of distinguishing by separate appellations, all such functions of simple elements as measure the intensity of physical properties, will be rendered obvious by referring to the well known controversy respecting motion.

Scarcely had the principles which regulate the action of bodies in motion become subjected to mathematical calculation, when a dispute arose as to the measure of motion itself; a dispute conducted with much more vehemence and acrimony than might be supposed incident to the nature of an abstract subject.

Several individuals of the greatest learning and reputation contended that weight, multiplied by velocity, ($w \times v$) gave a product always proportionate to the motion of bodies, as was proved by a comparison of their inertiæ, by all the properties relative to the common centre of gravity in planetary systems, &c. &c. &c.; while other persons, scarcely inferior to the former, adverting to the collision of elastic bodies, and to the extremely curious property of motion, the conservatio virium vivarum, contended with equal confidence, that the true measure of motion was weight multiplied into the velocity squared ($w \times v^2$); till, in the early part of the last century it was fortunately observed, that the different properties indicated by these two functions were not, in any respect, at variance with each other; and the terms *momentum* and *impetus* reconciled all opinions, by removing every ground for dispute.

For a full and detailed explanation of this subject, I would refer to the admirable treatise on "The Rectilinear Motion and Rotation of Bodies," by the late GEORGE ATWOOD, Fellow of Trinity College, Cambridge, F. R. S. printed in 1784; and to the Bakerian Lecture in the Philosophical Transactions for 1806, by an existing member of this Society, who has never touched any point of science that he has not elucidated and adorned.

In this Lecture it is observed, that neither impetus nor momentum have usually much to do with the action of ordinary machines; which is undoubtedly true; since neither of these functions measures directly their efficient power. The criterion of their efficiency is force multiplied by the space through which it acts ($f \times s$); and the effect which they produce, measured in the same way, has been denominated *duty*, a term first introduced by Mr. WATT, in ascertaining the comparative merit of steam engines, when he assumed one pound raised one foot high, for what has since been called, in other countries, the dynamic unit.* And by this criterion, one bushel of coal, weighing 84lbs. has been found to perform a duty of thirty, forty, and even fifty millions; augmenting

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* The dynamic unit used in France is a cubic metre (3,2809 feet) of water raised through a metre in height = 7220 pounds of water one foot high, being to a million as 1 : 138,5 as 72 : 10000 nearly; the log. 2,1413966.

with improvements chiefly in the fire place, which produce a more rapid combustion, with consequently increased temperature, and a more complete absorption of the generated heat; in addition to expansive working, and to the use of steam raised considerably above atmospheric pressure.

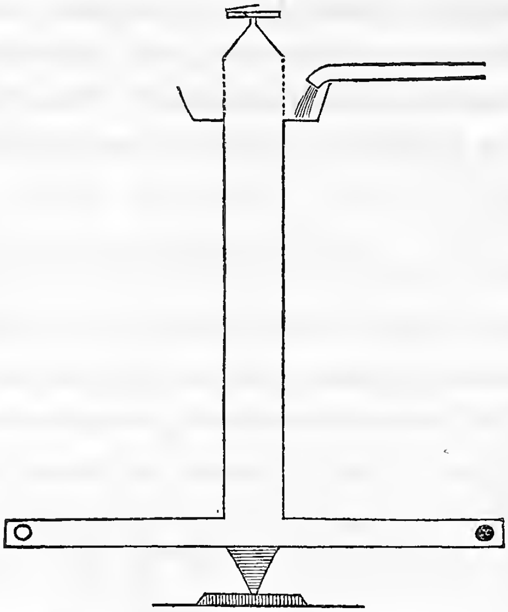
Since therefore a machine is efficient in producing duty, or effect, in proportion to the force applied, multiplied into the space through which it acts, I propose to denominate this function $(f \times s)$ *efficiency*; retaining the word *duty* for a similar function indicative of the work performed.

And it is obvious, that by a comparison of these two quantities, the *efficiency* expended on any machine, and the *duty* performed by it, an exact measure will be ascertained of its intrinsic worth.

Having never seen a full investigation of the engine moved by recoil, I am partly, on that account, induced to take it as an example of the utility to be derived from the use of this new term; and I am in part also induced to do so, by the circumstance of a most respectable society having last year held out the properties of BARKER's mill to the public, as fit objects for inquiry.

To investigate the Recoil engine.

Let every thing be taken in the abstract, and after a manner most advantageous to the machine. Suppose friction to vanish, and that the perpendicular tube, and the arms, are so capacious as to allow of the fluid issuing with a velocity equal to what a body would acquire by falling in free space, through an elevation equal to that of the perpendicular tube, or through, what is usually termed, the head. See Principia, B. 2. § 7.



Let l = the space in English feet through which a body freely descends in one second of time.

Then $2\,l$ = the velocity acquired.

h = the length of the perpendicular tube, or the actual height of the head.

Then $2\sqrt{h\,l}$ = the velocity due to the head.

Let R = the length of either arm from the centre to the orifice.

x = the velocity with which that point moves in revolving, expressed by feet in a second.

Then $\frac{x^2}{2\,l\,R}$ = the centrifugal force at the orifice.

And since the force exerted by each particle of the fluid varies as its distance from the centre

$\frac{x^2}{2\,l\,R} \times \frac{R}{2} = \frac{x^2}{4\,l}$ = the whole pressure from centrifugal force. An expression independent of the length of the arm; and the slightest reflection will make it appear that no alteration can arise from deflecting the form of the arm out of a straight line.

Then, since the pressure exerted by the actual head is expressed by its height (h), and the pressure from centrifugal force by $\frac{x^2}{4l}$,

$h + \frac{x^2}{4l}$ = the pressure sustained, or the virtual head, and $\left(h + \frac{x^2}{4l}\right) \times x$ = the efficiency applied to the machine, in relation however, not to the expenditure of fluid corresponding to the height (h), but to an expenditure greater in the proportion of $\sqrt{\left(1 + \frac{x^2}{4hl}\right)}$ to 1. But a portion of this efficiency must, of necessity, be expended in giving the rotary velocity (x) to the quantity of fluid issuing $= \sqrt{1 + \frac{x^2}{4lh}}$. Now the height due to x is $\frac{x^2}{4l}$, consequently $\sqrt{1 + \frac{x^2}{4lh}} \times \frac{x^2}{4l} \times x$ = the expenditure of efficiency in producing the rotary motion. And $\left(h + \frac{x^2}{4l} - \sqrt{1 + \frac{x^2}{4hl}} \times \frac{x^2}{4l}\right) \times x$ = the efficiency applied to the machine capable of producing duty.

Now substitute for x, (the velocity due to h) ($2\sqrt{hl}$) multiplied by an arbitrary quantity y; then $\sqrt{1 + \frac{x^2}{4hl}}$ (or the quantity of fluid issuing) $= \sqrt{1 + y^2}$ and $(y + y^3 - \sqrt{1 + y^2} \times y^3) \times 2 h^{\frac{3}{2}} l^{\frac{1}{2}}$ = the efficiency; and dividing by $\sqrt{1 + y^2}$ $\sqrt{1 + y^2} \times y^3 \times 2 h^{\frac{3}{2}} l^{\frac{1}{2}}$ = the efficiency that would be applied if no more fluid issued than what is due to the actual head (h).

The following Table exhibits the value of both these functions, taking (y) from *o* (where the duty must obviously be nothing) by steps of $\frac{5}{100}$ths to 1.25; after which the duty becomes negative.

$l = 16.0954$ feet the log. 1.2067016

If the angular velocity of a body in one second of time be expressed in whole revolutions in degrees, in minutes, or in seconds, the absolute velocity will be =

$$R \times 2cr = Rr \times 6.2831853 \quad . \quad . \quad . \quad \text{log. } 0.7981799$$

$$D^{\circ} \times \frac{2cr}{360} = D^{\circ} r \times 0.0174533 \quad . \quad . \quad \text{log. } 8.2418774$$

$$M' \times \frac{2cr}{360 \times 60} = M'r \times 0.00029089 \quad . \ \text{log. } 6.4637261$$

$$S'' \times \frac{2cr}{360 \times 60 \times 60} = S''r \times 0.00000484817 \ \text{log.} 4.6855748$$

y	The Duty for a given expenditure of fluid, on the function $\sqrt{1+y^2} \times y - y^3$	y	The Duty performed by the actual expenditure of fluid, or the function $y + y^3 - \sqrt{1+y^2} \times y^3$
.05	.04994	.05	.05000
.10	.09950	.10	.10000
.15	.14830	.15	.14997
.20	.19596	.20	.19984
.25	.24207	.25	.24952
.30	.28621	.30	.29881
.35	.32794	.35	.34762
.40	.36681	.40	.38607
.45	.40234	.45	.41159
.50	.43402	.50	.48525
.55	.46132	.55	.52649
.60	.48371	.60	.56410
.65	.50062	.65	.59706
.70	.51146	.70	.62432
.75	.51563	.75	.64454
.80	.51250	.80	.65632
.85	.50145	.85	.65812
.90	.48183	.90	.64824
.95	.45297	.95	.62479
1.00	.41421	1.00	.58578
1.05	.36488	1.05	.52908
1.10	.30427	1.10	.45233
1.15	.23170	1.15	.35311
1,20	.14646	1.20	.22878
1.25	.04785	1.25	.07660
1.28	—.01803 negative	1.28	—.02929 negative

The first of these functions reaches the maximum, when y exceeds .75 or $\frac{3}{4}$th by two or three thousandths; and the second when y is about .83.

From an inspection of the Table, it appears that when the machine is moving with the velocity productive of its greatest relative effect, namely, at three quarter parts of the velocity with which the fluid would issue under a pressure equal to that of the actual head, that the duty exceeds by a mere trifle one half of the efficiency expended; and from thence it obviously follows, that the recoil engine cannot in any case be employed with advantage. The water wheel, whenever the fall does not exceed forty or fifty feet; and the pressure engine, provided with an air vessel, in all cases where the fall is greater, receive an efficiency almost equal to the whole expended, instead of one half; and in practice, the duty will reach three quarters. The wheel moreover possesses a self-regulating power, sufficiently accurate for the present illustration, and somewhat analogous to the isochronism of the pendulum; that is, the actual weight of water on the wheel, and its velocity of rotation, must always be reciprocal to each other; thus maintaining a given efficiency, independent of velocity. The received efficiency of a water wheel being represented by the pounds of water passing over it × by the fall in feet, less the height due to the velocity with which the periphery moves in its rotation. But in the recoil engine, the moving power is expending at the rate due to the height even when the machine is actually standing still; and when it moves with a velocity exceeding that due to the length of the head, in the proportion of about 128 : 1 (where the duty is again nothing) the moving power will be expended at the increased rate of $\sqrt{1 + (1.28)^2} : 1$.

Here too may be easily shown the utter impossibility of executing a plan, which would not indeed deserve any notice, were it not that two ingenious practical engineers have within these few years, although at different times, and independently of each other, incurred large expense, and employed much pains in attempting to apply steam on the principle of recoil.

It is obvious that one of the factors, namely force (or the pressure of recoil), must in this case be extremely small, since an aperture of one inch square will discharge about $3\frac{2}{10}$ imperial gallons of water each minute in the form of steam, unassisted by centrifugal force, while the pressure amounts to no more than 14,66 pounds. The other factor (velocity) must therefore be made proportionably great to produce an adequate effect.

Now, since air rushes into a vacuum with the velocity of 1295 feet in a second - - - log. 3.1122698

And steam at 212° is said to have a specific gravity compared to air of 0.6235

Log. $9.7948365 \div 2 = 9.8974183$ arith. comp. 0.1025817

A velocity of 1640 feet in a second 3.2148515

Three quarters of this velocity in rotation will be requisite for imparting an efficiency to the machine of about one half of the efficiency expended. But such a velocity nearly equals that of a cannon shot, and the effect of centrifugal force on the arms $\left(\frac{x^2}{4l}\right)$ would produce a strain equal to the weight of 41776 feet of the material composing them, independently of the strain arising from centrifugal force of the fluid they contain. And as the modulus of tenacity for iron is estimated at 14800 feet, the centrifugal force will exceed that tenacity almost three times. But independently of this insuperable obstacle, any velocity adequate to the production of an useful efficiency must be utterly unmanageable in every other respect; nor is it easy to imagine the possibility of any expedient, similar to the air pump of Mr. WATT, for maintaining the vacuum in which their arms are to revolve.

The Steam Engine.

To estimate the efficiency of steam acting uniformly with its entire force, at a power corresponding to 30 inches of barometrical pressure, in reference to the consumption of one bushel of coal, I assume, from repeated experiments, that

14 cubic feet of water may be converted into steam, by that given consumption of fuel, occupying about 1330 times more space than the water from which it was produced. The whole space therefore filled by the steam amounts to 18620 cubic feet, capable of containing 1'150410 pounds of water; and as 30 inches of mercury at a specific gravity of 13.568 correspond to a column of water 33.92 feet in length, through this space the 1'150410 pounds of water may be supposed to be raised when a vacuum is formed, giving an efficiency of 39'361000, or above thirty nine millions of pounds raised one foot high.

But when this power of steam is applied through the medium of a piston, acting in the cylinder of a steam engine, large deductions must be made to arrive at the practical duty.

The air pump is usually constructed of a size to require about one eighth part of the efficiency;

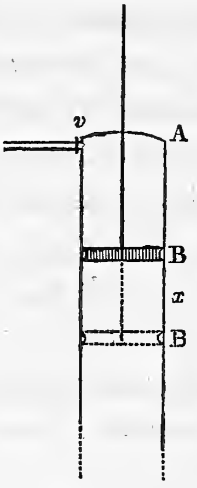
Some imperfection in the vacuum cannot be avoided;

And friction must exist to a considerable amount where bodies in motion have to confine an elastic vapour, ready to escape through the smallest aperture. So that thirty millions would probably have exceeded the attainable limit of duty, but for two expedients:

First. Causing the steam to act expansively, after exerting its whole force through a certain part of the cylinder; and

Secondly. Raising the temperature much above 212° of Fahrenheit, and thereby communicating to the steam a power beyond what is termed atmospheric strength; but this is, of course, accompanied by an additional expenditure of fuel.

The plan of working expansively is founded on the following investigation.



Let A B represent the portion of the cylinder through which the piston is urged by the entire force of the steam, and let its length be expressed by unity. Then let the supply of steam from the boiler be cut off by the valve at (v) and conceive the piston to have descended to B through the space (x) urged by the expansive elasticity of the steam, the density of which will then be $\frac{1}{1+x}$, and assuming that densities and elasticities are proportionate, $\frac{\dot{x}}{1+x}$ will be the fluxion of the efficiency, and the efficiency itself equal the nat. log. of $\overline{1+x}$. And the efficiency of the whole stroke from A to B will be 1 + nat. log. $\overline{1+x}$, giving this very curious result, that when x is infinite, the efficiency also exceeds any assignable limit.

$x=1$	$1+n.\overline{l.\,1+x}=1.693$
2	2.099
3	2.386
4	2.609
5	2.792
6	2.946
7	3.079
8	3.197
9	3.303
10	3.398

But it is obvious that several important circumstances have been omitted:

The reduction of temperature, and consequently of elasticity, caused by rarefaction, as the steam expands:

The increased relative effect occasioned by the reaction of the imperfect vacuum:

The increased relative burden of the air pump.

The relative increase of friction.

So that although this mode of working is highly advantageous, yet it is probable that, in practice, not more than from one half to two thirds of the cylinder can beneficially be occupied by expansive steam.

This mode of working has yet a further beneficial effect. The steam, beginning to act with its entire force, imparts the requisite velocity, by rapidly overcoming the inertia; and this force diminishing in the latter part of the stroke, allows the machine to exhaust the motion imparted, as it tends towards its close; thus also avoiding all violent percussion, which must obviously prove most injurious to the machinery, and in other respects be a pure loss.

It would be unfair towards the memory of a very ingenious man, were I to omit noticing, that much about the time when Mr. WATT introduced this method of using the expansive power of steam, a modification of the same principle was invented by Mr. JONATHAN HORNBLOWER, of Penryn in Cornwall. According to this method, instead of condensing the steam after it had passed under the piston, in the working cylinder, the steam is allowed to expand itself over a second piston in a second cylinder, when it is finally condensed in the usual manner; and if the second cylinder exceeds the first in the proportion of $1 + x$ to 1, the theoretical effect will be identical with that produced by the former method. But the mode invented by Mr. WATT excels the other in simplicity, and causes less friction.

The second expedient, that of employing steam at an high degree of temperature, and consequently of pressure, suggested itself in the following manner.

Experiments had shown, that a quantity of heat about nine hundred times greater than what is required to raise the temperature of a given weight of water one degree of Fahrenheit's thermometer, becomes latent in the conversion of that water into steam. That an elevation of temperature to

40° of Fahrenheit above 212° doubles the steam's elastic force, and that 30° more treble it; great expectations were consequently formed of the increased power that would be obtained, from a given consumption of fuel, by using high pressure steam; but the following most important circumstances were overlooked, till experience forced them into view.

That the temperature of steam cannot be raised, with a speed suited to mechanical purposes, in any other way than through the medium of its generating water.

And that water heated above 212° sends off more steam, with a proportionate absorption of latent heat, increasing the density of the steam previously generated of atmospheric elasticity, thus doubling its power at $212° + 40°$; but at an expense of fuel, not much inferior to the power gained.

If steam could be heated, as an independent elastic fluid, expanding $\frac{1}{480}$th of its bulk for each degree of Fah.; and if its capacity were the same as the capacity of water, a quantity of heat, equal to that rendered latent, would raise the power from unity to $\left.\frac{481}{480}\right)^{900} = 6\frac{1}{2}$. If the capacities of steam and water, according to some recent experiments, be as 1,55 to 1, the increase of power will be from unity to $\left.\frac{481}{480}\right)^{\frac{900}{1,55}} = 3\frac{1}{3}$. But the non-conducting property, common to all elastic fluids, has hitherto prevented this advantage from being obtained. Heated through the medium of its generating water, high pressure steam is believed however to be advantageous; and by its means additional power may be obtained, when an increase is requisite for any temporary use.

A contrivance has recently been attempted, by which a given small quantity of water is forced into a minute boiler at each stroke of the engine, and the boiler, presenting a very large surface in proportion to its content, is kept at an equable high temperature by immersion in a fuzed metal. Thus great degrees of elasticity and of rarity may be combined;

and the plan seems worthy of being fully prosecuted. But the main hope of augmented power, for still further elevating the condition of the human race, by increasing the empire of intellect over brute force, and for accelerating that splendid career, which, originating with the inventions of the plough and of the loom, has acquired such unhoped for rapidity in our own times, appears to rest on the application to mechanical purposes of some fluid more elastic than the vapour of water, according to the suggestion of our President in the Philosophical Transactions for 1823.

The following are a few instances selected from one of the Monthly Statements, published in Cornwall, of the work done by the principal steam engines, July 1826.

Mines.	Engine, and the diameter of the cylinder.	Load per sq. inch, on the piston.	Length of the stroke, in the cylinder	No. of Lifts	Depth.	Diameter of the Pump.	Time.	Consumption of coal, in bushels.	Number of strokes.	Length of the stroke, in the pump.	Load, in pounds.	Pounds lifted one foot high by consuming a bushel of of coal.	No. of strokes, per minute.	Remarks, and Engineer's Names.
		Lbs.	Ft. In.		Fms. ft.	Inches.				Ft. In.				
Wheal Vor	Trelawny's engine, 80 inches, single.	13,3	9 9	5 3 1 1 1	135 2 33 3 11 4 12 0 7 0	15 16 16 9½ 9	July 7th to July 31st.	2633	210490	7 0	89305	49,975,186	6,08	Drawing perpendicularly 135 fms.; and on the underlay 27 f. Main beam over the cylinder. Two balance bobs under ground. Srms and Richards.
Wheal Hope	60 inches single.	6,78	9 0	2 1 1 1	37 4 22 1 5 0 12 1	13 12⅛ 8 7	June 28th to July 29th	945	273730	8 0	21560	49,960,794	6,1	Drawing all the load perpendicularly. Main beam over the cylinder. 1 balance bob at the surface. Grose.
erland	Manor, 80 inches, single.	10,9	9 6	1 3 1	35 2 112 4 23 2	12 15 14⅞	June 27th to July 29th	2619	208880	7 6	72817	43,556,743	4,5	Drawing all the load perpendicularly. Main beam over the cylinder. One balance bob under ground, and one at the surface. Webb.
itto...	Fancy, 80 inches, single.	7,6	9 6	2 1 1	72 0 30 3 21 0	14½ 14¼ 13	Ditto	1825	213670	7 6	50890	44,686,299	4,6	Drawing all the load perpendicularly. Main beam over the cylinder. One balance bob under ground. Webb.

From this document it appears, that several of the large engines now at work in Cornwall, are actually performing a duty about one quarter part greater than would be the whole efficiency of steam, unaided by expansive working, or by high pressure; assuming that fourteen cubic feet of water is the quantity converted into steam by a bushel of coal; but other engines, apparently similar in every respect, fail in performing half this duty; and no satisfactory cause has yet been assigned for the important difference.

As examples of the power and energies of the human mind, these applications of fire, through the medium of an elastic fluid, to mechanical purposes, stand pre-eminently distinguished: as elaborate contrivances useful to man, steam engines are confessedly without a rival.

No series of investigations can therefore be more worthy of minute and accurate attention than one which may enable us to connect by general laws, the temperatures, the densities, the elasticities, the capacities for heat, and the quantities rendered latent in assuming the elastic form; first for steam, and then, if possible, for all other bodies capable of existing in a gaseous state, both in contact with their generating fluids, and in complete separation from them.

I have gone more at length into the principles of the steam engine, than the immediate object of this Paper either requires or can warrant; but I trust that the Society will admit the great importance of the subject as an excuse; a subject with which local circumstances have rendered me familiar during the whole of my life.

IV. *The Croonian Lecture for* 1826. *By Sir* Everard Home, *Bart. V.P.R.S.*

Read November 16, 1826.

The subject of the present Lecture, is an enquiry into the mode by which the propagation of the species is carried on, in the common oyster, and in the large fresh-water muscle.

Aided by Mr. Bauer's microscopical observations, illustrated by his representations of the facts that were ascertained, I have been enabled to lay before the Society many curious particulars respecting self-impregnating animals, which could only be brought to light by an examination of the organs of generation in the field of the microscope; and without the continuance of his assistance, I confess myself unable to prosecute the enquiry.

It is now a period of five years since we entered upon the present investigation, continuing it during the breeding seasons of these two species of bivalves. Having at last brought our labours to a satisfactory conclusion, I shall now detail the observations we have made.

The singular fact of pearls having their origin in the abortive ova of these bivalves, has been already recorded in the Philosophical Transactions. Before that discovery was made, a pearl was imagined to be the nacral covering which the animal has a power of secreting upon any extraneous body, introduced by accident, or otherwise, between the shells, to render its surface equally smooth and polished with the shell itself, and thus prevent it from injuring the substance of the animal in contact with it.

In the Hunterian collection there are many specimens, in which extraneous bodies of different kinds have been introduced within the shells, through holes bored for that purpose, while the animal was alive; which in the course of time received an external coat of nacre, and bear a general resemblance to the pearl; some of these were glass beads, some leaden shot; but the lustre of the pearl cannot, I believe, be imitated, since it is produced by the bright internal surface of the central cell shining through the semitransparent coats which are afterwards formed upon it.

Although the ova of the oyster and fresh-water muscle agree in this one particular, of becoming the nucleus on which pearls are formed, the process gone through before the young is completely formed, is not the same in both species.

As the oyster is more simple in its structure, from having no organs fitted to give it the power of loco motion, which the muscle is provided with, I shall take its mode of propagation first into consideration.

In the whole range of comparative anatomy in which separate organs are developed for the three essential purposes of animal life, sensation, digestion, and propagation of the species, those organs in the oyster are the smallest, the most simple, and have the least to occupy them. Their mode of propagation will be found even more simple than it is in many plants, and the processes that are gone through, are carried on in a much shorter time.

As the following account does not, I believe, accord entirely with those already before the public, either in this country, or in France, I have only to observe, that although others may have laboured the subject for a greater length of time, none, I am sure, have brought more diligence to the enquiry, or have more frequently revised the observations that were first made, with a view to correct any errors that were detected. With respect to the drawings by which the facts are illustrated, I can answer for their fidelity, should any voucher be necessary beyond the author's name.

On the mode of breeding in the Oyster.

The structure of the ovaria is so little developed, that it is difficult to discover these organs; and in the first instance, it requires the aid of the microscope for that purpose, even in the breeding season.

In this country, where the beds are not allowed to be disturbed during the season in which oysters spawn, we labour under a considerable disadvantage in the prosecution of this enquiry; and I am indebted to my friend Mr. COPELAND HUTCHINSON, who procured for me the opportunity of examining some oysters weekly, during the period they are prohibited from being sold in the public market, taken from a private bed near Sheerness.

The situation of the two ovaria, for I consider them to be double, as in fishes, is immediately within the membranes that line the two shells, having the liver placed between them; they consist of a membrane, whose use is not to be ascertained till the ova become visible attached by pedicles, and hanging from it. The structure of the liver resembles so closely that of the ovarium, while containing ova, as only to be distinguished by its more internal situation, and its colour, which is a shade darker.

In the month of March, the ova are so large as to be distinctly seen in the field of the microscope, and are then spherical; as they increase in size, the membrane to which they are attached becomes thickened. In June, they have arrived at their full size, and a white fluid like cream is now noticed surrounding them. That this is the impregnating liquor, is more than rendered probable by the ova dropping, or having dropped from their pedicles, and undergone a change in their appearance, a vesicle having formed, soon after which they leave the ovarium.

For this purpose a tube becomes visible, which before this period could not be detected; it originates by an opening between the two ovaria, which communicates with them both, and forms a sheath, in which the intestine is enclosed; it terminates externally by an orifice between the lips that surround the mouth of the œsophagus. This tube is the oviduct, which is single. The embryo is found when it enters the oviduct, to have already acquired a shell. The young begin to leave the ovaria in the end of June; and at the latter part of July none are to be found either in the ovaria or oviduct. On the 5th of August oysters are brought to market.

The ovaria, after the spawning has taken place, are not visible to the naked eye, but do not become evanescent, since in the microscope, fresh ova are seen in the very early part of their formation.

As fishes are what is called in high season while the ovaria are full of ova, it was to me not a little extraordinary that oysters should, by common consent, be admitted neither to be good nor wholesome, under the same circumstances; for no general law against dredging for them would prevent those possessed of private beds from indulging their appetites during the months of May, June, and July, if they found that oysters were better in those months.

I had an opportunity last July of setting this question at rest; for being at Dieppe, in France, where there is no restriction laid upon oysters, I ordered some for dinner: they had no flavour, none of the company could eat them. I found it equally true in Paris; but the fact is, the period respecting the oyster, which corresponds to the breeding season of fishes, is March and April, when the ova are getting ready for impregnation. In June and July the ova have been impregnated, and may be said to have spawned at the time the embryo is first received into the oviduct, which is in the month of June. At the time the young oysters leave the oviduct, there is a mucus of a purple colour which is voided at the same time, probably for the purpose of supplying them with nourishment while they remain enclosed within the enveloping mantle by which the gills are surrounded.

While in this situation they often become a prey to small sea-worms, which get between the shells, and gorge themselves with the young ones. I have seen these worms with their stomach completely distended with young oysters.

There are many curious structures met with upon the edge of the mantle which encloses the gills. As these appear enlarged in the breeding seasons, their uses may be applicable to the growth of the animal, of the shell, or the formation of nacre. Accurate representations of them are given, that others may be enabled to consider them, which is no part of the present enquiry.

The stomach and intestine of the oyster I have upon another occasion considered and delineated, and laid before the Society.

The heart has a near resemblance to that of the teredines, having two auricles and one ventricle.

On the mode of breeding of the fresh-water muscle.

In this bivalve, the ovaria in their situation and appearance are the same as in the oyster. The ova arrive apparently at the same size before they are impregnated, which in them also takes place in the ovaria.

The ova, while attached by their pedicles to the membrane of the ovarium, have an appearance only to be distinguished from the structure of the liver by the difference of colour.

About the 10th of August the ova are completely formed in the ovaria, and are detected about the 20th of the same month passing into the oviduct, which is a curiously trellised structure situated between the membranes that compose the bronchiæ; and about the 12th of September they have all arrived there.

That impregnation has preceded this change of situation is evident from the ovum having been formed into a vesicle, through the coats of which vesicle, very soon after it has been retained there, the embryo is distinctly seen surrounded by a fluid, opening and shutting the incipient shells for the aeration, and probably the nourishment of the fœtus in this stage of its growth.

While in this situation, many of the young were seen turning round as it were upon a centre. This motion had been taken notice of by LEWENHOEK, who thought it so extraordinary that he did not wish the fact to rest upon his own evidence, and called his wife and daughter, that they might bear testimony of its having taken place. When Mr. BAUER first met with it, the same notion occurred to him, of wishing to have other witnesses than his own eyes. He called in a young female servant, and having directed her eye upon the object, he asked what she saw?—a little white thing turning round and round.

This revolving motion of the embryo very naturally attracted my particular consideration; and having seen the porcelain manufactory at Worcester, it bore so close a resemblance to the circular motion given to the pieces of clay out of which plates and saucers are formed, that for some time I was completely deceived; but Mr. BAUER's close and persevering examination very soon detected the true cause of this strange phenomenon, which was produced by a small worm that had got into the vesicle, and while feeding on the embryo, performed these revolutions, carrying the young muscle round along with it, although itself concealed from the eye of the observer.

The young remain in the oviduct, the interior of which has a greater resemblance to the honey comb in which the young bees are deposited, than any thing else I am acquainted with, till they arrive at the size fitting them to provide for themselves; they leave the oviduct in October and November.

When the young are ready to leave their cellular prison, a canal is formed through which they pass out; and as the foot of the parent muscle is partly surrounded by a portion of the oviduct, when the foot is extended in the progressive motion of the animal, that portion of the oviduct is also carried beyond the external shells, so that the young will have every facility in being set entirely at liberty. This happens, as I have mentioned, in the months of October and November, towards the end of which they have all escaped; and even at this time young ova are found in the ovarium, preparing for the next season.

EXPLANATION OF THE PLATES.

Plate III. Fig. 1. An oyster with the convex shell uppermost, to identify the species to which it belongs.

Fig. 2. The same oyster, the convex shell being removed, showing the whole oyster, with the entire cloak, &c. &c.

Fig. 3. The same oyster, the cloak and one layer of the beard being removed, to show the ovarium and oviduct; from the edge of the ovarium is a small slice cut off, to exhibit the ova in their natural situation in an early state of pregnancy.

Fig. 4. An oyster in the act of emitting its living young ones, which are enveloped in a purple coloured mucus.

N. B. All these four figures are natural size.

Plate IV. Fig. 1. A perpendicular section of an oyster, to exhibit the course of the alimentary canal, the oviduct, the heart, &c. &c. the cloak and the beard being removed; magnified 2 diameters.

Fig. 2. A very small portion of the ovarium, with the ova imbedded in its substance; magnified 100 diameters.

Fig. 3. Some ova extracted from the same ovarium, as they

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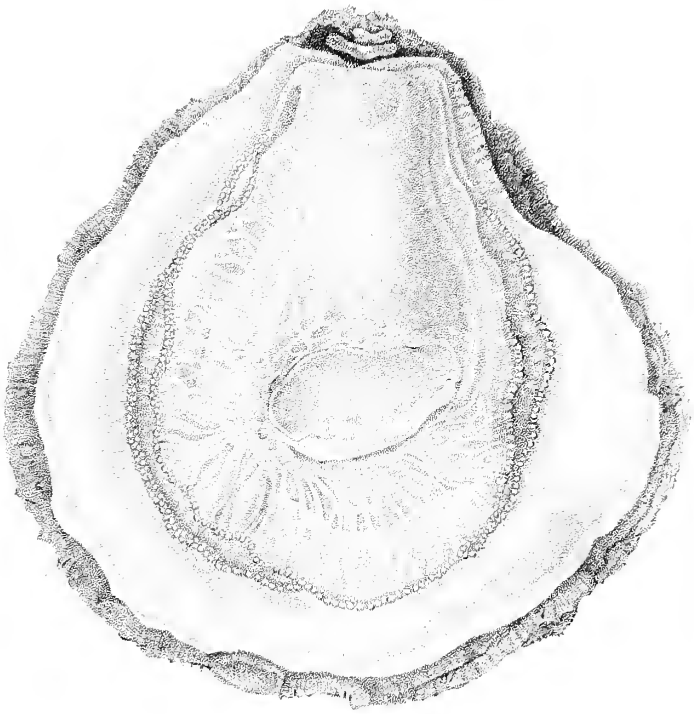

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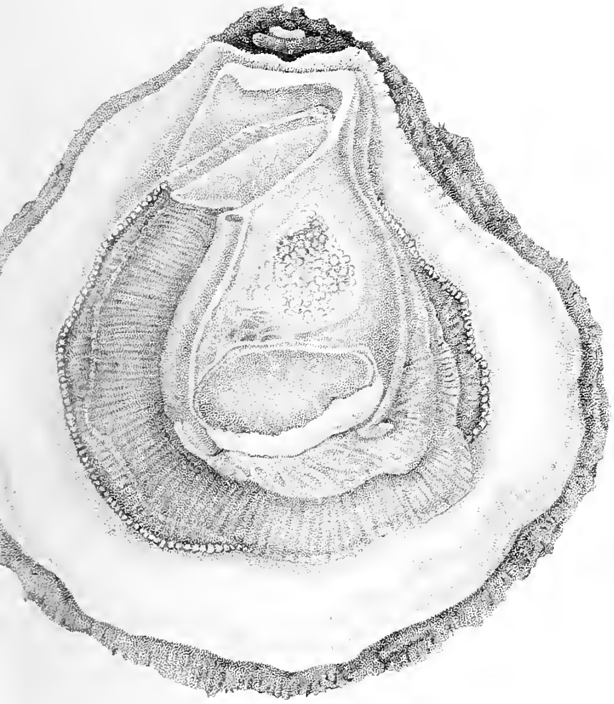

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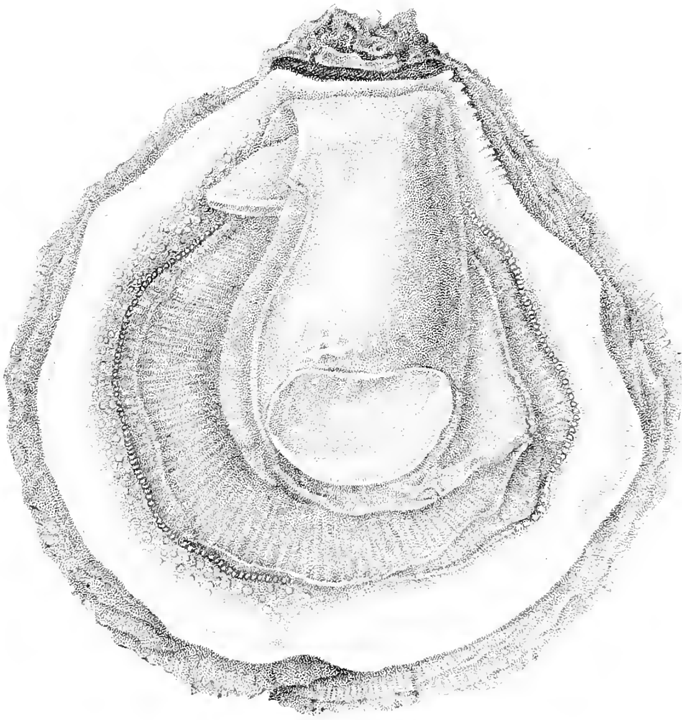

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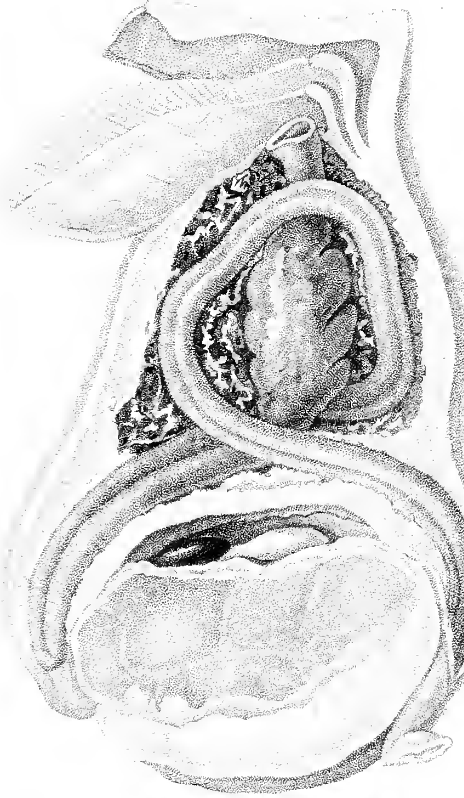

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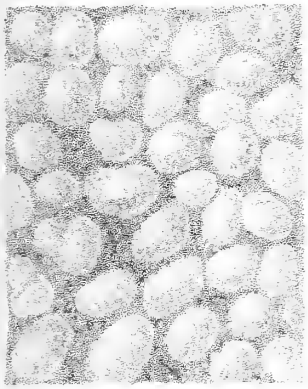

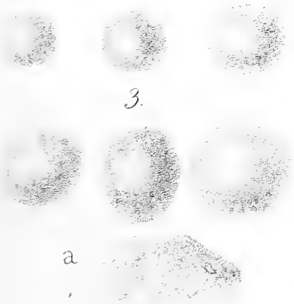

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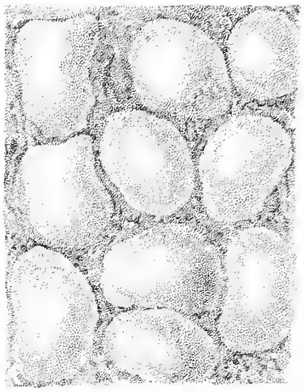

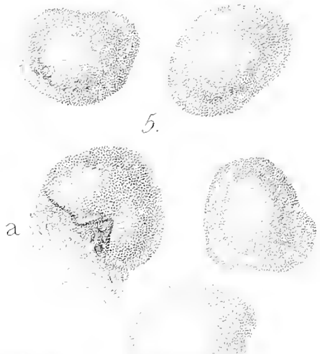

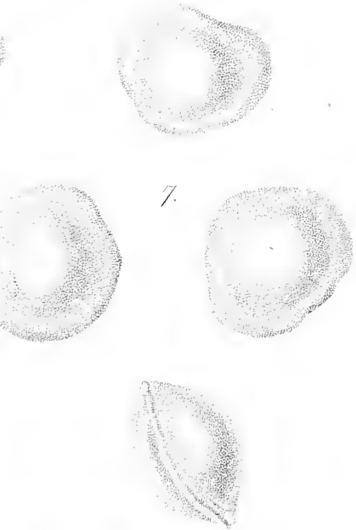

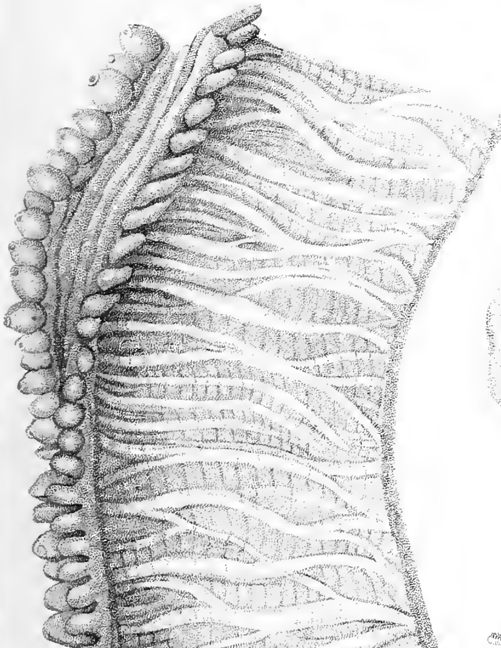

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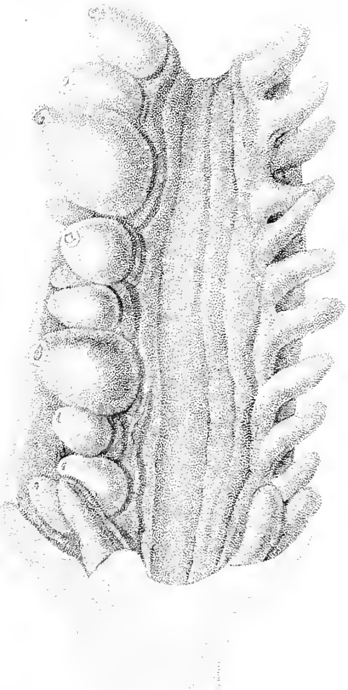

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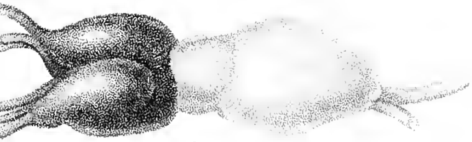

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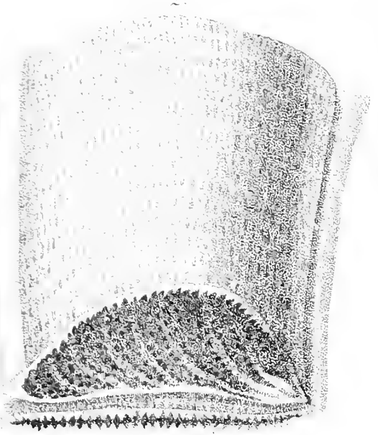

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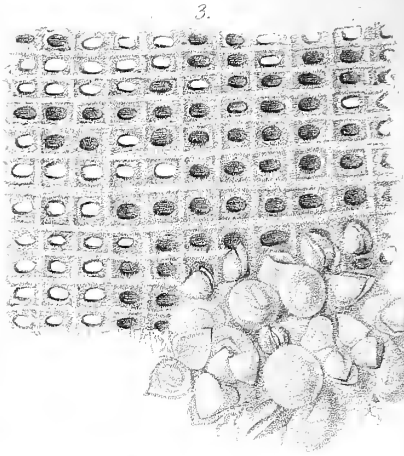

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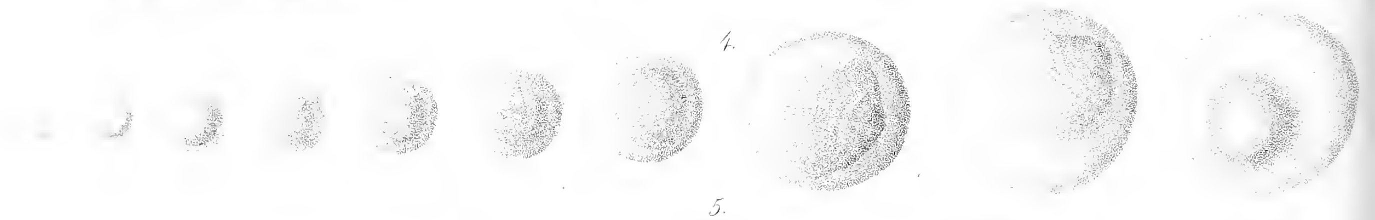

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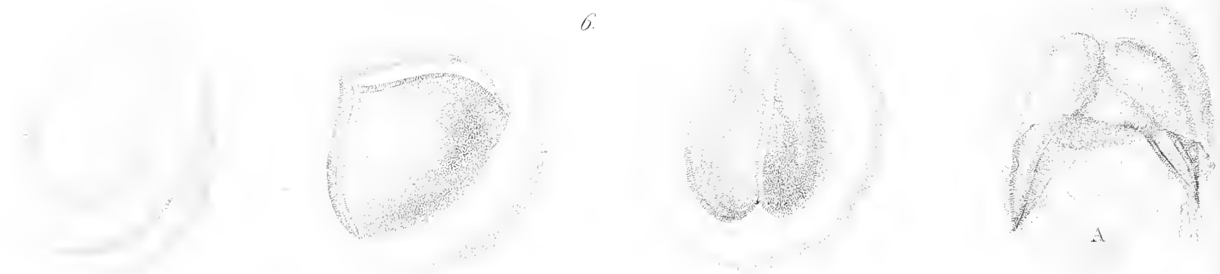

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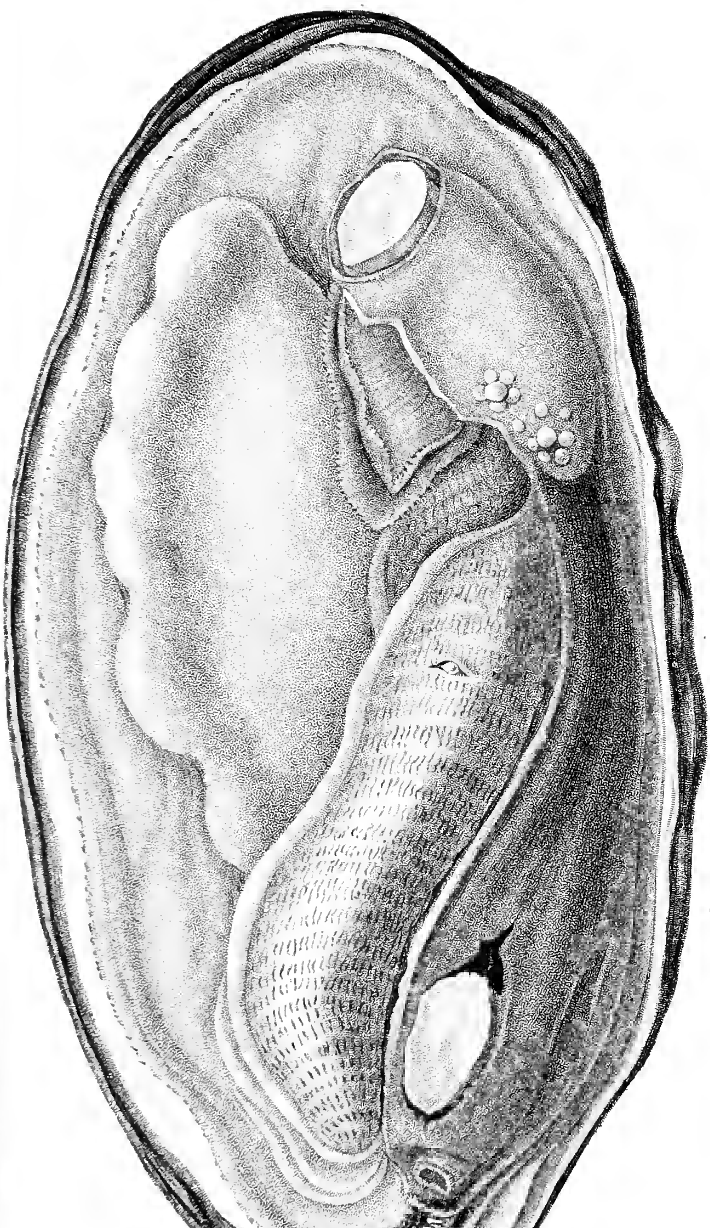

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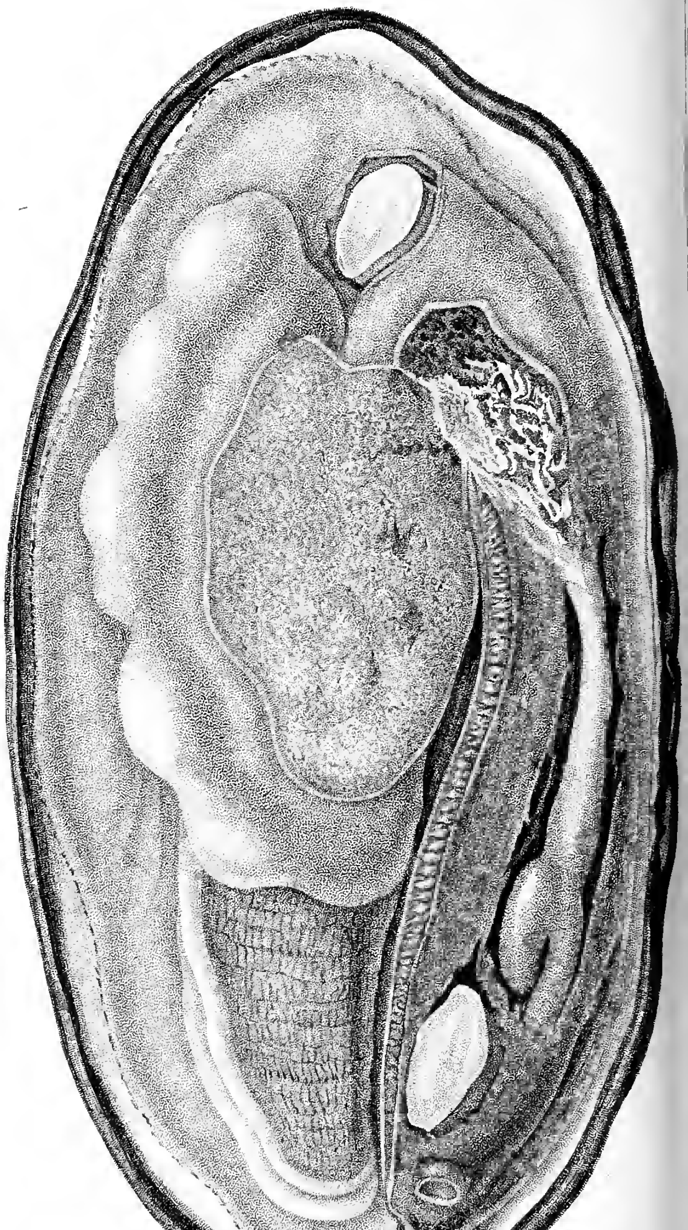

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appear when floating in water, when they assume nearly a spherical form: at *a* is one of the ova after having been about one hour in water, when they dissolve into a granulated mass; magnified 100 diameters.

Fig. 4. An equally small portion of an ovarium in a more advanced state of pregnancy; magnified 100 diameters.

Fig. 5. Some ova extracted from the above portion of ovarium, floating in water, magnified 100 diameters: at *a* is one of these ova bursting, and emitting its granulated substance; magnified 100 diameters; at *b* is an empty, unimpregnated probably, ovum; magnified 100 diameters.

Fig. 6. Several young oysters just emitted from the oviduct, and floating in water and their natural mucus; magnified 100 diameters.

Fig. 7. Some of the same young oysters, after having been some time in contact with the air, when they become quite opaque and assume a glossy appearance; magnified 100 diameters.

Fig. 8. A small portion of the cloak with its fringe; magnified 25 diameters.

Fig. 9. A portion of the fringe spread open; magnified 50 diameters.

Fig. 10. The heart, with its auricles in the natural position, as it lies in the oyster; magnified 4 diameters.

Plate V. Fig. 1. A fresh-water muscle, one shell, and its lining or cloak removed; natural size.

Fig. 2. The same muscle with the oviduct removed, and a perpendicular section of the ovarium; natural size.

Plate VI. Fig. 1. A very small portion of the ovarium, with the ova imbedded in the yellow granular substance of the ovarium, magnified 100 diameters.

Fig. 2. A transverse section of a portion of the oviduct; magnified 2 diameters.

Fig. 3. A small portion of the inside of the oviduct, with some of the ova ready for emission; magnified 20 diameters.

Fig. 4. Several ova, of various sizes, extracted from the ovarium; magnified 100 diameters.

Fig. 5. Ova, in the early state, extracted from the oviduct, and floating in water; magnified 100 diameters.

Fig. 6. Ova nearly ready for emission, extracted from the oviduct, and floating in water; at A is one just bursting its enclosing membrane or bladder; magnified 100 diameters.

Fig. 7. Some young muscles, just emitted, floating in water, where they are opening and closing in a very lively manner for several hours; magnified 100 diameters.

Fig. 8. Is the origin and progress of a singular worm, which is found within the oviduct of the muscle: originally it is an extremely minute globule, which is only to be distinguished from the usual granular substance by a curious rotatory motion, which it performs incessantly until it becomes quite organised; and it attains sometimes the length of an inch; magnified 100 diameters.

V. *On a newly discovered genus of Serpentiform Fishes. By* I. Harwood, *M. D. F. L. S. Professor of Natural History in the Royal Institution of Great Britain. Communicated by* Daniel Moore, *Esq. F. R. S.*

Read February 1, 1827.

In no department of natural history have descriptions been more unsatisfactory than such as relate to certain productions of the ocean, which, either from the immeasurable depths that conceal them, or the absence of those circumstances best adapted to their multiplication, very rarely present themselves to our notice, and from this rarity often excite impressions on our minds, ascribing to them properties foreign to their real natures, and at variance with that harmony which, even in the deepest recesses of the ocean, pervades the works of Omnipotence.

It is doubtless from a want of more frequent opportunities for investigation, that the ancients were induced to consider the sea as the abode of monsters and prodigies of the most incongruous characters; for, in addition to their prevailing opinion expressed in Pliny, " ut quidquid nascatur in parte naturæ ullâ, et in mare esse; præterque multa quæ nusquam alibi," we know that tritons, sirens, mermaids, and more lately krakens, and serpents of vast proportions, and varied properties, have been the frequent subjects of serious consideration; and even in the present day there are perhaps few whose imaginations, at some period of their lives, have not been highly interested by the contemplation of such ideal beings. As the difficulty therefore of eradicating error from the mind, when once received, imparts value to truth, though least adorned, on this ground I would presume to present to the attention of the Royal Society an account of a newly discovered, and a very extraordinary marine animal. Last autumn, whilst Captain SAWYER, of the ship Harmony, of Hull, was in pursuit of the bottle-nosed porpoise, in latitude 62° north, by about 57° west, he observed a body floating on the surface of the water, which was at first mistaken by himself and his seamen for an inflated seal's skin, such as the Esquimaux employ in the destruction of large aquatic animals, by attaching it to the harpoon by which they are speared, and thus tiring them out by its floating property. On a nearer approach however, the object which had excited their attention proved to be a living marine animal. The creature is still in the possession of Captain SAWYER, who preserved it in rum soon after being taken, and who obligingly afforded me an opportunity of examining it. Its capture was occasioned by its being, when first observed, almost worn out by unavailing efforts to gorge a fish of about seven inches in circumference, with which it appeared to have been long contending, as it exhibited very feeble signs of life. Its organs of motion being extremely small, and its body greatly elongated, this creature would, on a cursory view, be by all considered as an extraordinary kind of sea serpent—a tribe of animals concerning which so much equivocal matter has been written; and this idea would be even supported by a more close examination of some parts of its structure. The fact, that the sea contained animals nearly allied in form to serpents, has been remarked in the earliest ages; ARISTOTLE has expressly stated, that there are many species of sea serpents, among which tribe he evidently includes the branchial apodes, for he has particularized one of these as par excellence the Οφις ϑαλαττιος. This is the muræna serpens of LINNÆUS, or the ophisurus of LACEPÈDE, a creature of a more cylindrical form than the eel, and possessing other points of resemblance to the snake tribe; but although, in the present day, the term sea serpent would be ill applied to any animal which breathes by means of branchiæ, yet among such creatures, excluding the genera hydrus and hydrophis, and other true water snakes which inhabit tropical seas, I doubt if the subject of this communication be not at least as well entitled to that appellation as any hitherto described. From the several genera of animals however nearest allied to it, it offers points of disagreement so important, as to entitle it to a distinct place in classification, and especially from the formation of the jaws, which, with the exception of the apparent want of serpentiform inter-articular bones, are truly analogous to those of snakes; and, secondly, from the possession of an enormous elastic sac, which is seemingly a receptacle for air only. The first of these latter characters appearing to be the one, of all least liable to vary, I would suggest the term Ophiognathus as applicable to the genus; its characters are as follow:

OPHIOGNATHUS. Corpus nudum, lubricum, colubriforme, compressum, sacco amplo abdominali.

Caput anticè depressum, maxillâ superiore (paulo) longiore.

Dentes, in maxillâ inferiore, et ossibus intermaxillaribus, subulati, retroflexi.

Maxillæ elongatæ, patulæ, dilatabiles, (serpentium instar).

Lingua vix conspicua.

Spiracula ante et sub pinnas pectorales, magna.

Pinnæ pectorales, dorsales, analesque radiis mollibus; ventrales nullæ.

Oculi minimi, prope extremitatem maxillæ superioris positi.

Cauda elongata, in filamentum apterum producta.

As a genus, then, the association of the above characters distinguishes this creature from any others which I am aware have hitherto been described, although some of them may be met with in those genera nearest allied to it, as the muræna, ophisurus, gymnothorax, sphagebranchus, synbranchus, gymnotus, ophidium, gymnetrus, and trichiurus. In these however we find the following points of dissimilitude. In muræna the fins are adipose; ophisurus has teeth on the ossa palati, and gymnothorax has no pinnæ pectorales. In sphagebranchus the same fins are also absent, or imperfect, and the snout lengthened. In synbranchus the spiraculum is single; in gymnotus the dorsal fin is wanting. The genus ophidium has open branchiæ and large opercula; in gymnetrus the anal fin is deficient; and lastly, in addition to other important differences, this is supplied in the trichiurus by a dentated edge.

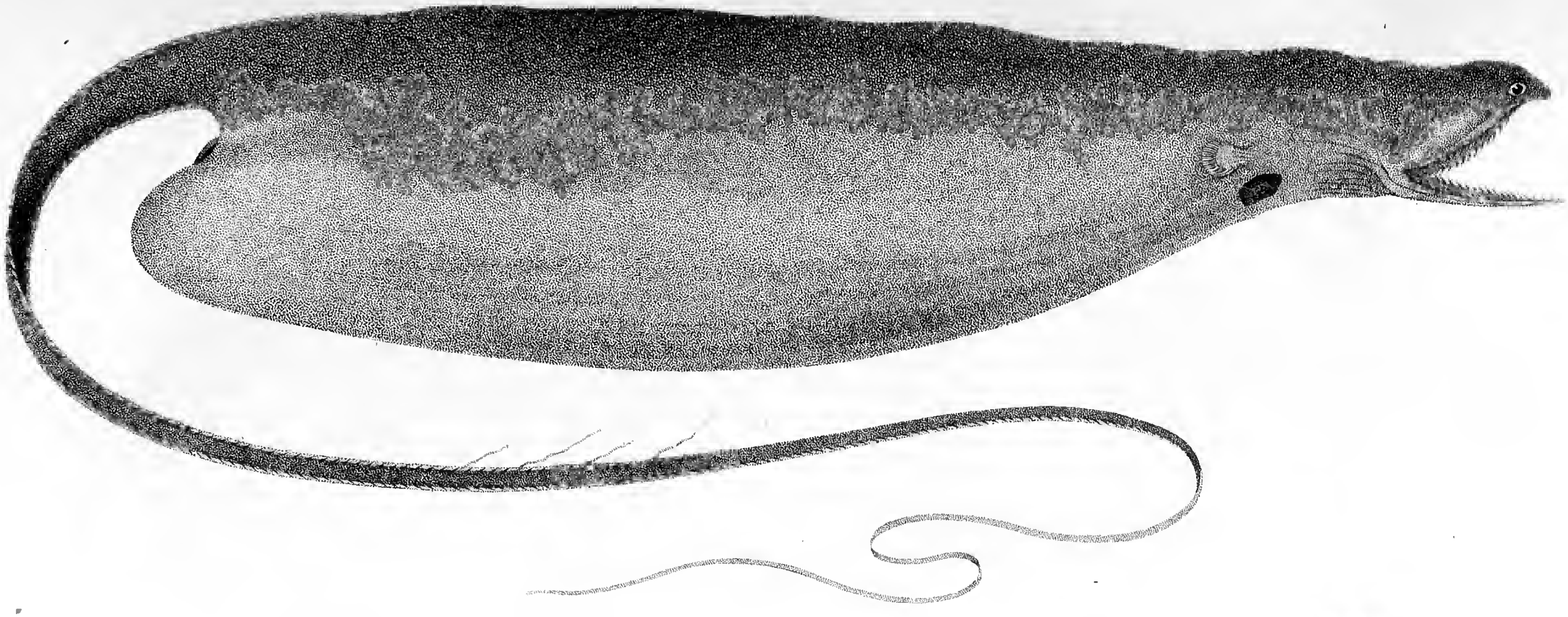
We now proceed to consider the only known species of Ophiognathus, which, from a character altogether anomalous among apodal fishes, we shall term *ampullaceus*.

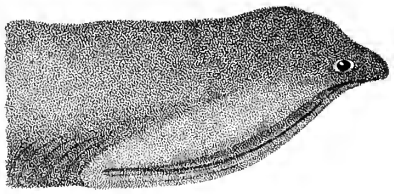
Its body is one uniform purplish black, except the filamentous extremity of the tail, which is much lighter. The total length of the specimen taken is 4 feet 6 inches. The enlarged and extremely elastic pharynx communicates with the enormous sac or air vessel, which extends in length from the extremity of the snout about 20 inches. The great delicacy of the parietes of this sac, and its apparent liability to rupture from the action of the spirit, prevented my inflating it to its full extent; but when partially filled with air, it measured about 9 inches in circumference below its union with the tail, and its greatest diameter, including the slender body to which it pertained, was 4 inches. At about one inch below the last point of its attachment with the body, the rectum was observed to perforate the sac, the tenuity of which rendered the course of that intestine, as indeed that of all the digestive organs readily traced. They are apparently sustained beneath the ribs (which latter appear very imperfect), by a membranous expansion, as they are not affected in their position by any inflation or emptying of the sac. That the sac itself communicates with the pharynx, is sufficiently proved by the fact, that if the blowpipe were further introduced, the digestive organs were alone inflated, the sac undergoing no change in its dimensions. The nearest point of analogy therefore to this structure observable in the class Pisces, is seen among the diodons and tetraodons, where a kind of ingluvies, or crop, formed of a very thin and extensible membrane, adheres closely to the peritonæum throughout the whole extent of the abdomen, by means of which their curious and rapid power of inflation has long excited surprise, though we cannot in the smooth Ophiognathus so satisfactorily account for its use, as among these fishes, which by its distention, mechanically elevate their spines, and thus float about in safety. This external sac might again be compared with the more internal air vesicle of most other kinds of fishes, and especially that of the sturgeon kind, which has so large and free a communication with the œsophagus; but in extending our enquiries on this subject further, we find no class among the vertebral animals which does not offer some modification of a similar structure; for even among the mammalia, the whales, which are called the balænæ boops and rostrata, also the balæna musculus, if it be a distinct species, have the anterior part of the body covered with an extremely elastic skin, plaited into deep folds, as Mr. HUNTER has expressed it, "like unto a ribbed stocking," and capable of a vast extent of dilatation. This is partly exhibited in the act of swallowing food, and appears reasonably intended, by communicating with the external air, to render the body so specifically light, as to enable them to sleep with greater ease upon the surface. Of this communication however, that accurate observer, Mr. HUNTER, has taken no notice, but adds, "why the skin should be so elastic, is difficult to say, as it covers the thorax, which can never be increased in size; yet there must be some peculiar circumstance in the economy of the species requiring this structure, which we as yet know nothing of." Among birds, several examples occur of the possession of external air vessels of considerable size, which in like manner appear to be intended to more perfectly increase or diminish their specific gravity at their will. Of this structure the gigantic stork, ardea argala LIN. presents a striking example. Lastly, among reptiles, such mechanical aids, either internal or external, are far more numerous, for to this end we must consider the large internal vesicle forming an appendage to the lungs of turtles; and probably to the same end is the saccular and membranous inferior half of the lung in snakes, which is at the same time transparent, and exhibits few traces of vascularity. To these might also be added many other instances; even among the lizards of the genus anolis, the lacertæ strumosa and bullaris, have beneath the anterior part of the body and throat, large elastic vesicles, which at will they quickly distend with air, though these species are not aquatic in their habits. But to return: the skin all over the body of the Ophiognathus is particularly soft and slimy, yet it has a slightly granulated appearance. The spiracula are of large size, of an irregular oval form, and are unprovided with externally perceptible branchiostegous rays; their edges partially conceal on each side three branchiæ. When the sac is contracted, these apertures are placed almost under the body, as in the sphagebranchus, having a narrow commissure between them; they are placed at about 5½ inches from the snout.

All the fins of the Ophiognathus are extremely small; in the formation of the pectorals there is a peculiarity not mentioned, I believe, in other apodal genera, they being principally composed of an adipose disk, which is terminated, and nearly surrounded by a narrow radiated membrane, instead of this latter taking its origin immediately from the body. The dorsal fin, which like the rest is very narrow, and provided with simple rays, commences at about 18 inches from the snout, and terminates insensibly upon that slender, tapelike filament, into which the tail becomes converted, and which is continued 20½ inches in length beyond the posterior extremity of the dorsal fin. About this part of the dorsal fin a few other minute filaments take their growth from it. The anal fin commences at the posterior union of the sac with the body, and ends at about 14 inches from the extremity of the caudal filament. The body exhibits no apparent lateral line; but perhaps the most curious structures which the creature presents to our notice, are connected with the head and jaws. The almost entire absence of a tongue, might perhaps prove one of its most characteristic distinctions, were we as yet sufficiently acquainted with the condition of this organ in those nearest allied to it. The teeth are disposed in a single row above and below; above, they exist only along the margins of the intermaxillary bones; below, they extend almost the whole length of the maxilla; but the ossa palati are entirely destitute of teeth. Lastly, the jaw-bones are so long, and their articulation is such, that their capability of expansion exceeds what I have seen in any other animal, the rattlesnake not excepted; and as in snakes, when fully distended, the edges of the jaws describe a large circle, and then appear but as the hemming of an ample sack, the pharynx, which usually occupies so small a space, being an equal participant in this extensile property. When the jaws were gently opened, they measured 2½ inches across, and 3½ inches from the front teeth above to those below; but while they possess this capability of distension, their contractile power is no less remarkable, as may be observed in Plate VII. fig. 1. which represents the usual appearance. Fig. 2. exhibits the jaws and pharynx more depressed and extended. Fig. 3. represents an anterior view of the same.

Thus provided then, we find the Ophiognathus to be one of the most predatory and voracious of all the belligerent inhabitants of the ocean, as was proved by the efforts of Captain SAWYER's specimen to gorge a species of perch of more than its own usual circumference, in striving with which, as before

Ophiognathus ampullaceus.





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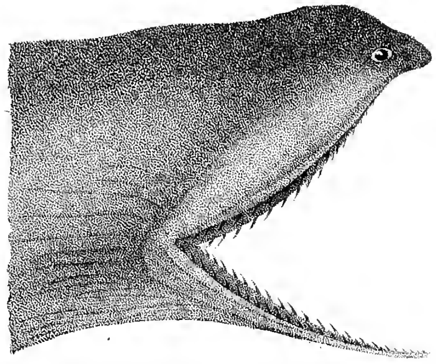

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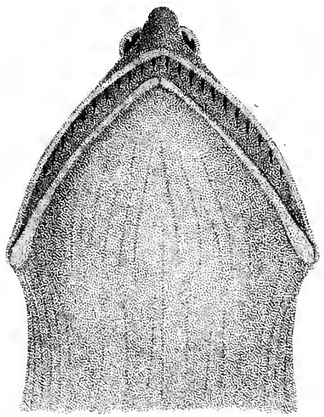

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observed, it appears to have lost all its muscular energy. Its entire form would indicate great swiftness of motion, which is doubtless effected by means of the same interesting sinuous inflections in the water, which excite our admiration in the class of serpents upon land; as it is indeed well ascertained, that some serpentiform fishes, especially the true murænæ, are capable of transferring with great effect, their aquatic locomotive powers to the surface of the earth. In what manner its enormous pouch is employed in its economy, I repeat, that I cannot presume to determine; its complete inflation with air, when on the surface, must, it would appear, afford a very effectual obstacle to the descent of the animal beneath. Whether it be capable of secreting the contained air, as has been thought of the common internal air vesicles of some other fishes, for which apparent purpose, eels have a peculiar gland connected with its centre, or whether water be allowed to enter its cavity, as it has been thought to enter the abdominal cavity of some rays, would form a subject of interesting enquiry. Having however entered on the field of conjecture, I shall not presume to longer engage the attention of the Royal Society, though I cannot but express a hope, that the discovery of another curious link in the vast chain of being, and especially in latitudes where such an one might have been least expected, will be considered sufficiently important to merit their notice.

Royal Institution, Jan. 10, 1827.

VI. *An examination into the structure of the cells of the human lungs; with a view to ascertain the office they perform in respiration.* *By Sir* Everard Home, *Bart. V. P. R. S.* *Illustrated by microscopical drawings from the pencil of* F. Bauer, *Esq. F. R. S.*

Read February 8, 1827.

No subject connected with physiological enquiry has more excited the attention of the anatomist and chemist, than respiration; but the association between this subject and animal heat, which has so long been supposed to exist, has led to the belief, for the last century, that both enquiries belong more particularly to chemistry than anatomy, and I may probably be considered as going out of my province in taking up this investigation. On the other hand, I see reason to believe that the process of respiration is in itself more simple than is imagined, and more within the reach of discovery by means of accurate anatomical knowledge of the parts employed, than by means of acquaintance with the intricacies belonging to chemical affinities: I carry this so far as to contend that no explanation of respiration upon chemical principles is to be depended on, unless it accord in all respects with the anatomy and physiology of *the lungs*, by which the assumed process takes place.

The present theory respecting respiration adopted by the chemists, is, that this process decarbonises the blood in the following manner; at every inspiration a compound of oxygen and nitrogen, mixed together, is received into the lungs, and in every expiration, the same volume is returned, measure for measure exactly, with this only difference, that what entered as oxygen is returned in the form of carbonic acid gas, which, according to *their* theory, proves that no part of the inspired atmospheric air has been retained in the lungs, but a quantity of carbon, equal to that of the oxygen inspired, has been extracted from the blood by the oxygen, making it become carbonic acid gas.

Nothing could be more ingenious than this theory, were it supported by the structure of the lungs themselves, and it could be proved that the blood requires no other changes for its purification ; since all the leading facts on which it is founded, are completely established upon the firm basis of experiment.

When this theory was formed, the structure of the air cells of the lungs had never been examined, the more minute structures in animal bodies being at that time considered beyond the reach of examination ; and it is the object of the present communication, to bring forward an explanation of the mechanism of the cells of the lungs, as well as of the different distributions of the vessels that ramify through those organs, acquired from Mr. BAUER'S microscopical observations, and to see how far they are fitted for the process, which by this theory is allotted to them.

In this investigation, I began by an enquiry into the circulation of the blood through the lungs, in the labour of which I have been very ably assisted by Mr. RUSSELL, a very intelligent student of St. George's Hospital, at present filling the highly respectable and important office of senior House

Surgeon. To him I am indebted for having taken the trouble of making injections of the arteries, the veins, and of the cells of the lungs, with different substances, so as to enable Mr. Bauer to expose and examine them on the field of the microscope.

The first new fact discovered in the course of this enquiry was, that although the common minute injection used by anatomists for filling the blood vessels, when thrown in by the trunk of the pulmonary artery, while the cells of the lungs are empty, returns again by the trunks of the pulmonary veins, yet when thrown in by the veins, it is not returned by the trunks of the arteries.

Another fact was discovered; that during the momentary distention of the air cells, an interruption is produced between the arterial and venal circulation in the lungs, the blood being carried no farther than the small arterial branches surrounding the air cells.

The following description of the air cells, and the parts surrounding them, is taken from the annexed microscopical drawings of Mr. Bauer.

As accurate representation surpasses all verbal description, I shall not have occasion to do more, to make myself understood, than to mention the parts themselves, and the circumstances under which they are represented.

The cells of the lungs were filled with quicksilver, to show their utmost capacity, and the parts were afterwards immersed in rectified spirit, to prevent the cells from collapsing, when the quicksilver was allowed to escape.

When the internal cavity of a single cell was exposed, immediately behind its internal membrane, the branches of the pulmonary artery, injected with red wax, were seen ramifying, as arteries do in common; these were accompanied by branches of the pulmonary vein, larger in proportion than those of the arteries, more numerous, and having valves, at apparently regular intervals, to prevent regurgitation of their contents. Besides the arteries and veins, there were innumerable absorbents opening into the cavity of the cell; their valves were at very short distances, and, in their course in the interstitial substance between the cells, they accompanied the veins. When the terminal branches of the pulmonary artery were traced, the injection was found to have stopped someway before the artery's termination, and the space beyond was filled with gas. The substance of the lungs, interstitial to the cells, when dried became transparent, and was found to be composed of a smaller order of cells, with transparent coverings, that freely communicated with one another, as well as with the cavity of the large cell they surrounded.

I cannot finish this description of an internal view of one of the cells of the human lungs, without expressing the obligations I am under to Mr. DOLLOND, who from a zeal for science, and the peculiar interest he has taken in these physiological enquiries, has fitted up the microscope (from the field of which the representations were taken), possessed of advantages beyond all the others that Mr. BAUER has met with, not excepting the achromatic microscope, as improved by Mr. CHEVALIER of Paris. This of Mr. DOLLOND, from the superiority in the clearness of the different glasses, and the facilities which are given to the variations of the nicer adjustments of the instrument, has enabled Mr. BAUER to give a more distinctly defined outline to all the parts, than he thought it possible to produce, when objects are so highly magnified.

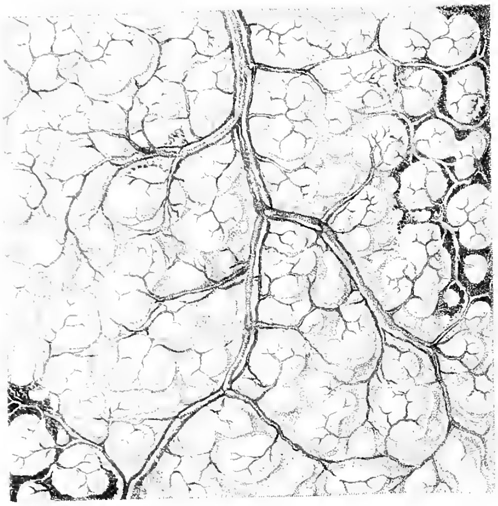
From this account of the anatomical structure of the lungs, it is evident they are calculated not only to receive supplies from the atmosphere, but to convey a part of them, with the greatest rapidity, as well as facility, to the heart; since the momentary interruption to the passage of the blood from the arteries to the veins, and the numerous valves in the absorbents, as well as those in the veins, are admirably fitted for that purpose; which is at variance with the theory of decarbonising the blood.

As carbonic acid gas has been occasionally detected by Professor Brande, both in the urine and perspirable matter, it must have been formed in the blood circulating through the arteries; and the supply of oxygen which enables this to take place, is now shown to be derived from the lungs.

This mode of decarbonising the arterial blood, by a portion of its oxygen uniting with the carbon, and carrying it off in the form of carbonic acid gas by the urine and perspirable matter, is extremely simple, and appears to require no elaborate chemical process.

Having ascertained upon a former occasion, that soon after digestion has been begun, the oxygen employed in that process unites with carbon, and the quantity of carbonic acid gas, met with in the venous blood, is greater than at any other time; this will sufficiently account for the blood in the branches of the pulmonary artery always containing a sufficient quantity of this gas to replace the oxygen that is removed from the cells of the lungs, and carried to the heart;

1



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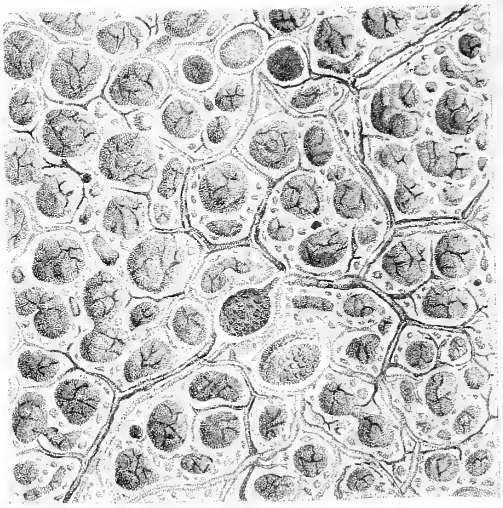

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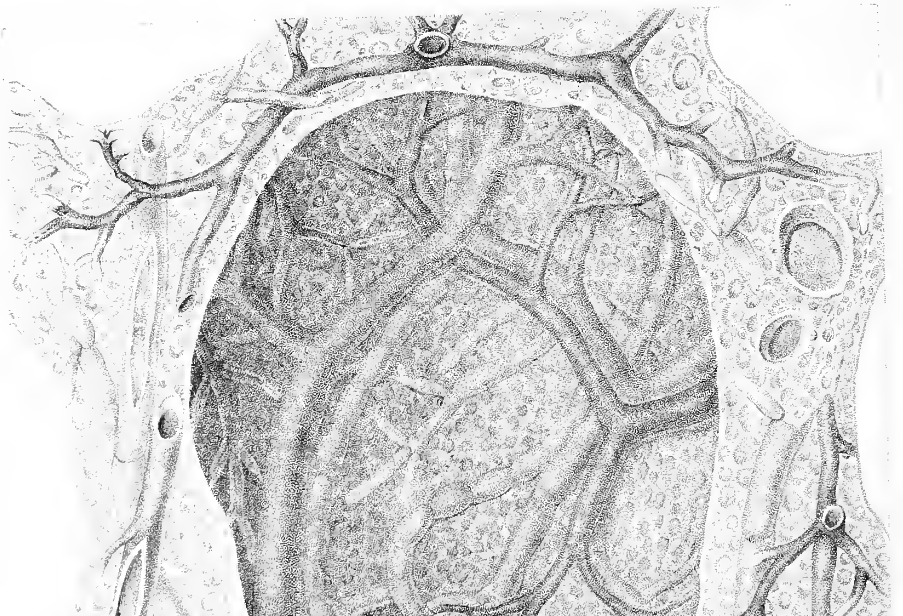


so that it is only such blood that is decarbonised by the carbonic acid that is carried off by expiration.

Respiration is necessary for carrying on the functions of life, by a supply of oxygen, and the removal of the excess of carbon; but it is not required for the simple support of the principle of life when no action is going on.

The garden snail excludes the atmospheric air from the lungs, by means of an operculum formed of mucus, during the winter season; and remains alive sealed up hermetically, till the warmth and the moisture of the spring dissolve the operculum, and expand a globule of air that had been shut up in the cavity of the lungs, the escape of which opens the communication with the outer air and restores respiration.

EXPLANATION OF THE PLATES.

Plate VIII. Shows the cells of the human lungs as they appear on the field of the microscope.

Fig. 1. represents $\frac{1}{64}$th part of an inch of the external surface of the human lung, the cells of which are filled with quicksilver; magnified 20 diameters.

Fig. 2. a transverse section of $\frac{1}{64}$th part of an inch of the human lung, in which the arteries are filled with red, and the veins with yellow minute injection; magnified 20 diameters.

Fig. 3. the transverse section of a single cell from fig. 2. with the parts immediately surrounding it; magnified 400 diameters.

Plate IX. Shows the structure of the human lungs immediately surrounding the air cells.

Fig. 1. the interstitial substance between the cells, in a

recent state, consisting of small cells every where opening into one another, and communicating with the large one; the arteries and veins injected; magnified 400 diameters.

Fig. 2. a portion of a terminal branch of the pulmonary artery at the air cell of the lungs, in which the injection has not reached the termination, and the remaining portion is distended with gas; magnified 400 diameters.

Fig. 3. a portion of an injected terminal branch of the pulmonary artery, into which the injection has been too much forced, which has occasioned extravasation through the terminating ends of the branches; magnified 400 diameters.

Fig. 4. a portion of an injected vein, accompanied by an absorbent vessel. At one terminating branch of the vein the injection has also been forced out; magnified 400 diameters.

Fig. 5. a portion of an absorbent vessel, in a dried state, in which some of the intervalvular portions are inflated, others collapsed; magnified 400 diameters.

Fig. 6. a portion of a vein dried, in which some of the intervalvular portions are distended with gas; magnified 400 diameters.

VII. *Remarks on a correction of the solar tables required by Mr.* SOUTH'S *observations.* *By* G. B. AIRY, *Esq. M. A. Fellow of Trinity College, Cambridge, and Lucasian Professor of Mathematics in the University of Cambridge. Communicated by Dr.* YOUNG, *F. R. S. &c.*

Read February 15, 1827.

THE discordances between the sun's true Æ, as observed by Mr. SOUTH, and his calculated Æ, as given in the Nautical Almanac, follow a law so simple, as to justify us in attributing them principally to the errors of the solar tables. The only exceptions to this assertion are the differences of March 1st, December 22d, and December 23d, 1822; and upon examination of the sun's calculated Æ for several days, previous and subsequent to those days, it appears that there is some irregularity in the second differences. I imagine therefore that some small errors have been accumulated in the calculations for those days; but as this is merely conjectural, I have not thought myself at liberty to reject them in the following computations.

A single inspection of the discrepancies, is sufficient to show that they are almost entirely produced by an error of the epoch, and an error in the place of the perigee. With these errors only I begun my calculations; but finding that the construction of the solar tables (contained in VINCE's Astronomy) gave great facilities for introducing an error in the eccentricity, I begun my calculations again, supposing the epoch, the place of the perigee, and the eccentricity, or the greatest equation of the centre, liable to error. The error of the equation of the centre is found to be so small that it may be neglected: but the errors of the epoch and place of the perigee are considerable.

The first part of the operation was to deduce from the errors in Æ, the corresponding errors in longitude. This was done by multiplying them by 15 sec. 23° 28′ cos.^{2} dec.; the multiplication by sec. 23° 28′ was however reserved to the end, when the results are multiplied by it. The next was, to give the errors which would be occasioned by assumed errors in the epoch, the place of the perigee, and the greatest equation of the centre. As the tables contain the variation of the equation of the centre for a variation of 10′ in the mean anomaly, and for one of $-17'',18$ in the greatest equation, this was very easily effected.

Supposing then that the epoch ought to be increased by x'', the mean anomaly by $y \times 10'$, and the greatest equation of the centre by $-z \times 17'',18$, I get the following equations, each of which is erroneous to the amount of the error of observation. The first side (as was mentioned) ought to be multiplied by sec. 23° 28′.

$$\begin{aligned}
10{,}57 &= x - y \times 19{,}7 - z \times 0{,}48 \\
11{,}87 &= x - y \times 19{,}6 + z \times 2{,}1 \\
10{,}38 &= x - y \times 19{,}4 + z \times 2{,}91 \\
13{,}87 &= x - y \times 19{,}1 + z \times 4{,}59 \\
12{,}8 &= x - y \times 18{,}9 + z \times 4{,}87 \\
10{,}31 &= x - y \times 16{,}9 + z \times 8{,}75 \\
8{,}94 &= x - y \times 16{,}7 + z \times 8{,}99 \\
12{,}49 &= x - y \times 15{,}6 + z \times 10{,}43 \\
10{,}76 &= x - y \times 15{,}4 + z \times 10{,}65 \\
8{,}43 &= x - y \times 14 + z \times 12
\end{aligned}$$

$14{,}16 = x - y \times 13{,}4 + z \times 12{,}54$
$10{,}51 = x - y \times 13{,}1 + z \times 12{,}74$
$10{,}85 = x - y \times 12{,}8 + z \times 12{,}93$
$10{,}85 = x - y \times 12{,}6 + z \times 13{,}12$
$9{,}81 = x - y \times 12{,}3 + z \times 13{,}31$
$9{,}73 = x - y \times 9{,}9 + z \times 14{,}79$
$9{,}90 = x - y \times 9{,}6 + z \times 14{,}94$
$9{,}76 = x - y \times 9{,}3 + z \times 15{,}08$
$9{,}76 = x - y \times 9 + z \times 15{,}22$
$11{,}99 = x - y \times 6{,}8 + z \times 16{,}05$
$10{,}19 = x - y \times 5{,}8 + z \times 16{,}34$
$12{,}09 = x - y \times 5{,}5 + z \times 16{,}43$
$11{,}6 = x - y \times 2{,}8 + z \times 16{,}96$
$10{,}52 = x - y \times 2{,}5 + z \times 17{,}$
$11{,}93 = x - y \times 0{,}1 + z \times 17{,}18$
$10{,}57 = x + y \times 6{,}5 + z \times 16{,}38$
$9{,}25 = x + y \times 6{,}8 + z \times 16{,}3$
$7{,}51 = x + y \times 9{,}1 + z \times 15{,}51$
$8{,}6 = x + y \times 9{,}4 + z \times 15{,}37$
$7{,}85 = x + y \times 11{,}5 + z \times 14{,}32$

$7{,}43 = x + y \times 17{,}9 + z \times 8{,}59$
$8{,}01 = x + y \times 18{,}3 + z \times 8{,}05$
$5{,}43 = x + y \times 18{,}4 + z \times 7{,}77$
$7{,}22 = x + y \times 18{,}5 + z \times 7{,}5$
$5{,}78 = x + y \times 18{,}9 + z \times 6{,}98$
$6{,}39 = x + y \times 19{,}3 + z \times 6{,}11$
$6{,}21 = x + y \times 19{,}8 - z \times 4{,}53$
$6{,}54 = x + y \times 19{,}8 - z \times 4{,}78$

$8{,}74 = x + y \times 12{,}6 - z \times 13{,}7$
$5{,}52 = x + y \times 12 - z \times 14{,}07$
$8{,}45 = x + y \times 11{,}7 - z \times 14{,}24$

$$\begin{aligned}
3{,}31 &= x + y \times 10{,}6 - z \times 14{,}87 \\
0{,}44 &= x + y \times 10{,}3 - z \times 15{,}01 \\
10{,}04 &= x - y \times 9{,}7 - z \times 14{,}87 \\
9{,}87 &= x - y \times 10 - z \times 14{,}72
\end{aligned}$$

$$\begin{aligned}
11{,}39 &= x - y \times 15{,}1 - z \times 10{,}99 \\
12{,}19 &= x - y \times 15{,}3 - z \times 10{,}77 \\
7{,}84 &= x - y \times 15{,}7 - z \times 10{,}31 \\
12{,}81 &= x - y \times 16{,}3 - z \times 9{,}6 \\
12{,}54 &= x - y \times 17 - z \times 8{,}62 \\
10{,}65 &= x - y \times 17{,}1 - z \times 8{,}41 \\
10{,}62 &= x - y \times 17{,}3 - z \times 8{,}16 \\
11{,}22 &= x - y \times 17{,}5 - z \times 7{,}9 \\
9{,}02 &= x - y \times 17{,}6 - z \times 7{,}64 \\
11{,}85 &= x - y \times 17{,}9 - z \times 7{,}12 \\
9 &= x - y \times 18 - z \times 6{,}86 \\
8{,}76 &= x - y \times 19{,}5 - z \times 2{,}8 \\
11{,}19 &= x - y \times 19{,}7 + z \times 0{,}6 \\
13{,}99 &= x - y \times 19{,}6 + z \times 1{,}44 \\
12{,}51 &= x - y \times 19{,}6 + z \times 2{,}29 \\
12{,}01 &= x - y \times 17{,}5 + z \times 7{,}94 \\
9{,}08 &= x - y \times 17{,}3 + z \times 8{,}2 \\
11{,}03 &= x - y \times 17{,}2 + z \times 8{,}45 \\
10{,}03 &= x - y \times 17 + z \times 8{,}7 \\
9{,}53 &= x - y \times 16{,}8 + z \times 8{,}95 \\
11{,}53 &= x - y \times 16 + z \times 9{,}88 \\
11{,}98 &= x - y \times 15{,}8 + z \times 10{,}15 \\
9{,}43 &= x - y \times 14{,}1 + z \times 11{,}9
\end{aligned}$$

$$\begin{aligned}
10{,}57 &= x - y \times 13{,}9 + z \times 12{,}1 \\
8{,}03 &= x - y \times 13{,}6 + z \times 12{,}31 \\
8{,}25 &= x - y \times 13{,}2 + z \times 12{,}68 \\
9{,}31 &= x + y \times 4 + z \times 16{,}9
\end{aligned}$$

$$\begin{array}{rcl}
6 & = & x + y \times 10{,}9 + z \times 14{,}69 \\
5{,}05 & = & x + y \times 12{,}3 + z \times 13{,}86 \\
4{,}65 & = & x + y \times 13{,}5 + z \times 13{,}09 \\
5{,}85 & = & x + y \times 13{,}7 + z \times 12{,}89 \\
\hline
4 & = & x + y \times 16{,}7 + z \times 10{,}2 \\
6{,}07 & = & x + y \times 17{,}3 + z \times 9{,}43 \\
6{,}96 & = & x + y \times 18{,}5 + z \times 7{,}59 \\
6{,}72 & = & x + y \times 18{,}7 + z \times 7{,}31 \\
5{,}05 & = & x + y \times 18{,}8 + z \times 7{,}02 \\
2{,}07 & = & x + y \times 20{,}3 + z \times 2{,}9 \\
2 & = & x + y \times 20{,}4 + z \times 2{,}6 \\
4{,}7 & = & x + y \times 20{,}5 + z \times 1{,}72 \\
3{,}53 & = & x + y \times 20{,}5 + z \times 1{,}09 \\
6{,}23 & = & x + y \times 20{,}5 + z \times 0{,}47
\end{array}$$

Grouping together those equations in which the sun's anomaly is included between $1^s\ 30^\circ$ and $4^s\ 30^\circ$, between $4^s\ 30^\circ$ and $7^s\ 30^\circ$, between $7^s\ 30^\circ$ and $10^s\ 30^\circ$, and between $10^s\ 30^\circ$ and $1^s\ 30^\circ$, as marked by the divisions above, we have the following results.

Summer of 1821.

$$110{,}42 = 10\,x - y \times 175{,}3 + z \times 64{,}81.$$

Autumn of 1821.

$$207{,}43 = 20\,x - y \times 82{,}2 + z \times 302{,}51.$$

Winter of 1821-1822.

$$53{,}01 = 8\,x + y \times 150{,}9 + z \times 35{,}69.$$

Spring of 1822.

$$46{,}37 = 7\,x + y \times 37{,}5 - z \times 101{,}48.$$

Summer of 1822.

$$250{,}2 = 23\,x - y \times 394{,}9 - z \times 20{,}68.$$

Autumn of 1822.

$$57{,}71 = 8\,x + y \times 13{,}7 + z \times 108{,}52.$$

Winter of 1822.

$$47{,}33 = 10\,x + y \times 192{,}2 + z \times 50{,}33.$$

And adding the groups for the same seasons of the two years,

Observations in Spring give $\left.\right\} 46{,}37 = 7x + y \times 37{,}5 - z \times 101{,}48$
in Summer - $360{,}62 = 33x - y \times 570{,}2 + z \times 44{,}13$
in Autumn - $265{,}14 = 28x - y \times 68{,}5 + z \times 411{,}03$
in Winter - $100{,}34 = 18x + y \times 343{,}1 + z \times 86{,}02$

Adding all,

$$772{,}47 = 86x - y \times 258{,}1 + z \times 439{,}7.$$

Subtracting the Winter from the Summer equations,

$$260{,}28 = 15x - y \times 913{,}3 - z \times 41{,}89.$$

Subtracting the Spring from the Autumn equations,

$$218{,}77 = 21x - y \times 106 + z \times 512{,}51.$$

By solving these equations we find $x = 8{,}23$, $y = -\frac{1}{6{,}555}$, $z = \frac{1}{17{,}18}$. These are to be multiplied by sec. 23°. 28′ = 1,0902. Performing this multiplication, and forming the quantities x'', $y \times 10'$, and $-z \times 17{,}''18$ we find that the epoch ought to be increased 8″,97, the mean anomaly ought to be diminished 99″,8, and the greatest equation of the centre diminished 1″09. The epoch of the perigee ought therefore to be increased 107″,8.

The correction of the equation of the centre is so small, that it may be doubtful whether it would be necessary to consider it. In that case, the solar tables would require no other alteration than in the tables of epochs. Every epoch of the sun must be increased by 8″,97 or 9″, and every epoch of the perigee by 1′,48″.

If we reject the equation corresponding to the observations of March 1st, December 22d, and December 23d, 1822, (the 43d, 82d, and 83d of the preceding list), we find that the epoch of the sun's longitude must be increased by 9″,3, and that of the perigee by 1′. 39″; and that the greatest equation of the centre must be diminished by 0″,66.

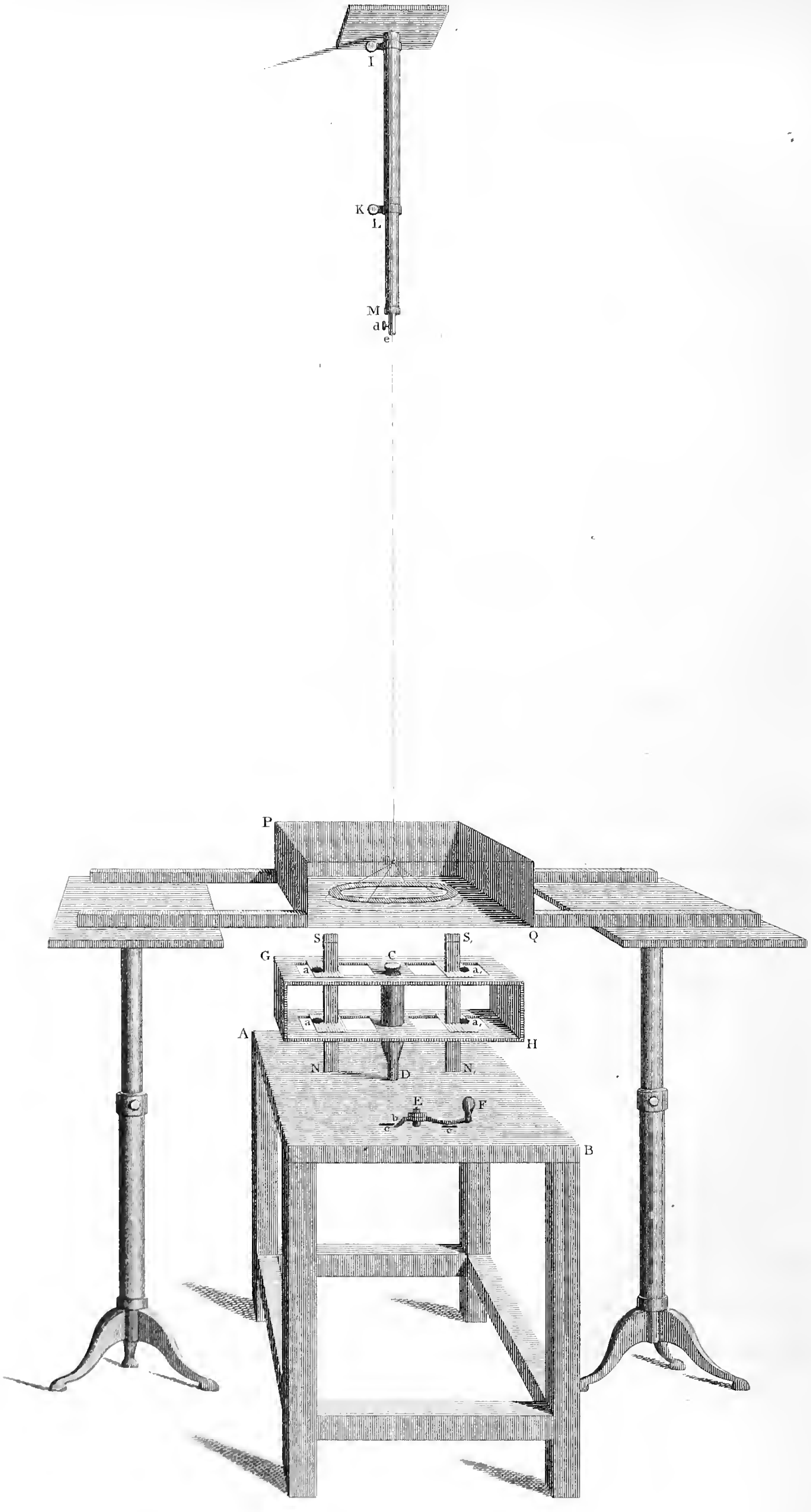
VIII. *On the mutual action of the particles of magnetic bodies, and on the law of variation of the magnetic forces generated at different distances during rotation.* *By* S. H. Christie, *Esq. M. A. F. R. S.*

Read February 15 and 22, 1826.

In my Letter to Mr. Herschel, published in the Transactions, I communicated the results of some experiments which I had made, with the view of determining according to what law of distance the magnetism developed in copper and other metals during rotation, varied. I was aware, that all I could offer at that time must be considered as distant approximations towards useful results; but as I had witnessed several of the very interesting experiments in which Mr. Herschel and Mr. Babbage were engaged, I was desirous that they should be in possession of whatever results I had obtained. In these experiments, I had made use of a thick copper plate revolving under a magnetized needle, and of magnets revolving under a copper disc; so that, at different distances, some of the forces which were brought into action during rotation, had very different angles of inclination to the plane of the needle, in the one case, and to the plane of the disc in the other; and in consequence of this, the results were by no means uniform. In order to remedy this, I proposed, instead of a disc, to make use of a copper ring, as, in this case, the poles of the magnet revolving vertically under it, no lateral forces would be called into action; and I expected that the law, according to which the force acting during rotation varied at different distances, would be determined with great precision. In this expectation I was not disappointed; and I now propose laying before the Society an account of these experiments, and of the results which I obtained from them.

In making some preliminary trials with thin flat rings, I found the effects produced so very much less than with a disc of the same weight, that, previously to pursuing the enquiry which I had at first in view, I was induced to make some experiments, in order to ascertain the effects that would be produced by a simple solution of continuity, in a circular disc, by concentric sections, and likewise by successively removing concentric portions. These experiments clearly show, that the intensity of the magnetism developed during rotation, is not alone materially affected by a separation across, what may be termed, the path of the induced pole, as has been found to be the case by Mr. BABBAGE and Mr. HERSCHEL; but that a separation, concentric with that path, by which the pole is undisturbed in its progress, is equally efficacious in diminishing the intensity of that pole; and that in the magnetism of the whole, when all the parts are continuous, there is in all cases a great accumulation of intensity above the sum of the intensities of the separate parts. This is so important a feature in the phænomena depending upon rotation, that these experiments may not be considered uninteresting: I shall therefore give an account of them previously to entering upon the principal object of this communication.

The nature of the apparatus which I employed on this and subsequent occasions, will be best understood by a reference to Plate X. A B is a very firm table, having a vertical axis



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C D passing through it, to which a rapid rotation can be given by means of the handle E F, to the axis of which, under the table, a wheel is attached, having a band passing round the lower part of the axis C D. In the upper and under sides of the wooden frame G H, firmly screwed on the axis C D, are openings in which the brass frames a, a, a_1, a_1, slide, and can be clamped at any distance from the axis, by means of milled headed screws passing through them, and working in brass nuts, holding on each side of the slits. In the brass frames a, a, a_1, a_1, are square openings to admit the magnets S N, S_1 N_1, either with their faces or their edges towards the axis of rotation, and in which they can be firmly wedged. By this means the distance from the axis of rotation at which the magnets revolve, and likewise the verticality of their axes, can be very accurately adjusted. From the handle E F, a brass spring b projects, and this striking on two small elevations c, c_1, diametrically opposite to each other on the table, indicates very accurately the time of half a revolution. I K is a tube fixed to the ceiling of the room at I, and having a clamping screw at K, by which the tube L M, sliding within the other I K, can be fixed. The suspending wire $e f$ is nipped tightly in a piece of slit brass by means of the screw d. The lower end of the suspending wire is similarly fixed between two small flat pieces of brass, which, below, form a broad stirrup, into which small wires, from the angles of a light rectangular wooden scale, may be passed, and there crossing each other, any torsion of the suspending wire immediately acts upon the scale, and *vice versa.* From the suspended scale a small piece of brass projects, which indicates in circles and degrees, on a large graduated ring, on the bottom of the screen P Q, whatever torsion is given to the wire, thus forming a very accurate balance of torsion. The screen P Q, having paper stretched tightly for the bottom and all the sides raised five inches, prevented currents of air, caused by the rotation of the magnets, affecting the disc or ring suspended over them. A B was so fixed that the axis C D, and the suspending wire *e f* were in the same vertical line, and with this line the centre of the graduated ring also coincided.

On the mutual action of the particles of magnetic bodies during rotation.

Having cut two discs of the same diameter from the same sheet of copper, I suspended one of them at the distance of an inch from the upper horizontal surfaces of the magnets, whose axes were vertical, and south poles upwards, and ascertained by means of this apparatus, the effects that would be produced by making the magnets revolve under it with their axes at various distances from the axis of rotation. I then observed the effect of making a circular cut through the disc, at the distance of an inch from its circumference; first, when a portion was left uncut; and next, when the outer ring was entirely separated from the remaining inner disc. The second disc was suspended by the same wire, and at the same distance from the magnets; and the effects which were produced, by making successively circular cuts, at different distances from the centre, ascertained, whether the interior portions were removed or retained.

The diameter of each of the discs was 8·4 inches; the weight of that which I designate as I. was 5298 grains; that of II. was 5232 grains. I preferred making observations with discs having this difference in weight, to employing a file to reduce them to the same. In observing with the disc II. glass, weighing 66 grains, was added; and in every case where the weight of copper suspended was less than 5298 grains, discs of wood and of glass were added to make up that weight, in order that the tension of the suspending wire might be the same in all cases. This wire was of hard brass of the size called by the wire-drawers No. 22, its length 45·6 inches.

The disc I. being placed in the scale at the distance of one inch from the upper surfaces of two 12 inch magnets, these were adjusted in the frame G H, with their flat faces towards the axis of rotation, so that their axes were vertical, and at the distance 4·2 inches from the axis of rotation, or that these axes revolved directly under the edge of the disc. The tube L M was turned until there was no torsion in the wire when the index on the scale pointed to zero on the graduated ring. The magnets were now made to revolve in the direction of screwing with the angular velocity of 5 revolutions per second, corresponding to one revolution of the handle in two seconds, (which velocity was always carefully preserved in all the subsequent experiments,) and the time in which the disc performed one, two, three, &c. revolutions observed, until it began to revolve in a direction contrary to that of the magnets; the instant when this happened was noted, and likewise the number of revolutions of the index, and the degree where it pointed when it began to retrograde. The same was done when the magnets revolved in the direction of unscrewing, and the means in the two directions taken. Having no assistance in making the experiments I could only note the time to the nearest second.

The numbers in the first column of the following table indicate the torsion of the suspending wire in circles, or the number of revolutions performed by the disc from rest; and in the following columns, are set down, the times in which these revolutions were performed when the magnets revolved at the several distances indicated above the respective columns.

Table I.

Distance of the axes of the magnets from the axis of rotation - - - =		Inches 4·2	Inches 3·7	Inches 3·2	Inches 2·7	Inches 2·2	Inches 1·7
		Time.	Time.	Time.	Time.	Time.	Time.
		sec.	sec.	sec.	sec.	sec.	sec.
Torsion of the wire in circles, or No. of revolutions of the disc -	1	91·5	67·5	58·0	59·0	70·5	96·0
	2	139·0	99·0	87·0	88·0	103·5	147·0
	3	190·5	125·0	109·0	111·0	132·5	206·5
	4	....	152·0	130·0	134·0	161·5	
	5	....	180·5	151·0	158·0	194·5	
	6	....	222·0	173·0	180·0	246·5	
	7	....	....	199·5	213·0		
	8	....	....	237·0			
Disc beginning to revolve in direction opposite to that of the magnets -	Arc =	3⊙ 238°	6⊙ 152°·5	8⊙ 107°	7⊙ 268°·5	6⊙ 52°	3⊙ 121°
	Time =	sec. 267·0	sec. 269·5	sec. 270·5	sec. 269·5	sec. 272·5	sec. 270·0

In order to deduce from these times, the force with which the disc was urged by the magnets revolving at different distances from the centre, let us suppose that this force is equivalent to a certain force acting at the distance 1 from the centre of rotation, and that this latter force would balance a torsion α° of the wire, or that it is equal to $m\,\alpha$, m being a constant to be deduced from the experiments: also let t be the time in which the index on the scale describes an angle

θ, or in which the torsion of the wire is θ; and v the velocity of a point in the disc at the distance 1 from the axis of rotation; α, θ and v being in degrees to the radius 1. We have therefore,

$$v\,dv = m\,(\alpha - \theta)\,.\,d\theta,$$

$$v^2 = m\,(2\,\alpha\,\theta - \theta^2),$$

$$\text{and } t = \frac{1}{\sqrt{m}}\;.\;\text{vers.}^{-1}\,\frac{\theta}{\alpha}\,.$$

Let the values of t and θ, when v becomes o, or the disc begins to turn back, be t_1 and θ_1, then we shall have

$$2\,\alpha\,\theta_1 - \theta_1^2 = o\,,\ \text{and } \alpha = \tfrac{1}{2}\,\theta_1\,.$$

$$\text{also, } t_1 = \frac{1}{\sqrt{m}}\,.\,\pi\,,\ \text{and } \sqrt{m} = \pi\,.\,\frac{1}{t_1}\,.$$

The mean value of t_1 from the experiments is 269·83 seconds, or very nearly 270 seconds; so that $\sqrt{m} = 2\,.\,\frac{1}{3}$.

We have therefore $\frac{\theta}{\alpha} = \text{vers}\;(2^\circ\,.\,\frac{t}{3})$; whence

$$\alpha = \frac{\theta}{\text{vers.}\,\left(2^\circ\,.\,\frac{t}{3}\right)}\,.$$

The corresponding values of θ and t, contained in the preceding table, being substituted in this formula, will give the following values of α, or of the relative forces with which the magnets urged the disc when revolving at different distances from the axis.

Table II.

Distance of the axes of the magnets from the axis of rotation =		Inches 4·2	Inches 3·7	Inches 3·2	Inches 2·7	Inches 2·2	Inches 1·7
		α	α	α	α	α	α
Values of θ from which the values of α are deduced.	360	698·8	1229·1	1642·3	1589·2	1132·1	641·0
	720	688·0	1213·6	1531·6	1500·0	1122·0	632·0
	1080	676·2	1221·8	1538·3	1491·0	1112·3	620·9
	1440	....	1203·5	1528·9	1457·0	1104·7	
	1800	....	1196·0	1518·9	1423·6	1098·7	
	2160	....	1168·8	1512·0	1440·0	1100·4	
	2520	....	....	1498·2	1409·4		
	2880	....	....	1494·4			
$\alpha = \frac{1}{2}\theta_1 =$		659·0	1156·3	1493·5	1394·3	1106·0	600·5
Mean values of α		680·5	1198·4	1528·7	1463·1	1110·9	623·6

The values of α deduced from the different values of θ, for the same distance of the magnets, agree with each other as nearly as could be expected, excepting in two cases, those corresponding to θ 360°, at the distances 3·2 inches and 2·7 inches, which are so much greater than the other values of α at those distances, that I am disposed to think the observations from which they are deduced inaccurate. A slight impulse given to the disc when it was released at zero, would diminish most sensibly the time of the first revolution, and consequently increase the corresponding value of α; and as it was difficult to avoid this in all cases, the inaccuracy in the observation might possibly arise from this cause. I was not aware of the incongruity in these values of α until it was too late to repeat the observations, having cut the disc and moved the apparatus before I made the computations.

Not only in these, but in all the observations which I have made, and from which I have computed the values of α, I have almost invariably found, that the values of α decrease as those of θ, from which they are deduced, increase: in 169 observations there are only 14 exceptions, and in these, in general, the differences are so small, that they most probably arose from small errors in the observations. It would appear then, that in all the experiments there must have been some circumstance which has a tendency to diminish the value of α as that of θ increased. It at first occurred to me that this might be a small deviation from the received law, in the connection between the force exerted by the wire and its torsion; but this I found was not the case, as the results which I obtained by comparing the deviations of a magnetized needle with the torsion of the wire, were extremely uniform. If the force exerted by the wire be proportional to the torsion, the torsion divided by the sine of the deviation should be constant. Now the values which I obtained for this quotient with the torsions 713°·8, 1427°·5, 2141°·17, were 6600, 6595, 6634, the greatest difference among which is only the 165th part of the mean. The resistance of the air would have a tendency to increase the whole time during which the disc continued to revolve in the direction of the magnets, and consequently to diminish the values of α as those of θ increased, but not to a sufficient extent; a diminution of 6 or 7 seconds in the whole time, and in some instances more, being required to account for this decrease in the value of α: nor can the decrease arise from the small changes which take place in the relative velocity of the disc and the magnets. Upon the whole, I think it very probable, that the values of α really decrease in the successive revolutions of the disc; and in this manner: according to the principle adopted in the very interesting Paper by Mr. HERSCHEL and Mr. BABBAGE, *on the magnetism manifested by various substances during rotation*, and so well supported by all the phenomena hitherto observed, the effect of the magnets upon the disc will depend upon the excess of the magnetism in those parts of the disc in the rear of the magnets above that in the parts in advance of them; and if this excess were constant, so would also be the value of α; but if on the magnets coming successively under any point, that point has not parted with all the magnetism which it is capable of losing by the removal of the magnets to the opposite side of the disc, there may be a gradual, though small, accumulation of the magnetism left in every point of the disc; and as only a certain portion of magnetism can be developed in each point during the time that the nets are in its vicinity, the excess which we have mentioned would be thus gradually diminished, and consequently also the value of α.

The forces with which the magnets, revolving at different distances with the same angular velocity urged the disc, are proportional to the mean values of α in Table II. and to deduce the relative forces with which the magnets would urge the disc when revolving at different distances with the same linear velocity, for instance, that at the distance 4·2 inches, the values of α must be increased in the inverse ratio of the respective distances, since the forces, *cæteris paribus*, are protional to the velocities. This reduction is made in the following Table.

Table III.

	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.
Distance of the axes of the magnets from the axis of rotation - =	4·2	3·7	3·2	2·7	2·2	1·7
Force with which the magnets, revolving with the same linear velocity would urge the disc =	680·5	1360·3	2006·4	2275·9	2120·8	1540·7

So that the distance at which the magnets would produce the maximum effect by revolving with the same linear velocity would be very nearly 2·44 inches.

The distance from the axis at which the magnets must revolve, in order that the magnetism may be developed with the greatest intensity, is less than this; since, in estimating this intensity, the length of the lever, at the extremity of which the magnets act, must be taken into account. We shall obtain, at least approximately, the relative intensities of the induced magnetism when the magnets revolved at different distances from the axis, by increasing the numbers in Table III. in the inverse ratio of those distances, or by increasing the values of α in Table II. in the inverse ratio of the squares of the distances.

Table IV.

Distance of the Axes of the magnets from the Axis of rotation =	Inches. 4·2	Inches. 3·7	Inches. 3·2	Inches. 2·7	Inches. 2·2	Inches. 1·7
Intensity of the induced magnetism =	680·5	1544·1	2639·6	3540·3	4048·8	3806·4

It therefore appears from these experiments, that the intensity of the induced magnetism would have been the greatest had the magnets revolved at the distance 2·07 inches nearly; that is, so that their axes had been at the distance of half the radius of the disc from the centre. This is what we might expect, whatever may be the law according to which magnetism is developed in each particle; but the great diminution in the intensity when the magnets revolved under the edge of the disc, leads to an inference respecting the developement of magnetism by induction by no means unimportant, viz. that continuity is much more essential than mass; and that, although a certain portion of magnetism is developed in each particle separately, yet the whole of this is considerably less than that which appears to accumulate by the mutual action of particle upon particle; for which action continuity appears to be requisite.

The measure of the magnetism developed in the copper, if the magnets revolved at the distance 2·1 inches from the axis of rotation, would, interpolating roughly from the results in Table IV. appear to be 4182 nearly; so that 2091 would nearly measure the quantity developed on each side of the axes of the magnets. The measure, therefore, of the magnetism developed when the magnets revolved with their axes under the edge of the disc, if continuity produced no accumulating effect, ought certainly not to be less than 2091 ; but as it is actually only 680·5, it would appear that the continuity of mass on each side of the axes of the magnets has the effect of increasing the quantity of magnetism developed at least in the ratio of 3 to 1.

The results which I obtained by making a circular cut in the disc, lead to a similar conclusion. In this case, the magnets were again adjusted to the distance 3·7 inches from the axis of rotation, and the times in which the disc completed successive revolutions was noted as before. A circular cut through the disc was now made at the distance of an inch from its edge, excepting for an inch at the extremities of two diameters at right angles to each other, as in fig. 1 ; after which the time at the completion of the revolutions was noted. The same was done when the disc was cut, as in fig. 2 ; and, finally, as in fig. 3 ; in which case tissue paper was placed between the ring cut off and the interior disc.

Fig. 1.

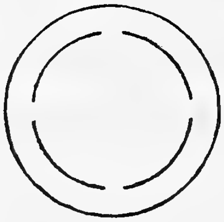


Fig. 2.

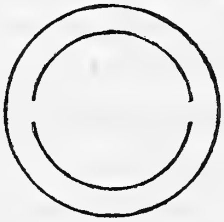
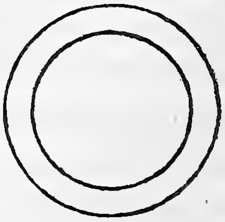


Fig. 3.



The results are arranged in the following Table :

Table V.

The magnets revolving with their axes at the distance 3·7 inches from the axis of rotation.

			Disc uncut.		Disc cut as Fig. 1.		Dise cut as Fig. 2.		Disc cut as Fig. 3.	
		θ	Time.	α	Time.	α	Time.	α	Time.	α
			sec.		sec.		sec.		sec.	
Torsion of the wire in circles, or No. of revolutions of the disc.	1	360	67·0	1246·6	88·0	751·3	128·5	389·4	131·5	375·3
	2	720	98·5	1224·5	132·0	746·0	217·5	395·8	236·0	374·5
	3	1080	125·0	1221·8	178·5	727·4				
	4	1440	153·0	1192·2						
	5	1800	182·0	1184·0						
	6	2160	224·0	1161·2						
Values of t_1 and $\alpha = \frac{1}{2}\,\theta^1$			269·5	1150·5	273·5	709·5	263·5	386·5	270·0	371·5
Mean values of α			......	1197·3	......	733·6	......	390·9	......	373·8

Here, the magnets revolving at the same distance in all cases, the mean values of α are measures of the magnetism developed under the different circumstances. The diminution of effect is sufficiently striking in the case of the ring remaining attached to the interior disc by four inches of its circumference, as in fig. 1. but it is still greater in proportion when the connection between them is further diminished by one half, as in fig. 2.; and no very striking effect, beyond this, is produced by rendering the separation complete. If any doubts could be entertained of the correctness of the inference respecting the effect of the absence of continuous matter, which I have drawn from the experiments when the magnets revolved at different distances from the axis under the entire disc, these last experiments, by exhibiting the effect itself in the most striking manner, precisely of the same nature and nearly in the same degree, must entirely remove them.

After having made these experiments, I ascertained what would be the effect produced when the magnets revolved under the separated ring alone, and likewise under the remaining disc alone: first, when the axes of the magnets were at the distance 3·7 inches from the axis of rotation, that is, remaining as in the foregoing experiments: secondly, when they revolved at the distance 3·2 inches from the axis, that is, having their axes under the inner edge of the ring, or the edge of the remaining disc.

Table VI.

The distance of the axes of the magnets from the axis of rotation.			
3·7 inches		3·2 inches.	
The magnets revolving under		The magnets revolving under	
The ring alone.	The disc alone.	The ring alone.	The disc alone.
$\alpha = 268{\cdot}0$	$\alpha = 120{\cdot}0$	$\alpha = 160{\cdot}5$	$\alpha = 283{\cdot}5$

The sum of the values of α, corresponding to the distance 3·7 inches, is rather greater than the value of α when the magnets revolved at the same distance under the disc and ring together; so that it would appear that no increase in the magnetism takes place in consequence of the proximity of the two masses. That the sum of the value of α, corresponding to the ring and disc separately, exceeds that of α corresponding to the ring and disc together, Table V. probably arises from a circumstance which affected slightly all the results. Although the direct communication between the disc and the currents of air, arising from the rotation of the magnets, was cut off by a screen of paper stretched on a frame, with sides considerably raised; yet a slight current of air was produced near the disc, which increased the final arc of torsion 20° or 30°, or the values of α 10 or 15, as nearly as I could determine by making the magnets revolve under discs of wood and glass, of the same weight as the large copper disc. As I did not consider that the torsion could in all cases be determined within much less limits than this, I preferred giving the observations as they were made, to applying a correction which was doubtful in its amount.

When the magnets revolved at the distance 3·2 inches, the sum of the values of α is 444; and we may take this as the value of α when the magnets revolved under the disc and ring together at that distance. Now we have seen (Table II.) that when they revolved at the distance 3·2 inches from the axis under the uncut disc, the value of α was 1528·7; so that here the magnetism developed was diminished by the circular separation in the ratio of 3·44 to 1.

I now placed the disc II. in the scale, and having determined the effects that were produced by making the magnets revolve under it, with their axes at the distance of 3·2 inches from the axis of rotation, their upper surfaces being at the distance of an inch from the disc, and their angular velocity 5 revolutions in a second, the same as before; I determined successively the effects produced by making circular cuts at the distances ·7 inch, 1·2 inch, 1·7 inch, 2·2 inches, from the centre. I ascertained the effects both when the pieces in the interior were removed and when they were retained, excepting in the first case of the small disc, 1·4 inch in diameter, which was removed, and glass of the same weight substituted in the first experiment, but was afterwards replaced, with the rings cut out, in the others. The differences in the results when the parts were retained, and when they were removed and glass substituted, were so small, that I shall only give the results where they were retained, excepting in the case of the largest circle: In all cases tissue paper was placed between the pieces to prevent contact.

Fig. 1.

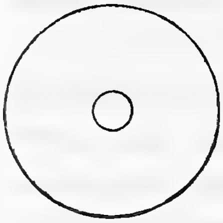


Fig. 2.

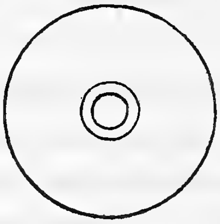


Fig. 3.

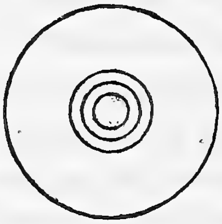


Fig. 4.

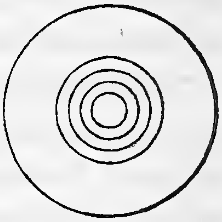


Table VII.

The magnets revolving with their axes at the distance 3·2 inches from the axis of rotation.														
			Disc uncut.		Disc cut as Fig. 1.		Disc cut as Fig. 2.		Disc cut as Fig. 3.		Disc cut as Fig. 4.		Disc cut as Fig. 4, the interior rings and small disc removed.	
		θ	Time.	α	Time.	α	Time.	α	Time.	α	Time.	α	Time.	α
			sec.		sec.		sec.		sec.		sec.		sec.	
Torsion of the wire in circles, or No. of revolutions of the disc.	1	360	58·0	1607·6	60·0	1506·1	63·0	1372·3	68·5	1170·2	79·0	896·3	78·5	907·1
	2	720	85·5	1549·5	88·0	1468·6	92·5	1342·8	100·5	1158·8	116·5	898·6	116·5	898·6
	3	1080	107·0	1558·0	111·0	1462·7	116·0	1357·6	126·5	1176·8	153·5	875.8	153·0	879·2
	4	1440	128·0	1542·2	134·5	1423·4	140·5	1330·6	154·0	1163·5	194·5	868·7	194·0	871·0
	5	1800	150·0	1517·7	157·3	1410·7	164·5	1326·7	184·5	1150·4				
	6	2160	172·0	1502·5	183·5	1389·2	192·5	1317·3	223·0	1155·7				
	7	2520	200·5	1474·4	220·0	1361·4	234·0	1308·7						
	8	2880	252·5	1450·5										
Values of t_1 and $\alpha = \frac{1}{2}\,\theta_1$			267·0	1448·0	267·5	1352·0	265·5	1308·0	265·0	1116·8	266·0	855·5	268·5	864·0
Mean values of α			.....	1516·7	.....	1421·8	.....	1333·0	.....	1156·0	.....	879·0	.....	884·0

As might be expected, the effects produced by the circular separation are not great when the line of separation is near to the centre; but as it approaches the circumference under which the magnets revolve, it increases rapidly. The portions of magnetism destroyed, or rather whose developement is prevented in the disc by circular cuts, at the distances ·7, 1·2, 1·7, 2·2 inches from the centre, appear, by these experiments, to be proportional to 94·9, 183·7, 360·7, 637·7. The last three numbers are very nearly proportional to the masses separated towards the centre, the quotients arising from dividing each by the square of the diameter of the section corresponding are 31·9, 31·2, 32·9; the mean 32·0; and the whole would have followed the same law, had the first number been 63 instead of 95, which difference is not greater than might take place in repetitions of the observations where the whole number of revolutions of the disc is 7 or 8, as in this case. When the magnets revolved under the disc I, with their axes at the distance 3·2 inches from the axis of rotation, the same as in the present instance, the value of α was 1528·7 (Table II.), and when they revolved at the same distance under the ring whose diameters were 6·4 inches, and 8·4 inches, its value was 160·5 (Table VI.); so that the quantity of magnetism whose developement was prevented by the removal of the disc diameter 6·4 inches, is proportional to 1368·2. Now, taking 32 as the quotient arising from dividing the number representing the quantity of magnetism destroyed, by the square of the diameter of the disc removed, 1310·7 will represent the quantity of magnetism destroyed by the removal of the disc diameter 6·4 inches. Supposing the law to be correct, the difference between this number, deduced from the observations with the disc 2, and the preceding, deduced from those with the disc 1, is not very far beyond the limits of errors in the observations, even had they, in all the cases, been made with the same disc; and certainly the agreement is as near as we could expect from observations made with different discs.

If the agreement of all these results with so simple a law is to be considered as fortuitous, the coincidence is certainly most singular; but I am disposed to think that the law in nature is really this, that the quantities of magnetism destroyed by removing concentric portions from the interior of the disc, are proportional to the mass removed, but that they have not generally, the same ratio to the whole magnetism developed in the uncut disc, which the portion removed from the disc has to the whole disc; that is, if d is the diameter of the uncut disc, and α represent the magnetism developed in it; d_{I} and d_{II} the diameters of portions removed, and α_{I}, α_{II} the magnetism developed in the disc when those portions are removed, the magnets revolving at the same distance from the axis in each case, then

$$\frac{\alpha - \alpha_{\mathrm{I}}}{\alpha - \alpha_{\mathrm{II}}} = \frac{d_{\mathrm{I}}^2}{d_{\mathrm{II}}^2},$$

and $\frac{\alpha - \alpha_{\mathrm{I}}}{\alpha}$ is not equal to $\frac{d_{\mathrm{I}}^2}{d^2}$, but $\frac{\alpha - \alpha_{\mathrm{I}}}{\alpha} = q \cdot \frac{d_{\mathrm{I}}^2}{d^2}$,

where q will be constant so long as the magnets revolve at the same distance from the axis, but will vary with that distance. In the present instance, the magnets revolving at the distance 3·2 inches from the axis, the values of q derived from the different observations are 1·48, 1·45, 1·53, 1·54, giving a mean 1·50. Here then the magnetism destroyed by the removal of concentric portions from the interior of the disc, has a considerably greater ratio to the whole magnetism developed than the mass removed has to the whole.

From the great diminution in the intensity of the induced magnetism which takes place on making a circular cut in the disc, especially near to the magnets, it would follow, that if a copper disc were reduced to a series of concentric rings of small breadth, the magnetism developed in it would be scarcely appreciable under whatever part the magnets might revolve: the same would follow from the experiment of Mr. Babbage and Mr. Herschel, if a copper disc, by continual cutting in the direction of radii, were reduced nearly to a series of very small sectors; so that if a disc were separated in both directions, so as to be reduced to pieces of small magnitude, even though not in a state of powder, there can, I think, be no doubt that the induced magnetism would be rendered insensible.

The value of α (Table VII.) when the disc was cut, as in fig. 4. the rings and small disc being retained, is rather less than its value when they were removed: this, however, is only to be attributed to the almost unappreciable effect produced by the pieces in the interior, and unavoidable errors of observation. This will be evident from the results in the following table, obtained by placing an entire disc, 4·4 inches in diameter in the ring, instead of the small disc and rings. The observations were made some time after the preceding, when the apparatus had been moved, and the disc was not at *precisely* the same distance from the magnets, which was the reason of the effects being less than before. The magnets revolved at the same distance from the axis as before, 3·2 inches; first under the ring, diameters 4·4 inches, and 8·4 inches, with the disc 4·4 inches in diameter in the interior; then under the ring alone, and afterwards under the disc alone. The effect of even a slight current of air was excluded by a glass cover over the screen, between the magnets and the disc.

Table VIII.

The magnets revolving with their axes at the distance 3·2 inches from the axis of rotation.							
		Under the ring, diameters 4·4 and 8·4 inches, with disc, diameter 4·4 inches,		Under the ring, diameters 4·4 and 8·4 inches, alone.		Under the disc diameter 4·4 inches alone.	
	θ	Time.	α	Time.	α	Time.	α
		sec.		sec.			
Torsion of the wire in circles, or No. of revolutions of the disk. 1	360	82·0	819·0	84·5	785·0		
2	720	123·0	806·1	127·0	770·2		
3	1080	165·5	777·9	170·5	752·4		
4	1440	226·5	757·1				
Values of t_1 and $\alpha = \frac{1}{2}\theta_1$		264·0	754·5	265·5	732·5	265·0	18·0
Mean values of α		....	782·9	....	760·0	....	18·0

The sum of the mean values of α, when the magnets revolved under the ring alone, and under the disc alone, is here as nearly as can possibly be expected equal to the value of α when they revolved under the ring and disc together. If we attribute the whole difference to error in determining the value of α, when the magnets revolved under the disc alone, which however I have no reason for thinking should be done, the value of α would still in this case be only 23. Increasing this in proportion to the values of α in Table VII. the magnetism developed in the uncut disc, diameter 8·4 inches, that in the ring alone, diameters 8·4 and 4·4 inches, and that in the disc alone, diameter 4·4 inches, would very nearly be represented respectively by 1517, 880, and 26.

That the magnetism destroyed by the removal of a portion of any mass should be more than a proportional part of the whole, does not appear extraordinary; but that the removal of a portion, in which, separately, the magnetism developed is represented by 26, should cause a diminution represented by 637 in the magnetism developed in the mass, or more than 24 times its separate effect; and that the separation should be nearly equivalent to the removal, are striking, and, I think, important facts in the investigation of the laws according to which magnetism is communicated to, and distributed in revolving bodies. These results are in perfect accordance with those which I have deduced from the experiments with the disc I.

It follows then from these experiments, that, in the developement of induced magnetism by rotation, every portion of a mass contributes towards the intensity of the magnetism developed, and that in a much greater ratio than would, by direct induction, be due to that portion according to its distance from the magnet; so that there appears to be, as it were, an accumulation of magnetism arising from the mutual action of the several particles upon each other; continuity throughout has thus a much greater influence than mass: a complete solution of continuity, when it does not take place in the parts adjacent to the magnet, is very nearly equivalent to an entire removal of the remoter mass so separated; and in all cases, the effect produced by complete separation is not much less than that produced by removal.

On the law of the variation of the magnetic forces generated at different distances during rotation.

I have already stated, that with the view of determining this law from experiment, I made use of a ring instead of a disc. This ring is of cast copper, circular, and of uniform thickness; its weight and dimensions as follow: weight 32·375 ounces troy; inner diameter 10·0 inches; outer diameter 11·9 inches; thickness ·24 inch. As I was desirous of avoiding using a file on it, the copper was simply cleared from the mould, and the small inequalities on its surface would consequently give all the measures in excess, so that its specific gravity is higher than 8·0954, which these dimensions would give. The mode which I adopted for suspending the ring was this:—an assemblage of four small wires (No. 18.) was fixed at the extremities of a diameter, so that when stretched, the middle was about two inches above the ring, and another similar assemblage fixed in the same manner at the extremities of the diameter at right angles: these were made to cross through the stirrup at *f*, as I have before described, so that there could be no play in it; that is, the ring could not be turned either way without immediately twisting the suspending wire. A double wire of the size No. 22, was attached to the stirrup, and after the ring had been suspended by it for several hours, its length was found to be 46·25 inches. To the ring, two pieces of wood, having small slips of brass projecting for indexes, were attached diametrically opposite to each other: these, with the wires fixed to the ring, made the whole weight supported by the suspending wire 32·56 ounces troy. The magnets before made use of, the dimensions of which are, length 12·1 inches, breadth ·9 inch, thickness ·36 inch, were fixed vertically in the frame G H, equally distant from the axis of rotation, their edges towards that axis, their south poles upwards and upper surfaces horizontal. The distance between the inner edges of the magnets was 10·05 inches; between their outer edges 11·85 inches; so that their axes revolved exactly under the middle section of the ring, and they described a ring directly under the copper ring, of as nearly as possible the same horizontal dimensions.

The screen P Q being removed, the ring was lowered until it just touched without resting upon the upper surfaces of the magnets, when the distance between K and M, on the tube L M, was ascertained, 7·33 inches: adding ·12 inch to this, 7·45 inches would have been the distance between K and M, if the middle horizontal section of the ring could have coincided with the upper surfaces of the magnets. By subtracting in any case the distance between K and M from this number 7·45 inches, I had very accurately the distance between the upper surfaces of the magnets and the middle horizontal section of the copper ring. After finishing the experiments the screen P Q was removed, and the ring again lowered until, as before, it just touched the upper surfaces of the magnets, when the distance between K and M was found to be 7·34 inches, so that the wire could not have stretched during the experiments; the small difference ·01 inch must be attributed to slight inequalities on the surface of the copper.

The magnets being made, as before, to revolve 5 times in a second, in the direction of screwing, and preserving this velocity very carefully in all cases, by making the intervals between the beats of the spring, *b*, on *c*, c_1, exactly a second, the times in which the ring completed successive revolutions, and in which it began to turn in the direction of unscrewing were noted; and also the number of revolutions and the degrees marked by the index when this took place. The same was done when the magnets revolved in the direction of unscrewing, and the means taken. The results obtained at different distances are tabulated below, where α, θ, θ_1, t, t_1, represent the same quantities as before.

Table I.

Distance of the middle section of the ring from the upper surfaces of the magnets.	Arc of torsion when the ring came to rest before retrograding. Values of θ_1	These are $= \frac{1}{2}\theta_1$ when θ_1 was less than 360°, and are the mean of the values of α when θ_1 was greater than 360°. Values of α
2·5 inches	22°·75	11·375
2·0	51·5	25·75
1·5	144·0	72·00
1·25	278·0	139·00
1·00	581·5	(1) 289·18
0·75	1344·5	(2) 706 25
0·50 *	3853·0	(3) 1982·5

Distance of the middle section of the ring from the upper surfaces of the magnets =			1·0 inch.		0·75 inch.		0·5 inch.	
		θ	t	$\alpha = \frac{\theta}{\text{vers.}\,\pi.\frac{t}{t_1}}$	t	$\alpha = \frac{\theta}{\text{vers.}\,\pi.\frac{t}{t_1}}$	t	$\alpha = \frac{\theta}{\text{vers.}\,\pi.\frac{t}{t_1}}$
Torsion of the wire in circles, or No. of revolutions of the ring.	1	360	sec. 183·0	287·6	sec. 102·75	748·6	sec. 59·25	2188·3
	2	720	......	......	159·75	718·4	88·5	2032·1
	3	1080	......	......	219·0	685·5	110·125	2038·7
	4	1440	......	......	......	......	131·75	1983·4
	5	1800	......	......	......	......	151·62	1961·3
	6	2160	......	......	......	......	170·37	1960·7
	7	2520	......	......	......	......	190·75	1942·4
	8	2880	......	......	......	......	212·5	1929·1
	9	3240	......	......	......	......	236·25	1928·0
	10	3600	......	......	......	......	269·25	1916·5
Values of t_1 and $\alpha = \frac{1}{2}\theta_1$			315·0	290·75	315·0	672·5	320·25	1926·5
Mean values of α			......	(1) 289·18	......	(2) 706·25	......	(3) 1982·5

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* When the copper ring was at the distance ·5 inch from the magnets, its outer circumference was at the distance of less than ·25 inch from the inner circumference of the graduated ring, on which the deviations and revolutions of the copper ring were measured, and which rested upon the screen; but this produced no effect, at least that was sensible even at this small distance; for after removing the graduated ring, the times of completing the several revolutions of the copper ring, and likewise the whole arc of torsion before it came to rest, were found to be as nearly as possible the same as they were previous to the removal.

In order to obtain from these experiments the law according to which the force urging the ring at different distances varied, I assume that the whole action of the magnets may be referred to a single point, near the extremity in each, as a pole ; and I consider that, within certain limits of the distances of the point acted on, no sensible error will arise from this supposition. My first trials were to ascertain whether this force varied inversely as any exact power of the distance, and I found that the supposition that it varied inversely as the 4th power of the distance, would give results approximating very closely to those obtained by observation. As however, on the principle that time is requisite both for the developement, and for the dissipation of magnetism, the induced poles in the ring would always be in the rear of the magnets, it appeared probable, that the supposition of the force varying inversely as some power of the distance between the pole of each magnet, and a point in the ring at a certain distance in the rear of it, would give results approximating even more closely to the observations ; and on trial I found that this was decidedly the case.

Calling c the vertical distance of the upper surface of the magnets from the middle horizontal section of the ring, p the distance of the poles of the magnets from their upper surfaces, and ε a constant horizontal distance from the pole of either magnet to a point behind it in the ring, I found that the formula,

$$\alpha = \left\{ \frac{M}{(p+c)^2 + \varepsilon^2} \right\}^2 \quad . \; . \; . \; . \; . \; (1),$$

where M and p are constants which, with ε, are to be determined from the observations, gave results approximating much more closely to the observations than the formula,

$$\alpha = \left\{ \frac{M_1}{p_1 + c} \right\}^4.$$

That an estimate may be formed of the degree of coincidence, I shall give the comparison of the observations with the results obtained in both cases. Putting $\frac{1}{\alpha^{\frac{1}{2}}} = a$ in the equation (1) we have

$$(p + c)^2 + \varepsilon^2 = M a \quad . \quad . \quad . \quad . \quad (2).$$

If we indicate the values of c and a, in the successive observations, by c_{I}, c_{II}, c_{III}, &c. and a_{I}, a_{II}, a_{III}, &c. and eliminate p and ε from three equations of the form (2), then

$$M = \frac{(c_l - c_m) \cdot (c_l - c_n) \cdot (c_m - c_n)}{a_n (c_l - c_m) - a_m (c_l - c_n) + a_l (c_m - c_n)} \quad . \qquad (3).$$

Since, in determining the value of M from this formula by means of the observations, errors of observation to the same extent would be the more sensible the less the intervals $c_l - c_m$, $c_l - c_n$, $c_m - c_n$, between the observations, instead of employing all the possible combinations of the observations, in order to deduce the mean value of M, I have excluded all combinations in which two consecutive observations entered. This however makes but a very small difference in the results, since the mean value of M deduced from the thirty-five combinations of the seven observations is 23·362, and its value deduced from the ten combinations in which consecutive observations are excluded is 23·314.

The mean value of M being obtained, the value of p will be found by eliminating ε from two equations of the form (2), and we have thus,

$$p = \tfrac{1}{2} \left\{ \frac{M (a_l - a_m)}{c_l - c_m} - (c_l + c_m) \right\} \quad . \quad . \quad . \quad . \quad (4).$$

In computing the mean value of p from the different combinations of the observations, I, as before, exclude the combinations of consecutive observations.

The values of ε^2 are computed from the several equations of the form (2), by substituting the mean values of M and p; and I thus obtain the mean value of ε^2.

The following table exhibits the values of a in the different observations, the several values of M and p deduced from the different combinations of the observations, and the values of ε^2 derived from the separate observations.

Table II.

Values of c in different observations.	Values of a in different observations.	Values of M determined from different combinations of $a_{I}, a_{II}, a_{III}, a_{IV}$, &c.		Values of p determined from the mean value of M and different combinations of a_{I}, a_{II}, a_{III}, &c.		Values of ε^2 determined from the mean values of M and p, and different values of a.
		Combinations.	Values of M.	Combinations.	Values of p	
inches.						
c_{I} = 2·5	a_{I} = ·296500	$a_{I}\ a_{III}\ a_{V}$	24·770	$a_{I}\ a_{III}$	·0825	·1255
c_{II} = 2·0	a_{II} = ·197061	$a_{I}\ a_{III}\ a_{VI}$	24·412	$a_{I}\ a_{IV}$	·0990	·1625
c_{III} = 1·5	a_{III} = ·117851	$a_{I}\ a_{III}\ a_{VII}$	24·022	$a_{I}\ a_{V}$	·0972	·1709
c_{IV} = 1·25	a_{IV} = ·084819	$a_{I}\ a_{IV}\ a_{VI}$	23·344	$a_{I}\ a_{VI}$	·0994	·1409
c_{V} = 1·00	a_{V} = ·058805	$a_{I}\ a_{IV}\ a_{VII}$	23·202	$a_{I}\ a_{VII}$	·0973	·1495
c_{VI} = 0·75	a_{VI} = ·037629	$a_{I}\ a_{V}\ a_{VII}$	23·318	$a_{II}\ a_{IV}$	·1195	·1459
c_{VII} = 0·50	a_{VII} = ·022459	$a_{II}\ a_{IV}\ a_{VI}$	22·614	$a_{II}\ a_{V}$	·1117	·1573
		$a_{II}\ a_{IV}\ a_{VII}$	22·553	$a_{II}\ a_{VI}$	·1118	
		$a_{II}\ a_{V}\ a_{VII}$	22·878	$a_{II}\ a_{VII}$	·1069	
		$a_{III}\ a_{V}\ a_{VII}$	22·026	$a_{III}\ a_{V}$	·1266	
				$a_{III}\ a_{VI}$	·1219	
				$a_{III}\ a_{VII}$	·1120	
				$a_{IV}\ a_{VI}$	·1002	
				$a_{IV}\ a_{VII}$	·0942	
				$a_{V}\ a_{VII}$	·0974	
		Mean	23·314	Mean	·1052	·1504

The differences here, between some of the values of M and p, and their mean values, may appear more considerable than they ought to be, if the formula from which they are derived be correct; but it is to be observed, that even very small errors in the observations are rendered very sensible by thus combining them. With regard to the differences in the values of ε^2, supposing the formula to be correct, and the observations to have been liable to no other errors than those in estimating the distances between the magnets and the ring, an error in this respect of $\frac{1}{100}$ inch would cause a greater difference than any here between the different values of ε^2 and the mean. As I took every precaution to ensure accuracy, I consider that although I might be liable to an error to this amount, I was not liable to a greater. The best criterion of the correctness of the formula is, however, the agreement of the observed values of α with those deduced from it by employing these mean values of M, p, and ε^2, and likewise the agreement in the values of these quantities deduced from the separate observations. The comparison between the observed values of α and those computed from the formula,

$$\alpha = \left\{\frac{M}{(p+c)^2 + \varepsilon^2}\right\}^2,$$

assuming $M = 23{\cdot}314$, $p = {\cdot}1052$ and $\varepsilon^2 = 1504$, is made in the following table.

Table III.

Values of c in the different observations.	Observed values of α	Values of α computed from $\alpha = \left\{\frac{M}{(p+c)^2 + \varepsilon^2}\right\}^2$	Difference between the observed & computed values of α.	Quotient of this diff. divided by the computed value of α.	Values of the constants deduced from the separate observations.		
					M	p	ε^2
inches.							
2·5	11·375	11·293	— 0·082	— ·00726	23·398	·1005	.1504
2·0	25·75	25·886	+ 0·136	+ ·00525	23·253	·1088	.1504
1·5	72·00	73·086	+ 1·086	+ ·01486	23·140	·1115	.1504
1·25	139·00	137·68	— 1·32	— ·00959	23·426	·1017	.1504
1·00	289·18	288·79	— 0·39	— ·00135	23·330	·1048	.1504
0·75	706·25	699·03	— 7·22	— ·01033	23·434	·1026	.1504
0·50	1982·5	2035·9	+ 53·4	+ ·02623	23·006	·1109	.1504
The mean values of M and p from the separate observations					23·284	·1058	

The quotients in the 5th column afford the best estimate of the degree of coincidence between the observed and computed values of α, since the magnitude of the errors to which the observations are liable depends upon the magnitude of α. The greatest discordance between the observed and computed values of α, whether estimated by their actual difference, or by dividing this difference by the computed value of α, is that corresponding to the least distance of the ring from the magnets; but even this amounts to little more than a fortieth of the whole. Perhaps it would have been as well to have omitted this observation altogether, since the distance of the ring from the magnets was so small, that supposing that distance to have been ·51 inch instead of ·50, or that an error of ·01 was made in estimating it, the difference between the observed and computed values of α would have been — 39·4 instead of + 53·4. Having however made the observations with great care, from this close agreement between the values of α deduced from the formula,

$$\alpha = \left\{ \frac{M}{(p+c)^2 + \varepsilon^2} \right\}^2,$$

and those observed, I cannot but conclude that this formula is correct.

If we were to suppose that

$$\alpha = \left\{ \frac{M'}{p+c} \right\}^4;$$

then

$$M' = \frac{c_l - c_m}{a_l - a_m},$$

where $a_l = \left(\frac{1}{\alpha_l}\right)^{\frac{1}{4}}$, $a_m = \left(\frac{1}{\alpha_m}\right)^{\frac{1}{4}}$.

Combining the observations as I have already mentioned, I obtained the mean values $M' = 5{\cdot}060$ and $p = {\cdot}240$. From the mean value of M′, deduced by combining all the observations, I had previously found $p = {\cdot}256$, and $M' = 5{\cdot}1263$ from the separate observations, but I afterwards discovered that the latter values were slightly affected by an error in one of the computations; as however I had previously determined the values of α by means of them, and these are rather nearer to the observed values than those obtained from $M' = 5{\cdot}060$ and $p = {\cdot}240$, I give the results of both to show the effects of such changes in the constants.

Table IV.

Values of c in the different observations.	Observed values of α	Values of α computed from $\alpha = \left\{\frac{M'}{p+c}\right\}^4$ $M' = 5{\cdot}060$ $p = 0{\cdot}240$	Difference between the observed & computed values of α	Quotient of this diff. divided by the computed value of α	Values of α computed from $\alpha = \left\{\frac{M'}{p+c}\right\}^4$ $M' = 5{\cdot}1263$ $p = 0{\cdot}256$.	Difference between the observed & computed values of α	Quotient of this diff. divided by the computed value of α
Inches.							
2·5	11·375	11·631	+ 0·255	+ ·02193	11·970	+ 0·595	+ ·04971
2·0	25·75	26·038	+ 0·288	+ ·01106	26·659	+ 0·909	+ ·03410
1·5	72·00	71·516	— 0·484	— ·00677	72·629	+ 0·629	+ ·00866
1·25	139·00	133·00	— 6·000	— ·04511	134·248	— 4·752	— ·03540
1·00	289·18	277·28	— 11·90	— ·04292	277·490	— 11·69	— ·04215
0·75	706·25	682·43	— 23·82	— ·03490	674·24	— 32·01	— ·04748
0·50	1982·5	2186·1	+ 203·6	+ ·09313	2114·1	+ 131·6	+ ·06226

The quotients in the 5th and 8th columns are not in any case great, but are decidedly greater than the corresponding quotients in Table III. Considering these quotients as the errors arising from the respective formulas, and that the sum of the squares of the errors is the best criterion of correctness, it appears that the sum of the squares of these errors in the 5th and 8th columns respectively, are ·014417 and ·012869, and that the sum of the squares of the errors in the 5th column of Table III. is only ·001190, or not a tenth of the least sum in the other case. It is manifest then, that the introduction of the quantity ε into the expression for the value of α, renders it essentially more accurate; and the result of these different comparisons is, I think, quite decisive of the correctness of the formula,

$$\alpha = \left\{ \frac{M}{(p+e)^2 + \varepsilon^2} \right\}^2 .$$

I have stated in my letter to Mr. Herschel,* that when a thick copper plate revolved under a needle, the force by which the plate urged the needle appeared to vary nearly as the inverse *4th power* of the distance, but that when magnets revolved horizontally under a copper disc, the force with which they urged it appeared to vary according to a law approximating rather towards that of the inverse *square* of the distance. The difference of the law in the two cases arose no doubt from a light copper disc having been suspended over strong bar magnets, in the second experiment, instead of suspending the heavy copper plate over the needle used in the first experiment, which could not have been conveniently done, as its weight is about 16 lbs. but which would have reversed the experiment. To remove any doubt that might arise from this apparent incongruity, I determined now precisely to reverse the experiment in the two cases, making the copper ring revolve *under* the same magnets, *over* which it had been before suspended. As however I could not suspend the magnets vertically over the ring, without increasing so considerably the weight to be suspended, by the apparatus to fix them in, that the wire by which the ring had been suspended would no longer sustain them, and

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* Phil. Trans. 1825.

indeed that no wire, on the torsion of which any sensible effect could be produced, would do so, the preceding experiments could not be made use of as strictly comparative with others in which the ring should revolve under the magnets. It therefore became necessary to make a different adjustment of the magnets, in the first instance, under the copper.

The magnets used in the preceding experiments were now placed horizontally, and parallel to each other, on their flat sides, on the top of the frame G H, with their poles of the same name adjacent, and their nearest sides at the distance ·34 inch from each other. The copper ring remained suspended by the same wire as in the preceding experiments, and being lowered until its under surface just touched, without resting upon two brass nuts, fixing the magnets to the frame, the distance between K and M was ascertained to be 9·575 inches: so that the distance from the under surface of the magnets to the under surface of the ring being ·59 inch, the thickness of the ring ·24 inch, and that of the magnets ·36 inch, the distance between K and M would have been 10·105, could the middle horizontal section of the ring have coincided with the axes of the magnets; which distance was therefore in this case to be considered as zero, on the scale measuring the distances between the middle horizontal section of the ring, in any case, and the horizontal plane passing through the axes of the magnets.

I have before mentioned, that, from the formula $\alpha = \left\{\frac{M'}{p+c}\right\}^4$, I had found p, or the distance of the poles of the magnets from their extremities, to be ·256 inch, which, as I was not aware at the time that any other formula would more closely represent the observations, I considered to be a near approximation, and that consequently this value of p was correct: and as I wished to see how far the effects produced by the magnets revolving with their axes vertical, agreed with the effects when they revolved with their axes horizontal, and their poles at the same distances from the ring, I regulated the distances at which I observed, in the latter case, according to this value of p. This will account for the peculiar distances at which I made observations which might otherwise appear somewhat singular. The results which I obtained are arranged in the following table.

Table V.

came to rest before retrograding. es of θ_1	These are $=\frac{1}{2}\theta_1$ when θ_1 was less than 360°, and are the mean of the values of α when θ_1 was greater than 360°. Values of α
71·0	35·5
46·0	73·0
42·5	171·25
63·0	(1) 282·45
56·0	(2) 481·3
25·0	(3) 871·2
27·5	(4) 1811·7

Distance of the middle section of the ring from the axes of the magnets. =			1·506 inch.		1·256 inch.		1·006 inch.		0·756 inch.	
		θ	t	$\alpha=\frac{\theta}{\text{vers.}\,\pi.\frac{t}{t_1}}$	t	$\alpha=\frac{\theta}{\text{vers.}\,\pi.\frac{t}{t_1}}$	t	$\alpha=\frac{\theta}{\text{vers.}\,\pi.\frac{t}{t_1}}$	t	$\alpha=\frac{\theta}{\text{vers.}\,\pi.\frac{t}{t_1}}$
			sec.		sec.		sec.		sec.	
Torsion of the wire in circles, or No. of revolutions of the ring.	1	360	185·4	283·4	131·7	485·0	95·3	880·0	64·5	1873·2
	2	720	......	......	210·2	480·9	141·3	876·8	93·4	1853·4
	3	1080	......	......	......	......	184·3	870·1	117·3	1836·0
	4	1440	......	......	......	......	233·0	866·6	138·8	1829·1
	5	1800	......	......	......	......	......	......	160·0	1822·6
	6	2160	......	......	......	......	......	......	181·1	1803·1
	7	2520	......	......	......	......	......	......	203·8	1789·0
	8	2880	......	......	......	......	......	......	228·9	1779·8
	9	3240	......	......	......	......	......	......	261·5	1767·4
Values of t_1 and $\alpha=\frac{1}{2}\theta_1$			317·3	281·5	317·3	478·0	319·0	862·5	321·5	1763·75
Mean values of α			......	(1) 282·45	......	(2) 481·3	......	(3) 871·2	......	(4) 1811·7

In the position which the magnets had here with regard to the ring, the points to which we may refer all the forces of the magnets, that is their poles, will not be at the same distance from their extremities as when the magnets were vertical; and the errors which will arise from considering that these points continue fixed for different distances of the ring, will be increased when those distances are small, in consequence of the great obliquity of the direction in which some of the forces are exerted. The pole of the magnet will no longer be directly under the ring; but if we call c the vertical distance of the middle section of the ring from the axis of the compound magnet, and ε, as before, a constant horizontal distance from the pole, at either end, to a point behind it in the ring, then the formula (1) will be

$$\alpha = \left\{ \frac{M}{c^2 + \varepsilon^2} \right\}^2 \qquad (5).$$

Putting, as before, a for $\frac{1}{\alpha^{\frac{1}{2}}}$, and calling the values of a corresponding to the distances c_l, c_m, a_l, a_m,

$$M = \frac{(c_l + c_m) \cdot (c_l - c_m)}{a_l - a_m} \qquad (6).$$

Combining these observations in the same manner as those which precede, I obtain the mean values of M, and then the values of ε^2 from the separate observations, by means of the formula,

$$\varepsilon^2 = M\, a - c^2 \qquad (7):$$

or we may obtain ε^2 independently of M from (5), then

$$\varepsilon^2 = \frac{c_l^2\, a_m - c_m^2\, a_l}{a_l - a_m},$$

or,

$$\varepsilon^2 = \frac{(c_l^2 - c_m^2) \cdot a^m}{a_l \quad a_m} - c_m^{\ 2} \qquad (8),$$

which latter is more convenient for computation; and the values of M may be computed from the separate observations.

The values of M computed from the formula (6), of ε^2 deduced from the separate observations with the mean value of M thus obtained, and also of ε^2 from the formula (8), are contained in the following table.

Table VI.

Values of c in different observations.	Values of a in different observations.	Values of M determined from different combinations of $a_{\rm I}, a_{\rm II}, a_{\rm III}, a_{\rm IV}$, &c.		Values of ε^2 determined from M = 48·518, and the separate observations.	Values of ε^2 determined from different combinations of $a_{\rm I}, a_{\rm II}, a_{\rm III}, a_{\rm IV}$, &c.	
		Combinations.	Values of M		Combinations.	Values of ε^2
inches.						
$c_{\rm I}$ = 2·756	$a_{\rm I}$ = ·167836	$a_{\rm I}$ $a_{\rm III}$	49·354	·5476	$a_{\rm I}$ $a_{\rm III}$	·6880
$c_{\rm II}$ = 2·256	$a_{\rm II}$ = ·117041	$a_{\rm I}$ $a_{\rm IV}$	49·177	·5891	$a_{\rm I}$ $a_{\rm IV}$	·6580
$c_{\rm III}$ = 1·756	$a_{\rm III}$ = ·076416	$a_{\rm I}$ $a_{\rm V}$	49·225	·6241	$a_{\rm I}$ $a_{\rm V}$	·6663
$c_{\rm IV}$ = 1·506	$a_{\rm IV}$ = ·059502	$a_{\rm I}$ $a_{\rm VI}$	49·147	·6189	$a_{\rm I}$ $a_{\rm VI}$	·6531
$c_{\rm V}$ = 1·256	$a_{\rm V}$ = ·045582	$a_{\rm I}$ $a_{\rm VII}$	48·662	·6340	$a_{\rm I}$ $a_{\rm VII}$	·5725
$c_{\rm VI}$ = 1·006	$a_{\rm VI}$ = ·033880	$a_{\rm II}$ $a_{\rm IV}$	49·036	·6318	$a_{\rm II}$ $a_{\rm IV}$	·6497
$c_{\rm VII}$ = 0·756	$a_{\rm VII}$ = ·023494	$a_{\rm II}$ $a_{\rm V}$	49·147	·5684	$a_{\rm II}$ $a_{\rm V}$	·6627
		$a_{\rm II}$ $a_{\rm VI}$	49·031		$a_{\rm II}$ $a_{\rm VI}$	·6458
		$a_{\rm II}$ $a_{\rm VII}$	48·297		$a_{\rm II}$ $a_{\rm VII}$	·5631
		$a_{\rm III}$ $a_{\rm V}$	48·842		$a_{\rm III}$ $a_{\rm V}$	·6488
		$a_{\rm III}$ $a_{\rm VI}$	48·700		$a_{\rm III}$ $a_{\rm VI}$	·6378
		$a_{\rm III}$ $a_{\rm VII}$	47·466		$a_{\rm III}$ $a_{\rm VII}$	·5455
		$a_{\rm IV}$ $a_{\rm VI}$	49·020		$a_{\rm IV}$ $a_{\rm VI}$	·6487
		$a_{\rm IV}$ $a_{\rm VII}$	47·114		$a_{\rm IV}$ $a_{\rm VII}$	·5356
		$a_{\rm V}$ $a_{\rm VII}$	45·545		$a_{\rm V}$ $a_{\rm VII}$	·4985
		Means	48·518	·6020		

Assuming $M = 48·518$, and $\varepsilon^2 = ·6020$, the values of α corresponding to the different values of c may be computed from the formula (5). The comparison between the values of α, thus computed, and those observed, is made in the following table.

Table VII.

Values of c in the different observations.	Observed values of α	Values of α computed from $\alpha = \left\{\frac{M}{c^2+\varepsilon^2}\right\}^2$	Diff. between the observed and computed values of α.	Quotient of this diff. divided by the computed value of α.	Values of M deduced from the separate observations.
inches					
2·756	35·5	35·03	— 0·47	—·01342	48·842
2·256	73·0	72·67	— 0·33	—·00454	48·628
1·756	171·25	173·31	+ 2·06	+·01189	48·229
1·506	282·45	285·79	+ 3·34	+·01471	48·234
1·256	481·3	495·56	+ 14·26	+·02878	47·814
1·006	871·2	903·65	+ 32·45	+·03591	47·639
0·756	1811·7	1709·4	—102·3	—·05985	49·949
Mean value of M from the separate observations - -					48·476

The agreement of the computed with the observed values of α, which I estimate by the quotients in the 5th column, is, upon the whole, as near as we could expect with this adjustment of the magnets. In the last observation, where the distance between the surfaces of the ring and magnets was only ·456 inch, the forces of some points in the magnets were exerted in directions so much more oblique than in the other observations, that this observation scarcely admits of comparison with the others, without taking into account the effect which this obliquity in the direction of the forces will have on the situation of the pole of the magnets. If in the comparison we reject this observation, the agreement between the computed and observed values of α becomes extremely close. Omitting in the values of ε^2, in the 7th column of Table VI. all those in which a_{vii} enters, we shall have the mean value of $\varepsilon^2 = \cdot 6559$; and the mean value of M deduced from the separate observations will be 49·093. The values of α at the different distances, computed from these values of M and ε^2, are compared with the observed values of α, in the following table.

Table VIII.

Values of c in the different observations.	Observed values of α	Values of α computed from $\alpha = \left\{ \frac{M}{c^2 + \varepsilon^2} \right\}^2$	Diff. between the observed and computed values of α.	Quotient of this diff. divided by the computed value of α.	Values of M deduced from the separate observations.
inches.					
2·756	35·5	35·398	— 0·102	— ·00288	49·163
2·256	73·0	73·013	+ 0·013	+ ·00018	49·089
1·756	171·25	172·36	+ 1·11	+ ·00644	48·935
1·506	282·45	281·90	— 0·55	— ·00195	49·140
1·256	481·3	483·18	+ 1·88	+ ·00389	48·998
1·006	871·2	866·36	— 4·84	— ·00559	49·230
Mean value of M from the separate observations -					49·093

The quotients in the 5th column are so extremely small, that there can be no doubt of the formula

$$\alpha = \left\{ \frac{M}{c^2 + \varepsilon^2} \right\}^2,$$

accurately representing the observations in all cases within the above limits of the value of c.

Having clearly ascertained the law of the force by which the magnets urged the copper ring during their rotation under it, I next proposed reversing the experiment by making the copper ring revolve under the same magnets. For this purpose the magnets were placed in the wooden scale, suspended by the wire which in the preceding experiments had carried the ring: they were placed on their flat sides at the distance ·34 inch from each other, with their poles of the same name adjacent, precisely as they were in the preceding case; and when the index on the scale between their south poles pointed in the magnetic meridian, there was no torsion in the wire. The suspending wire, if produced, would have bisected the line equidistant from the axes of the magnets, and passed through the axis of rotation. The ring was firmly fixed on the top of the frame, G H, so as to revolve horizontally in its own plane, about its centre, under the magnets; and the same precautions as before were taken in order to determine accurately the distance between K and M, corresponding to the zero of distance between the middle section of the ring and the axes of the magnets. I have before stated that the weight suspended in the preceding experiments was 32·56 ounces: in the present case, the weight of the magnets, scale, &c. suspended by the same wire was 33·475 ounces, so that the tension was rather more than before; but this difference was unavoidable, as the weight of the magnets alone is nearly equal to that of the ring.

The effects produced at different distances by the rotation of the ring being measured by the deviation of the magnets from the meridian, in order to compare these effects with those observed when the magnets revolved under the ring, it was necessary to determine the degree of torsion equivalent to any deviation of this compound needle. For this purpose the suspending wire was twisted 2, 4, 6 circles by making the magnets describe 2, 4, 6 circles, first in the direction of screwing, then in the contrary direction, noting the corresponding deviations of the index on the compound needle; and the mean of the deviations in opposite directions being taken, the ratio of the torsion to the sine of the deviation was obtained: the following are the results.

No. of turns of the wire.	Deviation of the index to the magnets.		Mean deviation.	Arc of torsion.	Torsion / Sin. deviation.
	Screwing.	Unscrewing.			
2	6° 05′ E	6° 20′ W	6° 12½′	713° 48′	6600·465
4	12 30	12 30	12 30	1427 30	6595·386
6	18 55	18 45	18 50	2141 10	6634·320
Mean value of $\frac{\text{Torsion}}{\text{sin. deviation}}$					6610·057

The ring was made to revolve with the same velocity as before, viz. 5 revolutions in a second; first in the direction of screwing; and this velocity was carefully maintained until the needle became steady in its direction, when the deviation marked by the index was noted: the same was done in the direction of unscrewing; and the mean of these deviations was taken as the deviation due to the rotation of the ring. When the distance between the middle section of the ring and the axes of the magnets exceeded 2 inches, the deviation was so small, that the errors to which the observation of it was liable, 2′ or 3′, bore too great a proportion to the whole for me to place much reliance on the observations, and I, consequently, have not noted the deviation at the distance 2·756 inches.

In the following table are contained the observed deviations, the torsion equivalent to the force urging the magnets during rotation, and likewise the values of M and ε^2 obtained from the torsions as before: the numbers in the third column are deduced from the observed deviations by substituting them in the expression,

Arc of torsion = 6610. Sin deviation;

and the values of α, or the torsion of the wire which is equivalent to the force with which the ring urged the magnets at different distances, are found by adding the deviation to the preceding arc of torsion.

Table IX.

Distance of the middle section of the ring from the axes of the magnets, or values of c_{II}, c_{III}, &c.	Deviation of the magnets caused by the rotation of the ring.	Torsion of the wire which is equivalent to the deviation.	Torsion of the wire which is equivalent to the force with which the ring urges the magnets, or value of α.	Value of a_n or $\frac{1}{\alpha_n^{\frac{1}{2}}}$.	Values of M determined from different combinations of a_{II}, a_{III}, a_{IV}, a_{V}, &c.		Values of ε^2 determined from M = 46·63 and the separate observations.
					Combinations.	Values of M.	
					a_{II} a_{IV}	47 828	
					a_{II} a_{V}	47·497	
					a_{II} a_{VI}	47·525	
					a_{II} a_{VII}	47·075	
$c_{II} = 2·256$	0° 36′	69·2	69·8	$a_{II} = ·119694$	a_{III} a_{V}	45·860	·4918
$c_{III} = 1·756$	1 23·5	160·5	161·9	$a_{III} = ·078592$	a_{III} a_{VI}	46·347	·5812
$c_{IV} = 1·506$	2 20	269 1	271·4	$a_{IV} = ·060701$	a_{III} a_{VII}	45·779	·5624
$c_{V} = 1·256$	4 06·5	473·6	477·7	$a_{V} = ·045753$	a_{IV} a_{VI}	46·859	·5559
$c_{VI} = 1·006$	7 30	862·8	870·3	$a_{VI} = ·033897$	a_{IV} a_{VII}	45·875	·5686
$c_{VII} = 0·756$	15 27·5	1761·8	1777·3	$a_{VII} = ·023720$	a_{V} a_{VII}	45·659	·5345
					Mean values	46·630	·5491

If we compare the torsions in the fourth column, which are equivalent to the force with which the ring by its rotation urges the magnets, with the mean torsions before obtained, which represent the force with which the magnets urge the ring when revolving horizontally under it, we shall find the agreement such, that there can be no doubt, that the forces are the same at the same distances in the two cases. The mean value of α at the distance ·756 inch when the magnets revolved was 1811·7; but it is to be observed that the value of α in the present instance, 1777·3, should not be compared with this, but with 1763·75, that value of α which is half the arc of torsion when the ring comes to rest, previously to turning in the contrary direction to that of the rotation of the magnets, since in the present case the rotation of the ring was continued until it balanced steadily the directive force of the magnets. This consideration greatly diminishes the difference in the two cases, and will account also in some measure for the values of M and ε^2 being different in Table VI. and Table IX.; but I must likewise state that the observations, when the ring revolved under the magnets, were liable to errors which did not affect those in the reverse form of the experiment, although I took great care to determine correctly the direction of the index to the magnets when the force of rotation balanced the directive force.

The comparison between the values of α contained in the preceding table, and those computed for the several values of c from the formula (5), making $M = 46{\cdot}63$ and $\varepsilon^2 = {\cdot}5491$, is made in the following table; and their agreement is such, that there can be no doubt, that the effects produced on the magnets by the rotation of the ring at different distances are correctly represented by this formula.

Table X.

Values of c in the different observations.	Observed values of α	Values of α computed from $\alpha = \left\{ \frac{M}{c^2+\varepsilon^2} \right\}^2$	Diff. between the observed and computed values of α.	Quotient of this diff. divided by the computed value of α.	Values of M deduced from the separate observations.
inches.					
2·256	69·8	68·39	− 1·41	− ·02062	47·108
1·756	161·9	164·78	+ 2·88	+ ·01748	46·221
1·506	271·4	273·98	+ 2·58	+ ·00942	46·410
1·256	477·7	480·79	+ 3·09	+ ·00643	46·480
1·006	870·3	892·21	+ 21·91	+ ·02455	46·054
0·756	1777·3	1731·5	− 45·8	− ·02645	47·242
Mean value of M from the separate observations					46·586

Whether the close agreement between the results deduced from the formulas (1) and (5), and the several observations which I have detailed, or the small differences in the constants deduced from the separate observations in each of the foregoing cases, be considered as a test of the correctness of these formulas, there can, I think, be no doubt that they will very accurately represent the effects that are produced, whether by the rotation of magnets on a copper ring, or on magnets by the rotation of a ring, within certain limits of the distance between the magnets and the ring.

Having succeeded in determining from experiment the law according to which the forces acting during rotation varied at different distances, for which purpose principally I had undertaken these experiments, my next object was to ascertain whether the formula I had obtained would result from the principle, that time is necessary both for the developement of magnetism by induction, and for its dissipation, and which appeared to be implied in the formula.

We have as yet no facts which indicate on what function of the time either the developement or dissipation of magnetism depends; and until this can be ascertained, there appears little prospect of obtaining a complete solution to this interesting but intricate problem in magnetism. It appeared to me not improbable, that when the distance of a magnetic pole from a body capable of becoming magnetic by induction, is suddenly decreased, the magnetism developed in any very small portion of time, varies as the time and the magnetism yet remaining to be developed, before it has attained the maximum intensity of which it is susceptible at this diminished distance, and which may be called the intensity due to this distance; and that on the contrary, on the removal of the pole, the magnetism dissipated varies as the time and the excess of the intensity above the minimum at the increased distance; the rate of developement and dissipation not being however, *cæteris paribus*, necessarily the same. I accordingly assumed this, in order to obtain the intensity of the magnetism in any point of the ring in terms of its distance from the pole of the magnet; but although the differential equation that would result from this assumption is integrable, so that the intensity of the point may be determined, yet as it is in a series whose convergency depends upon the smallness of the angular distance between that point and the point vertically over the pole of the magnet, and a second integration is required to deduce the force with which the magnets urge the ring, the result is rendered so complicated, that to reduce it to any form with which the formula I have obtained from experiment may be compared, appears almost a hopeless task. I consider therefore that, in order to obtain theoretical results, it is better to adopt in a more general way the principle to which I have referred.

According to this principle, the magnetism in the ring will not be developed with the greatest intensity in the point which is *vertical* to the pole of the magnet at any instant, but in a point at a certain distance *behind* it; and in every point in *advance* of this, and likewise in those *behind*, magnetism will be developed with an intensity depending upon their distances from the pole of the magnet. In the points *behind*, the intensity will be *greater* than, what I have termed, the intensity due to the distance of the magnet; in those in *advance*, *less:* but the same distribution of magnetism in the ring will constantly follow the magnet, during its rotation under it with the same uniform velocity. The effect of the magnetism in the points *behind* the pole of the magnet will be, to urge the ring with a constant force in the *direction of rotation;* that of the magnetism in those in *advance*, to urge it in a *contrary direction*, likewise with a constant force: so that the ring will be urged in the *direction of rotation* with a force which is equivalent to the *excess* of the *former force* above the *latter*. We may therefore assume that the *whole* force of the parts of the ring in *advance* of the magnet will be destroyed by the force of only a *portion* of the part *behind;* and that *the ring is continually urged in the direction of rotation by the undiminished force of the remaining portion of the part behind:* so that to obtain the force of the *whole*, we shall have to integrate from one extremity of *this portion* to the other.

Let r be the radius of the circle described by the poles of the magnets, or the radius of that circle in the ring vertically over them, and on which I suppose the whole action to take place; s the arc of the latter circle between the point which is vertical to the pole of one of the magnets and the portion $d s$, whose force is to be determined. Referring the induced magnetism developed in every magnetic particle in $d s$, to two poles, the force with which these poles act upon the pole of the magnet, and consequently with which this pole acts upon them, will vary directly as the intensity of the magnetism developed, and inversely as the square of the distance: the *south* pole of the magnet, that which in the experiments was nearest to the ring, will *attract* the induced *north* pole, and *repel* the induced *south* pole according to this law; and the axis of polarisation will be in the direction of the line joining the centre of the particle and the pole of the magnet. If we call the intensity of the magnetism developed in each induced pole, i; the distance of each pole from the centre of the particle, k; the distance of the centre of the particle from the pole of the magnet, ρ; then the action of the magnet on the particle in the direction of the line drawn from its centre to that of the magnet, being the difference of the attractive and repulsive forces on the two poles, will be represented by

$$\mu\, i \left\{ \frac{1}{(\rho - k)^2} - \frac{1}{(\rho + k)^2} \right\}^2 ;$$

or by

$$\mu\, i \,.\, \frac{4 \rho k}{(\rho^2 - k^2)^2} :$$

or, since k is exceedingly small compared with ρ, by

$$\frac{4 \mu k i}{\rho^3},$$

μ being a constant multiplier.

As this will represent the force on each magnetic particle in $d s$, the whole force on $d s$, taking m for the constant multiplier, may be represented by

$$\frac{4 m i d s}{\rho^3};$$

or, if φ be the angular distance of ds from the point vertically over the pole of the magnet, by

$$\frac{4\,m\,i\,r\,d\varphi}{\rho^3}.$$

The force on $d\,s$, or $r\,d\varphi$, in the direction of a tangent to the ring, that which urges it in the direction of the rotation of the magnet, will therefore be represented by

$$\frac{4\,m\,i\,r^2 \sin.\varphi\,d\varphi}{\rho^4}.$$

The intensity i, is, as I have before mentioned, greater than that due to the distance ρ: let us then suppose that it is the intensity corresponding to a position of the magnet at a very small distance ω behind its position at the instant at which we are estimating the force of the magnet on ds. Considering ω as a right line, the distance between this point and ds will be

$$\left\{ c'^2 + (r \sin.\varphi - \omega)^2 + r^2 (1 - \cos.\varphi)^2 \right\}^{\frac{1}{2}},$$

or

$$\left\{ c'^2 + 2\,r^2 (1 - \cos.\varphi) - 2\,r\,\omega \sin.\varphi \right\}^{\frac{1}{2}},$$

where c' represents the vertical distance between the pole of the magnet and the middle horizontal section of the ring, or $c + p$ in the first set of experiments. We shall therefore have, $\mu_{,}$ being a constant multiplier,

$$i = \frac{\mu_{,}}{c'^2 + 2\,r^2 (1 - \cos.\varphi) - 2\,r\,\omega \sin.\varphi};$$

or, since ω is extremely small,

$$i = \frac{\mu_{,}}{c'^2 + 2\,r^2 (1 - \cos.\varphi)} + \frac{2\,\mu_{,}\,r\,\omega \sin.\varphi}{\left\{ c'^2 + 2\,r^2 (1 - \cos.\varphi) \right\}^2},$$

very nearly.

If then, according to what I have before stated, $r\psi$ represents that portion of the ring, *behind* the magnet, whose action is destroyed by the action of the part in *advance*, the *whole* force with which the magnet urges the ring in the direction of its rotation will be represented by

$$2\,M^2 r^2 . \int \left\{ \frac{\sin.\varphi}{\left\{c'^2 + 2r^2(1-\cos.\varphi)\right\}^3} + \frac{2r\omega \sin.^2\varphi}{\left\{c'^2 + 2r^2(1-\cos.\varphi\right\}^4} \right\} . d\varphi,$$

the integral being taken from $\varphi = \psi$ to $\varphi = \pi$, and $\frac{1}{2}M^2$ being put for the constant multiplier $m\mu_{,}$; or if we consider the action of two magnets diametrically opposite to each other, the force will be represented by double this integral taken from $\varphi = \psi$ to $\varphi = \frac{1}{2}\pi$. The force in the latter case will therefore be represented by

$$\left. \begin{array}{l} \dfrac{1}{\left\{c'^2 + 2r^2(1-\cos.\psi)\right\}^2} - \dfrac{1}{(c'^2+2r^2)^2} \\ + \dfrac{4r\omega}{3} . \left\{ \begin{array}{l} \dfrac{\text{Sin}.\psi}{c'^2 + 2r^2(1-\cos.\psi)} . \left[\dfrac{1}{\left\{c'^2 + 2r^2(1-\cos.\psi)\right\}^2} - \dfrac{1}{2c'^2(c'^2+4r^2)} \left\{ \dfrac{c'^2+2r^2}{c'^2+2r^2(1-\cos.\psi)} + \dfrac{c'^4+4c'^2r^2+12r^4}{c'^2(c'^2+4r^2)} \right\} \right] \\ - \dfrac{1}{c'^2+2r^2} \left\{ \dfrac{1}{(c'^2+2r^2)^2} - \dfrac{c'^4+4c'^2r^2+6r^4}{c'^4(c'^2+4r^2)^2} \right\} \\ + \dfrac{6r^2(c'^2+2r^2)}{c'^5(c'^2+4r^2)^{\frac{5}{2}}} \left\{ \text{Tan}^{-1}. \dfrac{\sqrt{(c'^2+4r^2)}}{c'} - \text{Tan}^{-1}. \left(\dfrac{\sqrt{(c'^2+4r^2)}}{c'} . \text{Tan}\,\tfrac{1}{2}\psi \right) \right\} . \end{array} \right) \end{array} \right]$$

If ψ be a small arc, ω extremely small, and c' do not exceed $\frac{1}{2}r$, the first term here will greatly exceed any of the others; and the sum of all the terms multiplied by $\frac{4r\omega}{3}$ being plus, this will diminish the second term, which is minus: so that with these limitations we may consider

$$\left\{ \frac{M}{c'^2 + 2r^2(1-\cos.\psi)} \right\}^2$$

as a very close approximation to the value of the force with which the magnets urge the ring.

In order to obtain a more precise estimate of the value of the terms omitted, let us compare this value of the force with

$$\left\{\frac{M}{c'^2 + \varepsilon^2}\right\}^2,$$

obtained from the experiments.

It appears from the experiments (Tables II. and III.) that ε^2 is very nearly ·15, and r is nearly 5·5, or r^2 nearly 30: so that putting $2r^2(1 - \cos. \psi) = \varepsilon^2$, we have $\cos. \psi = ·9975$, and $\psi = 4° 3'$. In the extreme case, in the experiments in Table I. the distance from the upper surface of the magnets to the middle horizontal section of the ring is 2·5 inches, and therefore, from the value afterwards deduced for p, $c' = 2·6$ nearly, which is less than $\frac{1}{2}r$; but supposing $c' = \frac{1}{2}r$, and taking only the first two terms, the expression for the force will be

$$\frac{16M^2}{r^4}\left\{\frac{1}{1·0404} - \frac{1}{81}\right\}.$$

Here, without even taking into consideration the diminution of the second term by those multiplied by $\frac{4r\omega}{3}$, the error arising from the omission of this term in the expression for the force, will not in this, an extreme case, amount to $\frac{1}{80}$ of the whole.

We have obtained the expression for the force with which the magnets urge the ring, on the supposition that the magnets revolve with the same uniform velocity in all cases, considering that velocity as the unit of velocity; but it is evident that for any other velocity, this expression must be multiplied by the velocity. As the value of ψ will also depend upon the velocity, this value may be so considerably increased, that the first term will no longer give an approximate value of the force. Beyond a certain velocity, the value of ψ may increase with the velocity, and we may even conceive the velocity to be so far increased, that ψ becoming $\frac{1}{2}\pi$, the expression for the force will become equal to o; and with a still farther increase of the velocity, the force will become negative, and the motion of the ring consequently retrograde. The velocity that would be required to produce this effect with copper, may be much beyond what could be produced with the requisite apparatus; but as the value of ψ, the magnets revolving with the same velocity, must vary considerably with different substances, it is by no means improbable that with steel slightly hardened, or perhaps even with hammered iron, a retrograde motion might be produced by an angular velocity more within our command. The success of this experiment would afford a very striking illustration of the principle which is the basis of the preceding calculation.

With regard to the value of ω, all that we can infer from the experiments is, that it must be extremely small. If $c' = \cdot 6$, then $\omega = \cdot 00005$ would reduce the expression for the force to its first term; and the same would be the case when $c'^2 = 7$, or $c' = 2\cdot 646$, if $\omega = \cdot 038$. Taking ω to be $\cdot 01$, the error that would arise by neglecting the second and following terms in the expression for the force, considering the first term as 1, would be $- \cdot 0147$, when $c' = \cdot 6$; and it would be $+ \cdot 0084$ when $c' = 2\cdot 646$, the first term being also considered as 1 in this case. This probably is not far from the real value of ω in the present instance, since if it were much greater than $\cdot 01$, the error that would arise from neglecting it, when c' is small, would be considerable, and would be minus; and although this is the case in the results in Tables VII. and X. it is not so in that in Table III. The errors however by which the observations would be affected from a small error in the distance, when c' is so small as ·6 or ·7 inch, are considerable; and to this source we ought perhaps to attribute the error in the last observation in Table III. being plus instead of minus, as it ought to be from the omission of the second and following terms.

It appears then, that, within the limits of the values of the distance c', at which the observations were made, ω being extremely small, and ψ about 4°, we may, without any sensible error, omit the second and following terms in the expression for the force, which is thus reduced to the form which represents very accurately the several values of α, which is the measure of that force, in the experiments.

I am aware that it may be objected to the method of investigation which I have adopted, that the value of ψ may not be constant for different values of c', nor that of ω be constant for every point of the ring, behind the portion $r\,\psi$, even for the same value of c': but the variations of these being evidently within very narrow limits, no sensible error can arise from supposing them constant: the perfect coincidence between the formula deduced on this supposition, and that previously obtained from the experiments, proves clearly that this is the case, and that the assumptions which I have made are perfectly admissible.

After the very satisfactory explanation which Mr. BABBAGE and Mr. HERSCHEL have given of the general phænomena observed during rotation, on the principle that time is requisite both for the developement and for the dissipation of magnetism,* fully to establish the truth of the principle, it remained only to show, that the results obtained from

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* Phil. Trans. for 1825.

calculation, founded on that principle, perfectly accord with those obtained from experiment; and this, I trust, I have done by the preceding experimental and theoretical details. Considering this then as an established principle, future investigations must be directed to the discovery of the function of the time on which the intensity of the induced magnetism depends, during the approach of a magnetic pole towards a physical point susceptible of magnetism, and also during its recess. I have stated what I consider to be not an improbable law; but the whole time occupied either in the developement of magnetism by induction, or in its dissipation, is so minute, that it appears extremely difficult to devise experiments that would be a direct test of such laws; and to the more indirect tests, derived from a comparison of such experimental results as the foregoing, with theoretical results derived from these laws, difficulties of analysis in general oppose themselves.

Royal Military Academy,
7th June, 1826.

IX. *Corrections to the reductions of Lieutenant* Foster's *Observations on Atmospherical Refractions at Port Bowen; with Addenda to the Table of Magnetic Intensities at the same place. By Lieutenant* Henry Foster, *R. N. F. R. S.*

Read March 22, 1827.

I hasten to acquaint the Royal Society with an error I have inadvertently committed in my observations on Atmospherical Refractions at Port Bowen, published in Part IV. of the Philosophical Transactions for 1826. The error alluded to arises from my having used an erroneous value of the divisions on the long level of the repeating circle, with which the observations were made. For the detection of this error, I am indebted to Captain Kater, who recently having had occasion to use the same instrument, found the value of each division of the level to be more than ten seconds in arc, of which he very kindly informed me, knowing that I had considered them as equal to single seconds only; and at the same time he wished me to acquaint him, if any accident had happened to the instrument, by which the level might have been changed. I knew that no accident had befallen the instrument, and that the level was the same which I had used; I therefore immediately proceeded to town, and in Captain Kater's presence ascertained the value of each division of the level to be 10″.9.

In order to explain the source of the error into which I have fallen, I must state, that on receiving the repeating circle in question, I was given to understand that the value of each division of the level was equal to one second in arc:—and as this instrument was the same that had been employed by Captain SABINE, in his experiments on the pendulum, I took no steps to examine this point for myself, for it never once occurred to me that there could exist a doubt on the subject.

The following, however, are the corrected Tables, which are similarly numbered with those they are intended to be substituted for, in pages 220 and 223 of Part IV. of the Philosophical Transactions, for 1826.

Table VII. page 220, Philosophical Transactions, Part IV. 1826.

ervations for determining the apparent altitude of Arcturus at the time of setting, by Lieutenant FOSTER's *upper* telescope.

esponding observations for refraction are contained in Tables VIII. and IX. pages 221 and 222 in Part IV. for 1826.

Time.	No. of Obs.	Mean Reading of the Four Verniers.	Corrections for		Apparent Altitude.	Barometer at Temp. + 48°.	Temp. Fah.^{t}	Winds True.	Weather.	Remarks.
			Index.	Level.						
		° ′ ″	″	′ ″	° ′ ″	Inches.	°			
At 10^{h} A. M.	16	238 34 17,5	+ 10	+3 13,47	7 35 8,69	29,540	—26	ESE L^{t}.	Fine and Clear.	
11 A. M.	12	147 31 2,5	—	+1 13,57	7 35 10,12	29,610	—29	Easterly	Hazy, with slight Snow.	
10 A. M.	12	56 28 2,5	—	+0 49,05	7 35 10,92	29,626	—35	Calm	Clear and Fine Weather.	Thin haze near the horizon.
9 A. M.	6	190 55 57,5	—	+0 54,50	7 35 11,75	29,460	—40	Calm		
to	6	325 23 57,5	—	+0 10,90	7 35 17,18					Hazy near the horizon.
1½ P. M.	6	99 52 6,25	—	+0 2,72	7 35 18,09		—36			
10 A. M.	16	238 34 10,25	0,0	+2 38,05	7 35 11,98	29,600	—31	Calm	Clear and Fine.	

Mean to be used in Table VIII. 7 35 12,68 *instead* of 7° 35′ 18″,43
Micrometrical Measure as before + 1 49,82

Altitude to be used in Table IX. = 7 37 2,50 *instead* of 7 37 8,14

principal vernier was not reset to zero after the observations on the 18th, but the readings were continued in the observations, except on March 4th, when the principal vernier was reset to zero.

Table X. page 223; Part IV. for 1826.

Observations for determining the apparent altitude of α Aquilæ at the time of setting, by Lieutenant FOSTER'S *upper* telescope

The corresponding observations for refraction are contained in Tables XI and XII; pages 224 and 225 in Part IV. for 1826.

1825. Day.	Time.	No. of Obs.	Mean readings of four Verniers.	Correction for		Apparent zenith distance.	Apparent Altitude deduced from the apparent zenith distance in the ratio of the No. of observations.	Bar. at Temp. + 48°.	Temp. Fah.	Winds true.	Remarks.
				Index.	Level.						
th			° ′ ″	°	′ ″	° ′ ″	° ′ ″	Inches.	°		
Jan. 27	at 11h A.M.	8	323 7 57,5	0,0	+ 0 10,9	85 23 31,05	4 36 22,10	29,970	— 29,5	......	Occasionally squally.
——	1½ P.M.	14	* 78 38 30	—	+ 1 29,92	85 23 43,03					
Feb. 8	11 A.M.	8	323 8 20	0,0	— 0 5,45	85 23 31,82	4 36 22,02	29,455	— 22,5	North, moderate	
—	2 P.M.	8	* 286 18 5	—	+ 0 8,17	85 23 44,15					
9	10 A.M.	8	323 8 1,25	0,0	+ 0 32,7	85 23 34,24	4 36 19,17	29,701	— 35,7	Calm, fine and clear	
—	1 P.M.	14	* 115 32 16,25	—	+ 0 24,52	85 23 45,77					
10	10½ A.M.	6	* 152 21 51,25	—	+ 0 35,42	85 23 44,44	4 36 20,43	30,100	— 35,5	Calm	
—	1½ P.M.	6	* 304 43 41,25	—	— 0 21,80	85 23 34,70					
15	11 A.M.	12	* 304 43 8,75	—	+ 0 27,25	85 23 38,0	4 36 22,00	29,600	— 33	Cloudy overcast weather.	
28	11 A.M.	6	* 152 22 1,25	—	— 0 38,15	85 23 33,85	4 36 25,19	29,992	— 21	N. E. light, clear, & fine.	
—	1½ P.M.	6	* 304 43 25	—	+ 0 10,9	85 23 35,77					

Mean to be used in Table XI....................	= 4° 36′ 21″,82,	*instead* of	4° 36′ 23″,08
Micrometrical measure between Telescopes as before	+ 3 8,42		
Altitude to be used in Table XII.................	= 4 39 30,24,	*instead* of	4 39 31,50

* The principal vernier not reset to zero, and the observations are continued from the preceding reading.

P. S. It is to be regretted, that the observations by Lieutenant Ross, given at the end of the Paper alluded to, must now be wholly rejected; in consequence of the original observations involving the erroneous datum dependent upon the level, having been left on board the Fury at the time of her loss.

The extremely low temperature of the atmosphere, in which the Observations by Capt. Parry and myself for the Amount of Refraction at Port Bowen were made, renders it important that the identical thermometer employed should be preserved; I therefore beg to present it to the Society for their acceptance: it was prepared with every possible care by Mr. Daniell, for the occasion, and is a spirit thermometer, having an ivory scale, graduated from 110° degrees below zero to 94° above, of Fahrenheit :—It has also divisions according to Reaumur's scale.

I avail myself of this opportunity of noticing an omission in the Table of Abstracted Intensities, page 125 of Part IV. of the Philosophical Transactions, for 1826; in which, the means of the times of vibration of the horizontal magnetic needle only are given. The actual intensities would exhibit the law of variation much better than the times, and as I have detected some numerical errors in the Table alluded to, it would be better to recompute the whole, and convert the times into proportional intensities by squaring the reciprocals of the times, and multiplying those squares by 10,000,000,000, in order to render the results all integral. It is now however too late for such recomputation, and all that remains is to put the reader on his guard against the errors alluded to.

HENRY FOSTER.

H. M. S. Hecla, Deptford,
March 15, 1827.

X. *Correction of an error in a Paper published in the Philosophical Transactions, entitled, " On the Parallax of the fixed Stars." By* J. F. W. Herschel, *Esq. F. R. S.*

Read February 22, 1827.

In my Paper on the parallax of the fixed stars, published in the Philosophical Transactions for 1826, by inadvertence the letter l was written for λ, in the formula for computing the most advantageous times of year for observations on the parallax of a double star. The mistake was detected, and rectified in an erratum annexed to the 4th Part of the Transactions for that year, but not till the table computed by the erroneous formula was printed off, and too late to cancel the sheet. The errors produced fall wholly on the 5th column of the table in pages 277, 278, of the volume for that year, headed, " Times of the year most proper for observation ;" which column I have therefore had recomputed, and which is here subjoined. I am happy to be able to add, that except in a few instances, the differences are not so considerable as to render useless any observations which may have been made on the plan proposed, as in the great majority of instances the correct dates do not differ by more than a week from the erroneous ones ; and about the times proper for observation, a week or a fortnight one way or the other is of no essential moment.

General Number in Messrs. H. and S. Observations.	Star's Name, &c.	Times of the year most proper for Observation.	General Number in Messrs. H. and S's. Observations.	Star's Name, &c.	Times of the year most proper for Observation.
1	35 Piscium...	June 27, December 27.	188	39 Bootis	March 22, September 24.
20	γ Arietis.....	January 18, July 21.	193	44 ——	June 25, December 25.
25	α Piscium....	January 16, July 19.	194	Struve 474 ...	January 24, July 27.
38	32 Eridani ...	February 13, August 17.	201	η Coronæ B ..	January 22, July 25.
39	ε Persei......	February 13, August 18.	205	δ Serpentis ...	February 3, August 7.
46	55 Eridani ...	February 6, August 10.	206	Libræ 178 ...	February 12, August 16.
47	ω Aurigæ	March 6, September 8.	211	II. 85	March 4, September 6.
53	Rigel	March 20, September 22.	212	III. 103	March 10, September 12.
55	118 Tauri ...	March 10, September 13.	228	g 5 Ophiuchi	February 25, August 30.
56	32 Orionis ...	March 19, September 22.	240	——————	March 1, September 2.
59	33 Orionis ...	March 22, September 24.	242	μ Draconis ...	February 8, August 12.
67	ζ Orionis	February 28, September 2.	245	39 Ophiuchi .	March 8, September 11.
366	41 Aurigæ ...	March 23, September 26.	262	100 Herculis .	March 19, September 21.
69	8 Monocer. ..	April 2, October 5.	265	I. 86	March 29, October 2.
76	38 Gemin. ...	March 30, October 3.	269	39 Draconis ..	March 22, September 24.
80	δ ——	April 6, October 9.	271	——————	April 15, October 18.
88	11 Cancri	April 18, October 21.	274	——————	April 3, October 7.
93	φ^2 ——	April 18, October 21.	280	——————	March 28, October 1.
94	18 BodeHydræ	April 29, November 1.	287	——————	April 14, October 18.
96	144 of 145 ...	April 21, October 24.	295	III. 57	April 27, October 30.
98	57 Cancri....	May 4, November 6.	306	π Aquilæ	May 10, November 12.
99	17 Hydræ ...	April 28, October 31.	311	ε Draconis ...	April 20, October 23.
102	Cancri 194 ...	April 30, November 2.	312	ψ Cygni.....	April 18, October 21.
114	Leonis 145 ...	May 24, November 25.	313	I. 96	April 20, October 23.
128	90 Leonis....	June 5, December 6.	317	II. 96	April 6, September 10.
133	65 Ursæ	May 23, November 24.	320	I. 95	May 12, November 14.
134	2 Comæ	May 28, November 29.	323	ρ Capric.	April 23, October 26.
147	118 of 145 ...	June 3, December 4.	326	——	April 27, October 30.
152	Struve 422 ...	June 27, December 27.	343	Struve 751 ...	February 14, August 18.
155	—— 424 ...	June 30, December 30.	349	Aquarii 213 ..	May 28, November 29.
161	54 Virginis ..	January 14, July 16.	352	——————	June 24, December 24.
167	81 —— ..	January 12, July 15.	354	94 Aquarii...	May 21, November 22.
173	98 of 145	January 20, July 23.	356	107 —— ...	June 4, December 6.
176	Struve 456 ...	January 27, July 30.	359	σ Capric.	July 16, January 14.
177	—— 457 ...	January 23, July 26.			

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PHILOSOPHICAL

TRANSACTIONS

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OF

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FOR THE YEAR MDCCCXXVII.

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CONTENTS.

APPENDIX.

PHILOSOPHICAL

TRANSACTIONS.

XI. *On a new form of the differential thermometer, with some of its applications.* *By* WILLIAM RITCHIE, *A. M. Rector of Tain Academy.* *Communicated by* J. F. W. HERSCHEL, *Esq. Sec. R. S.*

Read December 21, 1826.

IN using metallic reflectors for experiments on radiant heat, the results are liable to considerable uncertainty, in consequence of the imperfection of the reflectors, and of the difficulty of placing the bulb of the thermometer exactly in the focus, when the source of heat is removed to different distances. The following contrivance is free from these objections, and will illustrate the various properties of radiant heat in a more simple and accurate manner than by the more expensive and imposing method of reflectors.

The instrument consists of two cylindrical chambers of very thin brass, or tin plate, from two to six or eight inches in diameter, and from a quarter of an inch to an inch thick. These chambers, like those of the photometer formerly described, are connected by a thermometer-tube bent in the form of the letter U, having small bulbs blown near its upper extremities. The tube, like that of the differential thermometer, contains a coloured fluid, such as sulphuric acid tinged with carmine, alcohol, &c. The exterior surfaces of the cylindrical chambers are coated with lamp black, for the purpose of absorbing the radiant heat, which is rapidly conducted to the interior of the chamber, and thus causes an expansion of the inclosed air. As the results of this instrument are only comparable with each other, the scale has no reference to a fixed standard, and consequently may be divided into any convenient number of equal parts.

This instrument is well calculated for ascertaining the relative radiating powers of different surfaces; but the only application which I shall now illustrate, is that of determining the law according to which radiant heat diminishes, as the instrument is removed to different distances from the radiating source.

Experiment 1.

Place a cylindrical vessel of tin plate filled with hot water, and having the same diameter as the chambers of the instrument, at different distances from the thermometer; the results will deviate very considerably from the ratio of the squares of the distances.

Experiment 2.

Repeat the preceding experiment with a canister having a smaller diameter, and the results will approach more nearly to the squares of the distances.

Experiment 3.

Instead of the canister, I employed iron balls about two inches in diameter, and found that the effects were, (within the limits of error) as the squares of the distances of the centre of the balls from the end of the instrument.

This property is demonstrated by LAMBERT to hold with regard to light. *Photometria*, page 56. Theor. VI.

Experiment 4.

Place two heated balls on one side of the instrument, and one on the other, (the whole being of the same temperature) and move the instrument till the fluid remains at zero, and the distances of the centres of the balls will be as 1 to the square root of 2.

Since the effects of radiant heat from the heated balls diminish as the squares of the distances, how are we to account for the striking deviation from this law, when the large canister was used? Professor LESLIE says " such a striking deviation from the properties of rectilineal emanations must originate somehow, either wholly, or in part, from an *imperfect reflection.*" As the same thing holds without *reflectors*, it appears that the cause assigned by Mr. LESLIE cannot be the true one.

XII. *On the structure and use of the submaxillary odoriferous gland in the genus Crocodilus.* *By* Thomas Bell, *Esq. F. L. S. G. S.* *Communicated by Sir* Everard Home, *Bart. V. P. R. S.*

Read March 1, 1827.

Few subjects connected with comparative anatomy and physiology have received less of careful and minute investigation than the structure and functions of those glands which produce *anomalous secretions.* The structure of those *general* organs, which are adapted to the functions of whole classes of animals, has, in all their modifications, been again and again examined and described; and in many instances little perhaps remains to be ascertained. But with regard to those secretions which belong to individual species, or to smaller groups, and which are formed only for the performance of a function required by their peculiar and exclusive habits, comparatively little information has hitherto been acquired. A detailed examination of each individual structure will be necessary to the establishment of any correct general views, or accurate classification of them; and every discovery, even of an isolated fact, exclusive of what individual interest it may possess, becomes of increased importance, from its possible relations to other analogous structures. In order to illustrate the truth of this remark, it is only necessary to refer to the laborious and profound investigations of Sir Everard Home, whose Papers in the Philosophical Transactions have so much extended our information on some of the most intricate subjects of comparative anatomy and physiology—the result principally of a minute examination of individual and peculiar structures. I make no apology therefore for the following short communication, to which the foregoing observations are, to a certain extent, applicable.

It has long been known, that beneath the lower jaw of the alligator and crocodile, there is situated, on each side, a gland, the office of which is to secrete an unctuous substance, having a strong musky odour: but although anatomists have not been ignorant of the existence of such a gland, our information has hitherto rested here, whilst its structure, and the apparatus connected with it, appear not to have been investigated, nor has any probable object of such a formation been suggested. In a careful examination however of this remarkable organ, which I made about two years since, I discovered a peculiarity of structure, which, as far as I have learnt, is without a parallel in the glandular system of animals, unless, as Sir Everard Home has kindly suggested, the muscle, which is described by Dr. Russell* as governing the poison gland in serpents, may be considered as in some measure analogous. The description of this structure, with which repeated subsequent dissections have made me more particularly acquainted, I now have the honour to submit to the Royal Society.

It is necessary to premise, that my observations have hitherto been confined to the common alligator of America.†

The external orifice of the gland, through which the secre-

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* Ind. Serp. vol. i. p. 90. t. 46.

† Crocodilus Lucius. Cuv:

tion passes, is situated about two thirds of the length of the lower jaw backwards from the symphysis. It consists of a longitudinal slit in the integuments, a little within the edge of the basis of the jaw. The scales with which it is immediately surrounded are much smaller than those which cover the neighbouring parts; and it is generally concealed by a duplicature of the integuments, so that the skin requires to be extended laterally before it can be brought into view. Through this opening exudes an unctuous substance of a consistence somewhat like suet, of a brownish colour, and with a powerful odour exactly resembling that of musk.

Having kept two of these animals for several months, I made occasional observations on this secretion, and found, as might have been expected, that during the warm weather, while they fed freely, and when all their functions were more perfectly developed, I could at any time procure it by slightly pressing the gland with the finger; but that in the winter, notwithstanding they were almost constantly kept in a room with a fire, and daily placed in warm water, it was greatly diminished in quantity, and its odour much less powerful.

On removing the integuments, the gland itself is seen closely attached to the skin, and lying between it and the under surface of the tongue. It consists of a simple follicle or sac, of a bluish colour, elongated and somewhat pyriform in its shape. In the alligators which I have dissected, and which have not exceeded four feet in length, the gland has been about half an inch long, and the sixth of an inch in diameter, its base being directed obliquely forwards and outwards. On cutting open the follicle, the internal surface is found to be lined with a soft, light-coloured, secreting membrane.

The most remarkable part of this apparatus remains to be described. The gland is enveloped by extremely fine delicate muscular fibres, disposed in an oblique or spiral direction, and consisting of two fasciculi, one of which passes over, and the other underneath the gland, each partially surrounding it, whilst a few fibres are spread on the cellular tissue and skin, to which the gland is thus slightly attached. At the base of the gland the fibres converge, and form a long, slender, round muscle, which after making a slight curve forwards, proceeds directly back to the *cornu* of the *os hyoïdes*, to which it is closely united. Throughout the greater part of its length it follows the course of a muscle apparently identical with the *mylo-hyoïdeus* in the mammifera, lying in contact with its anterior edge.

The use of the singular muscle I have described, appears to be twofold. It is obvious that the oblique position of the gland in a state of rest must prevent, or at least greatly impede the free passage of the secretion from the interior; but the longitudinal contraction of the muscle would tend to draw the base of the gland backwards and inwards, and having thus brought its axis in a direct line with the opening, would facilitate the exit of the musk; whilst, by the action of the fibres which surround the fundus of the gland, that organ would itself be compressed, and the secretion forced through the opening at the will of the animal.

When the *situation* of this gland is compared with that of other odoriferous glands occurring in various tribes of animals, so remarkable a discrepancy in this respect necessarily strikes us as indicative of some peculiar object to be attained by it. The secretion of powerful odours is generally confined, in other animals, to the neighbourhood of the generative organs; and we find glands of this description on each side the *cloaca*, even in the animal now under consideration. These must be considered in some cases as furnishing a defence against the attacks of enemies, and in others, as serving some office connected with the sexual function. In the present instance, however, these objects are obviously incompatible with the situation of the gland, and we must seek for some other use to which the secretion may be applied, in accordance with this peculiarity.

The predilection of many species of fish for all strongly odorous substances, is well known to every one who has observed the habits of this class of animals, and is often made subservient to the objects of the angler. From the earliest periods, in fact, at which angling was considered as a sport, and rules laid down for its successful prosecution, baits were directed to be imbued with strongly scented oils and extracts, for the purpose of rendering them more attractive. In a book printed by Wynkyn de Worde, in 1496, and which is a republication of the celebrated book of St. Albans, with the addition of "The treatyse of Fysshynge wyth an Angle," the following direction is given, amongst others, for taking pike. "Take a frosshe (frog) and put it in asa fetida, and caste it in the water wyth a corde and a corke, and ye shall not fayl of hym. And yf ye lyst to have a good sporte, thenne tye the corde to a gose fote; and ye shall se gode halynge whether the gose or the pyke shall have the better." Walton, and every subsequent writer on angling, has given receipts for the composition of pastes for the same purpose, in all of which tar, assa fœtida, camphor, oil of rhodium, musk, or some other powerful odour is the most important ingredient.

These facts strikingly coincide with the mode in which the alligators take their prey, as far as the observations I had an opportunity of making on those in my possession enable me to judge. This is done by suddenly snapping at it sideways, a mode perfectly consistent with the form of the mouth and the position of the eyes, and not less so with the situation of the gland itself, the secretion from which, would attract the fish directly to that part where they would become the most certain and easy prey. From all these circumstances then I am led to conclude, that the use of this secretion is to serve as an attractive bait for fish, which form the principal food of these animals. Sir EVERARD HOME, indeed, mentions a curious circumstance, which came under his own observation whilst resident in the island of Jamaica, and which led him to believe that their favourite food consists of birds. Although, however, it is certain that they will devour any animals that come within their reach, yet their aquatic habits seem to indicate that fish constitute their principal nourishment, whilst the observations of many writers, and especially those of BARTRAM,* abundantly confirm this opinion.

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* Travels in the Carolinas and Georgia.

EXPLANATION OF PLATE XI.

Fig. 1. A view of the under part of the lower jaw and throat of the alligator, showing the openings of the musk glands (*a. a.*)

Fig. 2. The parts dissected.

a. The musk gland.

b. The muscle of the gland.

c. Its insertion into the *cornu* of the *os hyoïdes.*

d. The *mylo hyoïdeus.*

Fig. 2.

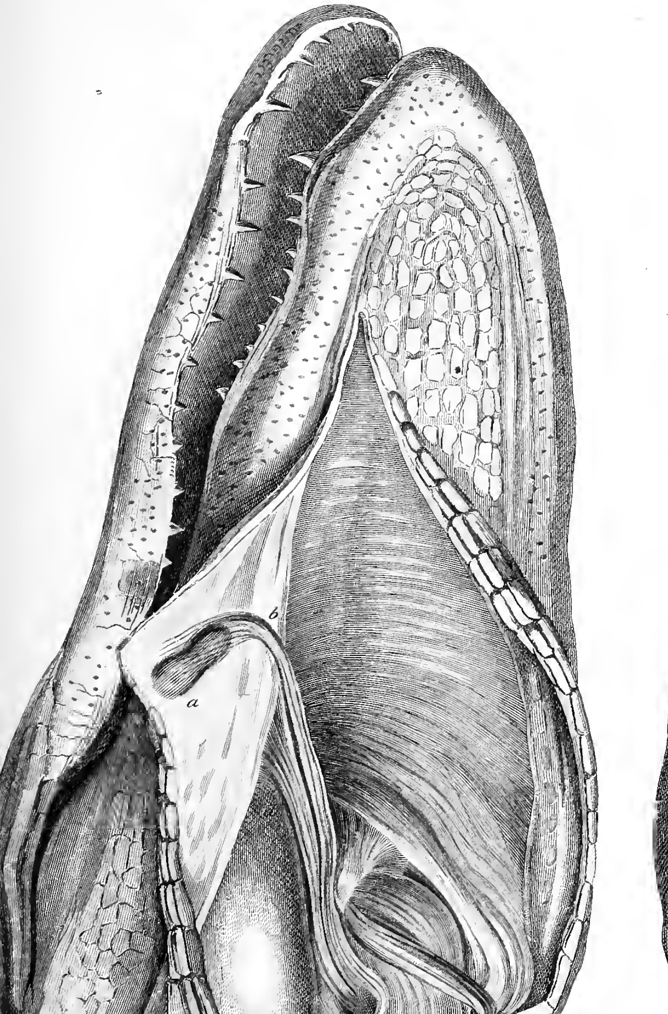
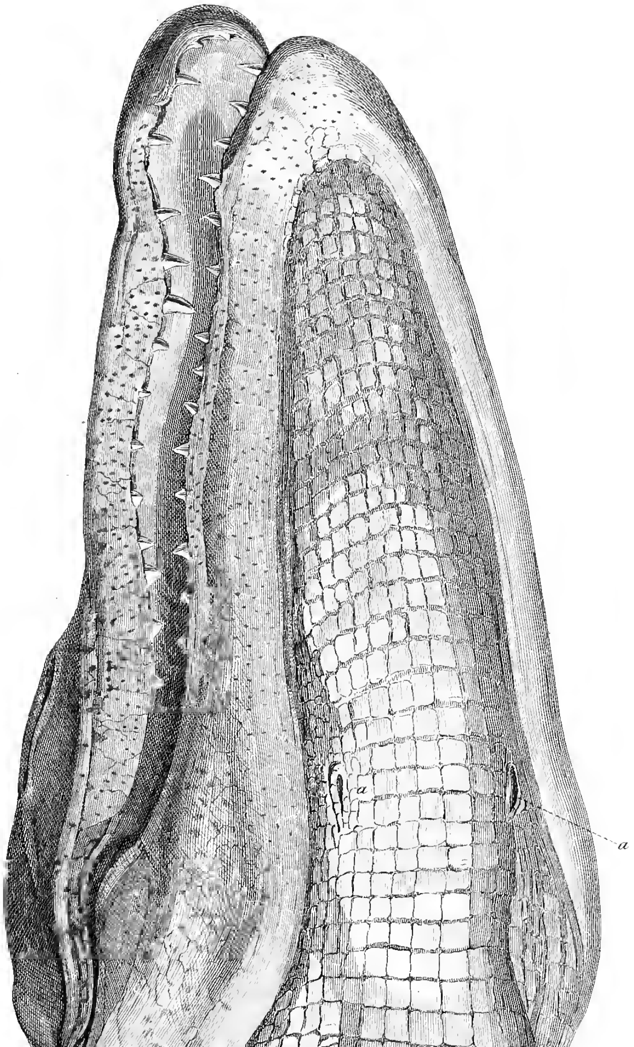


Fig. 1.



del.

[illegible]

XIII. *On the permeability of transparent screens of extreme tenuity by radiant heat.* *By* WILLIAM RITCHIE, *A. M. Rector of Tain Academy.* *Communicated by* J. F. W. HERSCHEL, *Esq. Sec. R. S.*

Read March 8, 1827.

Proposition.

INVISIBLE radiant heat, from a source at an elevated temperature, freely permeates thin transparent substances in the same manner as light.

Professor PREVOST, of Geneva, seems to have been the first person who endeavoured to establish this property of radiant heat, which was afterwards more fully investigated by M. DELAROCHE ;* but though the truth of the experiments have in general been admitted, the conclusions which these ingenious philosophers naturally drew from their experiments, have lately been called in question by several eminent experimentalists in Great Britain. It seems, therefore, that new experiments and observations are still wanting to place the fact beyond the power of controversy : the following appear to me quite sufficient for this purpose.

Experiment 1.

Let a large glass globe be blown so thin as to be almost iridescent. Fix a small portion of this globe opposite a circular hole about an inch in diameter, made in a sheet of

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* Journal de Physique, tome 72 et 75.

tin-plate. This may easily be done by applying electric cement to the circumference of the aperture, and then laying the film of glass on it when properly melted. Let a delicate air thermometer be placed opposite the disc of glass on one side of the plate, and a heated iron ball opposite to the bulb on the other. Let a current of cold air be made to play constantly against the disc of glass, which will keep it uniformly *below* the temperature of the ambient air. Things being thus arranged, the following facts will be observed.

1st. When the temperature of the ball is low, no sensible effect is produced on the thermometer.

2nd. When the temperature of the ball is high, though still invisible in the dark, the effect on the instrument is very considerable, even if the ball should be placed at a greater distance than formerly.

Here we have two sources of heat, which, on account of the change of distance, would produce equal effects in the naked bulb of the thermometer; but, by the intervention of a cold screen, the effect of the former is almost annihilated, whereas the effect of the other is still very considerable. This difference cannot possibly result from the difference of temperature in the screen, which is kept as near as possible at the same temperature by the influence of the current of cold air. We are therefore unavoidably led to the following conclusion; viz. That the progress of the heat was, in the first experiment, arrested by the screen; whereas in the other, a portion of it freely radiated through the screen, and found its way directly to the bulb of the thermometer.

Experiment 2.

Let two air thermometers be procured, with bulbs blown as thin as possible. Let the interior hemisphere of one of them be coated with a fine opaque coating of pounded charcoal. Place the bulbs of the thermometers at the same distance from a heated ball at the temperature of about 200 degrees. Divide the space through which the fluid descends in each into the same number of equal parts. Raise the ball to the temperature at which it has just ceased to be visible in the dark, place it a greater distance from the two thermometers, and it will be found that the liquid will have sunk farther in the thermometer having the *coated* bulb, than in the other.

This experiment evidently leads to the same conclusion as the preceding; viz. when the temperature of the ball was low, the whole current of radiant heat was arrested by the external hemispheres of the bulbs of the thermometers; but when the temperature of the ball was elevated, a portion of the radiant heat freely permeated the *transparent* bulb, which portion was arrested by the opaque coating in the other, and gave rise to an increase of temperature in the included air.

Experiment 3.

Procure a frame of a moderate size; stretch across it a number of very fine threads of glass, or of fine wire, parallel to each other. At right angles to these, and at the same distance, stretch other threads of glass or wire, so as to divide the whole frame into a great number of small squares. Brush the whole over with a very broad camel-hair pencil, dipped in the white of an egg, and a very delicate transparent liquid screen will be formed. Place the screen between the differential thermometer with cylindrical chambers, (a description of which has already been laid before the Society*) and the heated body, and the following facts will be observed.

1st. Raise the ball to a low temperature, keep the screen almost at the same temperature, by constantly applying the white of an egg mixed with cold water, to the upper side of the frame, and no sensible effect will be observed on the instrument.

2nd. Raise the ball to the temperature at which it has just ceased to be visible in the dark; place it at a greater distance, and a very striking effect will immediately be observed.

This experiment clearly proves that radiant heat freely permeates a very thin transparent liquid screen. I also find that heat begins to radiate through this screen when the ball is at a lower temperature than what is necessary to make it radiate through a screen of glass, or in other words, a liquid screen is more permeable by radiant heat than a solid one.

Experiment 4.

Place the screen at different distances from the heated ball, and very little difference will be observed in the descent of the fluid. In one experiment, the effect was 18 degrees with the screen close to the instrument, and the fluid rose only one degree when the screen was removed five inches towards the heated ball.

Professor Leslie has demonstrated that when the heat is absorbed by the screen, and radiated from its posterior sur-

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[*] Phil. Trans. 1827, p. 129.

face, the effect diminishes *rapidly* with every remove of the screen from the heated body.* This fact which he has established both by experiment and reasoning, is an infallible test for the opacity or transparency of all kinds of screens. Hence in the preceding experiment, the effect was not produced by heat radiating from the posterior surface of the screen, but by heat actually radiating *through* the screen in the same manner as light radiates through water, or other transparent fluids.

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* Leslie's Inquiry, page 74.

XIV. *On the derangements of certain transit instruments by the effects of temperature.* By Robert Woodhouse, *A. M. F. R. S. &c.*

Read April 26, 1827.

In the Philosophical Transactions for 1825, Part II. p. 418, I gave an account of the transit instrument belonging to the Observatory at Cambridge. Amongst other circumstances, I mentioned one of a derangement of the telescope arising from an unequal expansion of its braces. I established, as I thought, by direct experiments, the existence of such a derangement, and pointed out its cause. In a subsequent volume of the Transactions (1826, Part II. p. 75), I gave an instance of the quantity of such derangement caused by the sun's rays falling on the upper western brace. That circumstance caused a retardation of 20 seconds in the passage of Polaris at its lower culmination.

The removal of the braces, which, after the above experiments I judged to be a necessary measure, has, from one cause or another, been delayed till this time. I have however derived some good from my procrastination, since it has enabled me to institute the experiments which I am now about to detail.

These experiments have been made in consequence of some observations lately presented to the Royal Society, a copy of which Mr. South, their author, has been kind enough to send me. The results which Mr. South has drawn from these observations, in what regards the effect of the braces of his transit instrument, are very different from mine. They differ not in degree, but altogether. According to Mr. SOUTH, his transit instrument is alike steady and faithful to its duties, whether it be in shade or be exposed to the noon day sun. His instrument, in some respects, that is in the putting together of the tubes composing the telescope and axis, is different from mine; but it is furnished with similar braces; and that these should in no wise derange the instrument to which they are fixed, whilst similar ones, under certain circumstances, so greatly deranged mine, was a fact that considerably surprised me. Amongst other considerations a natural suggestion arose to my mind, whether I had not, from want of sufficient experience, or from negligence, committed some great mistake. To resolve this doubt I instituted the following experiments.

By a great number of previous observations the polar intervals of the wires of my transit (the perforated axis being towards the west) are

Upper culmination. m. s.	Lower culmination. m. s.
10 39.4	10 39.2
36.2	32.5
28.7	38.2
38.2	28.7
32.5	36.2
39.2	39.4

Hence the numbers to be added to the observed transits at the 1st, 2d, 3d, &c. wires, in order to compute the transits at the succeeding wires, will be represented by the two following tables.

Table I. Upper culmination of Polaris.

Nos. to be added to the transit at the 1st wire.	2d wire.	3d wire.	4th wire.	5th wire.	6th wire.
m. s.	m. s.	m. s.	m. s.	m. s.	m. s.
10 39.4	10 36.2	10 28.7	10 38.2	10 32.5	10 39.2
21 15.6	21 4.9	21 6.9	21 10.7	21 11.7	
31 44.3	31 43.1	31 39.4	31 49.9		
42 22.5	42 15.6	42 18.6			
52 55.	52 54.8				
63 34.2					

Table II. Lower culmination.

m. s.	m. s.	m. s.	m. s.	m. s.	m. s.
10 39.2	10 32.5	10 38.2	10 28.7	10 36.2	10 39.4
21 11.7	21 10.7	21 6.9	21 4.9	21 15.6	
31 49.9	31 39.4	31 43.1	31 44.3		
42 18.6	42 15.6	42 22.5			
52 54.8	52 55				
63 34.2					

Oct. 16, 1826. Lower culmination.

1	2	3	4	5	6	
m. s. 26 49	m. s. 37 29	m. s. 48 4	m. s. 58 43.5		m. s. 19 27	Polaris S. P.

After the star's passage over the middle wire, a warm blanket was applied to the upper eastern and lower western brace: light intervening clouds prevented the observation of the star's passage over the 5th wire, and rendered uncertain (not more, however, than to the extent of 4 seconds) that at the 6th wire.

		Computed transit at 6th wire.
From transit at 1st wire	$26^{m}\ 49^{s} + 52^{m}\ 54^{s}.8$	$1^{h}\ 19^{m}\ 43^{s}.8$
2nd	37 29 + 42 15 .6	44 .6
3d	48 4 + 31 43 .1	47 .1
4th	58 43.5 + 21 4 .9	48 .4
Computed mean transit		1 19 45 .97
Observed transit		1 19 27
Excess of computed above observed transit		18 .97

This acceleration of $18^{s}.97$ in the passage of Polaris indicates, since the culmination was an inferior one, a deviation of the telescope to the west, which the expansions of the upper eastern and lower western brace are both disposed to produce.

October 17. Upper culmination.

1	2	3	4	5	
m. s. 27 6	m. s. 37 45	m. s. 48 23	m. s. 58 52	m. s. 9 47	Polaris.

After the star had passed the middle wire a warmed blanket was applied to the same braces as before.

		Computed transit at 5th
From transit at 1st	$27^{m}\ 6^{s} + 42^{m}\ 22^{s}.5$	$1^{h}\ 9^{m}\ 28^{s}.5$
2nd	37 45 + 31 43 .1	28 .1
3d	48 23 + 21 6 .9	29 .9
4th	58 52 + 10 38 .2	30 .2
Computed mean transit at 5th wire		1 9 29 .173
Observed transit		1 9 47
		17 .827

$17^{s}.827$ is then the *retardation* of the passage of the star, indicating, since the culmination was a superior one, a deviation of the telescope to the west, as in the preceding experiment.

October 18. Lower culmination.

1	2	3	4	5	6	7	
m. s. 26 48		m. s. 48 6		m. s. 9 46	m. s. 20 15	m. s. 30 52	Polaris S. P.

Clouds prevented the observations of the star's passages over the 2d and 4th wires; but, after the *time* of the passage over the 4th wire, a warmed blanket was applied to the upper western and lower eastern brace, and suffered to remain on, till the star had passed the last wire.

		Computed transits at		
		5th	6th	7th
From transit at 1st	$26^m 48^s + \begin{cases} 42^m\ 18^s.6 \\ 52\ \ 54.8 \\ 63\ \ 34.2 \end{cases}$	$6^s.6$	42.8	22.2
at 3rd	$48\ \ 6 + \begin{cases} 21\ \ 6.9 \\ 31\ \ 43.1 \\ 42\ \ 22.5 \end{cases}$	12.9	4 1	28.5
	Computed mean transits	9.5	45.95	25.35
	Observed - -	46	15	52
	Excesses of observed above computed	36.5	29.05	26.65

which excesses indicate a deviation of the transit telescope to the east.

October 21. Upper culmination.

1	2	3	4	5	6	7	
m. s. 26 58	m. s. 37 39	m. s. 48 18		m. s. 9 31	m. s. 20 3	m. s. 30 47	Polaris.

After the *time* of the star's passage over the 4th wire (clouds preventing the observation of the passage) a warmed blanket was applied to the braces on the western side.

		Computed transits at		
		5th	6th	7th
From transit at 1st 26^m 58^s +	42^{m}22^s.5 52 55 63 34	20^s.5	53^s	32^s
at 2d 37 39 +	31 43 ·1 42 15 .6 52 54 .8	22 .1	54 .6	33 .8
at 3d 48 18 +	21 6 .9 31 39 .4 42 18 .6	24 ·9	57 ·4	36 .6
Mean computed passages		9^{m}22 ·5	19^m 55^s	30^{m}34.13
Observed - - -		9 31	20 3	30 47
Excesses of observed above computed - -		8 .5	8	12 .87

These excesses indicate a deviation of the instrument to the west; but their quantities show that the deviation arose from the *difference* of the effects produced by the expansions, since it is only one third of the deviation produced by applying the blanket diagonally to the braces.

It is not at all essential to the explanation I have given of the cause of the instrument's deviation, that the braces on the same side, even if equally heated, should exactly counteract each other. The effect of counteraction, under such circumstances would, probably, vary with the individual instrument. It might be greater in one than in another of similar construction; but, in the above experiment, it is not at all likely that the braces were equally heated, since, probably, they were not enveloped either with equal portions of the blanket, or portions equally heated.

Oct. 23. Upper culmination.

1	2	3	4	5	6	7	
m. s. 27 1	m. s. 37 43	m. s. 48 12	m. s. 58 46	m. s. 9 28	m. s. 20 19	m. s. 31 1	Polaris.

After the star had passed the 1st and 2d wire, fearing, from the appearance of the sky, that clouds might prevent me from observing the star on the middle wire, I applied a blanket (from want of sufficient preparation only moderately warmed) for about 9 minutes, to the upper western brace. After the star's passage over the fifth wire, the blanket, more heated than before, was applied to the lower western brace, and kept on till the star had passed all the wires.

		Computed passage at 3d wire.
From passage at 1st	$27^m\ 1^s + 21^m\ 15^s.6$ - - -	$48^m\ 16^s.6$
at 2d	$37\ \ 43 + 10\ \ 36.2$ - - -	48 19.2
	Mean computed passage -	48 17.9
	Observed - - - - -	48 12
	Excess - - - - - - -	5.9

The heating then of the upper western brace caused the instrument, or part of it, to deviate to the east, a result which agrees with all preceding ones. Next, if we suppose the instrument, after the lapse of 20 minutes, to have returned to its usual state, and assume $58^m\ 46^s$, and $9^m\ 28^s$ to be nearly the passages at the 4th and 5th wire, we have

	Computed passages at	
	6th	7th
$58^m\ 46^s + \begin{Bmatrix} 21^m\ 10^s.7 \\ 31\ \ 49.9 \end{Bmatrix}$ - - -	$19^m\ 56^s.7$	$30^m\ 35^s.9$
$9\ \ 28 + \begin{Bmatrix} 10\ \ 32.5 \\ 21\ \ 11.7 \end{Bmatrix}$ - - -	20 0.5	30 39.7
Mean computed passages - - -	19 58.6	30 37.35
Observed - - - - - - - - -	20 19	31 1
Excesses of observed above computed	20.4	23.65

The effect, therefore, of the expansion of the lower western brace was a retardation of the star, in its upper culmination, to the amount of about 20^s, a result which accords both with the explanation given of the cause of the instrument's deviation, and with former results.

The above experiments were not conducted, as I have already said, with much attention to nicety, which would have been an useless quality, seeing that the object of the experiments was not to enable me to modify the position or size of the braces, but to justify the measure of entirely discarding them as fallacious auxiliaries of the instrument. With the view, however, of communicating equal accessions of heat rather more accurately than by the preceding imperfect mode of applying a heated blanket, I held, in some of the following experiments, for the same time, and nearly about the same parts, the braces of the instrument, one in each hand.

October 24th. Upper culmination.

2	3	4	5	6	7	
m. s. 37 37	m. s. 48 19	m. s. 58 48	m. s. 9 29	m. s. 20 5	m. s. 30 56	Polaris.

After the star had passed the middle wire, I held, for about 9 minutes, the two western braces, one in each hand.

			Computed transits at	
			5th	6th
From passage at 2d wire	37^m 37^s +	31^m $43^s.1$ 42 15 .6	9^m $20^s.1$	19^m $52^s.7$
at 3d	48 19 +	21 6 .9 31 39 .4	9 25 .9	19 58 .4
at 4th	58 48 +	10 38 .7 21 10 .7	9 26 .7	19 58 .7
Mean computed	-	-	9 24 .23	19 56 .6
But observed	-	-	9 29	20 5
Excesses	-	-	4 .77	8 .4

In this case the deviation of the transit was to the west, and if we suppose equal communications of heat to have been given, by the above mode, to the two braces, it will follow, on the grounds of my explanation, that the lower western brace has more effect in deranging the instrument than the upper.

But that in the above experiment the two braces counteracted each other, will be understood from the latter part of the observation. After the star had passed the 6th wire, I held, in one hand, for about 9 minutes, the lower western brace.

	Computed passage at 7th wire.
Now 58m 48s + 31m 49s.9 - -	30m 37s.9
Observed passage - - - -	30 56
Excess of observed above computed	18 .9

In this case the effect produced by warming one brace was three times that produced by warming the two braces on the same side.

Oct. 25. Upper culmination.

4	5	6	7	
m. s. 58 48	m. s. 9 16	m. s. 19 58	m. s. 30 56	Polaris.

After the star had passed the middle wire, the lower eastern brace was held in my hand nearly 9 minutes; and after the star had passed the 6th wire the upper eastern brace was held for about the same time.

			Computed passage at 5th.
1st	$58^{m} 48^{s} + 10^{m} 38^{s}.2$	- - -	$9^{m} 26^{s}.2$
	Observed passage	- - -	9 16
	Deviation to the east	- -	10 .2
Next,			at 7th.
	$19^{m} 58^{s} + 10^{m} 39^{s}.2$	- - -	$30^{m} 37^{s}.2$
	Observed passage	- - -	30 56
	Deviation to the west	- - -	18 .8

Oct. 25. Lower culmination.

m. s.	m. s.	m. s.	m. s.	
58 40	9 8	19 48	30 27	P S. P.

After the star had passed the middle wire, the shutters to the south were unclosed, and the sun was suffered to shine on the lower western brace alone, which it began to do, but feebly and interruptedly,* after the star had passed the 5th wire.

	Computed transits at	
	6th	7th
$58^{m} 40^{s} + \begin{Bmatrix} 21^{m}\ 4^{s}.9 \\ 31\ 44.3 \end{Bmatrix}$ - -	$19^{m} 44^{s}.9$	$30^{m} 24^{s}.3$
$9\ 8 + \begin{Bmatrix} 10\ 36.2 \\ 21\ 15.6 \end{Bmatrix}$ - -	44 .2	23 .6
	19 44 .55	30 23 .95
Observed - - - -	48	27
Deviation to the west - -	3 .45	3 .05

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* During the observations of Polaris, the sky at the upper culminations, was generally clear; at the lower, almost always cloudy to the south, so that I was unable to expose, which I was desirous of doing, first, one brace, and then two braces *on the same side* to the sun's rays. It is not now worth the while to keep the braces on the instrument another half year for the chance of trying this experiment, especially since an English sky may again frustrate my plans.

October 26. Upper culmination.

4th	5th	
m. s. 58 48	m. s. 9 44	Polaris.

After the star had passed the middle wire I held, for about 9 minutes, the upper eastern and lower western brace, one in each hand.

Now,		
	$58^m\ 48^s + 10^m\ 38^s.2$ - -	$9^m\ 26^s.2$
	Observed passage - -	9 44
	Deviation to the west -	17 .8

October 28th. Upper culmination.

4th	5th	
m. s. 58 47	m. s. 9 29	Polaris.

After the star had passed the middle wire, I held, for about 9 minutes, the 2 lower braces, one in each hand.

	Computed transit at 5th.
$58^m\ 47^s + 10^m\ 38^s.2$ - - -	$9^m\ 25^s.2$
Observed - - - -	29
Deviation to the west - -	3 .8

In this case, as in those of pp. 149, 152, the expansions of the braces counteracted, and nearly balanced, each other, since the deviation does not much exceed the error of observation.

I think I may presume to say, that the foregoing experiments incontrovertibly show that the partial heating of the diagonal braces, or the partial heating of any one of the braces, deranges my transit instrument; the derangement taking place according to the explanation I gave of it in the Transactions for 1825 and 1826. According also to the principle of that explanation, and the preceding experiments, it follows, that the partial heating of the braces on the same side, or the partial heating of the upper or lower braces, produce counteracting effects, and may, in certain instruments, under certain degrees of temperature, produce balancing effects, as far as the time of the star's passage is concerned. This may be the case with Mr. SOUTH's instrument, and if so, we should at once have an explanation of its seeming inflexibility when exposed to the sun. I hope I am altogether within the bounds of courtesy and fair criticism, when I remark, that I see nothing in Mr. SOUTH's experiments that force me to the conclusion of his instrument being exempt from those infirmities with which mine is afflicted.

In these experiments of Mr. SOUTH's on Polaris, the braces on the same side, whether the eastern or western, are of the same temperature. The retardation, or acceleration, therefore of the star's passage, arises from the difference of the changes produced by temperature in the upper and lower parts of the tube of the telescope; which difference, estimating it by its effect on the time of the star's passage, may be very small, or not greater than the error of observation, or, (for there is nothing improbable in the supposition) insensible.

But it may be said, is not this the bending of facts to suit an hypothesis, instead of adopting the more natural supposition, that the steadiness of the instrument arises from the excellence of its construction. The construction, indeed, of Mr. SOUTH's instrument, in what regards the putting together of the tubes composing the telescope and axis, essentially differs from mine: but it cannot be *that* circumstance that confers inflexibility on Mr. SOUTH's transit instrument; for, were it so, the Greenwich instrument, which is similarly constructed, and by the same excellent artist, ought to possess the same rigid character. This, however, is not the case. Soon after the publication of my first Paper, the Assistants at Greenwich held in their hands the diagonal braces, and, by examining the meridional mark, detected a considerable deviation in the telescope. But in order to be more sure of the nature of the result arising from the partial heating of the braces, I requested the Astronomer Royal to try, with the Greenwich transit, the first experiment mentioned in this Paper. Mr. POND immediately, with his usual kindness, complied with my request, and the following are the details of the experiment.

Experiments on the braces of the transit at the Royal Observatory (Oct. 25, 1826.)

With warm flannel applied to one brace, the interval of the Pole star passing from centre to 4th wire - -	$9^{m}\ 41^{s}.5$
The true interval by observation - - -	10 49.7
Star accelerated - - - - - - -	1 8.2
With warm flannel applied to two opposite braces, from the 4th to 5th wire - - - - - -	$9^{m}\ 15^{s}.0$
The true time by observation - - -	10 47.5
Acceleration - - - - - - - - -	1 32.5
Total acceleration from the centre to the 5th wire - -	2 40.7

(Signed) THOMAS TAYLOR.

The peculiarity of construction then, which the excellent artist who made the Greenwich transit, has used in joining together the tubes composing the telescope and axis, does not prevent the braces from deranging the instrument; nor was it likely it should: it might render, under the same circumstances, the derangement less, or it might render it greater; for this is a point which I think should be determined solely by experiment. The experiment which I have just quoted, apparently renders the derangement greater; but this is a kind of result on which I lay no stress, since I have no grounds for not knowing that the heat applied at Greenwich was not three or four times what I applied.

From my own experiments, and from the testimony which I have just adduced from Greenwich, I find it, then, extremely difficult to believe that Mr. South's transit instrument should not obey the influence of expanding braces. I have stated some grounds for suspecting that it is not endowed with that inflexibility which it appears to assume in some of the observations. Not one of these, in my opinion, bears directly upon the point in question. A single observation of Polaris in October, with one brace alone of the instrument exposed to the sun, would have been a better test of its steadiness than all that are recorded. I invite that indefatigable and ardent observer to the trial of this test.

I am unwilling, and indeed not quite prepared, to enter on the difficult subject of the sun's meridional observations. The errors of the clock, or the difference of the sun's observed transits, and of his computed right ascensions, do not agree, as it is known, with the errors found by the transits of the stars. The former errors are always less. In this general result, the observations I have made agree with the Greenwich and Dublin observations, and with Mr. South's. The mean of the differences of the errors of the clock, as estimated by the sun and stars, is about six tenths of a second. And this can be partly explained from the fact, that the mean right ascensions of the stars having been increased in late catalogues by Mr. POND, by about $0^s.3$, are greater, by that quantity, than in the catalogues from which the solar tables are computed. But the differences are very variable; they are greater in summer than in winter. It is far from my intention to be cautiously aiming at a merely safe opinion, when I state that, probably, the inequalities of these differences arise partly from the errors of the solar tables, and partly from instrumental derangement. Certain I am that such a derangement, in degree at least, cannot fail to have taken place with the transit instrument of this observatory when exposed to the sun, and also with the Greenwich transit. A derangement of the instrument may also take place if a stream of hot, or cold air, should blow partially on one brace.

It is incumbent on me then, at least, to make the experiment of removing the braces from the Cambridge instrument; whether or not the good that will thence result will be more than balanced by the evil of depriving the instrument of its props or supports in certain positions, is a point which I hope soon to ascertain.*

* It is proper to state, that the fixing of braces to the Cambridge instrument, was contrary to the opinion of Mr. DOLLOND. They were fixed in consequence of instructions communicated to him through me (then, with my concurrence) " to make the transit instrument, in all respects, like the Greenwich one." It is now necessary to amend what was done by reason of these instructions, and, probably, (for I am anxious to render the instrument perfect by finding out its defects) we shall soon have other alterations to make: but if we can change for the better, I am sure of the zealous co-operation of Mr. DOLLOND, who has always been, and continues to be, extremely solicitous to render the instrument as perfect as possible.

XV. *On some of the compounds of chromium.* *By* Thomas Thomson, *M. D. F. R. S. L. and E. Professor of Chemistry, Glasgow.*

Read March 29, 1827.

The principal object of this Paper is to give an account of a singular combination of chromic acid, and chlorine, which I discovered about a year and a half ago. But as the investigation of this compound led me to a more careful examination of the oxides of chromium, and a more accurate knowledge of their composition than had been hitherto attained, I shall also state the facts which I have ascertained with respect to these bodies. In a Paper inserted in the Annals of Philosophy (1st Series, vol. 16, p. 321), I have shown that the atomic weight of chromic acid is 6·5. Two other compounds of chromium and oxygen being known, namely, the *green oxide*, and the *brown oxide*, I was induced, from analogy, to consider the atomic weight of the former to be 4·5, and that of the latter 5·5; and to make the atom of chromium 3·5. These views were rather favoured by some experiments on the chloride and muriate of chromium, which I have stated in my "First Principles of Chemistry," vol. ii. p. 52. But these experiments deviating more than two per cent from the theoretical number, could not be considered as decisive. How far my former notions on this subject were correct, will appear from the following statements.

I. *Chromium.*

The metallic chromium, which I employed in the following experiments, had been reduced by Mr. COOPER, of London, who possesses great skill in the management of furnaces. He was kind enough to give me a specimen of this rare metal several years ago; and I take this opportunity of thanking him for a present, which has been of considerable service in my investigations.

Pure metallic chromium is white, with a shade of yellow: it is very brittle, and easily reduced to a fine powder, which still retains the metallic lustre. It is not sensibly attracted by the magnet, even when in very fine powder. Magnetism then is not a property of this metal. The specimen found magnetic by RICHTER, must of course have contained iron. The specific gravity of chromium I found to be 5·093; but the specimen was not quite free from cavities.

Four grains of this metal, previously reduced to an impalpable powder, were boiled for an hour in nitric acid, without perceptible solution. The process was repeated with aqua regia instead of nitric acid, and the flask was left for two days on the sand bath. It was allowed to remain on the cold sand bath from the 3d June to the 18th July, 1826, during a period of uncommonly hot weather, when the thermometer in the shade was repeatedly at 86°. The acid liquid had assumed a green tinge, but the bulk of the powder was not sensibly diminished. The undissolved portion being separated, was found to weigh 3·73 grains: so that only 0·27 grain had been dissolved.

From this experiment it appears that acids do not answer well for converting chromium into an oxide; I therefore had recourse to alkalies. 3·14 grains of metallic chromium in powder, were mixed with a sufficient quantity of hydrate of potash and nitre, in the proportion of about 5 parts of the former to one part of the latter, and kept for 20 minutes in a red heat in a silver crucible by means of a spirit lamp. The water of the hydrate gradually went off, and a reddish yellow matter remained, which was quite liquid while hot, but became solid on cooling. It dissolved completely in water, and the liquid had the usual yellow colour of a solution of chromate of potash. It was neutralized by nitric acid, and then precipitated by acetate of lead. The chromate of lead thrown down, after being washed and dried upon the filter, weighed 16·8 grains, but was reduced to 16·23 grains when exposed to a red heat over a spirit lamp.

By this process the metallic chromium had been converted into chromic acid. Now chromate of lead is composed of

1 atom chromic acid - -	6·5
1 atom protoxide of lead -	14
	20·5

From this constitution of the salt, it is easy to deduce the weight of chromic acid in 16·23 grains of chromate of lead: it amounts to 5·146 grains.

Thus it appears that 3·14 grains of chromium, when converted into chromic acid, become 5·146 grains. This gives us 3·966 for the atomic weight of chromium. For

5·146 : 3·14 : : 6·5 (atom of chromic acid) : 3·966 = atom of chromium.

My stock of metallic chromium being small, I did not choose to repeat this experiment; but it had been made with such scrupulous attention, that I am satisfied it must approach very near the truth.

As 3·966 differs by less than one per cent from 4, we can scarcely hesitate about adopting 4 as the true atomic weight of metallic chromium. Indeed 4 must be the true number, if we adopt the law of Dr. PROUT, which I have so amply confirmed by numerous examples in my "First Principles of Chemistry," namely, that the atomic weights of all bodies are multiples of 0·125, the atom of hydrogen: for 0·125 × 32 = 4.

This conclusion will be corroborated by the experiments immediately to be detailed, which leave no doubt that the atomic weight of green oxide of chromium is 5. Now, as this oxide is a compound of 1 atom oxygen + 1 atom chromium, the atom of chromium must necessarily be 4. If we allow an error in weighing to have taken place in the preceding experiment to the amount 0·02 grain, the number 4 would come out as the result of the experiment. Now, I need hardly remark to practical chemists, how very difficult it would be to guarantee any similar experiment from so small an error as $\frac{1}{50}$th of a grain.

II. *Green oxide of chromium.*

This oxide is always obtained by depriving chromic acid of a portion of its oxygen. Many processes have been followed. When chromate of potash is digested with alcohol or tartaric acid, the chromic acid is pretty speedily converted into green oxide. A solution of chromate of potash speedily lets green oxide fall when a current of sulphurous acid is passed through the solution.* The same change takes place if we boil a mixture of chromate of potash and muriatic acid together. The addition of a little alcohol greatly promotes the rapidity of this process. The method proposed by BERZELIUS is one of the most tedious and unproductive. He passes a current of sulphuretted hydrogen gas through a solution of chromate of potash, previously acidulated with muriatic acid.

In whatever way the green oxide of chromium is procured, it is always at first in the state of a hydrate. If we dry the precipitate in the open air, it is a greenish blue tasteless light substance, which may or may not contain carbonic acid, according to the re-agent employed in throwing it down. If we dry it on the filter by artificial heat, its colour becomes a good deal darker, and it retains almost exactly half its weight of water. When it is dried in the open air without artificial heat, $\frac{13}{15}$ths of its weight are water. In this state of hydrate it dissolves readily in almost all acids. A moderate heat expels the water, and leaves the oxide in the state of an exceedingly beautiful green powder, scarcely soluble in any acid whatever. When this green oxide is heated nearly to redness in an open vessel, it generally *glows*, or becomes intensely red hot, so as to resemble the appearance of burning tinder. This glowing does not always take place, though it is a pretty common phenomenon. It has not yet been determined, upon what this curious property depends.

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* To prevent misapprehension, it will be proper to state, that the oxide thrown down by sulphurous acid has a brown colour; but if we dissolve it in muriatic acid, and throw it down by ammonia, the precipitate obtained will be green oxide. It will be shown hereafter, that brown oxide differs from green oxide, merely by retaining a small quantity of chromic acid, with which the green oxide is combined.

Many methods were tried to determine the atomic weight of this oxide. As it does not form crystallizable salts with acids, and as acids have the property of combining with it in various proportions, we cannot have recourse to the salts of chromium for this purpose. But as we know that the atomic weight of chromic acid is 6·5, it will be aufficiently satisfactory if we can determine exactly the number of atoms of oxygen, which must be abstracted from chromic acid, in order to convert it into green oxide of chromium. Now, sulphuretted hydrogen gas, and protosulphate of iron, possess the property of converting chromic acid into green oxide of chromium. I shall relate, in succession, the analysis of chromic acid made by means of these two bodies.

1. A quantity of neutral chromate of potash was dissolved in water, and a current of sulphuretted hydrogen gas passed through the solution till all action was at an end. A beautiful green-coloured precipitate fell: the liquid remained deeply coloured; but upon being heated sulphuretted hydrogen gas was exhaled, an additional portion of green precipitate fell, and the liquid became colourless. It was quite free from every trace of chromic acid or oxide of chromium.

The green precipitate being examined, was found to be a compound of one atom of sulphuretted hydrogen and one atom of green oxide of chromium. It was therefore a hydrosulphuret of chromium. This hydrosulphuret is tasteless, and insoluble in water. It dissolves with facility in acids, sulphuretted hydrogen gas being given off and sulphur remaining. From the experiment it appears, that this hydrosulphuret becomes soluble in water when an additional quantity of sulphuretted hydrogen is combined with it. Probably this soluble portion was in the state of bihydrosulphuret of chromium.

The residual liquid being evaporated to dryness left a deliquescent salt, possessing the following properties. Its taste was strongly alkaline, and it acted powerfully on cudbear paper, giving it a deep purple colour, as alkalies always do. Muriate of barytes dropt into the aqueous solution of the salt threw down a white precipitate, which was re-dissolved by adding a few drops of nitric acid. Acetate of lead occasioned a white precipitate. Sulphuric acid occasioned an effervescence, sulphurous acid was driven off, and abundance of sulphur was thrown down. These properties leave no doubt that the salt was a *hyposulphite* of potash.

Thus it appears, that when a current of sulphuretted hydrogen gas is passed for a sufficient time through a solution of chromate of potash, the whole chromic acid is converted into hydrosulphuret of chromium, while the potash becomes hyposulphite of potash. Before the rationale of these decompositions, and new combinations can be given, it will be necessary to make the reader acquainted with the true composition of the *hyposulphurous acid* of Mr. Herschel, which is the kind of acid formed in the process just described.

During the summer of 1825, which was remarkably hot and dry, there were formed in the soda leys of Mr. Charles Tennant, of Glasgow, numerous octahedral crystals possessing the following properties.

The crystals seemed to be regular octahedrons, though they did not admit of accurate measurement. The taste of the salt was very hot, bitter, and sulphureous. It was not altered by twenty-four hours exposure to the air; but when kept in a phial, gradually deliquesced into a brown liquid. Its alkaline properties were as powerful as those of a strong caustic potash ley: that is to say, it rendered cudbear paper purple, and dissolved the cuticle and nails of the fingers very speedily. When heated, it underwent the watery fusion, then became a solid salt, which caught fire and glowed or burnt like tinder for a considerable time with a very low yellow-coloured flame.

This salt had been noticed by VAUQUELIN in 1802. He obtained it from the carbonate of soda manufactured by PAYER and BOURLIER, and described its characters under the name of hydrosulphuret of soda.*

I dissolved 50 grains of these crystals in water, and added to the solution muriatic acid in sufficient quantity to saturate the soda, which I knew it, from previous trials, to contain. A smell of sulphuretted hydrogen was at first given out abundantly: this was soon followed by the odour of sulphurous acid, while at the same time sulphur was deposited. The solution being filtered and evaporated to dryness, left 25·3 grains of common salt, equivalent to 13·49 grains of soda.

50 grains of the salt being heated in a retort lost 29·2 grains of their weight. The retort was blackened by the action of the sulphur on the oxide of lead in the flint glass. The liquid collected in the receiver weighed 25·4 grains. It contained sulphuretted hydrogen and a small quantity of the original salt.

To determine the quantity of sulphur in this salt, 31·25 grains of sulphate of copper (containing 10 grains of oxide of

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* Ann. de Chim. 41, 190.

copper) were dissolved in water; and it was found that when this liquid was mixed with a solution of 29·75 grains of the crystals the whole copper was thrown down, and no residual sulphur remained in the liquid; for no effect whatever was produced by adding to it a few drops of the solution of sulphate of copper. Now, when sulphate of copper and a hydrosulphuret are mixed in the atomic proportions, the precipitate consists of bisulphuret of copper, composed of equal weights of sulphur and copper. But the copper in 31·25 grains of sulphate of copper is eight grains. Consequently, 29·75 grains of the crystals contain just 8 grains of sulphur; or, which is the same thing, 14·875 grains of the crystals contain 4 grains of sulphur.

As 50 grains of the crystals gave 13·49 grains of soda, it is obvious that 14·875 grains must contain 4·013 grains, which differs by only $\frac{1}{4}$th per cent from 4.

The salt being in regular crystals, and not altered by exposure to the air, the water contained in it must be a determinate number of atoms. Now, 50 grains of the crystals gave 25·4 grains of liquid; consequently, 14·875 grains would give 7·556 grains. But this liquid contained more than half a grain of saline matter: so that the quantity of water contained in 14·875 grains of the crystals does not amount to quite so much as 7 grains. If we suppose the water of the salt to be six atoms, its weight in 14·875 grains of the salt will be 6·75 grains. It is obvious from the experiment that it is not less than this, nor can it be more; for 7 atoms of water would be 7·875, which was above the whole weight of the liquid obtained, and yet above half a grain of the weight found was owing to saline matter.

It appears from the preceding analysis, that 14·875 grains of the salt contain

2 atoms sulphur	4
1 atom soda	4
6 atoms water	6·75
	14·75

The 0·125 wanting to make up the whole weight is equivalent to an atom of hydrogen. If we consider it as combined with the two atoms sulphur, making an atom of bisulphuretted hydrogen, then the constituents of the salt will be as follows:

1 atom bisulphuretted hydrogen	4·125
1 atom soda	4
6 atoms water	6·75
	14·875

It will appear immediately that these numbers exhibit the true composition of this hydrosulphuret. It is not, as VAUQUELIN supposed, a compound of sulphuretted hydrogen and soda, but of bisulphuretted hydrogen and soda.

If we dissolve this salt in water and add to the solution sulphurous acid, as long as this acid continues to lose its smell, one half of the sulphur contained in the salt is thrown down, and the liquid will be found to contain nothing but a solution of hyposulphite of soda. If we concentrate the solution sufficiently, it shoots into large transparent crystals of hyposulphite of soda.

These crystals are flat four-sided prisms, terminated by a bihedral summit. They have very much the taste of GLAUBER salt. When sulphuric acid is poured upon this salt, or still better, into an aqueous solution of it, sulphur is thrown down, and sulphurous acid is driven off, at least if heat be applied to the liquid. A careful analysis of this salt gave its constituents as follows:

1 atom hyposulphurous acid - -	5
1 atom soda - - - -	4
4 atoms water - - - -	4·5
	13·5

It is obvious, that the hyposulphurous acid in this salt is a compound of 2 atoms sulphur and one atom oxygen, and that its atomic weight is 5. It is equally obvious, that the hydrosulphuret employed in its fabrication contains not sulphuretted hydrogen, but bisulphuretted hydrogen.

For, let us suppose a mixture to be made of 14·875 of the hydrosulphuret of soda and 4 sulphurous acid, 2 of sulphur will precipitate, and a neutral hyposulphite of soda will be formed.

14·875 hydrosulphuret contain 2 atoms sulphur and only 1 atom hydrogen. There remain in solution (as half the sulphur falls) 1 atom sulphur + 1 atom hydrogen.

The 4 sulphurous acid are composed of 1 atom sulphur + 2 atoms oxygen.

One of these atoms of oxygen in the sulphurous acid will combine with the atom of hydrogen in the hydrosulphuret, and form water; so that there remain for the constituents of the hyposulphurous acid,

2 atoms sulphur = 4
1 atom oxygen = 1
5

There cannot have been more than 1 atom of hydrogen in the hydrosulphuret. For had there been two atoms, constistituting a bihydrosulphuret of soda, the whole oxygen of the sulphurous acid would have been converted into water, and the hyposulphurous acid would have contained no oxygen at all.

Such is the composition of the hyposulphurous acid of HERSCHEL. I verified this constitution by the direct analysis of several hyposulphites; the most easily analyzed of which were the hyposulphites of barytes and of lead.*

The reader being now aware of the composition of hyposulphurous acid, will be prepared for the theoretical explanation of the decomposition of chromic acid by sulphuretted hydrogen gas.

Let us suppose a solution of 125 grains of chromate of potash, containing 65 grains (equivalent to 10 atoms) of chromic acid. To reduce this to green oxide, 5 atoms of sulphuretted hydrogen gas are requisite, containing

5 atoms sulphur,
5 atoms hydrogen.

The sulphur is converted into sulphurous acid and must combine with - - - 10 atoms oxygen,
The hydrogen unites to 5 atoms oxygen,
forming water. 15

Thus the 10 atoms chromic acid, in order to become green oxide, must part with 15 atoms of oxygen; or (which is the same thing) 1 atom of chromic acid is converted into green oxide when it is deprived of 1·5 atom oxygen. But chromic

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* There is another acid which exists, composed of 1 atom sulphur + 1 atom oxygen. Its atomic weight is 3. It may be distinguished by the name of *sub-sulphurous acid.*

acid weighs 6·5; and 6·5 — 1·5 = 5 = atomic weight of green oxide.

The 5 atoms of sulphurous acid thus formed unite with 5 atoms of sulphuretted hydrogen; and these two bodies are mutually decomposed into 5 atoms hyposulphurous acid and 5 atoms water. The 5 atoms of hyposulphurous acid uniting to the 10 atoms of potash constitute dihypo-sulphite of potash, composed of

composed of	1 atom hyposulphurous acid	5
	2 atoms potash - -	12
		17

I conceive that this beautiful example of rather a complicated series of decompositions and new combinations, leaves no doubt that the atomic weight of green oxide of chromium is 5.

When we take bichromate of potash instead of neutral chromate, and treat it with sulphuretted hydrogen gas, the precipitated hydrosulphuret has a buff colour instead of a green. This may perhaps be ascribed to a portion of undecomposed chromic acid falling in combination with some part of the precipitate. For we shall see afterwards a similar coloured precipitate composed of chromic acid and green oxide.

This buff coloured precipitate is tasteless, and insoluble in water. It is either a hydrosulphuret of chromium, or at least contains a hydrosulphuret; for when it is heated sulphur sublimes, and sulphurous acid is given off. At a certain temperature it catches fire, and burns for some time with a yellow low flame. When digested in muriatic acid some sulphur precipitates. When digested in nitro-muriatic acid, a portion of the sulphur is converted into sulphuric acid. From 7·71 grains of the buff coloured precipitate I obtained

Green oxide of chromium	-	5·02
Sulphur	- - -	0·37

It was easy to drive off water, but not to determine its quantity. The 5·02 of green oxide contained $\frac{1}{6}$th of a grain of chromic acid. If we suppose the sulphur to be combined with hydrogen, and what is wanting to make up the weight to have been water, the constituents of the buff-coloured powder will be

Green oxide - -	4·85	or	185·44	or	37	atoms.
Chromic acid - -	0·17	or	6·5	or	1	atom.
Sulphuretted hydrogen	0·39	or	14·9	or	7	atoms.
Water - -	2·30	or	87·94	or	78	atoms.
	7·71					

The whole chromic acid, owing to an accident, was not collected; but from some experiments to be related afterwards, I am disposed to view the buff-coloured powder as a compound of

5 atoms hexa-chromate of chromium*	- -	36·5
1 atom hydrosulphuret of chromium	- -	7·125
12 atoms water	- - - - -	13·5
		57·125

Green oxide of chromium, when prepared by the usual processes, is not always free from chromic acid; but in consequence of the property which sulphuretted hydrogen has of reducing chromic acid to green oxide, we have it always in our power to free it entirely from all such admixture. For this purpose it may be dissolved in muriatic acid, and a

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* By hexa-chromate of chromium, I mean a compound of 1 atom chromic acid, and 6 atoms green oxide of chromium.

current of sulphuretted hydrogen may be passed through the solution, previously rendered as neutral as possible by evaporating it to dryness, and re-dissolving the residual matter in water. If the green oxide was pure, no precipitate appears; but if chromic acid was present, a quantity of the above described buff-coloured precipitate will be thrown down.

When sulphate of chromium in the state of a dry powder is put into a glass tube and heated to redness over a spirit lamp, while a current of hydrogen gas is made to pass over it, water is at first given off, and afterwards sulphuretted hydrogen. When all action is at an end, a black, tasteless, insoluble matter remains in the tube, composed (judging from the loss of weight) of

1 atom sulphur - - - -	2
2 atoms green oxide of chromium -	10
	12

It is therefore a *disulphuretted oxide* of chromium; or, perhaps, a *dihydrosulphuret* of chromium.

2. I shall now relate the experiments by which chromic acid was converted into green oxide of chromium, by means of protosulphate of iron.

When a solution of protosulphate of iron is mixed with one of chromate of potash, a buff-coloured precipitate falls, which is a combination of peroxide of iron and green oxide of chromium. In this case, the protoxide of iron is peroxydized at the expence of the oxygen in the chromic acid, which becomes green oxide. Thus we have it in our power to convert chromic acid at once to green oxide, by mixing it with a solution of protosulphate of iron. The determination of the quantity of protosulphate necessary to disoxygenise a given weight of chromic acid, will give us the quantity of oxygen which constitutes the difference between chromic acid and green oxide.

It will be necessary to remember that the atomic weight of protoxide of iron is 4·5, and that of peroxide 5; and that an atom of protoxide of iron is converted into peroxide by uniting with half an atom of oxygen. We must remember too, that crystallized protosulphate of iron is a compound of

1 atom sulphuric acid - -	5
1 atom protoxide of iron -	4·5
7 atoms water - - -	7·875
	17·375

Consequently 17·375 of protosulphate of iron constitute the equivalent for 1 atom of protoxide of iron.

If chromic acid be a compound of

1 atom green oxide - -	5
1½ atom oxygen - -	1·5
	6·5

as the analysis of it by means of sulphuretted hydrogen has shown it to be, it is obvious, that in order to reduce an atom of chromic acid to green oxide, we must mix it with three atoms of protoxide of iron. Now, 52·125 grains of protosulphate of iron contain the equivalent for 3 atoms of protoxide, while 12·5 grains of chromate of potash contain the equivalent for an atom of chromic acid. These facts being understood and remembered, we are prepared for following the details of the experiment.

12·5 grains of crystallized chromate of potash (containing 6·5 grains of chromic acid) were dissolved in a small quantity of boiling-hot distilled water. 52·125 grains of protosulphate of iron, recently crystallized, transparent, and of a light green colour, were dissolved in boiling-hot distilled water, after it had been kept boiling briskly for several minutes, in order to free it from air as completely as possible. These solutions being mixed, an abundant buff-coloured precipitate fell. It was separated by the filter, well washed and dried. The weight as dried on the filter was 25 grains, but by ignition it was reduced to 16·81 grains. It was a black shining powder, not magnetic, and similar in appearance to native *chromiron ore*, as it is called.

The residual liquid was neutral, and contained both peroxide of iron, and green oxide of chromium. It was diluted with water, and then mixed with benzoate of ammonia. The object of the dilution was to prevent any benzoate of chromium from being precipitated along with the benzoate of iron. The perbenzoate of iron, after being washed and dried, was found to contain 1·37 grain of per oxide of iron. The green oxide of chromium having been thrown down by an alkaline carbonate, washed, dried, and ignited, weighed 1·66 grains. It did not glow, and assumed a blackish colour.

From this experiment we see, that three atoms of protoxide of iron become peroxide when they are employed to reduce one atom of chromic acid to green oxide. But an atom of chromic acid weighs 6·5, and 3 atoms of protoxide of iron require 1·5 atom of oxygen to convert them into peroxide. Consequently, chromic acid is a compound of

Green oxide - -	5
Oxygen - - -	1·5

and 5 is the atomic weight of green oxide of chromium.

The whole protoxide of iron used in this experiment, when converted into peroxide, became 15 grains, and the 6·5 chromic acid, when reduced to green oxide, became 5 grains. Hence the total weight of the oxides of iron and chromium ought to amount to 20 grains.

Now the buff-coloured precipitate was	-	16·81
In solution { peroxide of iron	- -	1·37
{ green oxide of chromium	-	1·66
		19·84
Loss	- - - - - -	0·16

The cause of this loss became evident when the residual liquid was concentrated to a few drops. It should have contained sulphuric acid, potash, and the alkali employed to throw down the green oxide, and nothing else. But after the concentration had made considerable progress, the liquid assumed a perceptibly yellow tinge, showing that it still retained a small portion of chromic acid. The colouring powers of this acid are so great, that a very minute portion of it becomes visible. The protosulphate of iron, though I had been at great pains to have it as pure as possible, was not quite free from all admixture of peroxide. For when a crystal of it was put into a solution of prussiate of potash, a green colour was immediately struck. This incipient peroxidizement of the iron rendering the quantity employed insufficient to reduce the whole chromic acid, a minute portion still remained in the state of chromic acid.

Various repetitions of this experiment were made, but I was never able to obtain a solution of protosulphate of iron absolutely free from all admixture of peroxide. There was therefore always a loss. When the preceding quantities were employed, the smallest loss amounted to 0·07 grain, and the greatest to 0·2 grain; but notwithstanding this loss, never exceeding 1 per cent, and sometimes less than a half per cent, the experiment is conclusive, that 5 is the atomic weight of green oxide of chromium; and that 6·5 chromic acid become green oxide when deprived of 1·5 oxygen.

It is obvious that the 16·81 grains of black matter obtained were composed of

Peroxide of iron - -	13·63 or 20·404
Protoxide of chromium -	3·34 or 5
	16·97

That is to say, it is a compound of 4 atoms peroxide of iron, and 1 atom green oxide of chromium. On subjecting a portion of it to analysis, I obtained

Peroxide of iron - -	8·77 or 21·286
Green oxide - - -	2·06 or 5

Now, if we subtract from the 3·34 of green oxide the 0·16 of loss, it is obvious that the true composition of the powder will be

Peroxide of iron -	21·11
Green oxide of chrome - -	5

The analysis therefore, notwithstanding the smallness of the scale, comes near enough the truth to show that it had been conducted with care.

Having thus established the true atomic weight of chromium, and protoxide of chromium, the experiments which were made to determine the composition of phosphuret of chromium will be readily understood by the reader.

3·2 grains of phosphorus and 5·08 grains of anhydrous green oxide of chromium were put into a green glass tube, shut at one end and open at the other. The phosphorus occupied the bottom of the tube and the oxide the middle portion, extending about two inches, and distant rather more than one inch from the phosphorus. The tube was laid horizontally across a choffer, and the portion of it containing the green oxide was raised to a red heat by means of a charcoal fire. The phosphorus was then sublimed through the green oxide by means of a spirit lamp. A brilliant combustion took place, and the oxide was converted into phosphuret of chromium.

The phosphuret thus formed still continued an incoherent powder. It had a brown colour, was tasteless, and insoluble in water and acids. Before the blow pipe it was agglutinated together, but did not undergo complete fusion. It weighed 6·21 grains.

5·08 grains of green oxide of chromium are equivalent to 4·065 grains of chromium. Hence, the phosphuret was a compound of

Chromium	- -	4·065 or 4
Phosphorus	- -	2·145 or 2·11

1½ atom phosphorus weighs 2·25, which comes near 2·11. The compound thus seems to consist of 1 atom of chromium united to 1½ atom phosphorus. It is therefore a sesquiphosphuret.

I digested this phosphuret for a week in nitric acid. No solution took place, but the matter assumed a fine green colour. By this process the powder was converted into a phosphate of chromium. Part of the phosphoric acid was withdrawn, and remained dissolved in the liquid; for the phosphate weighed 9·545 grains. It was obviously a compound of

Protoxide of chromium	5·08	or 5	or 1 atom.
Phosphoric acid -	4·465	4·394	1⅓ atom.

There is still an excess of acid; but the excess, instead of being half an atom, as in the phosphuret, was only ⅓d of an atom.

My attempts to form a sulphuret of chromium by a similar process were not attended with success.

3. Besides the preceding experiments, by which the atomic weight of green oxide of chromium was determined, many others were tried, which did not terminate so satisfactorily. It will be worth while to give a short account of a few of the most promising of these methods, though they did not prove successful.

1. A quantity of chromate of lead was heated to whiteness in a charcoal crucible. It became black and agglutinated, and globules of metallic lead were visible in it. The under side of the cohering mass consisted chiefly of green oxide of chromium. 23·06 grains of this black matter were digested for a month in dilute nitric acid, and then filtered. The undissolved green oxide of chromium being collected on a filter, edulcorated, dried, and ignited, weighed 5·589 grains. The nitric acid solution being evaporated to dryness, left 27 grains of pure nitrate of lead, equivalent to 18·216 grains of protoxide of lead. Thus the 23·06 grains of black matter furnished

Green oxide of chromium	-	5·589
Protoxide of lead	- -	18·216
		23·805

which exceeds the weight of the matter employed by 0·745 grains. The oxygen in 18·216 grains of protoxide of lead amounts to 1·301. Of this only 0·556 gr. existed in the black powder. From this experiment it appears that $\frac{4}{7}$ths of the lead were reduced to the metallic state, while $\frac{3}{7}$ths still remained in their original state of protoxide.

The lead here exceeds the quantity which would have been obtained, had I been able to analyse the whole altered chromate of lead. The true proportions would have been

Green oxide of chromium	-	5·589
Protoxide of lead	- -	15·645

The excess of 2½ grains was owing to this: I took the bottom of the cake, because the upper part was not free from charcoal; and the great weight of the reduced lead occasioned an excess of it to be found in the bottom of the cake. Now the oxygen in 15·645 grains of protoxide of lead is 1·117, of which 0·556 gr. is very nearly the half. From this we see, that when chromate of lead is exposed to a white heat in a charcoal crucible, the chromic acid is totally converted into green oxide, while half the protoxide is reduced to metallic lead. The black matter formed is a compound of

2 atoms green oxide of chromium	-	10
1 atom protoxide of lead	- - -	14

42 grains of chromate of lead, in powder, were put into a green glass tube, which by means of a charcoal fire was kept at a cherry red heat, while a current of dry hydrogen gas

was made to pass through the tube till all action was at an end. The chromate underwent combustion, and water was evolved pretty copiously. The loss of weight was 4·6 grains.

Now, the constituents of 42 grains of chromate of lead may be represented as follows:

	Lead.		Oxygen.
28·68 protoxide of lead composed of	26·63	+	2·05
	Green oxide.		
13·32 chromic acid composed of -	10·246	+	3·074
Total oxygen - - -			5·124

By the process the chromic acid was totally reduced to green oxide. Globules of metallic lead could be detected in the residual matter. But had the whole lead been reduced, the loss of weight would have amounted to 5·124 grains, instead of 4·6 grains. The difference is 0·524, which rather exceeds $\frac{1}{4}$th of the oxygen in the protoxide of lead.

The whole of the residual matter having been digested in nitric acid for about a week, was dissolved, and formed a very dark bluish green liquid, from which the lead was precipitated by sulphate of soda, and the green oxide by carbonate of ammonia. When this last precipitate was dried it had a blue colour; when heated to redness it did not glow, as green oxide usually does, but assumed a dirty green colour. The weight was 9·36 grains. There had remained undissolved of the original matter 3·46 grains. It had a brownish yellow colour, and was chromate of lead, probably reproduced during the action of the nitric acid on the green oxide. It was composed of

Protoxide of lead - -	2·36
Chromic acid - - -	1·10
	3·46

If we subtract the 1·1 chromic acid from the 13·32, originally present in the chromate of lead, there will remain 12·22, which had yielded 9·36 of green oxide. According to this analysis chromic acid is composed of

Protoxide of chromium	9·36	or 4·9
Oxygen	2·86	or 1·5
	12·22	

The atomic weight of protoxide of chromium, by this experiment, comes out 4·9, instead of 5; but a few flocks of the green oxide of chromium accidentally escaped, and could not be weighed. Were we to estimate them at $\frac{1}{20}$th of a grain (and I think they amounted to that quantity), the atom of protoxide would amount exactly to 5.

This mode of experimenting is susceptible of considerable accuracy. The error was only 2 per cent, and the source of it was evident.

2. 6·62 grains of anhydrous green oxide of chromium were heated with hydrate of potash, over a spirit lamp, till water ceased to escape. The silver crucible containing the mixture was then raised to a red heat, and kept in that temperature for 15 minutes. The whole was now digested in water: 1·49 grains of the anhydrous green oxide still remained in its original state; so that by the process, 5·13 grains of green oxide had been converted into chromic acid. The yellow-coloured alkaline solution was saturated with acetic acid, and precipitated by acetate of lead. The chromate of lead obtained weighed after ignition 20·29 grains, equivalent to 6·433 grains of chromic acid.

According to this experiment chromic acid is composed of

Green oxide - -	5·13	or 5·183	or 5·89
Oxygen - - -	1·303	or 1·317	or 1·5
		6·500	7·39

This makes the atomic weight of green oxide 5·183, instead of 5; and if we consider 1·5 as the excess of oxygen in chromic acid over green oxide, we have the atomic weight of chromic acid 7·39, instead of 6·5, which is the true number.

The experiment was repeated twice, but without coming nearer the truth. I shall briefly mention the results of these two experiments.

10 grains of anhydrous green oxide were boiled in a silver crucible with a strong potash ley till water ceased to be driven off. The whole was then heated to redness, and kept in that state for a quarter of an hour. The dry mass being digested in water and filtered, there remained on the filter a blackish matter, which, dried on the sand bath, weighed 0·66 grain. When heated it glowed, and the weight was reduced to 0·33 grain. Thus 9·66 grains of green oxide had been converted into chromic acid. The potash solution treated as before gave 41·492 grains of chromate of lead, equivalent to 13·156 grains of chromic acid. Consequently, chromic acid is composed of

Protoxide	9·66	or 4·145	or 4·775
Oxygen -	3·496	or 1·5	or 1·727
	13·156	5·645	6·5

12·99 grains of anhydrous green oxide were mixed with a great excess of bicarbonate of potash which had been triturated with a little nitre. This mixture was slowly raised to a red heat: it was then brought into fusion, and kept in that state for about 20 minutes. The fuzed mass on cooling had a fine yellow colour. Being digested in water a dirty brown matter remained, weighing when dried on the filter 1·89 grain. When ignited it became green, but did not glow, and the weight was reduced to 1·19 grain. Thus 11·8 grains of green oxide were converted into chromic acid. The potash solution treated as before gave 49·21 grains of chromate of lead, equivalent to 15·603 grains of chromic acid. Thus, 11·8 grains of green oxide became 15·603 chromic acid. According to this experiment, we have chromic acid composed of

Protoxide	11·8	or	4·654	or	4·915
Oxygen	3·803	or	1·5	or	1·585
	15·603		6·154		6·5

These experiments all deviate a good deal from the truth. The mean of the three gives us the constituents of chromic acid, as follows:

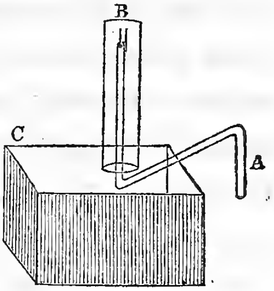
Protoxide - -	4·957
Oxygen - -	1·543

This gives us 4·957 for the atomic weight of green oxide. The deviation from the truth does not amount to 1 per cent. I expected by this mode of experimenting to have obtained much more satisfactory results.

3. A quantity of liquid chromic acid was evaporated to dryness, forming, as usual, a very deliquescent red mass, which I found by analysis to consist of

Pure anhydrous chromic acid -	88·97
Protoxide of chromium - -	4·30
Water - - - - -	6·73
	100·00

12·1 grains of this dry acid were put into a green glass tube, shut at one end, and bent as the tube (A) in the margin, so as to serve the purpose of a retort. The open end ascended to nearly the top of a glass jar (B) standing in the mercurial trough (C), and containing 3 cubic inches of air over a column of mercury 3·6 inches long. While the barometer stood at 30·13 inches, and the thermometer at 52°. The flame of a spirit lamp being applied to the end of the tube (A), containing the acid, fusion took place, and water was given out. A little of the acid assumed the form of vapour, and was deposited in the upper part of the tube. As soon as the tube became red hot, the acid began to give out oxygen gas. The heat was continued till all evolution of gas was at an end, and till the acid in the tube had assumed a fine green colour.



The oxygen gas evolved when reduced to the temperature of 60°, under a pressure of 30 inches of mercury, was 5·634 cubic inches, the weight of which is 1·909 grain. The real acid heated amounted to 10·765 grains. According to this experiment, chromic acid is a compound of

Protoxide - -	8·856	or 6·95
Oxygen - -	1·909	or 1·5
	10·765	

What makes this result so erroneous, is partly the impossibility of determining the quantity of chromic acid which had sublimed unaltered; but chiefly the impracticability of reducing the acid completely to the state of protoxide by the application of the greatest heat which the glass could bear without fusion. I made an attempt to determine how much of the acid still remained undecomposed, but did not succeed. Water would not separate the acid from the oxide, and fusion, or even digestion with an alkali could not be had recourse to, because it might have brought back a portion of protoxide to the state of acid.

III. *Brown oxide of Chromium.*

This oxide was first noticed by VAUQUELIN, who remarked at the same time that it contained only a very little more oxygen than the green oxide. It may be most conveniently prepared by passing a current of sulphurous acid gas through a solution of chromate, or bichromate of potash: the brown oxide soon precipitates, and may be easily collected on the filter, and washed and dried.

It has a flea-brown colour, and is destitute of taste, and does not undergo any alteration by several days exposure to the air. It is not quite insoluble in water. A quantity of it was put into a large glass jar, which was filled with water, and the powder well agitated in the liquid. When the oxide had subsided, the water was drawn off with a syphon, and clean water substituted. This was continued regularly every day for above two months. The water always assumed a yellow colour, and some chromic acid could be detected, dissolved in it, to the very last.

I did not succeed by this method in altering the colour of the oxide; but when caustic ammonia was substituted for water, the colour of the oxide, after the second washing, had become black; and after two or three more washings, it assumed the green colour of protoxide of chromium. From these trials I was led to suspect, that this supposed oxide might be nothing else than common green oxide of chromium combined with a certain determinate quantity of chromic acid. The following experiments were made with a view of determining how far this suspicion was well founded.

8 grains of brown oxide of chromium were heated on the sand bath till they ceased to lose weight. The brown colour was not altered, but the weight was reduced to 6·8 grains. When this matter was exposed to a red heat it did not glow, but became black, and similar to brown oxide after it has been once or twice washed in ammonia. The weight was reduced to 6·24 grains.

Brown oxide, when digested in nitric acid, was dissolved. The solution had a green colour, a very sweet taste, and possessed the usual characters of nitrate of chromium. It dissolved likewise in muriatic acid. The solution was green-coloured, sweet tasted, and precisely similar to common muriate of chromium.

These facts seemed to show decisively, that this supposed brown oxide does not combine with acids; and that when treated either with acids or alkalies, it is resolved into common green oxide and chromic acid.

To determine the quantity of chromic acid in brown oxide of chromium, the following experiments were made: 6·81 grains of brown oxide, previously exposed to an incipient red heat, were put into a green glass tube, through which a current of dry hydrogen gas was made to pass, while the oxide was kept at a red heat by means of a spirit lamp. No combustion took place; but a little water was formed, and the oxide assumed a fine green colour. The loss of weight was 0·18 grain.

12 grains of the brown oxide in its original state, as prepared, were put into a green glass tube, shut at one end and bent into a retort-like form, as before described. The open end of the tube rose to the top of a small glass jar, standing over mercury, and containing 3 cubic inches of air. Heat was applied to the oxide in the tube by means of a small charcoal fire. No gas was extricated till the oxide became red hot. At that temperature 1·169 cubic inch of oxygen gas came over. The heat was raised as high as the tube could bear, and it was continued till the evolution of gas had been for some time over, and till the oxide had assumed a fine green colour. During this process the oxide did not glow. The weight of 1·169 cubic inch of oxygen gas is 0·4009 grain.

From a preceding experiment it is evident, that 12 grains of the brown oxide employed, when exposed to a strong red heat, were reduced to 9·36 grains; or, the loss of weight was 2·64 grains. From the experiment just stated, it appears that 0·4009 gr. of that weight is oxygen. Consequently, the constituents of the brown oxide may be represented as follows:

Green oxide - -	9·36
Oxygen - - -	0·4009
Water - - -	2·2391
	12·0000

But the oxygen must have been combined with green oxide, so as to constitute chromic acid. We may therefore represent the constitution of brown oxide of chromium in the following manner.

Green oxide	8·024 or	30·02 or	6 atoms.
Chromic acid	1·7369 or	6·5	1 atom.
Water - -	2·2391 or	8·38	7½ atoms.

From the preceding analysis it appears, that brown oxide of chromium is a compound of 1 atom chromic acid and 6 atoms green oxide. The combination is not very intimate, since the chromic acid is gradually separated by water. It is not capable of combining either with acids or alkalies, or of forming peculiar compounds. When it is digested in acids, the common salts of green oxide of chromium are obtained. To alkaline solutions it gives out chromic acid, while green oxide of chromium remains.

In my "First Principles of Chemistry," vol. ii. p. 54, I mention that I formed a combination of brown oxide of chromium and soda, and I describe the process followed. I merely inferred the existence of brown oxide in this liquid, from its colour. I have since analyzed it, and found it a mixture of nitrate of soda and chromate of chromium. When nitric acid is digested on green oxide of chromium, and the liquid after being evaporated to dryness is fuzed in a crucible, the nitric acid is destroyed, and abundance of chromic acid formed. By this process, if we stop in time, we may form a brown-coloured liquid, which, when evaporated to dryness, leaves a substance quite similar in its properties to brown oxide of chromium.

From the preceding investigation it follows, that chromium combines with two proportions of oxygen, forming green oxide and chromic acid. The atomic weights and constituents of chromium and its oxides are as follows:

		Atomic weight.
Chromium	- - - - -	4
Green oxide	4 chromium + 1 oxygen	= 5
Chromic acid	4 + 2·5	= 6·5
Brown oxide	4 + $1{\cdot}\dot{2}14285\dot{7}$	= $5{\cdot}\dot{2}14285\dot{7}$

Two atoms and a half, the proportion of oxygen united to an atom of chromium in chromic acid, has not yet been observed, except in the hyposulphuric acid of GAY-LUSSAC, which is a compound of 1 atom sulphur, and 2·5 atoms oxygen.

IV. *Chloro-chromic acid.*

I proceed now to describe the properties of a rather remarkable compound, which I discovered about a year and a half ago, and which, from the great energy with which it acts upon combustible substances, will be very acceptable to those persons who are fond of exhibiting brilliant chemical experiments.

Chloro-chromic acid may be obtained with great facility by the following process. Triturate together in a mortar 190 grains of bichromate of potash, and 225 grains of common salt, till they are intimately mixed. Pour the dry powder into a tubulated retort: insert the beak of the retort into a dry glass receiver, fitting it by means of a perforated cork, which should not be quite air tight. Pour into the retort 500 grains of the sulphuric acid of commerce, and by agitation make it into a magma with the salts. Then apply the flame of a lamp to the bottom of the retort. An effervescence takes place, and beautiful red fumes soon make their appearance. These condense in the beak of the retort, and gradually drop into the receiver under the form of a red-coloured liquid. This process lasts about ten minutes, or a quarter of an hour, when the evolution of the red fumes suddenly stops. The matter in the retort has now assumed a fine green colour. If the distillation be continued, a liquid passes into the receiver, consisting chiefly of acidulous water, and destroys the red liquid. The process therefore must be stopped as soon as the evolution of red fumes is at an end.

The quantity of red liquid obtained from the above stated proportions of materials is about 200 grains; but it varies somewhat with the care with which the process is conducted. The quantity of protoxide of chromium remaining in the retort is also various. With the above proportions it usually amounts to 27·3 grains, equivalent to 35·5 grains of chromic acid, or $\frac{3}{11}$ths of the whole chromic acid in the salt; but it varies with the quantity of common salt employed. If we introduce only a portion of the common salt at first, and add the rest at intervals, till the matter in the retort has assumed a green colour, the protoxide of chromium remaining in the retort is a maximum, and the quantity of red liquor obtained a minimum. When the atoms of chlorine in the common salt are to those of chromic acid in the bichromate as 3 to 2, the product of red liquor is greatest. We may vary the sulphuric acid considerably, but the process is easiest when the quantity of acid used is sufficient to convert the salt into a semifluid magma. Chromate of lead or chromate of potash may be substituted for bichromate of potash.

The red liquid possesses the following properties: its colour is a deep, but lovely crimson; so intense indeed as to render the liquid opaque, except in very thin films.

Its taste is sweetish, astringent, and acid.

It has a smell of chlorine fully as strong and as disagreeable as chlorine gas itself.

It reddens vegetable blues.

Its specific gravity at 50°, was 1·9134; but its extreme volatility makes it rather difficult to estimate the specific gravity with very great exactness.

When dropt into water it falls to the bottom, and exhibits the appearance of a drop of oil. Globules of chlorine gas issue from it rather copiously. This evolution continues till the red liquor *dissolves* in the water, forming a yellow-coloured *mass*, which contains muriatic acid, chromic acid, and a little green oxide of chromium.

151·5 grains of the red liquor were poured into about an ounce of water. The temperature rose at once to 212°, and chlorine gas was given off in such abundance as to occasion an effervescence.

When the red liquid is dropt upon oil of turpentine, much heat is evolved and the oil catches fire, and burns with a lively flame, but having more of the blue tint than is usual with the flame of that oil. After the combustion, we find green oxide of chromium mixed with vegetable matter from the oil of turpentine.

When dropt into alcohol, even of a specific gravity not lower than 0·840, it sets it on fire. The combustion goes on tranquilly, the flame is lively, and has a beautiful bluish white colour.

When the red liquid is poured on flowers of sulphur, a pretty violent action takes place. After a few seconds the sulphur catches fire, and burns with a fine red flame.

When we drop it into olive oil, a violent action takes place;

great heat is evolved, a kind of effervescence is produced, and a quantity of green oxide of chromium formed; but the oil does not take fire.

The effects upon pyroxylic spirit are nearly the same as upon olive oil.

The red liquid has scarcely any action upon phosphorus. Even when the phosphorus is heated till it is brought into a state of fusion no action takes place. If the phosphorus be set on fire, the combustion is instantly extinguished by bringing the red liquid in contact with the burning body.

Neither does it produce any sensible effect upon charcoal powder, nor upon indigo.

When it is poured upon camphor no immediate action takes place; but after two or three minutes the camphor begins to swell out, and it gradually occupies many times its former bulk. By this action the camphor loses its smell and consistence, and seems as if it had been partially charred, the colour being changed into a dark brown.

The red liquid effervesces rather strongly when dropped into naphtha. Much heat is evolved, and the naphtha loses its oily nature, but does not catch fire.

Zinc and tin filings effervesce with the red liquid, but the action is gentle and soon at an end. On iron filings it does not act till we apply heat, when the iron dissolves with effervescence; but the volatility of the liquid prevents the process from going far. On copper and antimony it has no sensible action. When brought into contact with mercury, it forms with it a matter of the consistence of hog's lard. The mercury loses its fluidity, but retains its colour and metallic lustre.

When the protohydrate of acetic acid, in the liquid state, which it always has during the summer, is mixed with the red liquid, no immediate decomposition takes place; the mixture continues red and opaque, and as liquid as ever. The smell of the chlorine was much weakened, or overpowered by the acetic acid. No alteration in this mixture had taken place in a week; but in a fortnight the chloro-chromic acid was decomposed, and a quantity of reddish brown matter had precipitated.

A quantity of the red liquid was put into a receiver, and a current of ammoniacal gas was made to pass into the receiver from a small retort filled with a mixture of sal ammoniac and quick lime, to which the flame of a spirit lamp was applied. When the receiver was filled with ammoniacal gas, it was turned round so as to bring the red liquid in contact with it. A brilliant combustion took place, the red liquid assumed the form of a dark brown solid, and glowed for some time with an intense red heat. The residual matter being digested in water left a quantity of green-coloured muriate of chromium, or chloride of chromium, which would neither dissolve in water, nor nitric acid; but I ascertained the presence of muriatic acid by heating it with caustic potash. By this process much sal ammoniac is formed, and a quantity of muriate of chromium.

It is obvious from the preceding facts, that the property of supporting combustion, which this liquid possesses to so great a degree, is not owing solely to the chlorine which it contains, but likewise to the oxygen of the chromic acid; which, as we shall see afterwards, constitutes so considerable a portion of it. So that chromic acid is a supporter of combustion, as well as nitric acid, and nearly to as great an extent. Indeed this might have been inferred from several facts already known. I may mention, as an example, the property which it has of effervescing with tartaric acid, and giving out abundance of carbonic acid.

Neither nitric acid, muriatic acid, iodine, oxygen, or chlorine, have any sensible action on the red liquid.

When the red liquid is exposed to heat it gives out chlorine, and is gradually converted into a brown mass, solid while cold, but readily melting when heated. This brown matter deliquesces when left exposed to the air. When strongly heated it swells out, and at last assumes the form of a light grey slaggy looking matter. This in a strong red heat assumes a green colour, and is insoluble in acids. It resembles green oxide of chromium. I did not try whether or not it contained muriatic acid.

After various trials, I employed the following method to determine the constituents of this red liquid.

A cylindrical glass vessel, containing a few ounces of distilled water, was accurately balanced, and a quantity of the red liquid poured into it. The weight of this quantity was found to be 90·1 grains. By a little agitation. a complete solution was soon obtained, and the water assumed nearly the colour of sherry. A solution of carbonate of soda was gradually added till the acids contained in the liquid were saturated. The colour of the liquid had become darker, and upon applying heat a precipitate fell. This precipitate, after being collected on the filter, washed, and dried, weighed 1·37 grain. By ignition it glowed, and the weight was reduced to 0·71 grain. It was green oxide of chromium.

The liquid thus deprived of green oxide, and perfectly neutral, was mixed with a solution of nitrate of barytes, as long as any precipitate continued to fall. By this process the liquid was deprived of its yellow colour, the chromic acid having fallen in the state of chromate of barytes. This method of separating chromic acid does not succeed unless care be taken to leave no excess of acid in the liquid; for chromate of barytes dissolves easily in even dilute acids. If this inadvertency has been committed, the best way is to pour caustic ammonia into the liquid till you have supersaturated the acid. This will throw down all the chromate of barytes, previously held in solution, without acting upon nitrate of barytes, should an excess of that salt have been introduced. The chromate of barytes thus obtained being washed, dried, and ignited, weighed 148·37 grains. Chromate of barytes is a compound of

1 atom chromic acid - -	6·5
1 atom barytes - - -	9·75
	16·25

148·37 grains of the salt therefore contain 59·34 grains of chromic acid.

The residual liquid thus freed from chromic acid was precipitated by nitrate of silver. The chloride of silver obtained, weighed, after being washed, dried, and fused, 164·42 grains, equivalent to 40·54 grains of chlorine.

Thus the constituents obtained from 90·1 gr. of the red liquid were

Green oxide of chromium -	0·71
Chromic acid - - -	59·34
Chlorine - - - -	40·54
	100·59

The weight of the products is more than 10 per cent. greater than that of the red liquid subjected to analysis.* But 59·34 is to 40·54 as 6·5 to 4·4. I was induced from this to consider the red liquid as a compound of 1 atom chlorine and 1 atom chromic acid; and the following experiment was made to verify this supposition.

15·32 grains of the red liquid were dropt into a quantity of distilled water, and 50·15 grains of crystallized carbonate of soda were dropt into the bottom of the vessel containing the liquid. The solution of this salt took place slowly, and the carbonic acid escaped gradually, and without carrying with it any perceptible quantity of chlorine, as is apt to happen when a solution of carbonate of soda in water is poured into the dilute red liquid. The liquid being heated and agitated, to expel the carbonic acid gas, a slight flocky precipitate fell, and the liquid was found quite neutral; for it neither altered the colour of litmus nor cudbear paper.

The reason why 50·15 grains of carbonate of soda were used was this: if the red liquid be a compound of

1 atom chromic acid - -	6·5
1 atom chlorine - - -	4·5
	11

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* This augmentation of weight was occasioned by using a carbonate of soda, not quite free from common salt; and by adding the alkali slightly in excess. From these two causes there was an excess of chloride of silver, and the chromate of barytes was mixed with a little carbonate.

its atomic weight will be 11, and 11 grains of it will contain just the equivalent of 1 atom of chromic acid, and 1 atom of chlorine. The crystals of carbonate of soda being composed of

1 atom carbonic acid - - -	2·75
1 atom soda - - -	4
10 atoms water - - -	11·25
	18

It is clear, that in order to neutralize 11 grains of the red liquid, we must employ 36 grains of the carbonate; but 11 : 36 : : 15·32 : 50·15 = quantity of carbonate of soda employed. As this quantity just neutralized the liquid, it is plain that the quantity of acid in the red liquid was rightly estimated.

If 11 grains of the red liquid contain 6·5 gr. chromic acid, 15·32 gr. must contain 9·052 of that acid, which will require for complete precipitation 22·98 grains of nitrate of barytes.

The liquid was freed from the trace of green oxide of chromium which it contained by filtration, and a solution of 22·98 grains of nitrate of barytes was mixed with it. After the chromate of barytes had subsided the liquor was colourless, and was not altered by sulphate of soda. Consequently it contained no sensible quantity of chromic acid, or of barytes. It is plain that the quantity of chromic acid in the red liquid had been rightly determined.

The liquid thus freed from chromic acid still retained its chlorine, which, if the constitution of the red liquid has been rightly determined, should amount to 6·268 grains, and would just require 29·94 grains of nitrate of silver to throw it down. This quantity was therefore dissolved in distilled water, and mixed with the liquid. After the chloride of silver had subsided, the residual liquid was neither affected by common salt nor by nitrate of silver.

From this analysis it is clear, that the red liquid is a compound of 1 atom chromic acid, and 1 atom chlorine, and that its atomic weight is 11.

From an experiment formerly given, it is easy to deduce that 11 grains of the red liquid contain likewise 0·0866 grain of green oxide of chromium, doubtless, from the sweet taste of the liquid, held in solution by chlorine or muriatic acid. It would seem at first sight that this quantity, not taken into consideration in the analysis, vitiates the whole of the conclusions. But from the length of time taken up in weighing the liquid, I have no doubt that just as much moisture evaporated as this increase amounts to. The reader will observe, that the amount of green oxide is less than 1 per cent of the whole.

As this liquid possesses acid properties, I have given it the name of chloro-chromic acid. Water having the property of decomposing it, we have scarcely the means of trying whether it be capable of uniting with bases. The few trials which I made were not attended with success.

This body, so liquid and so volatile, is a compound of a solid and a gas. One cubic inch of chloro-chromic acid contains 259·22 inches of chlorine gas. Thus the gaseous particles are about 6½ times nearer each other than when they are in the gaseous state. This, together with the looseness of the combination, accounts for the abominable smell which distinguishes this liquid.

100 grains of the acid contain very nearly 41 grains, or 31¼ cubic inches of chlorine.

When chloro-chromic acid is left exposed to the air, it gradually loses its smell and its red colour, and is converted into an opaque dark brown liquid, having an acid and astringent taste, without the least perceptible sweetness. When this liquid is saturated with carbonate of ammonia, a yellowish brown precipitate falls, which becomes blue when washed, and is merely protoxide of chromium. The filtered liquid is yellow-coloured, and consists of a mixture of chromate of ammonia and sal ammoniac.

In some cases, when the chloro-chromic acid had been left for some days exposed to the air, it was converted into a solution of muriate of chromium in water; but this change was not constant. I ascribed it at first to small pieces of cork which had accidentally fallen into the liquid which had undergone this alteration; but upon trying whether I could convert chloro-chromic acid at pleasure into muriate of chromium, by leaving it in contact with cork, the experiment did not succeed. I am disposed however to ascribe the alteration to the action of combustible matter, as spirits and oils cautiously added convert it into a green coloured matter which has a considerable resemblance to muriate of chromium.

When chloro-chromic acid is exposed to heat in an open vessel much chlorine flies off, and a black scaly matter remains, which speedily deliquesces in the air, and when heated melts and gives out the odour of chlorine gas. 31 grains of this scaly matter were left for a month in an open dish covered with paper in my laboratory, which is rather damp. It was converted into a brown opaque liquid, weighing 58·36 grains. It had therefore absorbed 27·36 grains of water. The liquid being diluted with water and filtered, left 0·25 grain of a brown matter, which did not glow when heated to redness.

The filtered liquid was very dark-coloured, but not muddy. Its opacity was not destroyed by very large dilution with water. It reddened vegetable blues slightly, and had the sweet taste which characterizes muriate of chromium.

Being mixed with ammonia, abundance of green flocks fell, which weighed after ignition 10·22 grains.

The filtered liquid was mixed with nitrate of barytes as long as any precipitate fell. The chromate of barytes weighed after ignition 27·77 grains, equivalent to 11·108 grains of chromic acid.

The residual liquid being precipitated by nitrate of silver, the chloride of silver obtained weighed after fusion 23·6 grains, equivalent to 5·819 grains of chlorine.

Thus the constituents of the 31 grains analysed were

Brown insoluble matter -	0·25
Protoxide of chromium -	10·22
Chromic acid - - -	11·108
Muriatic acid - - -	5·980
Loss, considered as water -	3·442
	31

These numbers are not precisely in atomic proportion. There is a slight redundancy of muriatic acid or chlorine, amounting to very nearly half a grain. The constituents approach

1 atom muriate of chromium	acid	4·625	9·625
	oxide	5·000	
1 atom bichromate of chromium	acid	13	18
	oxide	5	
3 atoms water - - - - -			3·375
			31·000

And the matter when allowed to absorb as much water as possible contains 27 atoms of that liquid.

It is not easy to form a conception of the way in which the reduction of the chromic acid to green oxide takes place. The heat employed was never sufficient to drive off the oxygen. Had the chlorine been converted into muriatic acid, the oxide of chromium should have been acidified, at least in part. Hence I am disposed to think that it is not muriatic acid which the black matter contains, but chlorine. A similar disorganisement takes place, if chromic acid itself be left for some time on the sand bath. Hence, I am disposed to ascribe the reduction to the tendency which chromic acid has to unite with protoxide of chromium. We may conceive that the elasticity communicated to the oxygen of the acid by the heat, together with the affinity of chromic acid for protoxide, may occasion the reduction of a portion of the acid, and cause the oxide formed to unite with the acids in the liquid.

V. *Salts of chromium.*

The green oxide of chromium combines with the different acids and forms a genus of salts, not one of which, so far as I know, has been described by chemists. I shall therefore embrace the present opportunity to give a short account of them.

The greater number of them have a deep green colour; though some of them are blue, and some of them purple. The intensity of the colour is such, that the aqueous solutions of most of them are opaque, even when dilute. They have a very strong and rather agreeable sweet taste, very often slightly acidulous, from the great difficulty of freeing them from all excess of acid. None of them can be crystallized; but when evaporated to dryness, they assume the form of dark green, or nearly black powders.

When the infusion of nutgalls is dropt into a solution of muriate of chromium a *green* flocky precipitate falls.

Prussiate of potash occasions no precipitate, but when the mixture is heated it becomes dark brown and opaque, though no perceptible precipitate falls.

Sulphuretted hydrogen occasions no precipitate, provided the salt of chromium be free from all traces of chromic acid; but hydrosulphuret of ammonia usually throws down green flocks, owing, I presume, to an excess of ammonia, which that hydrosulphuret frequently contains.

Both carbonate of ammonia and caustic ammonia throw down the green oxide from muriate of chromium. But carbonate of ammonia does not answer for precipitating oxalate of chromium or acid phosphate of chromium, probably because it forms compound salts with the acid and base. Caustic potash throws down the green oxide, but an excess of the alkali again dissolves the precipitate.

When benzoate of potash is dropt into a concentrated solution of muriate of chromium, a green flocky precipitate falls, but no precipitate falls when the solution is dilute.

We must attend to this circumstance, when we employ a benzoate to free a salt of chromium from iron.

I. *Muriate of chromium.*

Muriatic acid dissolves the hydrated green oxide of chromium with great facility; but neither this nor any other acid answers as a solvent of anhydrous green oxide. Exposure to a red heat almost entirely destroys the solubility of this oxide. The muriatic solution has a very deep green colour, and always retains a great excess of acid. When evaporated to dryness, and kept for some time on the sand bath, much of this excess is driven off. The salt assumes a red colour and a scaly appearance. It still retains an excess of acid and dissolves readily in water, forming a deep green liquid, having a sweet, and slightly acidulous taste. The scales rapidly absorb moisture from the atmosphere, and deliquesce into a green liquid. In this state the salt is composed of

$1\frac{1}{4}$ atom muriatic acid	5·781
1 atom green oxide	5·000
1 atom water	1·125
	11·906

If we raise the heat sufficiently high to drive of all excess of acid, and to convert the salt into a chloride, we obtain it in the form of light, tasteless, green scales. In this state it is insoluble in water and acids. I did not analyse it, but conceive it to be a simple chloride composed of

1 atom chlorine	4·5
1 atom chromium	4
	8·5

As long as muriate of chromium is soluble in water it contains an excess of acid, varying from one half to one fourth of an atom according to the heat in which it has been dried.

2. *Nitrate of chromium.*

Nitric acid dissolves the hydrated oxide of chromium readily enough, if we assist the acid by the application of heat. The solution has a reddish blue colour, and always retains an excess of acid, what quantity soever of hydrated oxide we employ in the preparation. The solution is opaque, and if sufficiently diluted to be transparent, has a deep brownish purple colour by transmitted light. When evaporated to dryness it leaves a deep reddish blue matter, readily soluble in water, but not by any means so deliquescent as the muriate. A quantity of hydrate equivalent to 27·5 grains of green oxide was dissolved in nitric acid, and the solution evaporated to dryness. The residual matter was redissolved in water, and again evaporated to dryness. This process was repeated thrice. The weight of the dry salt was now 69 grains; its taste was acidulous, and very sweet, and the salt still reddened vegetable blues. The green oxide being thrown down by ammonia, and the liquid remaining evaporated to dryness, I obtained 33·53 grains of nitrate of ammonia, equivalent to 22·63 grains of nitric acid. This would make the composition of the salt

Nitric acid - -	22·63 or $\frac{2}{3}$ atom.
Green oxide - -	27·50 or 1 atom.
Water - - -	18·87 or 3 atoms.
	69·00

But no confidence can be put in this analysis; for nitrate of ammonia is apt to be decomposed by too high a temperature; and the salt had been left for 24 hours upon the sand bath.

When nitrate of chromium is exposed to heat in a platinum crucible, it melts, and copious fumes of nitrous gas are given off. When the heat is continued till these fumes cease to be disengaged, a great part of the green oxide is converted into chromic acid. The residue is soluble in water, and forms a dark brown acid liquid, consisting of chromic acid holding green oxide in solution. This affords an easy way of getting pure chromic acid; for the small quantity of green oxide present will not interfere with the application of the chromic acid to most purposes for which chemists are likely to employ it. When we prepare chromic acid by VAUQUELIN's method, besides the expence of the process, it is by no means easy to free the acid from all traces of sulphuric acid.

3. *Sulphate of chromium.*

I mentioned before. that when hydrated green oxide of chromium is dried in the open air, without the application of artificial heat, it contains $\frac{13}{15}$ths of its weight of water. When dried on the sand bath, it retains about half its weight of water. The last of these is darker coloured than the first. It is much easier to dissolve the former of these hydrates in sulphuric acid than the latter. With the former we can obtain a neutral salt; but I never was able to saturate sulphuric acid by digesting it over the latter hydrate.

Sulphate of chromium, while in solution, forms a dark green opaque liquid, having a sweet, and slightly acidulous taste. Like all the salts of chromium it reddens vegetable blues. When evaporated to dryness, there remains a dark matter, having a green colour, so intense, that it appears black. It is tasteless, does not affect vegetable blues, and is easily reduced to a fine powder. It is not altered by exposure to the air, and seems at first to be insoluble in water; but when we pour water over it, and apply heat, the sulphate gradually dissolves in the liquid, and the solution has the same colour and the same sweet acidulous taste as at first.

To determine the composition of this salt, 345 grains of the hydrated oxide, dried in the open air (containing 55 grains of oxide), were digested in a flask with a quantity of sulphuric acid of commerce, containing just 55 grains of real acid. The solution was complete; and the liquid being evaporated to dryness, the dry tasteless residue weighed 151·4 grains. Now, as the acid and oxide together amounted to 110 grains, it is clear that the water in the salt must amount to 41·4 grains. Thus the constituents of sulphate of chromium are

Sulphuric acid - -	55	or 1 atom.
Green oxide of chrome	55	or 1 atom.
Water - - -	41·4	or $3\frac{1}{3}$ atoms.
	151·4	

We may therefore without sensible error consider sulphate of chromium as composed of

1 atom sulphuric acid - -	5
1 atom green oxide - -	5
3 atoms water - - -	3·375
	13·375

Twenty grains of the dry salt being heated to incipient redness lost 9·97 grains of their weight, and when kept for ten minutes in a wind furnace, in a heat approaching to whiteness, the weight was reduced to 8·58 grains. 20 grains (as is evident from the preceding analysis) of this salt contain 7·26 grains of green oxide. The salt, after exposure to an incipient red heat, retained nearly $\frac{2}{5}$ths of its acid; and almost $\frac{1}{5}$th of the acid was retained after the strong heat of a wind furnace. I was unable by heat to bring sulphate of chromium to the state of green oxide.

4. *Carbonate of chromium.*

When muriate of chromium is precipitated by carbonate of soda, and the precipitate, after being well edulcorated, is collected on a filter, and dried in a heat not exceeding 212°, we obtain a light blue-coloured matter, which is very light, tasteless, and insoluble in water. In this state it is a dicarbonate of chromium composed of

1 atom carbonic acid - -	2·75
2 atoms green oxide of chrome	10
4 atoms water - - -	4·5
	17·25

I exposed 30·06 grains of this dicarbonate to a strong red heat. The loss of weight was 12·9 grains. The same quantity being dissolved in nitric acid effervesced, and lost 4·6 grains. This experiment requires some care, for the solution does not take place without the application of heat. Thus the constituents of the salt were found to be

Carbonic acid	-	4·6	or	2·75
Green oxide	-	17·16	or	10·25
Water	- -	8·3	or	4·96
		30·06		

The slight excess of green oxide and water in this analysis was, I believe, occasioned by the great caution employed in dissolving the salt in nitric acid; for I was so apprehensive of evaporating a portion of the nitric acid that I applied heat very cautiously, and stopt the process before the whole powder was dissolved. In another analysis of a portion of a similar dicarbonate, prepared in a different way, I obtained

Carbonic acid	- -	2·75
Green oxide	- -	9·33
Water	- - -	3·87

Here there is a deficiency of oxide and water; but this might be owing to errors in the analysis.

Finding that carbonate of soda, when employed to precipitate muriate of chromium, gives only a dicarbonate, I thought it not unlikely that a neutral carbonate might be formed by employing a solution of bicarbonate of potash to precipitate muriate of chromium. The matter obtained by this process was light blue, and resembled the dicarbonate in its external qualities. It was dried without the application of any artificial heat. Its composition determined in the same manner as above described was found to be

1 atom carbonic acid	-	2·75
5 atoms protoxide	-	25
21 atoms water	- -	23·625
		51·375

The result of the analysis was

Carbonic acid - -	2·75
Green oxide - -	26·44
Water - - -	23·69

In this analysis, as in the preceding, there is a slight excess both of oxide and water; I ascribe this to the same cause. Such analyses can scarcely be made with very great accuracy.

Thus I obtained a penta-carbonate instead of a neutral carbonate, when I employed bicarbonate of potash as a precipitant; a result quite unexpected, and not easily explained.

I tried to form a neutral carbonate by passing a current of carbonic acid through newly precipitated dicarbonate, still suspended in water; but the process did not answer.

5. *Phosphate of chromium.*

Phosphate of soda precipitates exceedingly dilute solutions of muriate of chromium. The precipitate while in the liquid state has a dirty whitish green colour. But when phosphate of chromium is in the state of a dry powder, it has a very fine deep but lively green colour. It is quite tasteless, and insoluble in water. To determine the composition of this salt, 16 grains of anhydrous phosphate of soda (containing 7·46 grains of acid) were dissolved in water and mixed with an excess of muriate of chromium, previously rendered as neutral as possible by evaporation to dryness. The mixed liquids were placed on the sand bath, in a porcelain dish, and evaporated to dryness. Water was digested over the dry residue till it ceased to dissolve any more. The insoluble matter weighed 18·75 grains. It was a fine chromium green powder, tasteless, and insoluble in water. When heated to redness it smoked, gave out the smell of muriatic acid, and became greenish black. The weight was reduced to 12·72 grains.

The water digested over this salt being examined by reagents, no phosphoric acid could be detected in it. Consequently the 12·72 grains of phosphate of chromium contained all the phosphoric acid employed in the experiment. It is clear that the phosphate formed was composed of

Phosphoric acid - -	7·46	or 7·09	or 2 atoms.
Green oxide of chrome	5·26	or 5	or 1 atom.
	12·72		
Water - - -	6·03	or 5·68	or 5 atoms.

The salt was a biphosphate, and obviously composed of

2 atoms phosphoric acid -	7
1 atom green oxide - -	5
5 atoms water - - -	5·625
	17·625

The slight excess of water was probably owing to the presence of a little muriatic acid in the salt before it was exposed to a red heat.

I did not succeed in my attempts to obtain a neutral phosphate of chromium. The biphosphate is soluble in phosphoric acid, and forms a deep green, sweet, and acidulous liquid; but the acid does not dissolve an atomic quantity; at least I did not succeed in obtaining such a solution. It is not unlikely that a quadro-phosphate of chromium exists.

6. *Arseniate of chromium.*

Arseniate of soda, when dropt into a solution of muriate of chromium, though very dilute, occasions a green flocky precipitate, at first dissolving in the liquid, but becoming permanent after the addition of a certain quantity of arseniate. This precipitate is an arseniate of chromium. When dry, it has a fine green colour, is tasteless, and insoluble in water.

I dissolved in muriatic acid a quantity of hydrated green oxide of chromium, containing 10 grains of oxide, evaporated the solution to dryness, and re-dissolved the salt in water. This solution was mixed with a solution of 41·5 grains of arseniate of soda (containing 15·5 grains of arsenic acid). This mixture was evaporated to dryness in a porcelain dish, and the saline residue digested in water till nothing more could be taken up. The water did not contain any trace of arsenic acid; but it still retained a green colour, and was found to contain 1·125 grain of green oxide of chromium. The arseniate formed was a compound of

Arsenic acid	-	15·5	- -	8·73	-	$1\frac{1}{8}$ atom.
Green oxide	-	8·875	- or	5	-	1 atom.

An accident prevented me from weighing the arseniate. It was very nearly, though not quite neutral.

There exists also a binarseniate of chromium, which was obtained in the following manner. A quantity of liquid arsenic acid was digested over hydrated oxide of chromium till it refused to take up any more. A dark green liquid was obtained, which was evaporated to dryness. There remained a dark green matter, which being digested in water gave out arsenic acid, and a dark green insoluble salt remained, which was binarseniate of chromium. It was tasteless, and insoluble in water. When heated to redness it became rose red, but on cooling the colour became a dingy buff. This salt was analyzed in the following way.

Six grains by ignition lost 1·45 grains, which were considered as water. 12 grains were dissolved in caustic potash, and the alkali being neutralized by muriatic acid, the green oxide of chromium was thrown down by carbonate of ammonia. The precipitate dried on the filter weighed 4·3 grains: but by ignition the weight was reduced to 2·18 grains. Hence the constituents were

Arsenic acid - -	6·92	or	15·8
Green oxide of chrome	2·18	or	5
Water - - -	2·90	or	6·65
	12·00		

If we consider it as a compound of 2 atoms acid, 1 atom green oxide, and 6 atoms water, the constituents will be

2 atoms arsenic acid - -	15·5
1 atom green oxide - -	5
6 atoms water - - -	6·75
	27·25

numbers which approach very near the actual result of analysis.

7. *Chromate of chromium.*

When chromic acid is digested on hydrated green oxide of chromium, a solution takes place, and the liquid assumes a dark brown colour. When we dissolve as much of the oxide as possible, and evaporate the solution to dryness, a brown insoluble matter is obtained, quite similar in appearance to brown oxide of chromium.

When we mix together solutions of chromate of potash and muriate of chromium, the mixture acquires the same brown colour, and a brown precipitate falls, evidently composed of chromic acid and green oxide of chromium. But this precipitate being soluble in water could not be edulcorated, or freed from the foreign salts with which it was contaminated.

8. *Acetate of chromium.*

When strong acetic acid is digested in a flask over hydrated oxide of chromium, several days elapse before any solution takes place, though the action be assisted by heat. By degrees, however, the acid acquires a green colour, which gradually deepens. I was unable by this process to saturate the acid completely. The smell of acetic acid still continued strong, and the liquid had an acid, though sweet taste. This solution was evaporated to dryness in a low temperature. The matter which remained had a fine dark green colour, but it was insoluble in water, though a solution was again obtained by digesting it in acetic acid.

Acetate of chromium then may be formed, but I was not able to obtain it in a state fit for analysis.

9. *Oxalate of chromium.*

Oxalic acid when digested over hydrated green oxide of chromium, dissolves it with considerable facility. The solution has a deep violet colour, and an excessively sweet taste. When evaporated to dryness it deliquesces again; but if we heat it so as to expel a considerable portion of the water of crystallization, we obtain a greenish-black matter quite tasteless, and similar to charcoal in its appearance. If this powder be digested in water it gradually dissolves, and the solution has the usual colour and taste of a solution of oxalate of chromium. It reddens vegetable blues.

A quantity of oxalic acid crystals, containing 45 grains of real acid, was dissolved in water and digested for several weeks over an excess of hydrated oxide. The quantity of oxide dissolved was exactly 45 grains, and the weight of the oxalate after evaporation to dryness was 120 grains. Hence, the constituents were,

Oxalic acid - -	45	or 5 -	or $1\frac{1}{10}$ atom.
Green oxide - -	45	or 5 -	or 1 atom.
Water - - -	30	or 3.333	or 3 atoms.
	120		

This oxalate was very nearly though not quite neutral. We see here the difficulty in obtaining exact chemical compounds of bodies which are not capable of crystallizing. Probably, I might have rendered the salt neutral by means of a slight admixture of ammonia.

10. *Tartrate of Chromium.*

Tartaric acid gradually dissolves hydrated oxide of chromium, and forms a dark blue-coloured liquid, having the usual sweet acidulous taste which characterises these salts. When the liquid is evaporated to dryness, a black, brittle, tasteless powder remains, which is tartrate of chromium.

To determine the composition of this salt, 148 grains of hydrated oxide (containing 23·58 grains of green oxide), were digested in a flask with 40·2 grains of crystallized tartaric acid, previously dissolved in water. This quantity contains 35·55 grains of real tartaric acid. After several weeks digestion the liquid was filtered. There was separated a light-blue powder, weighing after edulcoration and drying on the filter 24·52 grains. When heated it glowed, and its weight was reduced to 16·64 grains. Thus, 35·55 grains of tartaric acid had dissolved 16·64 grains of green oxide. The weight of the salt was 58·8 grains. Consequently, its constituents were,

Tartaric acid - -	35·55	or 10·68	or $1\frac{1}{4}$ atom.
Green oxide - -	16·64	or 5 -	or 1 atom.
Water - - -	6·61	or 1·99	or 2 atoms nearly.
	58·80		

This salt, like most of the others, contains an excess of acid. My attempts to obtain a neutral tartrate were not successful.

11. *Potash-tartrate of Chromium.*

This salt was accidentally formed in one of my processes to reduce the chromic acid in bichromate of potash to green oxide by means of tartaric acid. The liquid had assumed a deep green colour; but I was surprized that no precipitate could be obtained when ammonia was poured into it; though in a process of the same kind, which I had performed the day before, the green oxide had been readily thrown down by that alkali. On investigating the cause of this anomaly, I found that it depended upon the quantity of tartaric acid employed. If just the proportion requisite for decomposing the chromic acid be used, we can always throw down the green oxide of chromium by ammonia; but when such a quantity of tartaric acid had been employed that a notable portion of it remained undecomposed in the liquid, then no green oxide could be thrown down by ammonia, or by any other alkali. Knowing that the compound metalline tartrates are not precipitable by alkalies, I was naturally led to suspect that I had formed potash-tartrate of chromium. To verify this suspicion, I put bitartrate of potash and protoxide of chromium in the atomic proportions into a flask, with the requisite quantity of water, and digested the mixture for some days. By degrees the oxide entered into combination with the salt, and formed a deep-blue liquid, having a sweet acidulous taste. When evaporated to dryness, there remained a powder almost black and tasteless. It was still soluble in water, unless too much heat had been employed, and possessed exactly the characters of the substance obtained by digesting bichromate of potash with tartaric acid in such quantity as to form the compound not precipitable by ammonia.

This compound salt was subjected to analysis, and its constituents found to be,

2 atoms tartaric acid - -	16.5
1 atom potash - - -	6
1 atom protoxide of chromium	5
2 atoms water - - -	2·25
	29·75

The three first constituents are given exactly as they came out in the analysis. The water actually procured was only

2 grains. But as 29·75 grains of the salt contain 27·5 grains of the anhydrous salts, it is plain that the 2·25 grains wanting to make up the total must be water.

VI. *Chromates.*

In my "First Principles of Chemistry" I have given an account of twenty species of chromates, or combinations of chromic acid with a base. I shall take the present opportunity to give a short account of a few more of these salts which I have had occasion to examine, but without pretending to exhaust the subject.

1. *Perchromates of Iron.*

As the protoxide of iron has the property of reducing chromic acid to protoxide, it is evident that no protochromate can exist; but I thought it worth while to try whether I could not obtain a combination of chromic acid and peroxide of iron. 35 grains of iron were dissolved in dilute nitric acid, the solution was heated on the sand-bath and evaporated to dryness. The red-coloured residue, consisting chiefly of peroxide of iron, was redissolved by means of muriatic acid, and the muriatic solution was made as neutral as possible; first, by evaporation, and then by adding a few drops of ammonia.

125 grains of chromate of potash in crystals were dissolved in water.

These two solutions being mixed together, a dark reddish brown precipitate fell in large flocks. This precipitate was collected on the filter, edulcorated, and dried on the sand-bath. The brown matter thus collected weighed 30·45 grains; but when it had been exposed to a red heat the weight was reduced to 23·368 grains. It had a brown colour with a slight shade of red, was tasteless and insoluble in water, and was not in the least attracted by the magnet.

10 grains of this matter, as it had been dried on the filter, were dissolved in muriatic acid and precipitated by ammonia. The peroxide of iron thus collected being edulcorated, dried and ignited, weighed 6·14 grains. The residual liquid was saturated with acetic acid and precipitated at a boiling temperature by acetate of lead. The chromate of lead obtained weighed after ignition 4·82 grains, equivalent to 1·53 grain of chromic acid. From a preceding experiment, it is plain that the 10 grains of matter analyzed contained 2·33 grains of water; so that the constituents were,

Chromic acid - -	1·53 or	6·5 or	1 atom.
Peroxide of iron -	6·14 or	26·08 or	5 atoms.
Water - - -	2·33 or	9·87 or	9 atoms nearly.
	10·00		

We see that the salt is a compound of 1 atom chromic acid and 5 atoms peroxide of iron. Or it is a pentachromate of iron.

1 atom chromic acid - -	6·5
5 atoms peroxide of iron -	25
	31·5

It would appear from this, that when per-muriate of iron and chromate of potash are mixed in equal atomic proportions, one-half of the peroxide of iron falls united to one-tenth of the chromic acid, forming a pentachromate of iron.

The liquid which passed through the filter had a dark brown colour and was opaque. It was cautiously evaporated to dryness. An orange-coloured matter precipitated, which was sparingly edulcorated, because it was not quite insoluble in water. Of this orange powder I dissolved 10 grains in muriatic acid and threw down the peroxide of iron by ammonia. This precipitate after ignition weighed 4·102 grains. It was strongly magnetic, and therefore the oxidum ferroso-ferricum of BERZELIUS = 4·243 grains of peroxide.

The residual liquid was neutralized by acetic acid and thrown down by acetate of lead. The chromate of lead obtained weighed after ignition 9·46 grains, equivalent to 3 grains chromic acid. Thus, the constituents of the orange powder are,

Chromic acid - -	3·000	or 6·5	or 1 atom
Peroxide of iron -	4·243	or 9·193	or 2 atoms nearly.
Water - - -	2·757	or 5·973	or 5 atoms
	10·000		

Thus, the orange matter was a *diperchromate* of iron.

I could not ascertain the weight of the diperchromate formed; but if we suppose the whole peroxide of iron in the liquid to have been in the state of diperchromate, it is obvious that (supposing it anhydrous) it would amount to

2·5 atoms chromic acid -	16·25
5 atoms peroxide of iron -	25
	41·25

Thus, only about $\frac{1}{3}$ of the chromic acid had combined with the peroxide of iron, the remainder must have still continued in the state of chromate of potash.

2. *Dichromate of Lead.*

Chromate of lead constitutes one of the richest orange coloured pigments in existence; but it is too well known to require any description. The dichromate of lead is easily formed by digesting chromate of lead in caustic potash. Half the chromic acid combines with the potash, the remainder continues united with the oxide of lead. Dichromate of lead has a rich scarlet-colour: like chromate of lead it is tasteless, and insoluble in water. When heated to redness it becomes brick-red, but recovers its original colour on cooling. When digested in nitric acid, one-half of the oxide of lead is dissolved and chromate of lead restored. The following experiment will show the constituents of this salt. 58·96 grains of anhydrous chromate of lead were digested in caustic potash. The undissolved scarlet powder being edulcorated and ignited weighed 49·58 grains.

The potash solution was saturated with acetic acid and thrown down by acetate of lead. The chromate of lead obtained weighed after ignition 29·5 grains. But 29·5 is almost exactly the half of 58.96 grains, the original quantity of chromate of lead employed. It is plain, from this, that just one-half of the chromic acid in the salt had been removed. All the oxide of lead remaining, and the original salt having been neutral, it is clear that the red powder is a compound of

1 atom chromic acid -	6·5
2 atoms protoxide of lead	28
	34·5

3. *Bichromate of Silver.*

In the year 1820 I received a letter from Mr. Dowler, who was at that time in the laboratory at Guy's Hospital, informing me that "if a few drops of dilute nitric or sulphuric acid be added to a solution of chromate or bichromate of potash, so as to disengage the chromic acid; or, indeed, if a little of either of these acids be added to a solution of chromic acid, and, subsequently, a dilute solution of nitrate of silver be added, a precipitate will form, consisting of very small transparent scales, composed, I believe, of chromic acid and silver." He then mentions that he had not analyzed this salt, and that the same salt may be formed by adding dilute muriatic acid to the pulverulent chromate of silver, but not in sufficient quantity to decompose the whole, and applying heat. He mentions also some other cases in which these crystalline scales appeared during his experiments.

I have hitherto abstained from noticing this salt, from a notion that Mr. Dowler might perhaps have some intention of laying the analysis and properties of it before the public. But as six years have elapsed without any farther observations from Mr. Dowler, I think it right to state here the result of my trials to determine its nature.

If we dissolve bichromate of potash in water, and acidulate the solution with nitric acid, nitrate of silver cautiously added will occasion no precipitate; but on setting the liquid aside for 24 hours, if it be sufficiently concentrated, a number of small crystals are deposited: they are sometimes feather-shaped, sometimes oblique, four-sided prisms, terminated by rhomboidal faces, placed obliquely. They are opaque, have a splendent and metallic lustre, and a reddish-brown colour. They are tasteless, and insoluble in water. When digested in potash ley, chromic acid is separated, and oxide of silver remains. By this process I obtained from 2·95 grains of the crystals

Chromic acid - - 1·27 or 13 - 2 atoms.
Oxide of silver - - 1·68 or 17·19 $1\frac{1}{6}$ atom.

I consider the salt as a bichromate composed of

2 atoms chromic acid - 13
1 atom oxide of silver - 14·75
27·75

The small excess of silver in the analysis was probably owing to the digestion in the potash ley not having been continued long enough. The scale was rather too small to entitle me to expect very accurate results.

4. *Potash-chromate of Soda.*

This compound salt is easily formed by adding carbonate of soda to a solution of bichromate of potash in the atomic proportions. The carbonate of soda dissolves with effervescence, and the solution has the yellow colour of neutra chromate of potash. When the liquid is evaporated, crystals of the compound salt are deposited. These crystals have a yellow colour, and are four sided oblique prisms, but so irregular, that it was very difficult to make out the shape. When heated, they decrepitated and became red; but recovered their colour on cooling. 23·57 grains by exposure to a red heat lost 0·57 grain of their weight. Hence the constituents of the salt are

1 atom chromate of potash -	12·5
1 atom chromate of soda -	10·5
½ atom of water - -	0·5625
	23·5625

Though the water happens to amount almost exactly to half an atom, it is probable from the decrepitating property of the salt, that it is only mechanically lodged between the plates of the crystals.

This compound salt differs exceedingly from a mere combination of chromate of potash and chromate of soda. Chromate of potash is anhydrous, but chromate of soda contains 12 atoms of water. All this water is excluded when the compound salt is formed.

5. *Potash-chromate of Magnesia.*

Bichromate of potash, when digested over carbonate of magnesia, dissolves it with effervescence, and gradually assumes the yellow colour of the neutral chromates. This happens when the excess of acid in the bichromate is saturated with magnesia; so that 19 grains of bichromate of potash just dissolve 2·5 grains of magnesia. When the liquid is concentrated, it deposites a crust of the compound salt at the bottom of the vessel. This crust consists of a congeries of small hard crystals, so closely interwoven that it is scarcely possible to make out the shape. Some of the facets appear to belong to octahedrons, while others are more like faces of a prism, seemingly four sided, and rectangular. The colour is a fine yellow, and the salt is not altered by exposure to the air. The taste is bitter and unpleasant. The salt is pretty soluble in water. When heated, the colour becomes red, and it remains so, though with a stronger tint of yellow after the salt has become cold. In a strong red heat it fuses and effervesces, as if a gas were making its escape. The colour becomes brown, and the salt is no longer completely soluble in water.

To determine the composition of this salt, 190 grains of bichromate of potash were dissolved in water, and a quantity of magnesia alba, which contained 25 grains of magnesia, was digested in the liquid till the whole was dissolved. The colour of the solution was now a fine yellow, and no more magnesia would dissolve in it. The liquid being evaporated to dryness, the potash-chromate of magnesia obtained weighed 244·4 grains. Its constituents were obviously

Chromic acid -	130	or	13	2 atoms.
Potash - -	60	or	6	1 atom.
Magnesia - -	25	or	2·5	1 atom.
Water - -	29·4	or	2·94	$2\frac{1}{2}$ atoms.

In another experiment the quantity of water was only 2·12 grains. We may therefore consider the salt as composed of

2 atoms chromic acid - -	13
1 atom potash - - -	6
1 atom magnesia - - -	2·5
2 atoms water - - -	2·25
	23·75

This salt is a compound of 1 atom chromate of potash and 1 atom chromate of magnesia, containing the usual quantity of water of crystallization.

VII. *Chromiron Ore.*

In my "First Principles of Chemistry," vol. ii. p. 55, I express an opinion, that this mineral is a compound of 2 atoms brown oxide of chromium and 1 atom peroxide of iron. The facts stated in this Paper, showing that this opinion was ill founded, it became necessary to endeavour to determine the composition of the mineral by experiment.

The following table exhibits the result of all the analyses of this mineral that have come to my knowledge.

	* Vauquelin.	† Laugier.	‡ Klaproth.	§ Seybert.	¶ Seybert.	‖ Berthier.	‖ Berthier.	Thomson.	Thomson.
Green oxide of chrome	43	53	55·5	39·514	51·562	36	51·6	47·92	52·45
Peroxide of iron - -	34·7	34	33	36·004	35·140	37·2	37·2	34·08	24·33
Alumina - - - -	20·3	11	6	13·002	9·723	21·8	9·7	11·76	17·80
Silica - - - - - -	2·0	1	2	10·596	2·901	5	2·9	1·06	2·29
White matter - - -	......	......	......	......	......	......	......	4·18	1·61
Moisture - - - -	......	......	......	......	......	......	......	1	
	100	99	96·5	99·116	99·326	100	101·4	100	98·48

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* Jour. de Mines, x. p. 521. The specimen was sent from the departments of the Var. No description is given. Sp. gr. 4·0326.

† Philosophical Magazine, xxiv. 7. The specimen was from the Uralian mountains. Sp. gr. 4·0579.

‡ Beitrage, iv. 132. The specimen was from Styria. Sp. gr. 4·5.

§ Silliman's Jour. iv. 321. The specimen was from the Bare Hills, Baltimore. Sp. gr. 4·0639.

¶ Ibid. p. 323. The specimen was from Chester, Pensylvania.

‖ Ann. de Chim. et de Phys. XVII. 55. The first specimen from Isle de Vache, near St. Domingo; the second from the neighbourhood of Philadelphia. Obviously the same as Seybert's second specimen.

The first of the specimens in the table analysed by me was from the Bare Hills, Baltimore; its sp. gr. was 3·949. Its constitutents differed so much from those given by Seybert of a specimen from the same place, that I was induced to repeat the analysis four times. The results did not coincide with each other, but all gave nearly the same ratio between the green oxide of chromium and the peroxide of iron.

The second of the specimens analysed by me was from the Shetland Isles. Its specific gravity was 4·164 when purified by floating off the lightest portions, after reducing the mineral to powder.

My method of analysis was this: the mineral was reduced to a fine powder, and heated in a silver crucible with a mixture of caustic potash and nitre, amounting to thrice the weight of the powder. I usually operated on 25 grains of the mineral. After keeping the mixture in a state of fusion for half an hour, it was raised to a red heat, and kept in that temperature for about ten minutes; it was then allowed to cool, and lixiviated in water. A fine yellow alkaline liquid was obtained. The undissolved portion being digested in muriatic acid, the whole dissolved except about $\frac{1}{4}$ grain of brown matter, obviously consisting of ore that had escaped decomposition. It was deducted from the portion of ore subjected to analysis.

The alkaline solution was supersaturated with acetic acid, and then mixed with carbonate of ammonia in slight excess. The alumina was thrown down. After washing and ignition it was dissolved in muriatic acid to separate a little silica with which it was mixed.

The muriatic solution was concentrated, and then diluted with water. It became milky, and deposited a small quantity of white matter, which possessed the following properties:

Its colour was white, it was tasteless, and insoluble in water: it dissolved in muriatic acid, and was again precipitated by dilution. When digested in caustic potash it became black. This black matter again dissolved slowly in muriatic acid, and was precipitated white by potash. I had never more of it at once than half a grain. It seems to be a metalline salt. The characters agree best with a salt of base of manganese. The acid is neither the chromic, phosphoric, nor arsenic. This matter existed in all the specimens of chrome iron ore that I examined. The muriatic solution thus freed from the white matter was precipitated by caustic ammonia. The peroxide of iron after washing and ignition was dissolved in muriatic acid, to separate a little silica with which it was mixed.

Finally, the chromic acid solution was mixed with an excess of acetic acid, and the chromic acid thrown down by acetate of lead.

A glance at the preceding table, shows clearly that no deduction can be drawn from the analyses which it exhibits, because the ore was never pure. There is reason to believe that, in most cases, it is mixed with more or less of octahedral iron ore, besides the earthy matter with which it is obviously contaminated. I therefore reduced a quantity of the Baltimore ore to a coarse powder, and picked out a number of the small octahedrons, which constituted the pure chromium ore. They had more or less of the form of octahedrons, a brown colour with a splendent and almost metallic lustre. The specific gravity was 4·321. All the particles the least acted on by the magnet were scrupulously withdrawn. 25 grains of this pure ore being subjected to analysis, I obtained the following constituents:

Green oxide of chrome -	52·95	or	10
Peroxide of iron - -	29·24	or	5·52
Alumina - - -	12·22	or	2·30
White matter - - -	3·09		
Water - - - -	0·70		
	98·2		

A trace of silica.

If we exclude the unknown salt, which I have called white matter, we see that the ore is a compound of

2 atoms green oxide of chrome	10
1 atom peroxide of iron - -	5
1 atom alumina - - -	2·25

The very slight excess of iron and alumina, together with the trace of silica, were probably derived from some small green particles (obviously the gangue of the ore), which I could detect with a glass in the portion of the picked ore that had not been pounded. It is more than likely that one or two of them had got mixed with the pounded ore.

From the analysis of the Shetland ore, in the table, it seems to follow, that it contains no admixture of octahedral iron ore; for in it alone the green oxide of chromium, and the per oxide of iron, bear to each other the ratio of 2 to 1. All the other specimens subjected to analysis have been contaminated with iron ore.

APPENDIX.

Since the preceding paper was drawn up, I have found that when green oxide of chromium is precipitated by a fixed alkaline carbonate, we cannot free it from the fixed alkali by washing. This probably occasioned the want of success which attended some of the experiments related in the second section of the paper.

XV. *Rules and principles for determining the dispersive ratio of glass; and for computing the radii of curvature for achromatic object-glasses, submitted to the test of experiment.* *By* PETER BARLOW, *Esq. F.R.S. Mem. Imp. Ac. Petrop. &c.*

Read May 3, 1827.

1. IT is very remarkable, since the achromatic telescope is altogether of English origin, that in no one of our separate optical treatises are to be found specific rules for its construction, fitted for the use of practical opticians. Some essays of this kind have indeed been attempted; the first of which is found in MARTIN's "New Elements of Optics," published in 1751; but the principle there adopted is erroneous, and of course the deductions, although possessing a great appearance of simplicity, are wholly useless. Under the article Telescope, in the Encyclopædia Britannica, is another essay of this kind, which is correct in principle, but far from possessing the degree of simplicity which is desirable for practical purposes.

Under the like article in Rees's Cyclopædia is a treatise on the same subject, which may be considered wholly practical; it is founded however upon MARTIN's method, but corrected by an empyrical multiplier, which answers remarkably well in many instances, but is erroneous in all extraordinary cases.

Lastly, an elaborate and highly scientific investigation relative to these constructions was published by Mr. HERSCHEL, in the Phil. Trans. for 1821, to which I shall refer more at length in a subsequent page. These, I believe, constitute every attempt that has been made in this country to bring the strict laws of optics, applicable to these cases, within the reach of numerical calculation.*

More numerous attempts have been made by foreign mathematicians; but as far as my knowledge of them extends, they have in no instance been attended with the success that might have been expected from the deservedly high reputation of their authors.

I have spoken above principally of the methods of determining the radii of curvature of the lenses; but in order to enter upon this calculation, certain data are necessary, which require previous experiments and tedious numerical computations; so that upon the whole, to take two specimens of glass of unknown indices and dispersions, to form an object glass of them, free from colour and spherical aberration, requires very formidable calculations, involving in them, according to the best methods yet employed, certain principles and operations which we ought hardly to expect practical opticians to be masters of. At all events, every simplification that can be thrown into experiments and calculations of this kind must be desirable; and, I am greatly in hopes it will be found that I have, in the following pages, contributed

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* Since this Paper was written, Mr. Herschel has also published in the Encyclopædia Metropolitana, under the article Light, a still more extended investigation relative to this and other optical subjects; to which article it will likewise be necessary for me to refer as we proceed; and if, after all, any reference should be omitted which ought to be made, it must be attributed to this Paper having been written before the publication of the former.

a little towards this object. Probably, also, the immediate comparison of the computed results, with experiments on a large scale, will add a value to this Paper, which it might not otherwise have been thought to possess, and for which I am indebted to Messrs. W. and T. Gilbert, who very liberally engaged to submit to the test of experiments any theoretical deductions I might be led to in an investigation of these subjects.

On the determination of the index of refraction.

2. The following method of determining the index of refraction, by means of a lens, is not given as new; it has, on the contrary, been long practised; but as it forms the foundation of the method for determining the dispersive ratio, and will occupy but a few lines, I shall be excused for introducing it into this Paper.

It is simply this:—since by knowing the radii of curvature of a lens, and its index of refraction, we may compute the focal length; so conversely, by knowing the radii and measuring the focal length, we may compute the index of refraction.

The method which we employed for measuring the focal length of a lens, was as follows: a tube about $2\frac{1}{2}$ inches in diameter, and which exactly measured 10 inches from the back of the lens to its other extremity, was fitted with a draw tube of the same length, graduated to inches and tenths, and which, by means of a vernier, might be read to the hundredth of an inch. This was fitted with a positive eye-piece, which was adjustable to bring the cross wires exactly into its focus, and the graduations above-named commenced from this point or stop. A board about two feet square, covered with black crape, and having a clean circular piece of card paper, with fine cross lines upon it, was placed at a convenient measured distance from the lens, and then the draw tube was adjusted till we had the focus exactly coincident with the cross wires. This is easily ascertained, by moving the eye a very little upwards and downwards. Then, when the image does *not fall exactly* on the wires, this motion of the eye will produce an apparent motion between the cross wires on the telescope and those on the card; but when they are coincident, then, however the eye may be moved, the image and the cross wires will be at rest. This being determined, the focal length for this distance of the object is read off as above described. Let this focal length be f, the distance d, radii r, r', and index $1 + a$; then, by a simple inversion of the well known formula for the focal length of a lens, we have $a = \frac{r r'}{r + r'} \left(\frac{1}{f} + \frac{1}{d}\right)$.

But as for these experimental purposes we made the radii equal, or $r = r'$, this formula became simply in this case

$$a = \frac{r}{2} \left(\frac{1}{f} + \frac{1}{d}\right) \quad - \; - \; - \; - \; - \; - \quad (1).$$

The only possible source of error this method involves, is in the measurement of the tools; but this, from repeated experiments, we found might always be determined to within less than a five hundredth part of the radius, which can only affect the result to the amount of about $\frac{1}{1500}$th part of the index.

*Method of determining the dispersive ratio.**

3. The instrument employed for this purpose is similar to that used for determining the index, except that the tube, instead of being only ten inches in length, consists of three joints, one 20 inches, and two others 10 inches each, the draw tube being about 14 inches long (graduated as before); so that the length may be conveniently varied between 20 and 50 inches. The cell, which carries one of the lenses at the extremity of this tube, screws inside *flush* with the tube itself, and will thus admit another tube about 20 inches long to slide over it; at the extremity of the latter is another cell for carrying the plate lens.

This exterior tube is moved over the other by means of a tangent screw and handle, with HOOK's universal joint, as in the adjustment of transit and other instruments. Moreover, the exterior tube being opened for the space of two inches, and the interior tube graduated, the distance of the two lenses from each other may always be read off to the hundredth of an inch.

The instrument being thus described, the method of using it, and the principle on which the determination rests, will be readily understood. It is well known that in order to produce

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* I am not aware that this very simple method of determining dispersive ratios has been before practically employed; but it is suggested by Mr. HERSCHEL in his recent article refered to in a former page. He deduces it from considerations relative to the achromaticity, when the two lenses of an object-glass are placed at a distance from each other; his primary object being to complete any trifling want of correction by a change of distance. My views were not very dissimilar, and our resulting equations, although differently expressed, are of course equivalent.

achromatism in an object glass, it is requisite that the focal lengths of the two lenses be to each other in the ratio of their dispersive powers; that is, the ratio of the dispersive power of the flint being to that of the plate as $1 : d$, the focal length of the flint must be to that of the plate also as $1 : d$, the two lenses being in contact.

If therefore we have two lenses, viz. a concave flint, and a convex plate, in which the focal length of the latter bears a greater ratio to that of the former than 1 to d, we must open the two lenses from each other till the required ratio is obtained, when the object will be colourless, and therefore conversely, when the image is colourless, we shall be sure that the ratio of the focal lengths will be that also of the dispersive powers.

To illustrate this a little more particularly, let f, f' be the focal lengths of the plate and flint lens, and let δ be the distance of the lenses when the image is colourless. Then, first, it is obvious, that the effect will be the same as if we had a plate lens in contact with the flint, which had for its focal length $f - d$, but the actual quantity of its dispersion that due to the whole focal length f; that is, the same as a plate lens of focal length $f - \delta$, and whose dispersive power $= \frac{fd}{f-\delta}$.

And since in this state the image is colourless, it follows that

$$f' : f - \delta :: 1 : \frac{fd}{f-d}.$$

And therefore d, which is the quantity sought, is found from the equation

$$d = \frac{(f-\delta)^2}{ff'} \quad - \; - \; - \; - \quad (2).$$

The lenses we employed were about $2\frac{1}{2}$ inches in diameter, equally convex in the plate, and plano concave in the flint; their focal lengths varying in the plate and crown from $9\frac{1}{2}$ to $10\frac{1}{2}$ inches, according to their respective refractive indices, and in the flint from about 10 to $11\frac{1}{2}$ inches.

4. The flint lens, as will have been observed from the preceding description, is placed in the interior tube, and the plate in the exterior; and if when the two interior faces of the lenses are in contact, the index does not read zero, its actual reading is recorded; and ultimately, the index reading when the image is colourless is corrected by this quantity.*

This preliminary being attended to, the manner of conducting the experiments is as follows.

Fix up the black board with the circular white spot, as already described, at a convenient distance, and in a good light, directly opposite the tube properly mounted on its stand.

Let the two lenses be placed nearly in contact, and suppose the length of the tube reduced to about 20 inches. Now, move the plate lens gently forward by means of the handle and screw, the eye being placed at the eye-glass, and the image of the circular spot will, after a time, begin to appear in the field of the telescope, having a bright and strong violet spot in the middle; at this time a very little farther motion in the plate lens will give a distinct image of the object, but encircled by a strong violet shade.

If now the tube be lengthened to about 25 inches, and the experiment repeated by closing the glasses, the violet spot in the middle, and the circular ring when the focus is obtained, will have changed to a fine blue. If again we lengthen the

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* The different thicknesses of the lenses render the correction necessary.

tube considerably, that is to nearly 50 inches, we shall find by repeating the experiment, that is, by closing still more the two glasses, that the circular spot before the image is formed, and the surrounding atmosphere when the focus is obtained, will be red, orange or yellow; and between these extremes a focal length will be found where the circular spot in the middle will lose all distinguishing colour, showing itself a bright white; and when in the focus the image will be colourless, although surrounded by a visible atmosphere, principally proceeding from a want of spherical correction.

At this time the glasses are corrected for dispersion, and the compound focal length measured from the back of the flint, and the distance of the glasses must be accurately read off; and with these data the dispersion may be obtained by the formulæ already given, viz.:

$$d = \frac{(f-\delta)^2}{ff'} \quad - \ - \ - \ - \quad (2)$$

In this expression f is the focal length of the plate lens for the given object, and f' the focal length of the flint for parallel rays. The former may be found by direct observation with the index instrument, as already described, but the latter is best determined by means of the compound focus; that is, calling the compound focus f'', we shall have

$$\frac{1}{f'} = \frac{1}{f''} - \frac{1}{f-\delta} \quad - \ - \ - \ - \quad (3);$$

and f' being thus determined, is to be employed in the preceding formula.

As an example: suppose the compound focus measured from the flint to be 34·89 inches, the focal length of the crown lens 9·85 inches, and the distance between the lenses 1·41 inches.

$$\text{First } \frac{1}{f'} = \frac{1}{38\cdot89} - \frac{1}{8\cdot44},$$

$$\text{Whence } f' = 11\cdot13 \text{ inches.}$$

$$\text{Then } d = \frac{\overline{f-\delta}^2}{ff'} = \frac{8\cdot44^2}{9\cdot85 \times 11\cdot13} = \cdot649,$$

the dispersive ratio sought.

On the computation of the radii for correcting spherical aberration and colour.

5. We have seen, that to render an object-glass achromatic, it is only necessary to have the focal lengths in the direct ratio of the dispersive power of the two glasses.

Let this ratio be $1 : d$; then representing the compound focal length by f'', we shall have

$$f = f''\,(1 - d) = \text{focal length of plate.}$$

$$f' = f''\,\frac{(1-d)}{d} = \text{focal length of flint.}$$

And these focal lengths, without any other condition, will give a compound focal length f'', and produce achromatic correction.

Let $1 + a =$ index of plate
$1 + a' =$ index of flint,
r, r' the radii of curvature in the plate,
r'', r''' those of the flint.

The order of the radii reckoned from the object side being r, r', r'', r''', the two former being convex, and the latter concave, unless the contrary be stated.

Then by a known formula we shall have

$$\frac{1}{r} + \frac{1}{r'} = \frac{1}{f\,a}$$

$$\frac{1}{r''} + \frac{1}{r'''} = \frac{1}{f'\,a'}.$$

Here then are four quantities to be determined, and only two equations; so that if the condition of being achromatic was the only one, we might have any variety of answers at pleasure; but it is also required that the object-glass shall be free from spherical aberration, which is still only a third condition; and therefore, even with this, the question may still be considered as admitting of various solutions. But a fourth condition may be that the two interior surfaces shall be either actually equal, or very nearly so. And this last condition serves to bring the solution within very narrow limits, although it is still not strictly limited, unless we insist upon perfect contact surfaces, or some other specific condition.

Mr. HERSCHEL, in his very elaborate and valuable Paper on this subject in the Phil. Trans. Part II. 1821, instead of this last condition, has taken another, viz. "the destruction of aberration, not only for parallel rays, but also for rays diverging from a point at any finite distance."

The resulting equations by the introduction of this condition, make the radii of the two interior surfaces nearly equal; but in several cases the convex side is the deeper, and the two surfaces therefore *ride* in the middle, unless separated at the edges by paper, or some other substance interposed between them, which by many practical opticians is considered objectionable.* The contact surfaces are also in this construction deeper, and the actual quantity of aberration in either lens to be corrected is greater than would otherwise be necessary. Moreover, by insisting upon any fourth condition,

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* Mr. HERSCHEL suggests that the best way would be in all cases to frame each lens into a separate cell, and to adjust them to each other by screws. In this case, of course, it would be indifferent which of the two were the deeper surface.

equally rigid with the other three, the workman is restricted to a very exact accordance in the measure of all his four tools, and it leaves him no opportunity of matching a good flint lens with a plate, or a plate with a flint, which is in many cases a desirable convenience. I have not therefore insisted rigidly upon a fourth condition, but have made this subservient to the above convenience, by only requiring that the contact surfaces shall be either exactly, or nearly equal, and the concave the deeper, when there is any difference between them. The optician is thus enabled to make a choice, within certain limits, of the radii of one of his lenses, and has only to match the other to it. By this means the intricate equation arising out of the fourth condition is avoided. I am quite aware, that in this way a great sacrifice is made of analytical elegance; but as my object has been to bring the calculation fully within the reach of such practical opticians as have no pretensions to a knowledge of analysis, I have prefered a simple, although somewhat indirect method of computation, to one more direct and refined, but at the same time more intricate and laborious. The principle here proposed will be illustrated in the following paragraphs.

The investigation of the aberration produced at one spherical surface is found in most of our optical treatises, and need not therefore be repeated in this place; of these expressions, that which is given in Wood's Optics (art. 397) is perhaps one of the most simple. I shall therefore adopt this, and refer the reader to the work itself for the investigation.

Let d be the distance of a radiant point from a spherical convex surface of a denser medium whose radius is r, and semidiameter y; let also the sine of incidence to the sine of refraction be as 1 to 1 + *a*; that is, let the refractive index be 1 + *a*; then it is shown in the work above quoted that the aberration, *in this medium*, will be

$$p = \frac{a(d+r)^2}{(ad-r)^2} \times \frac{d+(a+2)r}{(a+1)d} \times \frac{y^2}{2r} \quad - \quad - \quad (4)$$

This expression is for the case of diverging rays on a convex surface of a denser medium; but it will apply to the case of a concave surface by merely changing all the signs of *r*. For parallel rays, *d* must be considered infinite; and for converging rays, *d* must be taken negative; so that this expression is general in all cases where the rays enter a denser medium.

When the rays pass from a dense to a rare medium, the ratio is $1 + a : 1$; but this, to be rendered symmetrical, must be reduced to $1 : 1 - b$, where $b = \frac{a}{1+a}$: substituting therefore in the above, every where $-b$ for a, we obtain for the case of diverging rays on a convex spherical surface,

$$-p = \frac{-b(d+r)^2}{(bd+r)^2} \times \frac{d+(2-b)r}{(1-b)d} \times \frac{y^2}{2r}$$

And the expression for converging rays on a concave surface is precisely the same, except in the sign of the last factor; because both *d* and *r* changing from positive to negative, leave the expression precisely the same, with the above exception; it becomes therefore in this case

$$p = \frac{b(d'+r')^2}{(bd'+r')^2} \times \frac{d'+(2-b)r'}{(1-b)d'} \times \frac{y^2}{2r'}, \quad - \quad - \quad (5)$$

merely writing *d'* and *r'* for *d* and *r*, for the sake of distinguishing between the two formulæ.

7. Now, in order to find the aberration of a lens, as caused by the refraction at the second surface, which is equivalent to the rays falling upon the spherical surface of a rarer

medium, the latter expression must be employed, in which therefore d' is not indefinite as in the former case, but is dependent upon the refraction at the first surface of the lens, being the distance to which the rays converge in consequence of that refraction; d' therefore in this case, by a well known expression for the focus of the rays at one surface, is

$$d' = \frac{(a+1)\,d\,r}{a\,d - r} \quad - \quad - \quad - \quad (6)$$

Where d must be taken positive or negative accordingly as the rays first diverge or converge, and r must be positive or negative as the first surface is convex or concave, and this value of d' substituted in equation (5), will give that part of the aberration which depends upon the rays, impinging on the second surface. But there is also another part depending upon the aberration of the first surface; for as the rays in consequence of the first aberration do not all converge to the distance d', whereas we have computed the second case as if they did, there will be an aberration on that account.

Let the aberration produced at the first surface be x; then the consequent aberration at the second surface will be (WOOD'S Optics, Art. 405.)

$$\frac{(1-b)\,r'^2}{(b\,d'+r')^2} \times x$$

And hence the entire aberration produced with diverging rays by a convex lens from a distance d, the radii being r, r', will be expressed by

$$\left.\begin{array}{l} \dfrac{a\,(d+r)^2}{(a\,d-r)^2} \times \dfrac{d+(a+2)\,r}{(a+1)\,d} \times \dfrac{(1-b)\,r'^2}{(b\,d'+r')^2} \times \dfrac{1}{2r} \\ + \dfrac{b\,(d'+r')^2}{(b\,d'+r')^2)} \times \dfrac{d'+(2-b)r'}{(1-b)\,d'} \times \dfrac{1}{2r'} \end{array}\right\} \times y^2 = p$$

And by substituting for b its value $\frac{a}{a+1}$, and making $\frac{d}{r'} = c$, $\frac{d'}{r'} = c'$, and $\frac{r}{r'} = q$, this reduces first to

$$\left.\begin{matrix} \dfrac{a(c+q)^2}{(ac-q)^2} \times \dfrac{c+(a+2)q}{c(ac'+a+1)^2} \times \dfrac{1}{2r} \\ + \dfrac{a(c'+1)^2}{(bc'+1)^2} \times \dfrac{(c'+2-b)}{c'} \times \dfrac{1}{2r'} \end{matrix}\right\} \times y^2 = p;$$

and ultimately to

$$\left.\begin{matrix} \dfrac{(c+q)^2}{(ac-q)^2} \times \dfrac{c+(a+2)q}{c(ac'+a+1)^2} \\ + \dfrac{(c'+1)^2}{(bc'+1)^2} \times \dfrac{(c'+2-b)q}{c'} \end{matrix}\right\} \times \frac{ay^2}{2r'} = p \quad - \quad - \quad (7).$$

And this I believe is the simplest form to which the general expression for the aberration of a single lens can be reduced.

In the above form it applies to the case of diverging rays and for a double convex lens; but it may be rendered applicable to every other case by attending to the proper signs of *d*, *r*, and *r'*; *d* being negative for converging rays, and *r*, *r'* being positive or negative accordingly as they are either or both convex or concave.

(8). When the distance is infinite or the rays parallel, then *c* being infinite, this expression becomes

$$\left.\begin{matrix} \dfrac{1}{a^2} \times \dfrac{1}{(ac'+a+1)^2} \\ + \dfrac{(c'+1)2}{(bc'+1)^2} \times \dfrac{(c'+2-b)q}{c'} \end{matrix}\right\} \times \frac{ay^2}{2r'} = p;$$

and since also in this case

$$d = \frac{(a+1)r}{a} \text{ and } c' = \frac{(a+1)q}{a},$$

this equation after farther reduction, that is, after substituting *c* in terms of *a*, becomes

$$\frac{\left.\begin{matrix} a^4 \\ 4a^3 \\ 6a^2 \\ 4a \\ +1 \end{matrix}\right\} q^2 + \left.\begin{matrix} 2a^4 \\ 6a^3 \\ 5a^2 \\ -1 \end{matrix}\right\} q + \left.\begin{matrix} a^4 \\ 2a^3 \\ +1 \end{matrix}\right\}}{\left.\begin{matrix} a^3 \\ 2a^2 \\ a \end{matrix}\right\} (q+1)^2} \times \frac{y^2}{2af} = p$$

Observing that $\frac{1}{2af} = \frac{1}{2r} + \frac{1}{2r'}$, f being the focal length; or writing A, B, C, D, for the several coefficients, making also $p' = 2paf$, and calling $y = 1$, it is

$$p' = \frac{Aq^2 + Bq + C}{D(q+1)^2} \quad - \; - \; - \; - \quad (8).$$

9. This equation in the common form of object-glasses belongs only to the plate or crown lens, which receives the direct or parallel rays; therefore the value of a, which enters into it, may always be considered to fall within the limits $a = {\cdot}50$, and $a = {\cdot}53$.

When $a = {\cdot}50$, this in numbers reduces to

$$\frac{4{\cdot}5\,q^2 + q + 1{\cdot}16}{(q+1)^2} = p',$$

and the solution gives

$$q = \frac{p' - {\cdot}50 \pm \sqrt{\left\{(p' - {\cdot}50)^2 + (p' - 1{\cdot}16)(4{\cdot}5 - p')\right\}}}{4{\cdot}5 - p'} \quad - \; - \; - \quad (9).$$

When $a = {\cdot}51$,

$$q' = \frac{p' - {\cdot}53 \pm \sqrt{\left\{(p' - {\cdot}53)^2 + (p' - 1{\cdot}146)(4{\cdot}47 - p')\right\}}}{4{\cdot}47 - p'} \quad - \; - \quad (10).$$

When $a = {\cdot}52$,

$$q' = \frac{p' - {\cdot}56 \pm \sqrt{\left\{(p' - {\cdot}56)^2 + (p' - 1{\cdot}127)(4{\cdot}44 - p')\right\}}}{4{\cdot}44 - p'} \quad - \; - \quad (11).$$

When $a = {\cdot}53$,

$$q' = \frac{p' - {\cdot}58 \pm \sqrt{\left\{(p' - {\cdot}58)^2 + (p' - 1{\cdot}11)(4{\cdot}42 - p')\right\}}}{4{\cdot}44 - p'} \quad - \; - \quad (12).$$

10. Having thus (equat. 7.) found a general expression for the aberration of a lens when the rays emanate from a given point, and in (equat. 8.) the expression for the aberration of a lens receiving parallel rays, the indirect method by which an equal and contrary aberration in the two lenses is produced may be thus described.

Since, when the aberration in the flint lens is so proportioned as to counteract that of the plate, all the rays converge to the mean focal point; so, conversely, if we suppose rays emanating from that focal point, they will have precisely the reverse route, after their refraction at the flint lens, to that which they have when they are converging towards it in an opposite direction from the plate lens; consequently, the aberration of the flint lens for rays emanating from the mean or compound focus, must be equal to the aberration from the plate lens for parallel rays in the opposite direction. And in the former case, when the amount of aberration has been ascertained, this will be also that due to the latter; whence the ratio of the surfaces which will produce this aberration, or the value of q may be computed by means of the general quadratic equation (8.) or the particular equation belonging to the given plate index. Hence we may proceed as follows:

11. *First, to compute the aberration of the flint lens for rays assumed to emanate from the compound focal point.*

Here the distance $= f''$, index $= 1 + a'$, dispersion $= d$; and let the radii of the surfaces (for distinction sake) be r''', r''; and the ratio of $r'' : r''' :: 1 : q$. Then

$$f = f'' (1 - d) = \text{focal length plate.}$$

$$f' = f'' \left(\frac{1-d}{d}\right) = \text{focal length flint.}$$

$$r''' = f' a' (q' + 1) = \text{outside surface flint.}$$

$$r'' = f' a' \left(\frac{q'+1}{q'}\right) = \text{inside surface flint.}$$

Whence again, $d' = \dfrac{(a'+1) f'' r'''}{a' f'' - r'''}$ - - (13.)

$$c' = \frac{d'}{r''} = \frac{(a'+1) f'' q'}{a f'' - r'''},$$

$$c = \frac{f''}{r''}, \; b = \frac{a'}{a'+1},$$

are all known quantities; and consequently,

$$\left.\begin{array}{l} \dfrac{(c+q')^2}{(a'c-q')} \times \dfrac{c+(a'+2)\,q'}{c\,(a'c'+a'+1)^2} \\ + \dfrac{(c'+1)^2}{(bc'+1)^2} \times \dfrac{(c'+2-b)\,q'}{c'} \end{array}\right\} \times \frac{a'\,*}{2\,r'''} = p$$

is also known. This is the amount of aberration for the flint lens for rays supposed to diverge from the compound focal point; and this, as we have seen, is also the amount of the aberration of the plate lens for parallel rays in an opposite direction; but this latter is equal to $\frac{p'}{2fa}$ (art. 8.). Multiplying therefore the last found value of p by $2fa$, and substituting for f'', r'', and r''' in the preliminary equations, we obtain

$$b = \frac{a'}{(a'+1)} \quad c' = \frac{d\,q'}{b\,(q'+1-d\,q')} \quad c = \frac{d\,q'}{a'\,(1-d)\,(q+1)}\,;$$

and lastly,

$$\left.\begin{array}{l} \dfrac{(c+q')^2}{(a'c-q')^2} \times \dfrac{c+(a+2)\,q'}{c\,(a'c'+a+1)^2} \\ + \dfrac{(c'+1)^2}{(bc'+1)^2} \times \dfrac{(c'+2-b)\,q'}{c'} \end{array}\right\} \times \frac{a\,d}{q'+1} = p' \text{ - } (14).$$

And this value of p' substituted in equations (8), will furnish the proper value of q for the ratio of the radii of the surfaces of the plate lens; and we shall then have

$f = f''\,(1-d) =$ equal focal length of plate.

$f' = f'''\left(\frac{1-d}{d}\right) =$ equal focal length of flint.

$$\left.\begin{array}{l} r = f\,a\,(q+1) = \text{1st surface} \\ r' = f\,a\left(\dfrac{q+1}{q}\right) = \text{2nd surface} \end{array}\right\} \text{plate.}$$

$$\left.\begin{array}{l} r'' = f'd'\left(\dfrac{q'+1}{q\ 1}\right) = \text{3d surface} \\ r''' = f'd'(q'+1) = \text{4th surface} \end{array}\right\} \text{flint.}$$

---

* The value of y being the same in both lenses is omitted, or considered as unity in both expressions.

The latter r''', being concave or convex accordingly as q' is positive or negative.

It is to be observed, however, that if in these results r' should be less than r'', or if it should exceed it too much, so as to leave the contact surfaces too wide, a new supposition of the value of q' must be made till the required approximation, stated in our fourth condition (art. 6.) is obtained.

12. It fortunately happens that the most laborious part of the above operation, viz. the solution of the equation from which the value of q in the plate lens is obtained, is readily reduced to a tabulated form, whereby this calculation is altogether avoided. This is done in the following short table, in which all the more practicable values of q are given for the several indices $a = {\cdot}500$, $a = {\cdot}510$, $a\ {\cdot}520$, and $a = {\cdot}530$; and it will be seen that so little change takes place in the value of q, for these changes of indices, that the number answering to any value of a between these limits may be readily found by simple proportion.

In this Table p represents the amount of aberration as determined by equation (14), and in the adjacent column is given the corresponding value of q: as to the method of using the table it will be readily comprehended from an example.

13. *Table showing the aberration of a lens for parallel rays to the four different indices* ·1500, ·1510, ·1520, ·1530.

p = the value of aberration.	Ratio of surfaces, or values of q for different indices.			
	1.500 q	1·510 q	5·20 q	5·30 q
1·05	Imaginary.	Imaginary.	Imaginary.	·180
1·10	·292	·291	·296	·303
1·15	·380	·374	·377	·380
1·20	·445	·445	·446	·447
1·25	·515	·510	·506	·509
1·30	·572	·570	·568	·567
1·35	·635	·627	·625	·626
1·40	·689	·686	·683	·683
1·45	·750	·743	·740	·740
1·50	·803	·800	·798	·798
1·55	·865	·858	·855	·855
1·60	·921	·917	·913	·913
1·65	·979	·975	·973	·972
1·70	1·042	1·037	1·034	1·034
1·75	1·103	1·098	1·096	1·095
1·80	1·166	1·161	1·159	1·159
1·85	1·230	1·226	1·223	1·223
1·90	1·296	1·292	1·289	1·290
1·95	1·360	1·356	1·357	1·359
2·00	1·434	1·431	1·429	1·430
2·05	1·506	1·503	1·503	1·504
2·10	1·581	1·578	1·579	1·580
2·15	1·658	1·657	1·658	1·659
2·20	1·738	1·738	1·738	1·741

Example.

14. Let it be proposed to compute the curves for an achromatic object-glass; the data being as below, viz.

Index of plate = 1·515, of flint = 1·600,
Dispersive ratio = ·66, diameter $5\frac{1}{2}$ inches, focus 80 inches.

Here $a = \cdot 515$, $a' = \cdot 600$, $d = \cdot 66$, $f'' = 80$.

First $f''(1 - d) = 80 \times \cdot 34 = 27\cdot 20 = f$,

and $f'' \frac{(1 - d)}{d} = \frac{80 \times \cdot 34}{66} = 41\cdot 21 = f'$.

We have thus the focal length of the plate and flint lens. Assume now for the ratio of the radius of the inside surface to the outside surface of the flint 1 : 10, that is,

$$\text{Let } r'' : r''' : : 1 : 10 \text{ or } q' = 10 \qquad \text{then}$$

$$r'' = f'\,a'\,\frac{(1+q')}{q'} = \frac{41{\cdot}2 \times {\cdot}60 \times 11}{10} = 27{\cdot}19 \text{ concave,}$$

$$r''' = f'\,a'\,(1+q') = 41{\cdot}2 \times {\cdot}60 \times 11 = 271{\cdot}9 \text{ concave.}$$

Therefore to find the values of the letters employed in formula (14.) for the aberration, we have, $r'' = -27{\cdot}19$ $r'' = -271{\cdot}45$,

$$q' = 10,\ c' = \frac{(a'+1)\,f''\,q'}{a'\,f'' - r'''} = 4{\cdot}03$$

$$c = \frac{f'}{r''} = \frac{80}{-27{\cdot}19} = -2{\cdot}94$$

$$b = \frac{a'}{a'+1} = \frac{{\cdot}6}{1{\cdot}6} = {\cdot}375.$$

These values substituted in the expression

$$\left.\begin{matrix} \dfrac{(c'+q')^2}{(a'c-q')^2} \times \dfrac{c+(a'+2)\,q'}{c\,(a'c'+a'+1)^2} \\ + \dfrac{c'+1)^2}{(b\,c'+1)^2} \times \dfrac{(c+2-b.)\,q'}{c'} \end{matrix}\right\} \times \frac{a\,d}{(q'+1)} = p$$

give in numbers

$$\left.\begin{matrix} \dfrac{49{\cdot}84}{138{\cdot}3} \times \dfrac{23{\cdot}06}{-47{\cdot}45} \\ + \dfrac{25\ 30}{2{\cdot}283} \times \dfrac{56{\cdot}55}{4{\cdot}03} \end{matrix}\right\} \times \frac{{\cdot}515 \times {\cdot}66}{11} = 1{\cdot}73 = p.$$

If now we were to employ the equations given for determining the value of q, this value of p must be substituted in the equation answering to $a = {\cdot}515$; but making use of the table, we must find the nearest value to p in the first column above and below 1·73, and thence the corresponding value of q. Here, since the values of q answering to $p = 1{\cdot}70$, and 1·75, are the same, for $a = {\cdot}510$, and $a = {\cdot}520$, we may infer they are the same for $a = {\cdot}515$; hence,

$$\begin{array}{lll} p = 1{\cdot}70\,, & q = 1{\cdot}03\,, & p = 170 \\ p = \underline{1{\cdot}75}\,, & q = \underline{1{\cdot}09}\,, & p = \underline{173} \\ \quad{\cdot}05 \quad : & \quad{\cdot}06 \quad :: & \quad{\cdot}03 \quad : {\cdot}036 \end{array}$$

Whence $q = 1{\cdot}066$.

Then again $r' = fa\,\frac{(q+1)}{q} = 27{\cdot}2$ very nearly.

$$r = fa\,(q + 1) = 28{\cdot}9.$$

Hence the required radii are

$$\left.\begin{array}{lll} r = & 28{\cdot}9 & \text{convex} \\ r' = & 27{\cdot}2 & \text{convex} \end{array}\right\}\text{plate lens.}$$

$$\left.\begin{array}{lll} r'' = & 27{\cdot}19 & \text{concave} \\ r''' = & 271{\cdot}9 & \text{concave} \end{array}\right\}\text{flint lens.}$$

Comparison of the preceding formulæ with the empirical rule said to be employed by Mr. Tully.

15. According to the description we have, under the article Telescope in Rees's Cyclopædia, of the principle of computation adopted by this ingenious optician, it appears that instead of computing the aberration of the flint lens from the focal point, it is calculated for parallel rays, and always for the index 1·500; the formula made use of being

$$p = \frac{27\,q^2 + 6q + 7}{6\,(q + 1)^2}$$

as first given by Huygens. This must necessarily give an erroneous result; and to correct it, a comparison of various experiments has led to the formation of an empirical multiplier, which is said to compensate for the erroneous supposition, and to have formed the ground work of the practice of this able artist.

Thus, for example: having first computed the focal lengths of the two lenses, we must, according to these directions, assume any ratio, at least within practicable limits, for the radii of the surfaces of the plate; viz. $q = \frac{r}{r'}$; then, by the above formula, find the value of p. Call now p' the aberration of the flint lens, computed by the same formula; then, independent of the correction above alluded to, we should have

$$\frac{p}{fa} = \frac{p'}{f'a'}, \text{ or } p' = p \times \frac{f'a'}{fa} = p \times \frac{a'}{da}.$$

But the value of p' thus found, as might be expected, does not produce a good object-glass; and from experiment it was ascertained, that the best effect was obtained when the multiplier, instead of $\frac{a'}{da}$, was made equal to $\frac{a' \sqrt{a'^3}}{da \sqrt{a^3}}$;

p' therefore is found by this formula

$$p' = p \times \frac{a' \sqrt{a'^3}}{da \sqrt{a^3}}.$$

Then, substituting this value of p' in the equation

$$p' = \frac{27q^2 + 6q + 7}{6(q+1)^2}$$

the value of q is obtained; and hence, of course, the radii sought.

Now, although this may furnish a very good approximation in some cases, it seems likely that it must, in others, deviate very considerably from the truth. I was desirous therefore of comparing the results obtained by this empirical formula, with those of the correct numbers as above determined, and also to ascertain experimentally, within what limits we might be in error without producing a sensible change in the correction of the object-glass; and through the assistance of

Messrs. W. and T. Gilbert I have been enabled to make various experiments, some of the most useful of which I will endeavour to describe.

First, however, let us ascertain what multiplier we should require, according to the practice we are speaking of, in the particular example computed in a preceding page.

16. Assuming our plate lens such as we have found it, viz. having $\frac{r}{r'} = 1{\cdot}066 = q$, this gives

$$p = \frac{27\,q^2 + 6\,q + 7}{6\,(q + 1)^2} = 1{\cdot}725,$$

$$\text{and the multiplier} = \frac{a'\,\sqrt{a'^3}}{d\,a\,\sqrt{a^3}} = 2{\cdot}218.$$

Now, the aberration of our flint, which theoretically corrects the aberration of this plate, computed by this formula, viz. by taking $q = 10$, is

$$p = \frac{27\,q^2 + 6\,q + 7}{6\,(q + 1)^2} = 3{\cdot}811\,;$$

whence $\frac{p'}{p} = \frac{3{\cdot}811}{1{\cdot}725} = 2{\cdot}209.$

The empirical rule therefore approaches extremely near in its result, in this case, to that obtained on strict optical principles; and in several other comparisons I have made, the agreement has been found equally close, although in others it differs too widely to be depended upon; and as the rule which I have given is strictly correct, and involves no greater difficulty of calculation than that we have been examining, there can, I think, be no doubt to which the preference should be given in any practical case of this kind.

Experimental examination of the limits within which an error in spherical aberration and dispersion may have place, without producing a sensible defect in the object-glass.

17. Although I feel convinced that the method we employed for measuring a tool, would give us a true result within $\frac{1}{500}$th part of the radius, yet it by no means follows that a new tool can be made within the same limits to meet any computed radius. In fact, if all the accuracy of radii were requisite which the strict theory requires, it would be almost impossible to construct an object-glass that would bear a practical examination; fortunately, however, this is not the case; some scope may be allowed, without any very sensible change in the performance of the telescope; and to ascertain within what limits it was necessary to confine the error, was the object of the following experiments.

Experiment 1.

18. According to the preceding computation (art. 14), we ought to have for an 80 inch focus the following curves; the index of the flint being 1·600, of the plate 1·515, and dispersion ·66, viz.

$$\left.\begin{array}{l}\left.\begin{array}{ll} r = & 28\cdot\ 9 \\ r' = & 27\cdot\ 2 \end{array}\right\}\text{plate} \\ \left.\begin{array}{ll} r'' = & -\ 27\cdot 19 \\ r''' = & -271\cdot\ 9 \end{array}\right\}\text{flint}\end{array}\right\}\text{compound focus 80 inches.}$$

Messrs. W. and T. Gilbert had by them two tools, which upon accurate measurement were found to be 26·4 and 264, and taking these instead of 27·19 and 271·9, the first surface to be rendered proportional ought to have been 28·05, and the focus 77.7 inches instead of 80 inches. A new tool was made for the first surface, but on measurement it turned out to be 28·4, viz. ·35 of an inch too long. We determined however to proceed with these radii, viz.

$$\left.\begin{matrix}28{\cdot}4\\26{\cdot}4\end{matrix}\right\}\text{ plate.}\qquad\left.\begin{matrix}26{\cdot}4\\264\end{matrix}\right\}\text{ flint.}\qquad\text{Focus } 77{\cdot}9 \text{ inches.}$$

The glass being accurately ground to these numbers and well centred, the result was satisfactory; the spherical aberration appeared to be very perfectly balanced, although the actual amount of the aberration of the plate lens, in consequence of the excess of the first surface, was 1·738 instead of 1.730. The focal lengths now also answered to a dispersion $d = {\cdot}664$ instead of ·660, and yet the correction for colour appeared perfect. It is clear, therefore, that an error to the amount here stated may exist between the computed and the practical radii, without producing any sensible detriment to the effect of the instrument. The plate was now reversed, carefully adjusted, and the observation repeated.

The achromatic correction of course was still perfect, and the spherical aberration seemed also tolerably well balanced, although the actual amount of the aberration of the plate was now only 1·62 instead of 1·73. The preference however appeared obviously to belong to the first arrangement.

Experiment 2.

19. In order to ascertain the effect of a known want of achromatic correction, a plate was employed which had been

ground to 26·4 and 30 inches, so that the arrangement was now

$$\left.\begin{array}{l} r = 30\cdot \\ r' = 26\cdot4 \end{array}\right\}\text{plate.}\qquad \left.\begin{array}{l} r'' = -\ \ 26\cdot4 \\ r''' = -264\cdot \end{array}\right\}\text{flint.}$$

This would be the proportionate focal lengths for a dispersion ·681. Here the focus was of course too long; and the blue colour was so strong as to prevent our judging of the effect of spherical aberration.

The plate was reversed, but the focus and colour of course remained the same, as did also the general appearance. As far as spherical aberration was concerned, that of the flint was obviously over-corrected in one case and under-corrected in the other; but the defect was too much buried in the colour to enable us to distinguish the difference.

Experiment 3.

20. Messrs. Gilbert having by them a concave flint lens of the same glass to the radii 28·4 and 264, this was matched with the last plate: the curves were therefore now

$$\left.\begin{array}{l} r = 26\cdot4 \\ r' = 30\cdot \end{array}\right\}\text{plate.}\qquad \left.\begin{array}{l} r'' = -\ \ 28\cdot4 \\ r''' = -264\cdot \end{array}\right\}\text{flint.}$$

which proportions answer to a dispersion ·638, about as much in defect as the preceding one was in excess. And it was found to have the same general defect; but the colour was now of course yellow.

Whatever the limits of error may be therefore that can be admitted with impunity, they must be far less than those in the last two experiments.

Experiment 4.

21. Here our flint lens was one of GUINAND's, and we assumed for its radii $r'' = -40{\cdot}4$, and r''' infinite. Its index was found to be 1·630, and its dispersion with our plate ·545; the index of the plate to be matched with it 1·515; and the proposed, or rather the resulting, compound focal length, 77 inches; diameter $5\frac{1}{2}$ inches.

Here $a = {\cdot}515$ $a' = {\cdot}630$, $d = {\cdot}545$, $f = 34{\cdot}95$, $f' = {\cdot}641$, $f''' = 77$; and since r''' and q' in this case are both infinite, we must substitute for q', $\frac{r'''}{-r''}$ in the expression for c', viz.

$$c' = \frac{(a'+1)f''q'}{a'f'' - r'''} = \frac{(a'+1)f''}{r''} = 3{\cdot}1$$

$$c = \frac{f''}{r'} = -1{\cdot}90$$

$$b = \frac{a'}{a'+1} = {\cdot}386.$$

Since q is infinite, our general equation (14) by rejecting all the terms into which q' does not enter, reduces to

$$\left.\begin{matrix} \dfrac{a'+2}{c\,(a'c'+a'+1)^2} \\ + \dfrac{(c'+1)^2}{(b\,c'+1)} \times \dfrac{c'+2-b}{c'} \end{matrix}\right\} \times a\,d = p$$

$$= (-{\cdot}107 + 5{\cdot}289) \times {\cdot}2806 = 1{\cdot}46.$$

This answers to $q = {\cdot}753$.

Whence $r = f\,a\,(q+1) = 31{\cdot}57$

$$r' = f\,a\,\frac{(q+1)}{q} = 41{\cdot}93.$$

The radii we really employed were 32·5 and 40·4, and the result was satisfactory in every respect with regard to correction; but the flint lens was very veiny, which prevented its being a good object-glass.

Experiment 5.

22. Here the curves of a flint lens were 22·2 and 56·4, both concave; the flint index 1·613; that of the plate to be matched with it 1·515; and the dispersion $d = {\cdot}637$.

By the formula we found $r = 12{\cdot}32$, and $r' = 27{\cdot}3$; but the tools actually employed were 12·3 and 27·7, and with these the effect was every thing that could be desired; the colour and spherical aberration being both perfectly corrected.

Experiment 6.

23. This was an object-glass which had been computed on the principles of Mr. Herschel. The index of the flint was 1·587, of plate 1·515, dispersion ·6775, and the focal length 29·5 inches.

The radii and foci, as determined by Mr. Herschel's rule, were

$$\left.\begin{array}{l} r = 19{\cdot}91 \\ r' = 6{\cdot}50 \end{array}\right\} \left.\begin{array}{l} r'' = -\ 6{\cdot}66 \\ r''' = +\ 34{\cdot}49 \end{array}\right\} \left.\begin{array}{l} f = 9{\cdot}52 \\ f' = 14{\cdot}06 \end{array}\right\}$$

In order to compare this rule with the preceding, I assumed the flint radii as above, and computed the radii of the plate.

Here $a = {\cdot}515$, $a' = {\cdot}587$, $d = {\cdot}6775$ $f = 9{\cdot}52$
$f' = 14{\cdot}06$ $f'' = 29.5$ $r'' = -\ 6{\cdot}66$, $r''' = 34{\cdot}49$

$$q' = \frac{r'''}{r''} = -\ 5{\cdot}17$$

$$c' = \frac{(a'+1)f''q'}{a'f'' - r'''} = 14{\cdot}0$$

$$c = \frac{f''}{r''} = -\ 4{\cdot}43$$

$$b = \frac{a}{a'+1} = {\cdot}37.$$

Whence our equation

$$\left.\begin{array}{l} \frac{(c+q')^2}{(a'c-q')^2} \times \frac{c+(a'+2)\,q'}{c\,(a'c'+a'+1)^2} \\ \frac{(c'+1)^2}{(bc'+1)^2} \times \frac{(c'+2-b)\,q'}{c'} \end{array}\right\} \times \frac{a\,d}{q'+1} = p$$

gives in numbers

$$(+\,{\cdot}569 - 33{\cdot}54) \times -\,{\cdot}0851 = 2{\cdot}805.$$

This answers to $q = 3{\cdot}064$; and then

$$r = fa\,(q+1) = 19{\cdot}913$$

$$r' = fa\,\frac{(q+1)}{q} = 6{\cdot}499$$

These numbers agreeing so very exactly with Mr. HERSCHEL's, was satisfactory; for although no doubt I believe could be entertained relative to either principle of computation, yet it was highly pleasing to me to see so close an agreement in the results of two numerical processes founded on such widely different bases.

24. In these numbers, however, we have an example of the inconvenience, (to which I have referred, p. 240), of rigidly enforcing a fourth condition; for the concave of the flint being less deep than the corresponding plate convex radius, it was thought necessary to alter these numbers: this was done by changing $r'' = 6{\cdot}66$ to $r'' = 6{\cdot}58$, and $r' = 6{\cdot}50$ to $r' = 6{\cdot}61$, which was the least alteration we could make in the contact surfaces to have the concave the deeper of the two; the other radii were necessarily altered to $r = 19{\cdot}0$, and $r''' = 32{\cdot}5$; so that our actual experimental radii were

$$\left.\begin{array}{l} r = 19{\cdot}0 \\ r' = 6{\cdot}61 \end{array}\right\} \left.\begin{array}{l} r'' = -6{\cdot}58 \\ r''' = +\,32{\cdot}5 \end{array}\right\}$$

the focal length and dispersive ratio, that is, the ratio of the focal lengths, being thus very exactly the same as required by the conditions of the problem.

The lenses ground to these numbers turned out very fine; the surface, centering, &c. was also very perfect, and the result, notwithstanding the discrepances between the computed and actual radii employed, was very satisfactory.

The scale of the experiment was however too small, the diameter of the lens being only 2¼ inches, so that the defect of correction for aberration was not very sensible; but in several other subsequent experiments, with very nearly the same proportional numbers for focal lengths of 5, 6, and 7 feet, with proportional apertures, the want of correct balance in the spherical correction was very manifest.

It seems therefore that we may in some cases deviate from the radii given by theory much more than in others, without producing the same defect in the instrument; and it will be seen that this ought, *a priori*, to be expected. We know that the amount of aberration (the focal length, aperture, &c. being given), varies with the ratio of the surfaces, and is least in the plate lens for all the usual indices for parallel rays, when $r : r' :: 1 : 6$, and is very little increased with the ratio of 1 : 1; all those results therefore, which require a ratio comprised within, or near, these limits, will have but a small quantity of aberration in the plate lens to be corrected by the flint lens; but when we employ such numbers as require a ratio of 3 to 1, or 4 to 1 in the plate, then the aberration to be corrected by the flint is very considerable, and a small discrepance between the computed and practical radii will produce a much greater error than the same discrepances would in the former instance; and to this circumstance I attribute the difficulty which we certainly found in submitting Mr. HERSCHEL's numbers to practice. Nothing can be desired more accurate nor more elegant than the principles and the analysis on which it is founded, nor any thing more simple than the ultimate result; but it happens, that except the most rigid agreement has place between the computed radii and the radii employed, the discrepance has a very considerable effect upon the correction of the object-glass.

The rule which I have endeavoured to explain in the preceding pages is, I believe, equally correct, but it possesses none of the elegance of investigation which distinguishes the other. To compensate for this, however, it has an extensive range of application, and will enable us in all cases to select those particular radii which will produce the required correction with the least liability to error, and with the closest contact surfaces. We may also, by rejecting the latter condition, match any flint whatsoever with its proper plate; which is I believe a great practical convenience.

The above are only a few out of a great number of experiments, but I have selected them so that they embrace all the varieties which can ever occur; viz. with the flint double concave when q is positive; with the flint plano-concave when q is infinite; and with it concavo-convex when q is negative. So that I hope no one who has any knowledge of the meaning of an algebraical formula, can be at any loss in submitting the rules I have endeavoured to illustrate to a practical application.

Approximate method of computing the curves for an achromatic object-glass.

25. On refering to the formula (14.) for finding the value of p, it will be seen that the first term which expresses the aberration due to the aberration at the first surface, is very inconsiderable with respect to the other term, and that the former may be omitted in all common cases without producing any sensible error. This omission serves to contract the operation very considerably, while by a simple inspection of the table (art. 13.) it will be seen that its several columns are so nearly the same, that any mean one may be adopted instead of the whole: availing ourselves of this circumstance, every rule and principle for constructing an object-glass may be comprised in the following short synopsis, and the result may be used with every degree of confidence in all ordinary cases; although in large telescopes, and in cases where the index and dispersions are very extraordinary, it will be necessary to employ the exact formula already illustrated. According to the approximation here alluded to the rules for the computation may be stated as follows.

APPROXIMATE FORMULA

For the Construction of Object-glasses.

Index plate $= 1 + a$. Index flint $= 1 + a'$, dispersive ratio $1 : d$, focal length $= f''$.

$$f = f''\,(1 - d) = \text{focal length plate,}$$

$$f' = f''\,\frac{(1 - d)}{d} = \text{focal length flint.}$$

Assumed ratio of flint surfaces $1 : q'$,

$$\left.\begin{aligned} r'' &= f'\,a'\,\frac{(1 + q')}{q'} = \text{inside radius} \\ r''' &= f'\,a'\,(1 + q') = \text{outside ditto} \end{aligned}\right\}\text{flint,}$$

$$\text{Find } b = \frac{a'}{a + 1}\quad c' = \frac{(a' + 1)\,f''\,q'}{a'\,f'' - r'''}$$

$$p = \frac{(c' + 1)^2}{(b\,c' + 1)^2} \times \frac{c' + 2 - b}{c'} \times \frac{a\,d\,q'}{q' + 1},$$

and the corresponding value of q in the following table.

p	q	p	q	p	q	p	q
1·15	·374	1·40	·683	1·65	·972	1·90	1·29
1·20	·446	1·45	·739	1·70	1·03	1·95	1·36
1·25	·506	1·50	·798	1·75	1·09	2·00	1·43
1·30	·568	1·55	·855	1·80	1·16	2·05	1·50
1·35	·625	1·60	·913	1·85	1·22	2·10	1·58

$$\left.\begin{aligned} \text{Then } r &= f\,a\,(q + 1) = \text{radius 1st surface} \\ r' &= f\,a\,\frac{(q + 1)}{q} = \text{radius 2nd surface} \end{aligned}\right\}\text{plate.}$$

Method of practically determining the index of refraction and the curvature of the surfaces of any given convex or concave lens.

26. It is frequently convenient for a practical optician to be enabled to determine the radii of curvature of a given lens, and I am not aware of any rule being given for this purpose; the following therefore may be acceptable. The method of measuring the radii of a given concave lens is very well known: it is simply to measure the reflected solar focus of each of the two surfaces; then double these numbers will be the radii sought.

The same simplicity of calculation does not present itself in the convex lens; still, however, the following method of deducing the radii will be found by no means difficult.

Obtain, as in the case of the concave lens, the focus by reflection from *the back surface* of the convex lens, exposing first one surface and then the other to the solar rays; measure also accurately the solar focal length of the lens by refraction; and then by means of these three quantities, equations may be formed which will give the radii of curvature and index of refraction.

Let r, r' be the radii of curvature of the two surfaces, and $1 + x$ the index of refraction let the lens be exposed to the sun's rays, so that the latter are first received upon the surface r. Then by known optical principles the refracted focus at the first surface will be

$$f = \frac{1 + x}{x} r.$$

We may now therefore (disregarding the thickness of the lens, consider these rays as converging towards the back surface to a focus f; and from this surface a part of them will be reflected to a focus f'; which will be expressed by

$$f' = \frac{f r'}{2 f - r'} \cdot$$

This, by substituting for f, its preceding value, and making $y = \frac{x}{1 + x}$ becomes

$$f' = \frac{r r'}{2 r - \frac{x}{x + 1} r'} = \frac{r r'}{2 r - y r'} \cdot$$

These rays will be refracted at the first surface to a focus which we suppose to have been measured. Let this measured distance be m; then by known principles for expressing the refraction at the surface of a rarer medium, we have

$$\frac{1}{m} = \frac{-y}{(1 - y) r} - \frac{1}{(1 + y) f'} \cdot$$

Or substituting for f', we obtain

$$\frac{(1 - y) r'}{2 (1 + y) - \frac{r'}{r}} = m.$$

And of course by simply inverting the lens, or changing r to r', we have (calling the other measured focus n)

$$\frac{(1 - y) r}{2 (1 + y - \frac{r}{r'}} = n.$$

Let φ be the measured solar focus by refraction; then $1 + x$ being the index, we have

$$x \left(\frac{1}{r} + \frac{1}{r'}.\right) = \frac{1}{\varphi}.$$

From which three equations, and the known relation between y and x, the three required quantities x, r, and r' may be obtained.

If we make $\frac{r'}{r} = q$: these equations are

$$\frac{(1-y)\,r'}{1+qy} = 2\,m,$$

$$\frac{(1-y)\,r}{1+\frac{y}{q}} = 2\,n,$$

$$(q+1)\,\varphi\,x = r' \text{ and } \frac{q+1}{q}\,\varphi\,x = r.$$

Substituting for r' and r in the two former, we have (observing that $(1-y)\,x = y$,

$$\frac{y\,(q+1)}{1+yq} = \frac{2\,m}{\varphi} = m',$$

$$\frac{y\,(q+1)}{q+y} = \frac{2\,n}{\varphi} = n'.$$

$$\text{Hence } m' + m\,y\,q = y\,(q+1),$$

$$\text{and } y = \frac{m'}{(1-m)\,q+1},$$

$$q+y = \frac{m' + (1-m')\,q^2 + q}{(1-m')\,q+1}.$$

And substituting the last two values in the equation preceding them, we have

$$\frac{m'\,(q+1)}{m' + (1-m)\,q^2 + q} = n',$$

$$\text{or, } m'\,n' + (n' - m'\,n')\,q^2 + n'\,q = m'\,(q+1).$$

$$\text{Whence } (m'\,n' - n')\,q^2 + (m' - n')\,q = n'\,m' - m',$$

$$\text{or, } q^2 + \frac{m'-n'}{m'\,n'-n'}\,q = \frac{n'\,m'-m'}{m'\,n'-n'}.$$

And since here $q = -1$ is obviously one of the roots, the other will be $\frac{m'\,n'-m'}{m'\,n'-n'} = q$,

$$\text{or} \qquad q = \frac{2\,m\,n - n\,\varphi}{2\,m\,n - n\,\varphi} \quad - \quad - \quad - \quad (1.)$$

Again, since $y = \frac{x}{x+1}$, and $y = \frac{m'}{(1-m')\,q+1}$, we may readily obtain $\qquad x = \frac{2\,m}{(\varphi - 2\,m)\,(q+1)} \quad - \quad (2).$

Whence the index $1 + x$ is known.

As also, $\varphi\, x\, (q + 1) = r'$ - - (3).

$$\varphi\, x\, \frac{(q+1)}{q} = r \qquad - \quad - \quad (4).$$

The radii sought.

27. In order to determine the index of a given concave lens, we must combine it with any proper convex lens to produce a compound focus. Let this focus be φ, that of the convex lens f, and the required focus of the concave x, then by known principles $\frac{1}{x} = \frac{1}{\varphi} + \frac{1}{f}$.

Whence x becomes known. Having then measured the radii of curvature as already stated, and calling them r, r', and index $1 + a$, we have

$$a'\left(\frac{1}{r} + \frac{1}{r'}\right) = \frac{1}{x},$$

and since r, r', and x are known, a' and $1 + a'$ will of course be known also.

XVII. *On the change in the plumage of some Hen-Pheasants. By* William Yarrell, *Esq. F. L. S. Communicated by* William Morgan, *Esq. F. R. S., March* 19, 1827.

Read May 10, 1827.

The latter part of the last shooting season has been unusually productive of those hen-pheasants which assume, more or less, the appearance of the male, and considerable discussion has, in consequence, arisen as to the cause of this change in the plumage.

Chance, rather than design, having supplied me with many opportunities of observation both on pheasants and the common domestic fowl, I am induced to notice the internal peculiarities that have been observed invariably to accompany this change of feather, and such other circumstances as appear connected with this subject, some of which I think will be found new and interesting.

A Paper on this subject, by Mr. John Hunter, published in the 70th Volume of the Philosophical Transactions, and afterwards reprinted in his "Animal Economy," details the appearance of several female birds having the feathers of the male, in which account he is led to observe, "that this change of character takes place at an *advanced age* of the animal's life, and does not grow up with it from the beginning."

In the Third Volume of the Memoirs of the Wernerian Society, Mr. John Butter devotes a Paper to a consideration of the change which takes place in certain hen-birds at an *advanced period* of their lives, and concludes, " that all hen-pheasants as well as common-fowls would assume the plumage of the cock to a certain degree, if they were kept to a certain age."

Some further observations on this subject by Monsieur Isidore G. St. Hilaire, will be found in the " *Annales des Sciences Naturelles,*" and in the Eleventh Number of Dr. Brewster's Edinburgh Journal.

The remarks I shall have occasion to introduce, will be found somewhat at variance with the opinions of the writers above referred to, who appear to consider that age is absolutely necessary to produce this change : I shall be able to show, that certain constitutional circumstances producing this change, may, and do, occur at any period during the life of the fowl, and that they can be produced by artificial means.

Besides various opportunities during former seasons, I had the advantage, in the months of December and January last, of examining seven hen-pheasants, in plumage more or less resembling the male, in all of which the sexual organs were diseased, but with some variation as to extent, and the progress of change observable in the plumage bore a corresponding analogy. The ovarium was contracted in size, of a purple colour, and hard to the touch ; the spherical shape of the ova destroyed in some ; the oviduct also diseased throughout its whole length, and the canal obliterated at the upper part immediately preceding the funnel-shaped enlargement at the bottom of the ovarium.

The parts were all preserved, and deposited in the Museum of the Zoological Society.

Desirous of possessing a specimen of the organs from a healthy female to contrast with the preparations of such as were in a diseased state, a hen-pheasant in the natural plumage was opened for examination; but in this instance disease prevailed throughout the whole of the ovarium, but had not affected the oviduct; proving, that this disease exists in the sexual organs previous to the change in the feather; and this corresponds with the recorded observations of others, where hen-pheasants in confinement, and females of the common-fowl in the poultry yard, had been known to have ceased producing eggs two years before any change was observed in their plumage.

That the obliteration of the true character of the female organs by disease, and the consequent alteration of feather, takes place at various periods, are inferred from the following circumstances. Among the large broods of young pheasants, frequently from fifty to one hundred birds in number, which some gamekeepers are exceedingly successful in rearing by hand, produced from eggs laid by birds in confinement, nests deserted from various causes, or eggs exposed by mowing; it is by no means unusual in the months of August and September, when the young birds put forth the first plumage indicative of the sex, that one or two females are observed to produce the brighter coloured feathers of the male. These birds are then about four months old only. In two instances, among the hen-pheasants before mentioned, as shot in a wild state, some of the first plumage, usually called nest feathers, had not been shed, evidence sufficient to prove that they also were both birds of the year.

A partridge sent me by a friend in December last, on account of its having a white bar across the breast, and the first three primaries of each wing also white, was opened in the presence of two persons, and found to be under the influence of the same sort of disease apparent in the organs of a hen-pheasant examined at the same time by way of comparison. This partridge was one of a covey bred during the summer of 1826, several of the young birds of which exhibited some white feathers. This circumstance was often noticed by the keeper, but only one bird was procured. The clay-coloured legs, as well as the plumage in this specimen, were additional evidence that this partridge also was a bird of the year.

A few of the feathers on the breast bore the chesnut colour peculiar to the male of this species.

It may be objected to this example, that the colour of the altered feather was not entirely that of the male; but I have quoted this instance in order to show that it was a young bird; that the female organs were destroyed by disease; and that a change in the colour of the plumage had taken place.

The assumption of plumage decidedly resembling that of the male, must not however be confounded with accidental varieties. All variations of feathers are not caused by an alteration of the sexual organs. I have examined several birds of various species in which those parts were perfectly healthy; but such birds are generally smaller than the natural size of the species to which they belong; and the variety of plumage in them probably originates in an imperfect secretion arising from weakness.

That this disease arises at later periods during the life of the bird, but still long previous to a natural cessation of the powers of reproduction as a female, seems almost certain from the circumstance that in some of the preparations of the parts of the hen-pheasants examined, the distinct globular forms of numerous ova are still apparent, but altered in colour; from which it would appear probable, that had not this disease occurred, these embryos would in due season have been matured and deposited.

Having shown that a particular change of feather follows the destruction of the sexual organs by disease, I shall proceed to describe the effects produced upon both sexes of the common fowl, when obliteration of the same parts is effected by artificial means, that is to say, by an operation.

The breeder of poultry, who practises the art of making capons, is apprized by the attempts of the young male bird to crow, that a sufficient enlargement of the testes has taken place to enable him to perform the operation of extraction with ease and certainty; but this act completed, the bird never crows after. The comb and gills do not attain a size equal to those of other males not subjected to this operation; the spurs appear, but remain short and blunt; and the long narrow feathers of the neck and lower part of the back, so characteristic in the true male, put on an appearance in this bird, intermediate between the hackled appearance in the cock, and the ordinary web of the hen.

The operation performed on the female of the common fowl is much more simple than might be expected. It consists in making a small incision through the thin skin of the flank on the left side; the oviduct, which lies immediately within, is thus easily brought into view; and it is then only necessary to cut away a small portion of it, that the continuity of the canal may be destroyed. The ova do not afterwards enlarge, and the connexion between the sexual organs and those of the voice are not less remarkable in the females than that before observed to exist in the male. She makes an imperfect attempt to imitate the crow of the cock, there is an increase in the size of the comb, and a spur or spurs shoot out, but remain short and blunt. The plumage undergoes an alteration, which is called by the breeders getting foul feathered, becoming hackled in form, and altered in colour. But a more singular point is, the peculiar shape of the lower part of the back in these birds, from the want of that enlargement of the bones, observed in all true females, by which they obtain a breadth of pelvis sufficient to allow a safe passage to the perfect egg, an object the more particularly necessary, when it is recollected that a slight fracture of its brittle shell is sufficient to prevent the developement of the chick.

As the object in performing this operation upon fowls is to gain an increase in size, still preserving the delicate texture of the flesh, these birds when ten or twelve months old are sent off to the London markets, and farther observation prevented; but so great is the similarity at this age between some examples of this description, that it is frequently difficult to determine the sex by such external characters as remain. Thus, males and females, becoming as it were neuter in gender by the deprivation of the sexual organs, put on a corresponding appearance, and both assume characters decidedly intermediate between the true sexes.

The influence exercised by the sexual organs upon the colour of the feathers, as well as the voice, is not confined to this effect alone. The summer plumage of some birds, and the brighter tints of others, called by French authors *plumage des noces*, do not make their appearance till the sexual organs begin to dilate under the genial influence of spring.

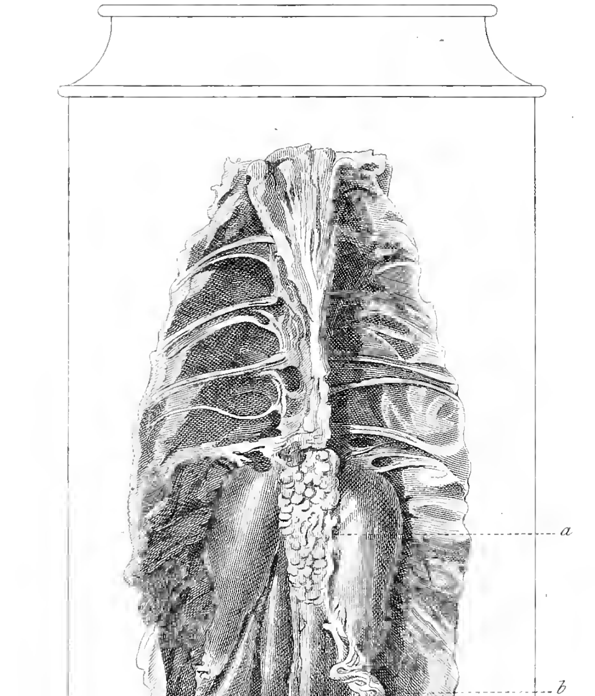
The various songsters pour forth their constant and most melodious strains only during the season of producing and rearing their young; and some birds, as the cuckoo, quail, &c. appear capable of exercising their voice but for a limited time, confined to the same period of the year. With the decline of summer the sexual organs again contract, the voice subsides, and the plumage losing its brilliancy, assumes by degrees more valuable shades of grey and white for defence during the rigour of winter.

Returning again to the subject of hen-pheasants that are said to exhibit the feathers of the cock, it may be stated generally, that at best it is but an approximation to the plumage of the male.

It is probable that they do not live many years after the commencement of the change, since so few are found to arrive at any great degree of splendour. Of the many I have had opportunities of examining, none possessed either the full-sized broad scarlet patch round the eye, the fine blue zone at the end of the red feathers of the breast, or much of the bright straw-coloured mark on the scapulars and wing-coverts, one specimen alone excepted; nor have I seen a female pheasant with spurs; and a bird belonging to Mr. LEADBEATER, which is by many degrees the finest I have ever seen, is also without any.

From these detailed observations it will probably be granted that age is not necessary, but that this disease, with its consequences, may arise at any period during life; and that the

1.



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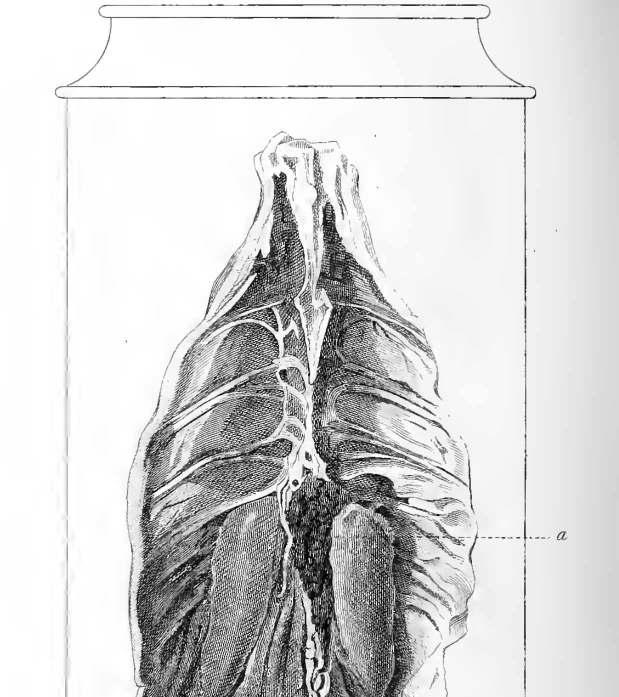

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changes in the external character depending on the destruction of the sexual organs, may be effected by artificial means.

From several examples in different classes and orders, I am induced to believe it will be found a law of nature, that in all animals bearing external characters indicative of the sex, besides the sexual organs, those characters will undergo a change, and exhibit an appearance intermediate between the perfect male and female, whenever the animal happens to be deprived of the influence of the true sexual organs, whether from original malformation, subsequent disease, or artificial obliteration.

February, 1827.

EXPLANATION OF PLATE XII.

No. 1. Part of the body of a female pheasant, the sexual organs in the natural state: all the other viscera removed.

a, the ovarium, with its ova ; natural state.

b, *b*, *b*, the oviduct.

c, part of the intestine, tied.

No. 2. Part of the body of a female pheasant.

a, the ovarium diseased, discoloured, spherical character of the ova destroyed.

b, *b*, *b*, the oviduct, exhibiting the same disease, and its consequent discoloration throughout the whole length.

c, part of the intestine, tied.

XVIII. *On the secondary deflections produced in a magnetized needle by an iron shell, in consequence of an unequal distribution of magnetism in its two branches. First noticed by Captain* J. P. WILSON, *of the Honourable East India Company's Ship Hythe.* *By* PETER BARLOW, *Esq. F.R.S. Mem. Imp. Sc. Petrop.*

Read May 17, 1827.

CAPTAIN WILSON, in a very early stage of my magnetic experiments, took considerable interest, not only in their application to the purposes of navigation, but in the fundamental laws on which that application was founded; and in pursuit of his own particular views he undertook, in his last voyage, to decide, by actual experiment, some of the points which were not in the beginning universally admitted; amongst which one of the most important, was that relating to the change of position of the ideal magnetic sphere which I had imagined for the purpose of magnetical reference, and for the convenience of reducing the laws of action to their most simple and general form. According to the results which I had obtained, it was presumed, but not confidently asserted, (see page 65, 1st edition "Essay on Magnetic Attractions") that this sphere, in different parts of the world, would take up different positions with regard to the horizon, following in all cases the changes of position of the dipping needle. Captain WILSON proposed to repeat these experiments at different places, where he might have an opportunity, on his outward and homeward voyages: viz. at St.

Helena, Bengal, and in China; and he had the satisfaction of finding the sphere nearly inverted in his different experiments, the law in all cases strictly following those which had been predicted from my first series of results.

The confirmation however of this fact is now of but secondary interest, because it has been demonstrated, and admitted by all who have taken any part in the enquiry; I shall therefore pass over these experiments, and mention only the curious observations alluded to in the title of this Paper.

The apparatus which Captain Wilson employed was exactly similar to that which I made use of in Woolwich, viz. a large and strong round table with a hole in the centre, and a 13-inch mortar shell, with a contrivance for raising and lowering the latter through the centre hole.

It appears that while in China, endeavouring to trace out the magnetic equator to the shell, which I have in my Essay called the circle of no attraction, or of no deviation, he found, that although in this circle no deviation was observed with the needle in its natural state, yet, if one end of the needle (for example the south) was slightly touched with the south end of a magnet, a considerable deviation followed, and on repeating the experiments in different positions on the magnetic sphere, different results were obtained; the end of the needle, whose magnetism had been deteriorated, in some cases approaching, and in others receding from the centre of the ball.

Captain Wilson having erected this apparatus at his house in town, I spent some time with him and Captain Beaufort, R. N. F. R. S. in examining and repeating these curious experiments, but without being able at that time to reduce them to uniform laws. From the experiments we did make, it appeared, however, that if the ball was raised just above the plane of the table, and the compass carried round the table, proceeding, for example, from the north towards the east or west, the deteriorated branch of the needle receded from the ball; and this happened also beyond the east and west points to a certain azimuth, after which the deteriorated branch approached the ball. Precisely the same occurred when the ball was placed just below the table, beginning however now at the south instead of the north. The points of change being in this case between the north and east or west point, and in the former between the south and east or west point. If the ball was placed exactly with its horizontal section in the plane of the table, the law appeared still more anomalous; but in carrying the compass round the ball in the magnetic equator, or plane of no deviation, the deteriorated branch in all cases approached the ball.

In page 55 of the first edition of my essay, I have stated that some of the discrepancies I found between the observed and computed deviations, were probably due to an unequal distribution of magnetism in the two branches of the needle; and there could be no doubt that this was actually the case in the present instance; but I had no idea of the great amount of error to which my first observations might have been subject, had this inequality of magnetism been greater than it was. In the experiments above referred to, the error amounted to 2°, 3°, and even 5° and upwards; but as the actual amount depended principally upon the extent of deterioration (which we had no means of measuring) I have not attempted to give numerical results.

After attentively considering Captain Wilson's experiments, and repeating some of them on my original apparatus at Woolwich, aided also by the explanation I had formed in my own mind as to the cause of these apparent anomalies, I was at length enabled to reduce the several results to a sort of general law, which may be thus enunciated.

We may distinguish the following several cases of deviation and magnetic action between a magnetized needle and an iron ball or shell.

1. The needle may be placed in any part of the magnetic meridian of such a ball; in which case there is no deviation in the needle; nor is there any secondary deviation by an unequal distribution of magnetism in its two branches.

2. The needle may be placed any where in the magnetic equator of the ball. In this case, whichever end of the needle has its magnetism deteriorated, that end will approach the ball, and the same obtains generally while the poles of the needle are in opposite hemispheres of the ball.

3. Generally, in other positions one branch of the needle will be nearer to the centre of the ball than the other; then, if the near end has its magnetism deteriorated, the needle will approach its natural meridian, but if the more distant end be deteriorated, the needle will be more deflected, or recede from the meridian. And this happens whether the near end lies between the ball and meridian, or the meridian between it and the ball.

These however must be considered rather as general than as particular descriptions of the latter cases, because as the needle approaches those points in which its direction is at right angles to the line joining its pivot and the centre of the ball (which are the points of change alluded to in the preceding experiments) the secondary deflections are small and somewhat equivocal, the precise point of change seeming to have a reference, not to position only, but also to the amount of deterioration produced in the needle.

These curious results are important for two reasons; first, as showing the necessity, in making numerical magnetic experiments, of being very particular as to the most perfect uniformity in the construction of the needles employed, as well as in the communication of magnetism to them; and as it is probable that this uniformity can never be completely attained, it will enable us to account for some of those small irregularities, which will attend the most careful experiments, without attributing them to errors of observation or adjustment

Secondly, these results are interesting, as amounting almost to a demonstration of the truth of that theory of magnetism, so very generally, but not universally admitted, viz. "that iron becomes magnetic by induction from the earth."

In the first edition of my Essay, I was led by the apparent simplicity of the hypothesis, to adopt a particular view of this subject, which had been pointed out to me, and which referred the deflection of the needle to the simple central attraction of the ball on its two opposite extremities; and although, from various analogies and other circumstances, I saw reason to change my opinion on the subject in the second edition, and to adopt the induction hypothesis, yet I never could contrive a cross experiment to decide positively between the two theories; for every experiment I could imagine, and every result I could ever obtain, which might be explained on one of those principles, could be as easily illustrated by the other. The present order of secondary deflections, however, is quite decisive of the point in question, these being all perfectly consistent with the one, and generally inconsistent with the other hypothesis.

The first of the cases pointed out above, viz. when the needle is in the magnetic meridian of the ball, requires no illustration; we may pass therefore to the second, in which the needle is supposed to be placed in the magnetic equator. Now here, on the one hypothesis, the equilibrium of the needle in its natural meridian is attributed to an equal and opposite repulsive power on its two branches, these being each found respectively in that hemisphere of the ball of the same name with itself, and are each therefore under repulsion.

Consequently, if any deterioration takes place in either branch, that branch will be less repelled, and will therefore approach the ball.

On the other hypothesis, as no repulsion is admitted, the equilibrium must be due to equal and opposite attractions; consequently, the effect of deteriorating either branch would be, that that branch would recede from the ball, which is contrary to observation; and the same applies generally while the poles of the needle are in opposite hemispheres of the ball.

In the third case, the explanation is nearly as simple; for example: the branch of the needle nearest the centre of the ball will be the most powerfully acted upon; or, which is the same, the centre of all the actions on the needle will be in that branch; if, therefore, that branch be found between the ball and the meridian, the attractive powers prevail over the repulsive; but if the meridian be found between the ball and the nearest branch of the needle, then the deflection is due to an excess of the repulsive forces over the attractive.

In the first case, by deteriorating the near branch, we diminish the attractive forces, and in the other case, the repulsive; so that in both instances the needle ought to approach the meridian; as is found to be the case. But by deteriorating the other, that is the most distant branch, we increase the preponderance of attractive power in the one case, and the repulsive in the other, and, consequently, the needle will be more deflected, or recede farther from the meridian.

With respect to the rather uncertain character of the secondary deflections near the points of change, the explanation appears to me to rest on this: that, admitting the attractive and repulsive principle, the centre of attraction and of repulsion may fall both in the same branch of the needle, or in opposite branches. In the one case, the needle is deflected by only the difference of the two forces, and in the other, by their sum. And it is probable that in or near the points of change, the degree of deterioration may produce this uncertain result, by changing these centres from one branch to the other, according to the intensity of the deteriorating power.

After all, however, it will not be expected that the results due to such a complex system of forces can be illustrated in common language, since in their more uniform state, they require the aid of the most powerful analysis to reduce them to determinate laws. It is sufficient for the present purpose to have shown, that the order of secondary deflections, discovered by Captain Wilson, are, in a general point of view, consistent with that hypothesis, which supposes the magnetism of iron to be due to induction from the earth, and that they are inconsistent with that which attributes the deflection of a magnetised needle to the general central attraction of the iron on its two poles or extremities, or on an imaginary needle passing through the pivot in the line of the dip.

In adopting the hypothesis of induced magnetism in the Second Edition of my Essay, I only attempted the calculation for an indefinitely short needle, or magnetic particle. Since this M. Poisson has, by means of the powerful analysis he knows so well how to apply, obtained a general formula for a needle of any length; and I have little doubt, if we possessed the means of estimating the amount of deterioration, or the actual inequality of magnetism in the two branches of the needle, that all the facts I have stated would become by his formula a subject for calculation.

The following experiments may perhaps in some measure assist towards rendering the results numerical: they were undertaken after the preceding part of the paper had been written on the suggestion of Captain Beaufort.

Three needles were procured from Messrs. W. and T. Gilbert, as nearly equal in weight, length, and power as possible, all applicable to the same pivot and compass-box. The radius of each was three inches, and the number of vibrations made by each in a minute when in their natural magnetic state, was eighteen. No. 1. was left in that state. No. 2. was deteriorated in its northern branch; No. 3. in its southern; after which No. 2. made only 11 vibrations in a minute, and No. 3. 12 vibrations in the same time. With these needles the experiments were conducted as follows.

The ball was raised till its centre was 10 inches above the pivot of the needle, and the latter placed at 13½ inches from the centre of the table, making the distance between the pivot and the centre of the ball 16·8 inches, which distance was preserved in all the experiments. The box containing the needle was placed in the situation above mentioned, first north of the ball, then N. 20° E, N. 40° E. and so on all round to the north again. And in each of these positions the deviation of each needle was successively registered, the results being as in the first division in the following table. Precisely a similar set of observations were made with the centre of the ball 10 inches below the pivot of the needle, as in the 3d division of the table; and lastly, a like set were obtained with the centre of the ball level with the pivot of the needle, the latter in this case being placed at the whole distance 16·8 from the centre of the table.

Situation of compass.	Deviations; ball 10 inches above.			Deviations; ball on the plane of the table.			Deviations; ball 10 inches below.		
	Needle No. 1.	Needle No. 2.	Needle No. 3.	Needle No. 1.	Needle No. 2.	Needle No. 3.	Needle No. 1.	Needle No. 2.	Needle No. 3.
North	—	—	—	—	—	—	—	—	—
N 20° E	1°	3°	2½°	7½	10½	4½	14	22	4
N 40 E	21¾	25¾	15½	12½	16	8	21	29	10
N 60 E	30	36½	22	11½	13¾	9¾	27	30	21½
N 80 E	34½	43½	27	5½	8½	7¼	32¾	28	37½
S 80 E	34	41	30½	3¼	5¾	9	34	30½	43
S 60 E	27½	22	31	12	10	14	30½	21½	35
S 40 E	20½	9½	28½	13	8	16	22	15½	26
S 20 E	12½	4	22½	8	4½	11	—	—	—
South	—	—	—	—	—	—	—	—	—
S 20 W	11½	3½	21½	7¾	4	9½	—	—	—
S 40 W	20	9½	28	13	7½	16	21	15	26
S 60 W	26½	21	29½	11½	9¾	15	31	21	34¾
S 80 W	33	39	29	4	7	2½	34½	29¾	43
N 80 W	34	43¼	30½	……	……	……	33	29	39
N 60 W	30	36	22	10½	12	8¾	26	29	21
N 40 W	21	25½	15	11½	15¼	7½	20	28	9½
N 20 W	……	……	……	7¼	9¾	3¾	11½	21½	3½

XIX. *On the difference of Meridians of the Royal Observatories of Greenwich and Paris. By* THOMAS HENDERSON, *Esq. Communicated by* J. F. W. HERSCHEL, *Esq. Sec. R. S.*

Read May 17, 1827.

IN the Philosophical Transactions for 1826, Part II. Mr. HERSCHEL has given a detailed account of observations, which were made in the month of July, 1825, for the purpose of ascertaining the difference of the meridians of the Royal Observatories of Greenwich and Paris, with a computation of these observations, from which the most probable value of the difference of longitude appears to be $9^{m}\ 21^{s}\cdot6$. But I have perceived that in the copy of the observations delivered to him from the Royal Observatory of Greenwich, an error of one second has been committed; as the true sidereal time of the observation made there on 21st July, ought to be $17^{h}\ 38^{m}\ 57\cdot12$ in place of $17^{h}\ 38^{m}\ 56^{s}\cdot10$, set down in the Table p. 104, which he informs me was computed at the Observatory, and officially communicated to him from the Astronomer Royal. This error seems to have had its origin in the little Table at the bottom of page 103; for, on subtracting the error of the clock, $47^{s}\cdot37$, from the time $18^{h}\ 8^{m}\ 30^{s}\cdot40$, the true sidereal time is $18^{h}\ 7^{m}\ 43^{s}\cdot03$, instead of $18^{h}\ 7^{m}\ 42^{s}\cdot03$, there given. The error in the result of that day's observations, arising from this cause, has been partly compensated by a mistake of three tenths of a second, which has occurred in calculating the combined observations of the same day, the gain of mean on sidereal time being stated to be — $4^{s}\cdot54$ (pp. 120 and 122), in place of — $4^{s}\cdot24$. On checking the other observations, a few trifling alterations appear to be necessary upon the Greenwich Table of sidereal time, from the *data* given along with it. These seem to be occasioned by different methods of calculation, and indeed are hardly worthy of notice. The French astronomers not having given the *data* on which the calculations of the sidereal times at Paris are founded, they are assumed to be correct.

The effect of the alterations thus made upon the elements of computation, is to redeem the result of the observations of 21st July from the suspicion which attached to it, in consequence of its disagreement with the results of the other days, and to produce a change of one-tenth of a second (corresponding to one hundred feet nearly on the earth's surface,) upon the most probable value of the difference of meridians forming the subject of investigation, it now appearing to be $9^{m}\ 21^{s}\cdot5$. At the same time the chronometer, which was employed for the observations at Fairlight down, is shown to have kept a more uniform rate than what previously appeared. In this important national operation, the utmost accuracy is desirable; and it has therefore been thought proper to subject the whole observations to a new computation. This will make more apparent the near coincidence of the results of the different days observations, and the great precision to be expected from experiments of the nature of those in question, when, as has happened in the present instance, they are made with the utmost care and attention on the part of the observers.

The details of the new computation are as follows.

As mentioned by Mr. HERSCHEL, rockets were exploded at Wrotham, which were observed at Greenwich and Fairlight down; at La Canche, on the French Coast, which were observed at Fairlight and Lignieres; and at Mont Javoult, which were observed at Lignieres and Paris.

Before the difference of meridians can be eliminated from the observations, the rates of the chronometers employed at Lignieres and Fairlight must be ascertained. Let P be the sidereal time at Paris, L the corresponding time indicated by the chronometer at Lignieres (these times being determined by simultaneous observations of signals) P' and L', the same times for a subsequent night, then $(P'-P)-(L'-L)$ is the retardation of the chronometer at Lignieres on sidereal time during the chronometer time $(L'-L)$; $\frac{24^h \times [(P'-P)-(L'-L)]}{(L'-L)} = r$, is the rate of the chronometer, or the equation to be added to 24 hours of chronometer time to obtain the corresponding interval of sidereal time; and $\frac{r(L'-L)}{24^h}$ is the equation to be added to any portion of chronometer time $(L'-L)$ to reduce it to sidereal time.

In like manner with regard to the chronometer at Fairlight, let G and F be the corresponding sidereal time at Greenwich and chronometer time at Fairlight, G' and F' the same times for a subsequent night, then

$\frac{24^h \times [(G'-G)-(F'-F)]}{(F'-F)} = r'$ is the chronometer's rate, or the equation to be added to 24 hours of chronometer time, to obtain the corresponding interval of sidereal time; and $\frac{r'(F'-F)}{24^h}$ is the equation to be added to any portion of chronometer time $(F'-F)$, to reduce it to sidereal time.

Computation of the rates of the Chronometers.

1. Lignieres Chronometer.

From July 18 to July 19.

	h. m. s.		h. m. s.
19th P′ =	18 19 41·83	L′ =	10 29 33·97
18th P =	18 32 21·88	L =	10 46 13·60

$P'-P = 23\ 47\ 19{\cdot}95 \quad L'-L = 23\ 43\ 20{\cdot}37$

$(P'-P)-(L'-L) = 3^m\ 59^s{\cdot}58$

$$\frac{24^h \times 3^m\ 59^s{\cdot}58}{23^h\ 43^m\ 20^s{\cdot}37} = r = 4^m\ 2^s{\cdot}38.$$

From July 19 to July 20.

	h. m. s.	h. m. s.
20th	17 43 31·30	9 49 29·60
	18 3 32·55	10 9 27·80
	18 23 35·60	10 29 27·20
	18 43 42·15	10 49 30·85
	Mean	Mean
20th P′ =	18 13 35·40	L′ = 10 19 28·86
19th P =	18 19 41·83	L = 10 29 33·97

$P'-P = 23\ 53\ 53{\cdot}57 \quad L'-L = 23\ 49\ 54{\cdot}89$

$(P'-P)-(L'-L) = 3^m\ 58^s{\cdot}68$

$$\frac{24^h \times 3^m\ 58^s{\cdot}68}{23^h\ 49^m\ 54^s{\cdot}89} = r = 4^m\ 0^s{\cdot}36.$$

From July 20 to July 21.

	h. m. s.		h. m. s.
21st P′ =	18 14 15·18	L′ =	10 16 14·18
20th P =	18 13 35·40	L =	10 19 28·86

$P'-P = 24\ 0\ 39{\cdot}78 \quad L'-L = 23\ 56\ 42{\cdot}62$

$(P'-P)-(L'-L) = 3^m\ 57^s{\cdot}16$

$$\frac{24^h \times 3^m\ 57^s{\cdot}16}{23^h\ 56^m\ 42^s{\cdot}62} = r = 3^m\ 57^s{\cdot}70.$$

From July 21 to July 22.

	h. m. s.		h. m. s.
22d P′ =	18 11 24·77	L′ =	10 9 24·63
21st P =	18 14 15·18	L =	10 16 11·48

$P'-P = 23\ 57\ 9{\cdot}59 \quad L'-L = 23\ 53\ 13{\cdot}15$

$(P'-P)-(L'-L) = 3^m\ 56^s{\cdot}44$

$$\frac{24^h \times 3^m\ 56^s{\cdot}44}{23^h\ 53^m\ 13^s{\cdot}15} = r = 3^m\ 57^s{\cdot}56.$$

2. Fairlight Chronometer.

From July 17 to July 18.

	h. m. s.	h. m. s.
17th	17 12 38·06	9 31 36·15
	17 22 39·55	9 41 35·90
	17 32 42·39	9 51 37·10
	17 42 44·31	10 1 37·50
	17 52 43·98	10 11 35·65
	18 2 49·09	10 21 38·85
	18 12 52·67	10 31 40·85
	18 22 54·52	10 41 40·95
	18 42 55·39	11 1 38·55
	Mean	Mean
17th	G = 17 53 53·33	F = 10 12 44·61
18th	G′ = 17 53 32·40	F′ = 10 8 28·13

$$G' - G = 23\ 59\ 39\cdot07 \quad F' - F = 23\ 55\ 43\cdot52$$

$$(G' - G) - (F' - F) = 3^m\ 55^s\cdot55$$

$$\frac{24^h \times 3^m\ 55^s\cdot55}{23^h\ 55^m\ 43^s\cdot52} = r' = 3^m\ 56^s\cdot25.$$

From July 18 to July 19.

	h. m. s.	h. m. s.
19th	G′ = 18 12 20·31	F′ = 10 23 17·57
18th	G = 17 53 32·40	F = 10 8 28·13

$$G' - G = 24\ 18\ 47\cdot91 \quad F' - F = 24\ 14\ 49\cdot44$$

$$(G' - G) - (F' - F) = 3^m\ 58^s\cdot47$$

$$\frac{24^h \times 3^m\ 58^s\cdot47}{24^h\ 14^m\ 49^s\cdot44} = r' = 3^m\ 56^s\cdot04.$$

From July 19 to July 20.

	h. m. s.	h. m. s.
20th	18 4 55·93	10 11 57·85
	18 15 4·26	10 22 4·45
	Mean.	Mean.
20th	G′ = 18 10 0·10	F′ = 10 17 1·15
19th	G = 18 12 20·31	F′ = 10 23 17·57

$$G' - G = 23\ 57\ 39\cdot79 \quad F' - F = 23\ 53\ 43\cdot58$$

$$(G' - G) - (F' - F) = 3^m\ 56^s\cdot21$$

$$\frac{24^h \times 3^m\ 56^s\cdot21}{23^h\ 53^m\ 43^s\cdot58} = r' = 3^m\ 57\cdot25.$$

From July 20 to July 21.

	h. m. s.	h. m. s.
21st	G′ = 17 38 57·12	F′ = 9 42 7·65
20th	G = 18 10 0·10	F = 10 17 1·15

$$G' - G = 23\ 28\ 57\cdot02 \quad F' - F = 23\ 25\ 6\cdot50$$

$$(G' - G) - (F' - F) = 3^m\ 50^s\cdot52$$

$$\frac{24^h \times 3^m\ 50^s\cdot52}{23^h\ 25^m\ 6^s\cdot50} = r' = 3^m\ 56^s\cdot25.$$

From July 21 to July 22.

	h. m. s.	h. m. s.
22d	G′ = 17 47 55·64	F′ = 9 47 8·59
21st	G = 17 38 57·12	F = 9 42 7·65

$$G' - G = 24\ 8\ 58\cdot52 \quad F' - F = 24\ 5\ 0\cdot94$$

$$(G' - G) - (F' - F) = 3^m\ 57^s\cdot58$$

$$\frac{24^h \times 3^m\ 57\cdot58}{24^h\ 5^m\ 0^s\cdot94} = r' = 3^m\ 56^s\cdot76.$$

Collecting together the rates of the chronometers, we have

Lignieres Chronometer.

	r
From July 18th to July 19th - -	4ᵐ 2ˢ·38
19th —— 20th - -	4 0·36
20th —— 21st - -	3 57·70
21st —— 22d - -	3 57·56

Fairlight Chronometer.

	r'
From July 17th to July 18th - -	3ᵐ 56ˢ·25
18th —— 19th - -	3 56·04
19th —— 20th - -	3 57·25
20th —— 21st - -	3 56·25
21st —— 22d - -	3 56·76

As the rate of the Lignieres chronometer is irregular, it seems advisable to deduce the rate for reducing the observations there by interpolation from the two nearest rates, supposing that each answers to the middle of its interval. In this manner are obtained the following rates for the observations at Lignieres.

July 18th. $4^{m}\ 3^{s}\cdot41$; but as this differs considerably from the rate which the chronometer had on leaving Paris, the rate $4^{m}\ 2^{s}\cdot38$, derived from the observations of the 18th and 19th, has been retained, as being probably nearer the truth.

19th. $4^{m}\ 1^{s}\cdot38$.

21st. $3^{m}\ 57^{s}\cdot63$.

22d. $3^{m}\ 57^{s}\cdot49$.

The rate of the Fairlight chronometer being sufficiently uniform, the mean of the whole, $3^{m}\ 56^{s}\cdot51$, has been adopted for all the observations there, which cannot produce any sensible error.

The difference of meridians is now obtained from the following formula.

Let P be the sidereal time at Paris, L the corresponding chronometer time at Lignieres, L′ and F, corresponding chronometer times at Lignieres and Fairlight, and F′, chronometer time at Fairlight, and G the corresponding sidereal time at Greenwich; the intervals of chronometer times $(L' - L)$ and $(F' - F)$ must be reduced to intervals of sidereal time $(l' - l)$ and $(f' - f)$ by the formulæ given above. Then $P + (l' - l) + (f' - f) - G =$ difference of meridians required as is evident.

Computation of the difference of meridians; all the observers taken jointly.

July 18.

P	L	L′	F	F′	G.
h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.
18 15 40·37	10 29 34·40	9 54 52·00	9 46 29·75	9 41 46·20	17 26 46·17
18 35 41·13	10 49 32·80	10 14 54·00	10 6 31·40	9 51 49·60	17 36 50·97
18 45 44·13	10 59 33·60			10 1 50·30	17 46 53·59
				10 11 48·60	17 56 53·29
				10 21 46·90	18 6 53·41
				10 41 47·20	18 26 56·95
Mean	Mean	Mean	Mean	Mean	Mean
+18 32 21·88	10 46 13·60	10 4 53·00	9 56 30·57	10 8 28·13	- 17 53 32·40
+ 11 59·53	= $f' - f$			$l' - l =$	- 41 27·57
	L′ — L =	— 41 20·60			
+18 44 21·41	Reduction =	— 6·97	F′ — F =	+ 11 57·56	- 18 34 59·97
—18 34 59·97			Reduction =	+ 1·97	
	$l' - l =$	— 41 27·57			
9 21·44	= Diff. Merid^{s}		$f' - f =$	+ 11 59·53	

July 19.

P	L	L′	F	F′	G
h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.
17 29 29·60	9 39 30·40	9 44 49·40	9 36 33·10	9 42 0·45	17 30 56·55
18 39 52·50	10 49 41·10	9 54 49·90	9 46 33·65	9 51 53·65	17 40 51·34
18 49 43·40	10 59 30·40	10 34 49·70	10 26 33·70	10 1 56·50	17 50 55·76
		10 54 53·40	10 46 37·55	10 22 2·45	18 11 5·07
				10 32 24·75	18 21 28·63
				10 41 59·80	18 31 5·57
				10 51 59·60	18 41 7·08
				11 2 3·40	18 51 12·47
Mean	Mean	Mean	Mean	Mean	Mean
+18 19 41·83	10 29 33·97	10 17 20·60	10 9 4·50	10 23 17·57	—18 12 20·31
+ 14 15·41	= $f' - f$			$l' - l =$	— 12 15·42
	L′ — L =	— 12 13·37	F′ — F =	+ 14 13·07	
+18 33 57·24	Reduction =	— 2·05	Reduction =	+ 2·34	—18 24 35·73
—18 24 35·73					
	$l' - l =$	— 12 15·42	$f' - f =$	+ 14 15·41	
9 21·51	= Diff. Mer^{s}				

July 21.

P	L	L′	F	F′	G
h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.
17 37 23·10	9 39 24·70	9 54 50·40	9 46 38·95	9 42 7·65	17 38 57·12
17 47 32·10	9 49 32·70	10 4 53·10	9 56 41·50		
18 7 40·95	10 9 38·60	10 14 51·20	10 6 39·80		
18 17 30·30	10 19 26·40	10 34 49·60	10 26 38·25		
18 37 40·75	10 39 33·20	10 44 59·40	10 36 47·90		
18 57 43·90	10 59 33·30	11 4 51·80	10 56 41·10		
Mean	Mean	Mean	Mean	Mean	Mean
+18 14 15·18	10 16 11·48	10 26 32·58	10 18 21·25	9 42 7·65	—17 38 57·12
+ 10 22·81	= $l' - l$			$f' - f$ =	— 36 19·55
	L′ — L =	+ 10 21·10	F′ — F =	— 36 13·60	
+18 24 37·99	Reduction =	+ 1·71	Reduction =	— 5·95	—18 15 16·67
—18 15 16·67					
	$l' - l$ =	+ 10 22·81	$f' - f$ =	— 36 19·55	
9 21·32	= Diff. Merids.				

July 22.

P	L	L′	F	F′	G
h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.
17 31 12·15	9 29 18·60	9 44 50·80	9 36 39·80	9 32 8·95	17 32 53·31
17 51 18·70	9 49 21·90	9 54 53·50	9 46 42·45	9 42 6·95	17 42 53·38
18 1 15·65	9 59 17·10	10 4 53·10	9 56 42·50	9 52 8·55	17 52 56·58
18 11 21·70	10 9 21·70	10 15 8·60	10 6 57·85	10 2 9·90	18 2 59·29
18 21 43·60	10 19 41·70	10 24 48·30	10 16 37·50		
18 31 31·80	10 29 28·40	10 34 58·70	10 26 48·05		
18 51 29·80	10 49 23·00	10 44 57·50	10 36 47·00		
		10 54 47·70	10 46 37·15		
		11 4 48·80	10 56 37·90		
Mean	Mean	Mean	Mean	Mean	Mean
+18 11 24·77	10 9 24·63	10 24 54·11	10 16 43·36	9 47 8·59	- 17 47 55·64
+ 15 32·03	= $l' - l$			$f' - f$ =	— 29 39·63
	L′ — L =	+ 15 29·48			
+18 26 56·80	Reduction =	+ 2·55	F′ — F =	— 29 34·77	- 18 17 35·27
—18 17 35·27			Reduction	= — 4·86	
	$l' - l$=	+ 15 32·03			
9 21·53	= Diff. Merids.		$f' - f$ =	= —29 39·63	

These various results differ so little from each other, that their arithmetical mean, $9^{m}\ 21^{s}\cdot45$, may be assumed to be near the truth. But it may not be improper to ascertain the most probable value, and its probable error, by the calculus of probabilities as practised by GAUSS, BESSEL, &c. to serve as a rule for other investigations of a similar nature, in which it may be more requisite. Each night's result is liable to an error occasioned by the errors in the observed times of the signals, and of the transits of stars, whereby the clocks were compared with the heavens. The probable error of a single observation of a signal and a transit, appears from a considerable number of observations, to be one tenth of a second; and this divided by the square root of the number of these phenomena observed at any station, gives the probable error of the mean of the observed times at that station. But the results are exposed to other causes of error, such as the small deviations of the transit instruments from their meridians, the peculiar state of the eyes of the different observers, atmospheric circumstances, and various others which fluctuate from night to night, but may be supposed constant during the same night. Each night's result is equally liable to these errors, which have no tendency to be diminished by an increased number of observations upon that night. The probable error of each result arising from this cause is assumed to be two tenths of a second, and it is not likely to be more. Errors in the comparison of the chronometers employed for the observations of signals at Greenwich and Paris with the transit clocks, are also to be apprehended, which errors at each observatory may be supposed to be one tenth of a second. A probable error of one tenth of a second in the reduction of the interval at Lignieres, on the 18th, ought also to be taken into account. By the theory of probabilities, the square of the probable error in the result of each night's observations is equal to the sum of the squares of the various errors, which by their combination produce the error in the result; and the weight to be attributed to each result is expressed by the reciprocal of the square of its probable error. The computation of these quantities is exhibited in the annexed Table, in which Δ denotes the difference of meridians obtained from each night's observations, *e* its probable error, and W its weight.

Day of Observation.	Δ	e^2	W.	$(\Delta - 9^m 21^s) \times$ W.
	m. s.	s.	s.	s.
18th	9 21·44	$\frac{0{\cdot}01}{5}+\frac{0{\cdot}01}{3}+\frac{0{\cdot}01}{3}+\frac{0{\cdot}01}{2}+\frac{0{\cdot}01}{2}+\frac{0{\cdot}01}{6}+\frac{0{\cdot}01}{6}+\frac{0{\cdot}01}{6}+0{\cdot}04+0{\cdot}01+0{\cdot}01+0{\cdot}01=0{\cdot}09366$	10·68	4·699
19th	9 21·51	$\frac{0{\cdot}01}{3}+\frac{0{\cdot}01}{3}+\frac{0{\cdot}01}{3}+\frac{0{\cdot}01}{4}+\frac{0{\cdot}01}{4}+\frac{0{\cdot}01}{8}+\frac{0{\cdot}01}{8}+\frac{0{\cdot}01}{3}+0{\cdot}04+0{\cdot}01+0{\cdot}01=\quad 0{\cdot}08083$	12·37	6·309
21st	9 21·32	$\frac{0{\cdot}01}{4}+\frac{0{\cdot}01}{6}+\frac{0{\cdot}01}{6}+\frac{0{\cdot}01}{6}+\frac{0{\cdot}01}{6}+\frac{0{\cdot}01}{1}+\frac{0{\cdot}01}{1}+\frac{0{\cdot}01}{1}+0{\cdot}04+0{\cdot}01+0{\cdot}01=\quad 0{\cdot}09917$	10·08	3·226
22d	9 21·53	$\frac{0{\cdot}01}{5}+\frac{0{\cdot}01}{7}+\frac{0{\cdot}01}{7}+\frac{0{\cdot}01}{9}+\frac{0{\cdot}01}{9}+\frac{0{\cdot}01}{4}+\frac{0{\cdot}01}{4}+\frac{0{\cdot}01}{5}+0{\cdot}04+0{\cdot}01+0{\cdot}01=\quad 0{\cdot}07408$	13·60	7·208
			46·73)	21·442(0ˢ·46

Most probable mean of the whole $= 9^m\, 21^s{\cdot}46$.

The probable error of this mean is equal to the reciprocal of the square root of the sum of the weights; or $= \frac{1}{\sqrt{46{\cdot}73}} = 0^s{\cdot}15$.

It may therefore be said that $9^m\, 21^s{\cdot}46$, or to the nearest tenth of a second, $9^m\, 21^s{\cdot}5$ is the most probable value of the difference of meridians in question; that it is likely that this determination is within two tenths of a second of the truth; and that additional observations, even to a very considerable number, would not materially diminish the small uncertainty that still exists.

The above rectification of the observations made to ascertain the difference of longitude between Paris and Greenwich, not only adds greatly to the merit of the distinguished observers employed in the work, but trebles the value of their results by narrowing the extreme range of the experiments from $0^s{\cdot}65$ to $0^s{\cdot}21$.

THOMAS HENDERSON.

Edinburgh, 30th March, 1827.

ERRATA IN MR. HERSCHEL'S PAPER.

Page 85, July 20, *for* " 9 49^m $39^s{\cdot}6$ " *read* " 9^h 49^m $29^s{\cdot}6$."
19, — " 9 44 50 " —— " 9 54 50."
90, 21, — " 10 26 28·3 " —— " 10 26 38·3."
22, Signals No. 6, erroneously set down. As they stand they are repetitions of No. 7.
102, 21, *for* " clock 1^h 9^m $50^s{\cdot}88$ " *read* " 17^h 9^m $50^s{\cdot}88$."
104, 17, — " —— 17 32 44·40 " —— " 17 32 42·40."
—— — — " —— 18 22 52·48 " —— " 18 22 54·48."
109, line 10 from bottom, *for* " or " *read* " on."
111, — 7 from top, *for* " — " *read* " ×."
—— — 12, ———— *for* " *z* " *read* " Z."
119, July 19, *for* " C′ 9^h 46^m $36^s{\cdot}65$ " *read* " 9^h 46^m $33^s{\cdot}65$."
120, — 22, line 2d from bottom, *for* " — $3^s{\cdot}03$ " *read* " $0^s{\cdot}03$.

XX. *Some observations on the effects of dividing the nerves of the lungs, and subjecting the latter to the influence of voltaic electricity.* *By* A. P. W. Philip, *M. D. F R. S. L. and E.*

Read May 10, 1827.

The Royal Society did me the honour in 1822 to publish the results of some experiments, from which it appeared that the secreted fluids of animals are so deranged by dividing the nerves of the secreting organs, and separating the divided ends, that they are no longer capable of their functions; and that after these functions are thus destroyed, they may be restored by transmitting voltaic electricity through the secreting organs by the portions of the divided nerves attached to them.

In the statement of these results, the attention was chiefly directed to the function of the stomach. In the present communication I shall make a few additional observations respecting the lungs.

However much the secreting surface of the stomach may be deranged by the means just mentioned, its appearance, owing we have reason to believe to the extreme minuteness of its structure, is the same as when the nerves have been left undisturbed, or nearly so, and with the exception of occasional efforts to vomit, no symptom shows itself after the division of the nerves indicating the derangement of function which has taken place. Both in the symptoms and appearances after death, the derangement occasioned in the lungs by their division is much more remarkable.

Soon after the operation the animal begins to breathe with difficulty, and this symptom gradually increases, and is at length evidently the cause of death. On inspecting the lungs after death, the air tubes and cells, as far as they can still be traced, are found filled with a viscid fluid; and in a considerable proportion of the lungs, generally more or less according to the time the animal has survived the operation, every trace of both tubes and cells is obliterated, the lungs both in colour and consistence assuming much of the appearance of the liver. The portions of lungs thus changed sink in water; and although examined with the greatest care, and the aid of a powerful magnifying glass, both by Mr. CUTLER, who was so kind as to give me his assistance, and myself, we could not perceive in them the least remains of the structure peculiar to this viscus.

I wished however to ascertain, by means less fallacious than the sight, whether the structure of the lungs in the parts most effected, be really so changed as to cause the obliteration of their cavities. Mr. CUTLER, at my request, was so obliging as to make the following experiments, the account of which I shall give in his own words.

" If you cut out a portion of each of the eighth pair of " nerves in the neck of a rabbit, it seldom dies within eight " hours, and rarely survives more than twenty-four hours.

" On examination after death, the lungs are found, in " many parts, covered with dark red patches.

" To ascertain the mischief done to the substance of the " lungs, I endeavoured to fill them with mercury by the " trachea, but from the delicate structure of the air cells a " rupture took place, and the mercury escaped.

" I then endeavoured to inject the air cells through the " trachea with the finest vermilion injection. In the healthy

" lungs the attempt was invariably successful, making the " whole of a bright scarlet colour, and, on cutting into them, " every part was found to be uniformly filled with the in- " jection.

" After injecting the diseased lungs, the dark red patches " remained on their surface: other parts of the lungs were " of a bright red colour: some parts were partially injected, " and other parts retained their natural appearance.

" This was explained on dissection. Those parts of the " lungs which were completely injected had not suffered from " disease, other parts had suffered sufficiently partially to " obstruct the injection, while some parts were so completely " hepatised that not a particle of injection could enter them, or " the parts beyond them, which were not equally diseased.

" Those portions of the lungs which were completely " injected, sunk in water, from the weight of the injection.

" The hepatised portions, from their diseased state, sunk " also, whilst the portions beyond them, having their natural " appearance, floated."

If, as I have repeatedly ascertained, and various gentlemen have witnessed, after the nerves are divided, and the divided ends separated, the due degree of voltaic electricity be transmitted through the lungs, by those portions of the nerves which remain attached to them, no affection of the breathing supervenes, and the lungs, after death, are found quite healthy, unless the electricity has been applied of such power, or continued for such a length of time, as to excite inflammation, and *then the appearances on dissection are those of inflammation, not those produced by the division of the nerves of the lungs.*

It appears from these facts, that the effect of dividing the nerves of a vital organ, and separating the divided ends, is not merely that of deranging its secreting power, but *all those powers on which its healthy structure depends*; and that the effect of voltaic electricity, is that *of preserving all these powers.* It is particularly to be observed, that the voltaic apparatus should be so arranged that its influence may be transmitted through the lungs as soon as the nerves are divided, the delay of even a short time appearing to give rise to more or less morbid appearance in the lungs.

The present Paper may be considered as the concluding part of an inquiry in which I have been engaged for many years; two papers relating to which the Royal Society did me the honour to publish in the Philosophical Transactions for 1817 and 1822.* To the first of these papers I have already had occasion to refer; the other was entitled, "On the effects of Galvanism in restoring the due action of the lungs." The objects of this inquiry were to ascertain how far the nervous power is essential to the function of secretion, and the other assimilating processes of the animal body; and whether the voltaic electricity, applied as far as possible, in the same way in which the nervous power is applied, is capable of supplying its place in these processes. It appears from the various experiments, the results of which have now been laid before the Society, that the answer in both instances is in the affirmative.

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* The contents of these Papers have, with the consent of the President and Council of the Royal Society, been re-published more in detail, in the third edition of my Inquiry into the Laws of the Vital Functions.

XXI. *On the effects produced upon the Air Cells of the Lungs when the pulmonary circulation is too much encreased.* *By Sir* Everard Home, *Bart. V. P. R. S.*

Read May 31, 1827.

While Mr. Bauer was engaged in making the microscopical observations contained in a former paper on the structure of the human lungs, he compared the appearances with those in the lungs of other animals, and found in the quadruped the principal difference to be, the more minute branches of the bronchiæ have imperfect cartilaginous rings to a greater extent towards the air cells than in the human lungs.

In cold blooded animals, as the turtle, the bronchiæ do not in their ramifications diminish as in the quadruped, but remain of a considerable size, and the lateral branches degenerate into a trellis work, which is only bounded by the circumference of the organ.

The cells of the human lungs are not dilatations of the bronchial tubes, but are regular cells in which the tubes terminate. The animals Mr. Bauer examined were the hare, the sheep and the turtle. In the human lungs and those of the hare the superficial cells are larger than the interior. In examining the air cells of a hare that had been coursed, he found the superficial large cells filled with colourless coagulable lymph, forming white specks, and the smaller more interior ones filled with coagula of red blood. No such appearance was met with in a hare that had been snared, or one that had been shot.

To determine whether this effect had been produced by the animal's over exertion, I procured two hares which had had an unusually long run; and this appearance was conspicuous in the lungs of both of them, but more extensively in the one than in the other, most probably from that hare having been more pressed by the greyhounds. This appearance, which gives us the exact form of the large superficial cells, is represented in the annexed drawing.

Never having been present at a coursing party, I applied to the gamekeeper of Richmond Park, where they course regularly during the season twice a week to supply the King's table. Mr. Sawyer informed me that a run of fifteen minutes with greyhounds was rarely exceeded, and when a hare is pressed for so long a time, it often sinks from exhaustion and dies, although the dogs have not reached it; the greyhounds themselves after so long a run are so blown as to be often unable to seize the hare with their teeth, although within their reach. He considers a run of a quarter of an hour with greyhounds, to press the hare equally with a run with harriers in hunting for three hours; and in both cases the animal frequently dies from over exertion.

That the natural state of the air cells in the lungs in which the white specks were so abundant, while the more interior were filled with extravasated red blood, might be ascertained, I got Mr. Russell, whose name is mentioned in the former communication, to pour quicksilver into some of the bronchiæ, and fill the air cells, that they might be distended to the utmost, and then immerse the portion of lung so injected into rectified spirit, to prevent them from afterwards collapsing. This was very effectually done, and Mr. Russell remarked that the quicksilver did not pass so quickly into the cells as in his former attempts to fill those of the human lungs.

Mr. BAUER some days after examined the internal structure in the microscope, magnifying the parts 20 diameters. He found not only the cells full of mercury, but the branches of the bronchiæ which terminated in them also distended with it, which was not the case in the human lungs; the terminations of the bronchiæ being composed of elastic membrane, had squeezed out the quicksilver.

This difference of structure of the more minute branches of the bronchiæ, may be a provision of nature to give a ready admission of air into the cells at the time the animal is hard run, while in the human lungs, the elastic membranous structure admits of the volume of air being varied according to circumstances, as we find it is in the act of singing, and in playing on wind instruments, which I understand is often attended with bad consequences in such individuals whose lungs are of a delicate texture; and when long persevered in has even proved fatal, from producing probably the same effect as in animals that are hard run. The white specks that have been described appear to be portions of coagulable lymph, separated from the circulating blood, in consequence of the disturbed state of that fluid in its passage through the branches of the pulmonary artery, and afterwards deposited in the larger and superficial air cells, in the same manner as coagulable lymph is deposited on the internal membrane of veins during a state of inflammation of these canals. That such a separation frequently takes place while the blood is in a fluid state, both in the body and when drawn from the arm, I have upon another occasion given several illustrations.

Every thing that is valuable in the pursuits of comparative anatomy arises from its making us better acquainted with the structure of the human body, and the uses of its different organs, respecting which we have no other mode of acquiring information. What works can we consult for the improvement of anatomy and physiology, from which knowledge we are to derive our rudiments in the treatment of diseases so as to relieve the miseries of mankind, than those of the Great and all wise Author to whom we owe our being; and who has spread before us the whole animal creation, not only for the purposes of affording food and raiment, but also to make us better acquainted with the mechanism of our own bodies?

I am led to make these remarks in this place, since it appears to me that the specks met with in the lungs of the hare when the velocity of its progressive motion is overstrained, gives us great insight into the disease of the lungs called tubercles, one of the most prevalent, and, I may say, the most destructive to the natives of our climate.

On Tubercles in the Human Lungs.

In proof of our ignorance of the origin of tubercles in the lungs, it will only be necessary to examine Dr. BAILLIE's valuable account of this disease, both in his verbal description, and the representation he has given of the appearances that tubercles put on. He had opportunities of referring to what the anatomists before him had done on the subject, and, as a teacher, the natural, healthy structure of these organs was familiar to him. The disease in all its stages must have been constantly under his eye in the numerous dead bodies with which the dissecting room was for twelve years, during which he taught anatomy, regularly supplied, and yet he neither had become acquainted with its origin nor its nature.

He considers it to consist of white specks situated between the pleura and air cells, too small to admit of particular examination.

In his Morbid Anatomy, his words are, "Tubercles consist of rounded bodies of a white colour, interspersed "through the substance of the lungs; they are probably "formed in the cellular structure which connects the air "cells of the lungs together, and are not a morbid affection "of glands, as has been frequently imagined. There is no "glandular structure in the cellular connecting medium of "the lungs; on the inside of the bronchiæ, continued from "the trachea, where there are follicles, tubercles have never "been seen. They are at first very small, not being larger "than the heads of very small pins, and in that case are "frequently accumulated in small clusters. The smaller "tubercles of a cluster grow probably together and form "one large one. The more ordinary size of a tubercle is "about that of a garden pea, but they are subject in that "respect to great variety; they adhere closely to the substance of the lungs, but have no particular covering, or "capsule, and have little or no vascularity; when cut into, "they are found to consist of a white smooth substance "having a firm texture, and often contain in parts a thick "curdly pus." Dr. Baillie has given two plates, in each of which there are two figures. In the first, the tubercles are of a small size, but from that representation no conclusions can be drawn, either as to their origin, or their real situation respecting the healthy structure of the organ.

In the second, which he mentions to be more rarely met with, they are shown both on the convex and concave surfaces close to one another, immediately under the surface, and projecting through the pleura; these have acquired a considerable size, and it is much to be regretted that they were not met with in an earlier stage, and that the history of this particular case has not been registered; but when the appearance, which is shown, and the section of the tubercle is accurately observed, no one can doubt that the origin of this species of tubercle must have been from particles of coagulable lymph deposited in the superficial air cells, similar to those met with in the hare.

EXPLANATION OF PLATE XIII.

Fig. 1. Represents the superficial cells of the lungs of a hare that had been coursed, filled with colourless coagulable lymph; magnified 20 diameters.

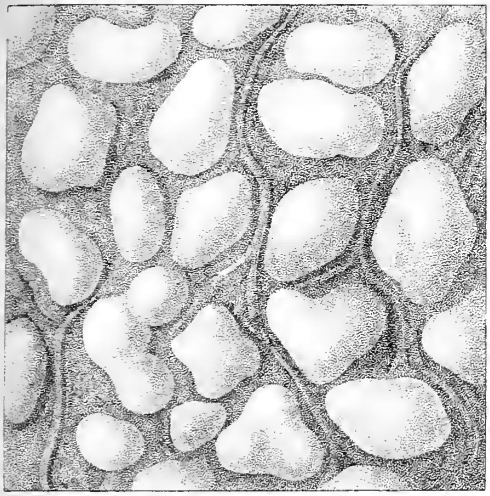
Fig. 2. A section of the same lungs, in which the deeper seated cells are exposed; magnified in the same degree.

Fig. 3. A perpendicular section of the superficial cells; in the same degree magnified.

Fig. 4. The bronchial branches and superficial cells of a hare that had been coursed, filled with mercury where not previously occupied by coagulable lymph; magnified 20 diameters.

Fig. 5. A section of another portion of the same lung, in

1.



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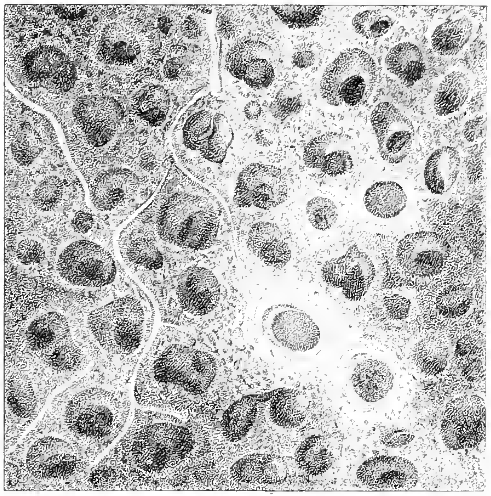

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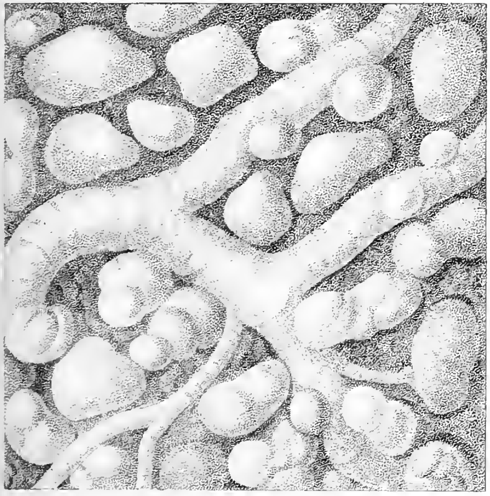

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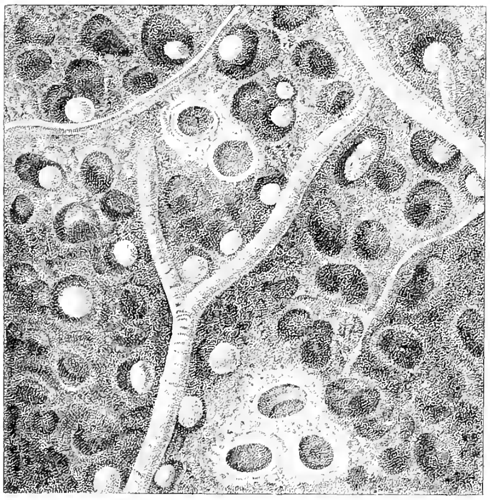

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which the deeper seated cells are filled with mercury; magnified in the same degree.

Fig. 6. The bronchial ramifications traced to their termination at the air cells in the hare; magnified in the same degree.

Fig. 7. A similar portion of the bronchial ramifications in the sheep; equally magnified.

XXI. *Theory of the Diurnal Variation of the Magnetic Needle, illustrated by experiments.* *By* S. H. Christie, *Esq., M. A. F. R. S.*

Read June 14 and June 21, 1827.

In a Paper published in the Transactions for 1823, I stated my opinion, that the diurnal variation of the needle was probably due to the influence of temperature, but that the principle adopted by Canton would not account for the morning easterly variation. In a subsequent paper,[*] I pointed out that the changes in direction and intensity appeared always to have a reference to the position of the sun with regard to the magnetic meridian; the direction of the needle being undisturbed nearly at the time the sun passed that meridian; and the horizontal intensity being the least at the same time. Having taken this view of the subject previously to my being aware of Dr. Seebeck's discovery, that magnetical phænomena will arise from a disturbance in the equilibrium of temperature, my knowledge of that discovery and of subsequent experiments, particularly those of Professor Cumming, confirmed me in the opinion, that temperature must have a considerable effect in producing some of the phænomena of terrestrial magnetism, although I considered that this influence might be modified by the effects produced by rotation, or by peculiar influence in the sun's rays.

At the conclusion of the Paper[†] describing the experiments

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[*] Philosophical Transactions, 1825.

[†] It appears that this Paper was read before the Cambridge Philosophical

to which I have referred, Professor CUMMING makes this remark : " Magnetism, and that to a considerable extent, it appears, is excited by the unequal distribution of heat amongst metallic, and possibly amongst other bodies. Is it improbable that the diurnal variation of the needle, which follows the course of the sun, and therefore seems to depend upon heat, may result from the metals and other substances which compose the surface of the earth, being *unequally* heated, and consequently suffering a change in their magnetic influence ?" And in the second part of a paper detailing some thermo-magnetical experiments, read before the Royal Society of Edinburgh, Dr. TRAILL considers, " that the disturbance of the equilibrium of the temperature of our planet, by the continual action of the sun's rays on its intertropical regions, and of the polar ices, must convert the earth into a vast thermo-magnetic apparatus :" and " that the disturbance of the equilibrium of temperature, even in stony strata, may elicit some degree of magnetism."* I am not, however, aware that any thermo-magnetical experiments bearing directly on the subject of the phænomena of terrestrial magnetism, have yet been published.

By varying the original experiment, Professor CUMMING,

Society in April, 1823, but was not published for a considerable time afterwards; and I was not aware of its having been read when I sent my second Paper to the Royal Society in 1824.

* This interesting communication appears to have been read in February, 1824, but I believe it has not yet appeared in the Edinburgh Philosophical Transactions; and I only know it from a short abstract in the Edinburgh Philosophical Journal for October, 1824, with which, however, I was not acquainted, until I had made nearly the whole of the thermo-magnetic experiments described in the present Paper.

Dr. TRAILL, M. BEQUEREL, and others, obtained many highly interesting results; but in all these, the metals had been united only at *particular points*, and heat or cold applied at *the joints;* and I am not aware of any experiments having been made with different metals *symmetrically united throughout.* Perhaps however the following experiment, mentioned in the abstract of Dr. TRAILL'S Paper, may be considered an exception: "When a piece of metal has one of its surfaces applied *flatly* to another equal metallic plate, a thermo-magnetic combination is formed." It is an exception, and one bearing directly on the subject, if we are to look to "*the disturbance of the equilibrium of temperature in stony strata,*" as the cause of any of the phænomena of terrestrial magnetism. If however these phænomena are to be attributed to *electro-magnetism excited by heat*, it appears to me more probable, that *the disturbance of the equilibrium of temperature* arises from the *inequality of the conducting powers of the atmosphere, and of the land or water, with which it is every where in contact*, or *of those of the land and water*, than from the difference in the conducting powers of the various *strata* of the earth itself.

Thermo-magnetic phænomena have as yet only been exhibited by *metallic* combinations, but this may have arisen from the smallness of the masses on which we may have been enabled to experiment; and it is by no means improbable, that, were experiments made on a large scale, these phænomena might be exhibited in combinations of *any substances whose conducting powers differ greatly.*

Admitting then the possibility of electro-magnetism being excited in such combinations as the *earth and its atmosphere*, their contact being supposed to take place as between *a bar and a wire connecting the two ends*, heat being applied at *a joint*, we must first enquire, whether substances which exhibit magnetic phænomena when *thus joined*, and heat is *so applied*, will *also* exhibit them when their contact is *symmetrical throughout*, and heat is applied to *any point;* and if so, whether the directions in which the forces elicited would act, according to the distribution of heat on the earth, will account for any of the phænomena of terrestrial magnetism.

In the first experiments which I made with these views, I employed a compound ring of bismuth and copper, the bismuth having been cast in a circular ring, the outer circumference of which was formed by a slip of thin copper having the two ends rivetted together, and holes punched through at regular distances: the metals were thus in contact throughout. With this ring I immediately found, that, to whatever point heat was applied, magnetic phænomena were exhibited, a needle being affected differently, according to the different positions in which the ring was placed with regard to it. It is not my intention to detail the results which I obtained, as, although they were as uniform as I could expect, the experiments were made only for a first trial, to ascertain whether any sensible effect would be produced on a needle, by copper and bismuth so united, to whatever point in the combination heat was applied. They removed all doubt on this part of the subject; but after making them, I was so much otherwise engaged, that more than two years elapsed before I had sufficient leisure to repeat and extend them with a more efficient apparatus.

In the experiments which I have more recently made, and which I shall now describe, I made use of a flat ring of cast copper, the outer diameter of which is 11·9 inches, the inner diameter 10·05 inches, ·24 inch in thickness, and weighing 31 oz. Tr. In the interior side of this ring, three grooves were turned, and this side was covered with solder, consisting of 2 parts bismuth and 1 part lead, which, being fusible at a very low temperature, ensured a perfect contact between the copper ring and the bismuth which was cast in the interior of it. This formed a circular plate nearly 12 inches in diameter, and weighing 119 oz. Tr. A wooden axis passed through a hole, ·3 inch in diameter, in the centre of the plate, so that it could be made to revolve in its own plane.

This plate being placed vertically or horizontally, a small compass, with a needle two inches in length, very delicately supported, and having the rim within the box, which is of silver, accurately divided to degrees, was placed in different positions with regard to it; the directive force of the needle being always diminished in the ratio of 9 to 1, by means of a bar magnet placed at an invariable distance to the south of the needle. Heat was then applied to a point in the periphery of the plate for a certain time, by means of a lamp with a small wick, giving a small and well defined flame. The lamp being removed, and the deviation of the needle observed, different points of the plate were successively brought under, or opposite to, the needle, according as it happened to be placed over, or by the side of, the plate, by turning it, in its own plane, about the axis at right angles to it, and the deviation of the needle corresponding to each position of the plate, noted. When the plate had been brought into its original position, and the deviation corresponding to this position again noted, the lamp was again applied to the same point, for the same time as before, and a second set of observations made, by bringing the several points of the plate to the compass in the order contrary to that which had been previously used. By this means, the deviation of the needle corresponding to each position of the plate, and to a temperature of the heated point nearly a mean between its temperature at commencing and that at concluding the observations, was obtained.

The experiments which I proposed to myself were these: to place the plane of the plate vertical, and likewise horizontal; in the former case both in the plane of the meridian, and at right angles to it: to adjust the compass to certain positions with regard to its centre and surface, and to observe the effects produced on the needle when the heated point on the plate had various positions. By this means, I expected to be able to determine the nature of the laws which these deviations observed in all cases, or the species of polarity in the plate which would account for them. Having made these observations, I found that, if through the place of heat a diameter were drawn, which, for distinction, I call *the axis of heat*, and another at right angles to it, then looking on to either side of the plate, with the place of heat downwards, if I supposed a *south pole* in the lower quadrant on the *right hand*, and a *north pole* in that on the *left*, the character of the deviations would agree with such polarity of the plate.

It is not my intention to enter upon any theoretical views with regard to the nature of the forces which are the cause of thermo-magnetical phænomena: whether they arise from electric currents being excited in particular directions, and acting in a peculiar manner on a magnetised needle, or not, is not the object of my present enquiry, although the experiments which I shall detail may be of a nature to throw some light on this part of the subject. I therefore adopt the term *pole*, as a convenient one, to indicate in a general manner, the points towards which the attractive and repulsive forces, apparently acting upon the needle, seem to tend; and the term *axis of polarity*, to indicate the line joining two poles, the forces towards one of which are attractive, and towards the other repulsive, for the same end of the needle.

My first object was to ascertain that, whatever might be the action of the plate, it was tolerably uniform throughout. This was done in the first instance by placing the plate with its plane in that of the meridian, and the centre of the needle in the vertical diameter produced, at the distance ·55 inch from the upper edge, and then observing the deviations, in the manner I have described, when different points of the plate had been heated, by applying the lamp to them for two minutes, taking care, after a set of observations had been made for each heated point, to cool the whole previously to heating another. The circumference of the plate was on one face divided into eight equal parts, and the points of division numbered 0, 45, 90, 135, 180, 225, 270, 315, so that this face being towards the west and 0 downwards, 90 was south. The observations are contained in the following table. The centre of the plate being taken as the centre, the angular distances between the centre of the needle and the heated point are indicated in the first column: they are measured from the vertical towards south, and always in the same direction to 360°. The observed deviations of the needle, corresponding to these different angular positions of the heated point, are inserted in the columns over which are indicated the points on the plate which were successively heated; the deviations in the first column of each set being due to the particular angular positions of the heated point when that point approached the compass through the north; those in the second, when it approached through the south; and those in the third, the means of these two, and which are considered as the deviations due to the several positions of the heated point, had that point preserved its mean temperature throughout the revolution of the plate.

0 the heated point.			180 the heated point.			90 the heated point.			270 the heated point.			Means of the mean Deviations.
Deviations.			Deviations.			Deviations.			Deviations.			
the points on the brought under the ass in the			When the points on the plate brought under the compass in the			When the points on the plate brought under the compass in the			When the points on the plate brought under the compass in the			
ect er.	Reverse order.	Mean.	Direct order.	Reverse order.	Mean.	Direct order.	Reverse order.	Mean.	Direct order.	Reverse order.	Mean.	
′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′
0 E	0 30 E	2 15 E	0 45 E	0 30 W	0 15 E	1 30 E	0 30 W	0 52 E	0 30 W	1 30 W	1 22 W	0 30 E
0 W	12 30 W	12 30 W	22 30 W	17 45 W	20 07 W	23 30 W	16 45 W	20 07 W	15 00 W	17 00 W	16 00 W	17 11 E
0 W	23 30 W	28 00 W	34 00 W	25 45 W	29 52 W	34 30 W	24 30 W	29 30 W	26 30 W	26 30 W	26 30 W	28 28 E
0 W	26 30 W	29 15 W	31 00 W	25 30 W	28 15 W	30 30 W	25 00 W	27 45 W	26 30 W	33 00 W	29 45 W	28 45 E
0 W	7 00 W	5 15 W	1 30 E	1 45 W	0 07 W	5 15 E	0 15 E	2 45 E	4 00 W	8 45 W	6 22 W	2 15 W
0 E	33 00 E	27 30 E	23 30 E	31 30 E	27 30 E	26 00 E	33 00 E	29 30 E	16 15 E	34 30 E	25 22 E	27 28 E
0 E	35 30 E	28 00 E	24 30 E	36 00 E	30 15 E	25 00 E	34 30 E	29 45 E	16 30 E	38 30 E	27 30 E	28 52 E
0 E	25 00 E	18 30 E	16 30 E	23 30 E	20 00 E	16 30 E	25 00 E	20 45 E	9 15 E	23 30 E	16 22 E	18 54 E
0 E	4 30 E		0 30 E	0 15 E		1 00 E	2 30 E		0 30 W	3 00 W		

In comparing the mean results when the different points of the plate were heated, it is to be considered that minute accuracy could not be attained, either in applying the lamp to the *precise* point indicated, or *exactly* to the middle point between the two surfaces of the plate; and that the angular distances of the heated point were liable to errors arising from the first circumstance, as well as from adjustment; also, that the needle not always coming to rest in the same time, it was not possible to make the successive observations at precisely the same intervals throughout. Such care was however taken to guard against this last source of error, that each set was made in very nearly the same time: the whole set, when the point 0 was heated, occupied 18 minutes; that when 180 was heated, 18 minutes; that when 90 was heated, again 18 minutes; and that when 270 was heated, 19 minutes. The observations then clearly show that the deviations of the needle, corresponding to the several positions of the heated point, did not arise from a peculiarity in the arrangement of the particles, or in the contact of the two metals, in particular parts of the plate; but that to whatever polarity in the plate, or to whatever electric currents these deviations were due, these poles were symmetrically situated, or these currents were uniformly excited, to whatever point in its circumference heat was applied: and that, consequently, whatever might be the results obtained by applying heat in all cases to the same point in the circumference, they would not differ sensibly from the mean of the results obtained from corresponding observations, if heat were applied to several points successively, as in the foregoing instance, in each particular case. To have adopted the latter method in every particular position of the compass and the plate, in which I proposed to investigate the effects, would indeed have been almost an endless task: however, to leave no doubts respecting the results which I might obtain, whenever the deviations were of a nature that might possibly arise from a want of uniformity of action in the plate, I invariably observed the deviations corresponding to the heating of different points. In some positions of the compass, observations were made when 0, 45, 90, 135, 180, 225, 270, 315 had each been the heated point, and likewise when the plate had been reversed; but in no instance did the results differ in character, or even the corresponding deviations vary much in their extent, to whatever point heat had been applied.

To determine the laws which govern the magnetic phenomena resulting from the application of heat to a point in such a metallic combination as I made use of, I first adjusted the plane of the plate at right angles to the plane of the meridian, and placed the compass with its centre in the plane of the meridian passing through the centre of the plate, with its centre 6 inches above that centre, or opposite to the outer edge of the copper ring, and 1·3 inch to the north of the plane of the plate, that is, as near to it as the compass-box would, without touching, admit. The lamp having been applied for 5 minutes to the outside of the copper ring at the point 0, which was the lowest, the deviations of the needle corresponding to the different angular positions of the heated point were observed, as in the preceding instance. Corresponding observations were made when the compass was in the position diametrically opposite, or below the centre of the plate; and also when in the same horizontal plane with that centre, to the east or to the west of it, opposite the outer edge of the copper, at the same distance, 1·3 inch north from its surface: and similar observations to these were likewise made with the centre of the needle, in the corresponding positions, to the south of the plate. Observations also, of precisely the same nature as these, were made with the centre of the compass opposite to the line of junction of the copper ring with the circular plate of bismuth. I thus obtained the deviations of the needle when the compass was opposite to the outer edge of the copper, and when it was opposite to the line of junction of the copper and bismuth, in eight different positions of the compass, in each case; the heated point being, in each position of the compass, in four different positions with respect to the centre of the needle.

The observed deviations, corresponding to the different angular positions of the place of heat from the needle, the centre of the plate being the angular point, are contained in the following table, where this angle is measured from the needle, in the direction of the sun's daily motion, round the whole circle.

Table of the deviations of the needle when the plane of the plate was at right angles to the plane of the meridian, and the centre of the needle opposite to the outer edge of the copper.

Angular distance of the place of heat from the centre of the compass.	The centre of the needle being in the plane of the meridian passing the plate's centre.				The centre of the needle being in the horizontal plane passing through the plate's centre.			
	Above the plate's centre, and		Below the plate's centre, and		To the east of the plate's centre, and		To the west of the plate's centre, and	
	1·3 inch north of its surface.	1·3 inch south of its surface.	1·3 inch north of its surface.	1·3 inch south of its surface.	1·3 inch north of its surface.	1·3 inch south of its surface.	1·3 inch north of its surface.	1·3 inch south of its surface.
°	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′
0	29 30 W	26 45 E	29 00 E	28 15 W	6 15 W	10 00 E	7 15 E	11 30 W
90	24 30 E	24 45 W	6 00 W	5 00 E	35 30 E	32 30 W	30 00 W	33 00 E
180	5 00 E	9 15 W	7 45 W	7 00 E	0 05 E	0 30 W	2 30 W	5 00 E
270	3 00 E	5 00 W	11 30 W	11 00 E	34 30 W	34 00 E	25 30 E	26 30 W

Table of the corresponding deviations of the needle when its centre was opposite to the line of junction of the copper and bismuth.

0	47 30 W	48 00 E	44 45 E	31 15 W	8 30 W	10 45 E	6 00 E	13 45 W
90	10 45 E	5 15 W	6 00 W	6 45 E	14 45 E	13 15 W	14 20 W	18 20 E
180	23 30 E	26 00 W	15 30 W	11 30 E	0 30 W	2 30 W	3 00 W	4 15 E
270	164 30 E	167 30 W	135 00 W 61 15 W	165 00 E 43 30 E *	51 30 W	44 45 E	45 30 E	52 00 W

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* The upper numbers indicate the directions of the needle when first observed, the lower ones those which it assumed almost immediately afterwards. These directions correspond to different positions of equilibrium noticed in a former paper; Phil. Trans. 1823.

If these deviations are to be considered as due to polarity in the plate, the situations of the poles may be deduced from them, pretty nearly, without entering into any calculations; for which purpose indeed the observations could scarcely be made with sufficient precision. For example: the needle being below the centre, and the heated point opposite to it, it is evident, from the character of the deviations, that, looking from the compass on to the plate, either there must be a *south* pole to the *right* of the needle, *or* a *north* pole to its left; or a *south* pole to the *right and* a *north* pole to the *left*. As however the direction of the deviation is changed by making the heated point describe a quadrant, on *either* side of the compass, both these poles must exist, the *south* pole being within the lower quadrant on the right, and the *north* pole within that on the left. The positions of these poles are more precisely fixed by some of the other observations. When the compass was opposite to the line of junction of the copper and bismuth, above the centre of the plate, and to the *north* of its surface, the angular distance of the place of heat from the centre of the needle being 180°, the deviation was 23° 30′ east; when this distance became 270°, the place of heat being 90° to the *east*, or to the *left* of the compass, the deviation became 164° 30′ east; and as the place of heat approached the needle, the *north* end passed through *south*, the deviation becoming N 47° 30′ W, when the place of heat was opposite to the compass. This effect is precisely that which would have taken place had a strong *north* pole existed in the *north*, or marked surface of the plate, in the radius forming an angle of about 64° with the axis of heat, to the *left* or *east*, supposing the place of heat downwards; this *north* pole, as the plate turned on its axis, passing to the south of the needle and repelling its *south end*, or *north pole*. When the needle was to the *south* of the plate, similar changes in its direction fix the position of a *south* pole in a point on its *south*, or unmarked surface opposite to the *north* pole on the *north* side. The observations below the centre indicate the same positions for the pole. I should mention that these observations were made when o was the heated point: but to leave no doubt with regard to these results being due to a uniform action in the plate, I made corresponding observations when 315, 270, 225, 180, 135, 90, 45 were successively the heated points, and in all cases obtained corresponding results. By reversing the sides of the plate, that is, making the marked face south, the observations showed that the positions of the poles were independent of this circumstance.

From the deviations being of the same character, but of greater extent, when the needle was opposite to the line of junction of the copper and bismuth than when opposite to the outer edge of the copper, it would follow, that the poles were nearer to the centre of the plate than this line of junction. I afterwards found, by placing the compass in the horizontal plane passing through the centre of the plate, and at different distances from it towards the east, that when the place of heat was the lowest point, the *north* pole on the *north* side of the plate was in the vertical 4 inches to the *east* of the centre, and *below* the compass; and when the centre of the needle was at this distance to the east, by making the place of heat describe 45° towards the compass, the *north* pole appeared to be a little to the east of the needle's centre, and *above* it. I thus determined, pretty accurately, that the lines drawn to the poles on the east side of the axis of heat, made an angle with that axis of about 64°, and that the poles were at the distance 4·5 inches from the centre.

The same positions of the poles will account for the deviations when the compass was to the east, or to the west of the plate's centre. It however appears from these, that the plate did not act with *perfect* uniformity; that is, that the poles were not *quite* symmetrically situated with respect to the axis of heat. If the *axis of polarity* be supposed to cut the *axis of heat* about 1·9 inch from the centre towards the place of heat, and to be inclined to it so that, supposing the place of heat downwards, the angle below *the axis of polarity* on the western side is slightly obtuse, and on the eastern side, acute, it would perfectly account for the nature of the deviations when the angle between the place of heat and the compass was 0, or 180°.

It would appear then, that such deviations as those observed with the plane of the plate at right angles to the plane of the meridian would arise from four poles in the plate, two near each surface, in a line cutting the axis of heat nearly at right angles, between the centre and the place of heat, the *south* pole being on each surface of the plate on the *right* hand, looking on that surface from the place of heat.

The same polarity will also account for the deviations that I observed with the plane of the plate in the plane of the meridian, and likewise with its plane horizontal. The observations with the plane of the plate in the plane of the meridian were made in precisely the same manner as the preceding, with the addition, that the deviations were likewise observed when the heated point was 45° on each side of the needle. In these the marked face of the plate was west, and the lamp was applied to the point marked o, as before. They are contained in the following Table.

Table of the Deviations of the Needle when the plane of the Plate was in the plane of the Meridian, and the centre of the Needle opposite to the outer edge of the Copper.

Angular distance of the place of Heat from the centre of the Compass.	The centre of the Needle being in the vertical plane at right angles to the plate and passing through its centre.				The centre of the Needle in the horizontal plane passing through the Plate's centre.			
	Above the Plate's centre, and		Below the Plate's centre, and		To the North of the Plate's centre, and		To the South of the Plate's centre, and	
	1·3 inch East of its surface.	1·3 inch West of its surface.	1·3 inch East of its surface.	1·3 inch West of its surface.	1·3 inch East of its surface.	1·3 inch West of its surface.	1·3 inch East of its surface.	1·3 inch West of its surface.
°	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′
0	2 15 E	2 15 E	11 00 E 1 00 E	2 52 E 0 15 E *	4 45 E	3 15 E	2 45 E	1 30 E
45	13 00 W	14 00 W	14 15 W	17 00 W	37 15 W	13 30 W	16 30 W	30 00 W
90	10 00 W	9 00 W	8 45 W	11 30 W	36 30 W	10 30 W	11 07 W	24 15 W
180	0 15 W	0 15 W	0 00	0 15 W	0 30 E	1 15 E	0 30 W	0 52 W
270	9 15 E	7 00 E	9 15 E	10 45 E	15 37 E	16 00 E	17 45 E	12 15 E
315	12 45 E	12 15 E	10 45 E	13 00 E	20 30 E	22 00 E	21 45 E	14 30 E

Table of the corresponding Deviations of the Needle when its centre was opposite to the line of junction of the Copper and Bismuth.

0	6 00 E	9 00 E	10 00 E 0 37 W	7 45 E 0 15 W *	12 00 E	15 15 E	3 15 E	5 30 E
45	34 30 W	45 00 W	34 00 W	40 00 W	77 00 W	38 45 W	36 30 W	61 00 W
90	39 00 W	29 45 W	25 45 W	36 00 W	74 00 W	29 00 W	27 45 W	59 15 W
180	0 22 E	1 15 W	1 07 E	0 15 E	0 00	0 37 W	2 00 W	2 00 W
270	40 00 E	29 00 E	25 15 E	36 30 E	30 15 E	65 30 E	51 00 E	28 30 E
315	36 30 E	44 00 E	30 00 E	31 00 E	38 30 E	72 00 E	58 30 E	34 30 E

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* It will be remembered, that, in all cases, the first observation of the deviation was made, after removing the lamp, when the point to which the lamp had been applied was the lowest, and the deviation was again observed when this point was brought into the same situation, by the plate having been turned on its axis, in order to observe the deviations due to other positions of the heated point; and likewise, that similar observations were made when the plate was turned in the contrary direction. In each of these four positions, then, of the compass, there were four observations corresponding to the angular distance 0°: the upper numbers are the means of the deviations first observed on removing the lamp; and the lower, the means of those when the plate was again brought round into its original position. In the position of the needle opposite to the line of junction of the copper and bismuth, and to the east of the plate, during the time that the lamp

In this position of the plate, the deviations of the needle arising from the action of four poles in the plate, in the situations I have indicated, with the compass in corresponding positions on contrary sides of the plate, would, in each, be in the same direction, the preponderating force on the one side being attractive, on the other side repulsive. These deviations would not however be equal; the limit of the deviation due to the attractive force, however great, being the azimuth of the attracting pole from the centre of the compass; and the limit of that due to the repulsive, the supplement of this angle. So that when the force is considerable, and this angle much less than a right angle, the deviation arising from the repulsive force on one side, will greatly exceed that due to an equal attractive force on the other. On examining the deviations in these tables, it will be found, that, when the deviation on one side of the plate greatly exceeds the corresponding one on the other, as 77° W., 38° 45′ W.; 74° W., 29° W.; 65° 30′ E., 30° 15′ E.; 72° E., 38° 30′ E.; &c., the situations of the poles were such that the deviations in the former cases were due to repulsion, and in the latter to attraction.

The deviations corresponding to the angular distance 0, being in this position of the plate all easterly, is in accordance with what I have stated with regard to the inclination of *the axis of polarity* to *the axis of heat.* According to this,

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was applied, the deviation was N. 140° E., and when it was removed, the needle stood for an instant at N. 117° E., and then moved gradually back till it came to N. 10° E., when the observation was made. This great deviation, probably, arose from some irregularity in the action of the flame during the time it was applied; or, possibly, of the plate itself, rendered, in this position of the compass, at that time particularly sensible.

likewise, if the sides of the plate were reversed, that is the marked face placed east instead of west, the corresponding deviations should be all westerly; and on examining the observations which I made with the marked face so placed, the compass being in the same horizontal plane as the centre of the plate, and opposite to the line of junction of the copper and bismuth, I find them to have been; with the needle to the north of the centre of the plate, 8° W., when it was 1·3 inch to the east of it, and 5° 45′ W., when 1·3 inch to the west; also 12° W., when it was 1·3 inch to the east, and 14° 45′ W., when 1·3 inch to the west, and on the south side of the centre. The other observations agreed with those in the preceding table.

In the observations which I made with the plane of the plate horizontal, and which in every other respect were similar to the preceding, the marked face, or that which in the other observations had been towards the west or the north, was upwards. The compass was placed north, south, east, and west of the centre of the plate, and, as before, its centre 1·3 inch from the surface, and vertically above and below the outer edge of the copper ring. It was likewise placed in corresponding positions vertically over the line of junction of the copper and bismuth; but the corresponding observations could not be made below the plate, as the extremity of the needle was in general hid by it. The plate was, as before, turned in its own plane, and the deviations of the needle noted when the place of heat had different positions with respect to the compass. The observations are contained in the following table, where the angular distance of the place of heat is measured from the needle in the direction of the sun's daily motion round the whole circle.

Table of the Deviations of the Needle when the plane of the Plate was horizontal, and the centre of the Needle vertically above or below the outer edge of the Copper.

Angular distance of the place of Heat from the centre of the Compass.	The centre of the Needle being in the plane of the meridian passing through the Plate's centre.				The centre of the Needle in the vertical plane at right angles to the meridian and passing through the Plate's centre.			
	To the North of the Plate's centre.		To the South of the Plate's centre.		To the East of the Plate's centre.		To the West of the Plate's centre.	
	1·3 inch above its surface.	1·3 inch below its surface.	1·3 inch above its surface.	1·3 inch below its surface.	1·3 inch above its surface.	1·3 inch below its surface.	1·3 inch above its surface.	1·3 inch below its surface.
°	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′
0	5 45 E	11 30 W	9 00 W	8 45 E	5 45 W	2 22 E	1 00 E	2 15 W
45	0 38 E	5 00 W	3 45 W	1 30 E	18 15 E	25 45 W	16 15 E	24 30 E
90	2 30 W	5 00 E	3 00 E	2 15 W	14 00 E	23 15 W	15 30 W	22 15 E
180	3 00 W	3 15 E	2 00 E	3 30 W	4 22 W	1 15 W	0 38 E	0 08 E
270	4 00 W	2 30 E	2 30 E	5 30 W	22 30 W	21 45 E	15 30 E	21 00 W
315	0 52 E	3 15 W	1 20 W	3 08 E	25 30 W	22 00 E	17 00 E	27 00 W

Table of the corresponding Deviations of the Needle when its centre was vertically above or below the line of junction of the Copper and Bismuth.

0	11 00 E	18 00 W*	16 00 W	12 45 E*	0 30 E	Observations could not be made in this position of the compass.	1 30 E	Observations could not be made in this position of the compass.
45	1 15 E	8 00 W	6 00 W	2 45 E	20 15 E		11 30 W	
90	4 00 W	6 45 E	4 45 E	3 15 W	16 00 E		14 30 W	
180	5 00 W	6 30 E	4 00 E	6 00 W	0 00		1 00 W	
270	6 15 W	4 30 E	5 00 E	8 30 W	17 00 W		15 00 E	
315	1 30 E	5 00 W	3 30 W	3 30 E	18 45 W		14 00 E	

The character of these deviations perfectly accords with the position which I have assigned to the poles in the plate, but when the centre of the needle was in the plane of the meridian, and the heated point directly under or over it, the extent of the deviations, compared with others, was greater than would arise from this position of the poles, supposing

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* These observations were made with the centre of the compass vertically below a point ·35 inch from the line of junction towards the outer edge, the extremity of the needle not being visible when nearer to the centre of the plate.

them to be the points of greatest intensity. Throughout the observations, however, a certain degree of irregularity was manifested when the heated point was that nearest to the compass. This, supposing the action to be produced by currents excited in the plate, would arise from the much greater energy of these currents in the immediate neighbourhood of the heated point, and likewise from their being more influenced by variations in the temperature of that point there, than elsewhere. And it is to be observed, that although I have in a general manner referred the action of the whole plate to four poles, in certain positions, yet I by no means suppose, that the forces acting upon the needle tend to points absolutely fixed, whatever may be the position of the compass: and, indeed, I have rather adopted the term, *pole*, to illustrate, in a general manner, the nature of the effects, than as supposing that the action which takes place between the plate and the needle can, in all cases, particularly in the immediate vicinity of the heated point, be *precisely* represented by that which would take place between the poles of two magnets.

It appears, then, from all these observations, that, when heat was applied to a point in the copper ring, the characters of the deviations of the needle, and generally also their extent nearly, were such as would arise from a polarising of the plate in lines nearly at right angles to *the axis of heat*, and cutting that axis between the centre and the place of heat, near each surface, *contrary* poles being *opposite* to each other in the two surfaces. We may therefore, I think, infer, that, if magnetic phænomena are manifested by heat being applied to the joint of two substances, united in any of the usual ways, they will likewise be exhibited when the line of junction is symmetrical throughout, as with a ring of one substance surrounding a circular plate of another, heat being any where applied; and that these phenomena will nearly correspond to a polarising of the compound disc, similar to that which I have indicated for the plate of bismuth and copper; the precise positions of the points to be considered as poles depending upon the nature of the substances united.

Admitting, then, that the *earth and the atmosphere* are subtsances in which such action can, under any circumstances, take place, these experiments would indicate that *any portion of the earth, bounded by parallel planes, with the atmosphere surrounding it, would become similarly polarised, if one part were more heated than another.* Thus, considering alone *the equatorial regions of the earth,* we should have *two magnetic poles on the northern side, and on the southern side two poles similarly posited; the poles of different names being opposed to each other on the contrary sides of the equator.*

In order fully to investigate this subject, it would be necessary that we should know the times of maximum and minimum heat, at least in the equatorial regions, both on the continents and likewise over the surface of the sea; if however we assume, that, in general, the greatest heat occurs about 3 o'clock in the afternoon, and the least about 5 o'clock in the morning, when the sun is vertical to the equator, which is probably very near the truth, it will serve to give an outline of the effects that would be produced on the needle by the revolution of such poles, developed on each side of the equator. According to this view, the coldest point on the equator will be 150° west, or 210° east from the hottest point. On the *northern* side of the equator, we should therefore have the *north* pole which is to the *west* of the place of heat, of *greater* intensity, and *nearer* to that place, than the *south* pole which is to the *east* of it; but, supposing the hypothesis to be correct, the precise situations of these poles could only be determined from accurate observations of the diurnal variation of the needle. Different points in the earth's equator becoming successively those of greatest heat, these poles would be carried round the axis of the earth, and would necessarily cause a deviation in the directions of the horizontal needle in different parts of its surface. We have then to consider, whether the deviations that would arise from magnetic poles, so circumstanced, correspond with the observed diurnal variations of the needle.

With this view, I had proposed to compare the deviations of the needle, caused by the compound plate of bismuth and copper, with the diurnal changes in the needle, observed at different places, by adjusting the plate so that its plane should make with the horizon an angle equal to the complement of the latitude of the place of observation, and the axis about which it revolved an angle with the magnetic meridian equal to the variation at that place: the centre of the needle being then placed vertically over the centre of the plate, the poles in the plate would represent those which I have supposed to be developed on each side of the equator. I however found that at such a distance as the compass would here be placed from the plate, the deviations caused by it would not be of a magnitude to be determined with sufficient accuracy, with the small compass which I had employed. A pretty close approx-

approximation, however, to the nature of these deviations, may be obtained by making two magnetic poles revolve, relatively to a needle, in the same manner as I have supposed those near the equator.

For this purpose I made use of an instrument somewhat resembling an altitude and azimuth circle. To the vertical graduated limb an axis, in the direction of a diameter, is firmly attached. Perpendicular to this axis is a graduated circle carrying two arms, to the ends of which slender bar magnets 6 inches in length are attached perpendicularly to the plane of the circle, and so that their upper poles, which are of contrary names, are in the plane of the circle. These arms are moveable, and may be fixed so that the poles shall subtend any required angle at the centre of the circle, which, with the magnets, may be made to revolve round the axis. It is evident, that if the axis be inclined to the horizon at an angle equal to the latitude of the place of observation, and so that the azimuth of its lower extremity from south of the magnetic meridian be the same as the variation of the north end of the needle at that place, then, a needle being placed vertically over the centre of the revolving circle, the magnets would revolve, relatively to the needle so placed, as the poles which I have supposed near the equator, with respect to a needle at the place of observation; and the deviations of this needle, corresponding to particular positions of the magnets, would indicate the nature of the changes due to the action of poles, similarly placed, near the equator of the earth.

For the purpose of comparison, I have selected the observations made by Lieut. Hood in 1821, at Fort Enterprise, lat. 64° 28′ N. long. 113° 06′ W, where the variation was 36° 24′ E; those made by Canton in London in 1759, when the variation was nearly 19° W; and the highly interesting observations made by Lieut. Foster in the early part of 1825, at Port Bowen, lat. 73° 14′ N, long. 88° 54′ W, and where the variation was 124° W.

As Colonel Beaufoy had for so many years observed the course of the variation of the needle with the most zealous care, and having the advantage of the best instruments, I hoped to have been able to procure a set of his observations at every hour during the day, for a considerable period of time; and I have no doubt, had it not been for the recent loss which science has sustained by the death of that able and indefatigable observer, that whatever observations he had made, that could at all have been applicable to the purpose I had in view, would have been placed at my command, with that liberality for which he was so much distinguished. His published observations, which were commenced in the year 1813, were generally made between 8 and 9 o'clock in the morning, and 1 and 2 in the afternoon, with the view of determining the maximum easterly and the maximum westerly deviations. The last observations to which the times are given are for March 1822, and the mean of these gives 8^{h} 32^{m} A.M. as the time of the maximum east, and 1^{h} 29^{m} P.M. as the time of the maximum west; the mean daily variation being 8′ 58″, and the mean maximum westerly variation 24° 36′ 36″. An observation was likewise made between 6 and 7 P.M. but the maximum easterly deviation in the evening does not generally appear to have been determined: however, after the observations for July 1813, Colonel Beaufoy states, " from several observations it is after eleven at night."

The observations at Fort Enterprise were made every day from the 10th January to the 16th March, and from the 22d March to the 7th April; but as considerable disturbance took place in the direction of the needle, in taking the mean of the deviations at the several hours of observation, it is necessary to exclude those days on which observations were not made at each of these hours. The following table contains the mean, so taken, of the variations at different hours in the months of January, February, and March.

	10h A. M.	1h P. M.	5h P. M.	9h P. M.	12h P. M.	No. of Day's Observations.
January - - -	35° 54′ E	35° 42′ E	35° 42′ E	35° 44′ E	35° 51′ E	18
February - - -	35 54	35 47½	35 40	35 41½	35 41⅓	19
Means for Jan. and Feb.	35 54	35 45	35 41	35 43	35 46	
March - - -	9 A. M. 35 57	35 39	35 30	35 30	35 33	13
Means for Jan. Feb. and March	....	35 43	35 37	35 38½	35 42	

Lieut. Hood states, that the maximum variation east occurred at 9 A. M. and the minimum at 3 or 4 P. M.; but these means clearly indicate that the minimum happened after 5 P. M. Some latitude however must certainly be allowed in determining these points.

Canton's observations in 1759, which are published in the 51st vol. of the Philosophical Transactions, appear to have been made with great care, with the view of determining the amount of the variation at different seasons of the year. On a few days in each month great irregularities, attributed by CANTON to the effect of the aurora borealis, are observable; and in deducing the mean times of maximum and minimum variation, I have excluded all the observations on the days on which these irregularities happened. These are however few, and I am not aware that including them would have much affected the results; but as my object is to determine the more regular effects, I consider that, in taking the mean, they are better excluded. CANTON'S observations were not always made at corresponding hours on different days; so that, to obtain the mean variation at or near to particular hours, I have taken the observations nearest to such hours, on the several days in each month; and taken a mean of the times, and of the observed variations. The following table contains the results thus deduced.

Abstract of the Means deduced from **Canton's** observations, in 1759, of the variation of the needle in London.

	First observation in the morning.		Morning, Minimum West.		Observation between 10h and 11h A. M.		Maximum West.		Evening Minimum West.		Observation nearest to 9h P. M.		No. of Days Observations.
	Time A. M.	Variation West.	Time A. M.	Variation West.	Time A. M.	Variation West.	Time P. M.	Variation West.	Time P. M.	Variation West.	Time P. M.	Variation West.	
	h. m.	° ′ ″	h. m.	° ′ ″	h. m.	° ′ ″	h. m.	° ′ ″	h. m.	° ′ ″	h. m.	° ′ ″	
January	0 15	18 55 21	9 07	18 55 03	10 32	18 57 20	1 35	19 01 47	9 14	18 55 51	8 57	18 56 00	30
February	0 22	18 57 03	9 17	18 54 57	10 51	18 56 49	1 58	19 03 33	9 30	18 57 00	8 48	18 57 57	22
March	0 17	18 58 04	9 09	18 54 45	10 49	18 58 30	1 43	19 05 48	8 35	18 57 56	8 58	18 58 00	30
April	0 22	18 58 30	8 37	18 53 45	10 45	18 59 10	1 33	19 05 53	8 43	18 58 39	9 46	18 58 48	24
Means	0 20	18 57 52	9 01	18 54 29	10 48	18 58 10	1 45	19 05 05	8 56	18 57 52	9 10	18 58 15	
May	0 21	18 58 43	8 29	18 54 11	10 35	19 00 25	1 26	19 05 47	8 22	18 58 54	8 48	18 59 14	27
June...........	0 22	18 59 00	8 29	18 54 31	10 43	18 59 55	1 19	19 07 06	8 24	18 59 02	9 20	18 59 25	29
July	0 14	19 00 20	8 15	18 56 00	10 07	19 00 18	1 42	19 08 16	8 29	19 00 26	9 13	19 01 52	26
Means	0 19	18 59 21	8 24	18 54 54	10 28	19 00 13	1 29	19 07 03	8 25	18 59 27	9 07	19 00 10	
August	0 19	19 00 40	8 29	18 57 41	10 17	19 01 23	1 18	19 09 19	7 37	19 00 18	9 17	19 01 23	28
September.....	0 26	19 03 28	8 51	19 01 03	10 25	19 05 28	1 20	19 12 58	8 46	19 02 52	9 11	19 02 42	24
October.......	0 05	19 05 27	8 55	19 01 03	10 54	19 07 26	1 17	19 11 46	8 58	19 04 27	9 03	19 04 39	22
Means	0 13	19 03 12	8 45	18 59 56	10 32	19 04 46	1 18	19 11 21	8 27	19 02 32	9 10	19 02 55	
November.....	0 15	19 07 48	9 04	19 07 15	10 46	19 08 43	1 37	19 14 30	9 20	19 07 00	9 05	19 06 51	24
December	0 19	19 06 07	9 06	19 06 21	10 48	19 08 48	1 45	19 12 39	9 15	19 05 50	9 12	19 06 03	28
Means with preceding Jan.	0 16	19 03 05	9 06	19 02 53	10 42	19 04 57	1 39	19 09 39	9 16	19 02 54	9 05	19 02 58	
Means for the whole year..	0 17	19 00 53	8 49	18 58 03	10 38	19 02 01	1 33	19 08 17	8 46	19 00 41	9 08	19 01 05	314

From these it appears, that the maximum westerly variation occurred about $1^h\ 30^m$ P. M. from which time the needle deviated towards the east until nearly 9 P. M.; after which a small deviation to the west took place during the night, until an early hour the next morning; from which time the deviation was easterly until nearly 9 A. M. when the easterly deviation attained its maximum, this maximum being considerably to the east of the minimum westerly variation in the evening. This excess appears to be different for different seasons of the year. The mean of the winter months, November, December, January, gives it very nearly nothing, the whole amount of daily variation being 6′ 46″. For February, March, April, it is 3′ 23″, or not quite a third of the whole variation 10′ 36″. For May, June, July, it is 4′ 33″, or more than a third of the daily variation 12′ 09″. And for August, September, October, it is 2′ 26″, or not quite a fourth of 11′ 25″, the whole daily change. In this table, the mean daily variation for different months is rather less than that deduced by CANTON. This arises from the exclusion, in the mean, of those days on which the variation was irregular, and on which, in general, the maximum west was considerably greater, and the minimum less, than on those days on which it was regular.

The observations at Port Bowen have been so recently published, that it is unnecessary for me here to point out the nature of the results; and any remarks I may have to make on them, I shall reserve until I have given the experiments with which I propose to compare them.

In order to determine how nearly the changes in the direction at Fort Enterprise, in London, and at Port Bowen, might be represented by the rotation of two poles, as I have described, I placed the small compass I had before made use of vertically over the centre of the circle in the plane of which the upper poles ~~of the~~ magnets were to revolve, and adjusted the arms carrying the magnets, so that the upper poles were at the distance 10·25 inches from that centre. In the first instance, the distance of the centre of the needle above that of the circle described by the poles was 12·25 inches; and after observations had been made with the compass at this distance, it was lowered until the distance was 10·25 inches, and again until the distance was 8·25: the first of these distances corresponds to the poles being developed interior to the surface of the earth; the second on the surface; and the third exterior to it. Observations were at first made with the arms carrying the magnets so adjusted, that the angle between the poles or their difference of longitude was 130°; and the observations were repeated when this angle had been altered to 170°.

For the observations corresponding to those at Fort Enterprise, latitude 64° 28′ N. variation 36° 24′ E, the axis about which the poles revolved was inclined to the horizon at an angle of 64° 28′, and the azimuth of its lower extremity from magnetic south was made 36° 24′ E: so that, the north end of the needle pointed N 36° 24′ E from the plane of the circle representing the true meridian. Adjustments similar to these, and corresponding to the latitudes of the places and the variation, were made for the observations to be compared with those at London, variation 19° W, and variation 24°40′ W, and at Port Bowen, latitude 73° 14′ N, variation 124° W.

The observations which I made of the deviation of the needle, under these different circumstances, at every change of 15° in the angular position of the poles relatively to the meridian, corresponding to their positions at intervals of an hour throughout the day, are contained in the opposite Table. The time indicated in the first column is that elapsed since the magnet having its *north* pole *upwards*, which represents the *north* pole near the *equator* on its *northern* side, was on the *northern* true meridian: and in the following columns are indicated the deviations of the north end of the needle, from the magnetic meridian, at the respective hours, and under the circumstances indicated above the several columns. The magnet having its *north* pole *upwards* was considerably *stronger* than the other. When this magnet was on the *southern* meridian, that which had its *south* pole *upwards* was to the *east* of it: the angular distance between the poles is indicated above the columns containing the deviations of the needle. It is scarcely necessary for me to mention, that had I adjusted the compass and the magnets so that the deviations should not have exceeded the observed diurnal variation, I should not have been able to observe the effects with any precision: the adjustments therefore were made so that the deviations should greatly exceed the diurnal changes. The deviations in each set are only comparable among themselves.

In order to compare the deviations corresponding to different hours, reckoned from the passage of the north pole over the true north meridian, with the observed changes in the direction of the needle at different times in the day, it is necessary to assign some particular time in the day for that which is taken as the zero in this table. Considering that the diurnal changes have been most accurately observed by

Angle described by the north magnetic pole from true north, reduced into time.	Latitude 64° 28′ N, variation 36° 24′ E, the same as at Fort Enterprise in 1821.						Latitude 51° 30′ N, variation 19° W, the same as in London in 1759.						Latitude 51° 30′ N, variation 24° 40′ W, the same as near London in 1820.						Latitude 73° 14′ N, variation 124° W, the same as at Port Bowen in 1825.					
	Angle between the magnetic poles.						Angle between the magnetic poles.						Angle between the magnetic poles.						Angle between the magnetic poles.					
	130°			170°			130°			170°			130°			170°			130°			170°		
	Height of the needle's centre above the centre of the circle described by the poles.			Height of the needle's centre above the centre of the circle described by the poles.			Height of the needle's centre above the centre of the circle described by the poles.			Height of the needle's centre above the centre of the circle described by the poles.			Height of the needle's centre above the centre of the circle described by the poles.			Height of the needle's centre above the centre of the circle described by the poles.			Height of the needle's centre above the centre of the circle described by the poles.			Height of the needle's centre above the centre of the circle described by the poles.		
	Inches 12·25	Inches 10·25	Inches 8·25	Inches 12·25	Inches 10·25	Inches 8·25	Inches 12·25	Inches 10·25	Inches 8·25	Inches 12·25	Inches 10·25	Inches 8·25	Inches 12·25	Inches 10·25	Inches 8·25	Inches 12·25	Inches 10·25	Inches 10·25	Inches 12·25	Inches 10·25	Inches 8·25	Inches 12·25	Inches 10·25	Inches 8·25
	Deviation.	Deviation.	Deviation.	Deviation.	Deviation.	Deviation.	Deviation.	Deviation.	Deviation.	Deviation.	Deviation.	Deviation.	Deviation.	Deviation.	Deviation.	Deviation.	Deviation.	Deviation.	Deviation.	Deviation.	Deviation.	Deviation.	Deviation.	Deviation.
h	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′
0	0 15W	0 15W	0 35W	1 45W	1 55W	2 40W	2 30 E	3 40 E	4 20 E	2 35 E	3 50 E	4 50 E	2 45 E	3 40 E	4 35 E	2 50 E	4 25 E	5 15 E	1 40 E	2 20 E	3 00 E	2 55 E	3 50 E	5 05 E
1	0 15 E	0 30 E	0 20 E	0 45	0 40	1 00	2 20	3 15	3 50	3 15	4 40	5 40	2 25	3 25	4 05	3 30	5 00	6 10	1 05	1 20	1 35	2 15	2 40	3 40
2	0 35	1 05	1 00	0 15 E	0 35 E	0 25 E	2 10	2 45	3 35	3 20	4 35	5 40	2 10	2 55	3 40	3 30	4 40	6 00	0 20	0 20	0 20	1 20	1 25	1 50
3	1 00	1 20	1 40	0 50	1 20	1 30	2 10	2 45	3 30	3 15	4 15	5 35	2 10	2 55	3 40	3 15	4 20	5 40	0 15W	0 30W	1 00W	0 20	0 15	0 00
4	1 20	2 00	2 25	1 30	2 15	2 35	2 10	2 55	3 50	3 10	4 10	5 20	2 10	2 55	3 50	3 05	4 15	5 25	0 55	1 25	2 20	0 25W	1 00W	1 40W
5	2 00	2 40	3 20	2 15	3 05	3 40	2 25	3 30	4 30	3 10	4 15	5 40	2 25	3 25	4 30	3 05	4 15	5 35	1 40	2 20	3 30	1 30	2 20	3 26
6	2 35	3 45	4 40	2 45	4 00	5 00	3 05	4 20	5 35	3 30	4 45	6 25	3 05	4 15	5 25	3 25	4 40	6 15	2 25	3 25	4 50	2 25	3 30	5 00
7	3 25	4 55	6 20	3 40	5 15	6 40	3 40	5 20	6 50	4 10	5 40	7 35	3 40	5 10	6 35	3 55	5 35	7 20	3 20	4 30	6 25	3 20	4 45	6 35
8	4 30	6 20	8 15	4 45	6 40	8 35	4 30	6 20	8 20	4 45	6 40	8 50	4 15	5 55	7 40	4 25	6 20	8 20	4 05	5 35	7 40	4 15	5 50	8 00
9	5 30	7 45	10 15	5 45	8 05	10 35	4 45	6 45	9 10	5 05	7 20	9 50	4 20	6 05	8 00	4 30	6 20	8 35	4 40	6 30	8 35	4 55	6 45	9 10
10	6 10	8 40	11 30	6 25	9 15	12 00	3 50	5 35	7 10	4 10	6 00	7 55	2 50	3 50	5 15	3 15	4 15	5 40	4 55	6 45	8 55	5 20	7 10	9 25
11	5 55	8 25	11 20	6 15	9 00	12 00	0 00	0 30W	1 10W	0 15	0 00	0 40W	1 20W	2 50W	4 15W	1 00W	2 10W	3 50W	4 50	6 35	8 30	5 20	6 45	9 05
12	4 20	6 20	8 15	4 40	6 40	8 50	5 45W	9 30	13 35	5 25W	9 05W	13 15	7 00	11 30	15 30	6 45	10 50	15 25	4 05	5 30	7 00	4 30	5 50	7 40
13	1 40	2 15	2 50	2 10	2 50	3 40	9 35	14 20	18 45	9 15	13 50	18 45	10 05	15 00	19 25	9 50	14 45	19 35	2 50	3 40	4 50	3 20	4 15	5 35
14	1 05W	1 45W	2 45W	0 35W	1 05W	1 40W	9 35	13 35	17 30	9 20	13 05	17 20	9 45	13 40	17 35	9 25	13 25	17 30	1 20	1 35	2 20	1 50	2 10	3 00
15	3 10	4 30	6 40	2 25	3 40	5 15	7 55	11 00	14 10	7 30	10 30	13 40	7 55	10 30	13 45	7 25	10 20	13 25	0 05 E	0 30 E	0 20 E	0 25	0 15	0 35
16	4 15	6 05	8 20	3 25	4 50	6 50	6 15	8 20	10 50	5 40	7 40	10 15	5 50	7 50	10 35	5 30	7 25	10 00	1 30	2 25	2 35	0 50 E	1 30 E	1 40 E
17	4 40	6 40	9 10	3 45	5 25	7 30	4 40	6 15	8 15	4 10	5 30	7 40	4 20	5 45	7 40	3 55	5 15	7 20	2 40	3 45	4 30	1 55	2 50	3 30
18	4 40	6 40	9 10	3 45	5 30	7 35	3 30	4 35	6 10	3 15	4 20	5 50	3 00	4 00	5 25	2 55	3 55	5 30	3 30	4 50	6 00	2 40	3 45	4 50
19	4 35	6 05	8 20	3 45	5 30	7 35	2 10	2 45	3 40	2 40	3 25	4 45	1 40	2 15	2 55	2 20	3 00	4 20	3 45	5 25	6 55	3 15	4 35	5 50
20	3 50	5 15	6 55	3 45	5 25	7 25	0 25	0 30	0 50	2 15	2 50	3 50	0 05 E	0 00	0 00	1 55	2 20	3 20	3 45	5 30	7 05	3 35	5 15	6 35
21	2 55	3 50	5 15	3 40	5 00	6 50	1 20 E	2 05 E	2 15 E	1 40	1 50	2 30	1 45	2 40 E	2 55 E	1 15	1 15	1 45	3 40	5 20	6 45	3 40	5 20	6 50
22	1 50	2 25	3 20	3 25	4 25	5 55	2 30	3 35	4 15	0 35	0 00	0 20	2 50	4 00	3 40	0 05	0 35 E	0 25 E	3 10	4 30	5 45	3 40	5 20	6 40
23	0 55	1 05	1 45	2 40	3 20	4 25	2 40	3 55	4 40	1 05 E	2 15 E	2 30 E	3 05	4 20	5 05	1 30 E	2 40	3 20	2 25	3 25	4 25	3 30	4 45	6 15
24	0 15	0 15	0 40	1 45	1 50	2 40	2 35	3 35	4 20	2 35	3 50	4 45	2 50	3 40	4 35	2 55	4 25	5 20	1 40	2 20	3 00	2 55	3 50	5 05

CANTON and Colonel BEAUFOY, in London and its vicinity, I shall determine this from their observations. It appears from these, that the maximum west variation happens about 1^{h} 30^{m} P. M. Now, in the table, when the variation is 19° W., or 24° 40′ W., this maximum corresponds to about 13^{h} 30^{m}, or 13^{h} 20^{m} after the north pole has passed the northern meridian. So that the hours in the first column of the table must, in all cases, nearly indicate the times since midnight. This being the case, it will be seen, that when the angle between the poles is 130°, the times of the maxima, and the characters of the deviations, in the respective cases, approximate to the observations at Fort Enterprise, in London, and at Port Bowen, and perhaps as nearly as we ought to expect, when we consider that, admitting the correctness of the hypothesis, many circumstances in the experiment must necessarily be different from those accompanying the phænomena in nature. But to form a more just estimate of the nature of the agreement, I shall point out the principal discordances and coincidences between these results and the observed diurnal changes. I may premise, that, whether the poles revolve in the interior of the sphere, on the surface of which the needle is supposed to be situate, on its surface, or exterior to it, the character of the changes which take place in the direction of the needle is nearly the same. I will, however, for the purpose of comparison, take the case where the poles and the needle are at the same distance from the centre.

Observations at Fort Enterprise. Lieutenant HOOD states that the maximum east variation occurred at 9^{h} A. M., but the first observations in the morning which are given for January and February, were at 10^{h} A. M., and none are recorded after this hour until 1h P. M.: for March, the first observation is at 9h A. M. Even with instruments best adapted for the purpose, the time of maximum cannot be determined with great precision; and although Lieutenant HOOD possessed the requisites of zeal and ability, yet the instruments that were made use of were not such as would have been employed, had not the nature of the expedition in which Captain FRANKLIN was engaged, restricted him to carrying only those most essential. Much latitude, with respect to the time of maximum, must therefore, under these circumstances, be allowed; and I think the time by experiment, 10h A. M., is probably not far from the truth. The time of minimum east or maximum west appears by the table to be between 5h P. M. and 6h P. M. Lieutenant HOOD states it at 3h or 4h P. M.; but the mean of his observations certainly indicates that the time was later than 5h P. M.

Observations in London, variation 19° *W.* The mean of CANTON'S observations for the months nearest the equinoxes, February, March, April, August, September, October, gives 8h 40m P. M. as the time of minimum west in the evening: the experimental result is about 11h P. M., making the difference in the times considerable. In both cases, however, the course of the deviation is the same; the needle deviating slightly towards the west during the night and early part of the morning; then proceeding towards the east, and attaining the maximum east, or minimum west, at almost precisely the same time in the two cases: namely, 9h A. M. in the one, and 8h 53m A. M. in the other. CANTON'S observations give 11′ as the mean difference for these equinoctial months, between the maximum west and morning minimum west, and

8′ as the difference between the maximum and afternoon minimum: the corresponding differences in the table are, 21° 10′ and 18° 20′, which have the same character, but are not in the same ratio; so that the ratio of the intensities of the poles in these experiments, is not precisely that which the observed diurnal changes require.

Observations near London, variation 24° 36′ W. I have already mentioned the reason that I have not been able to refer more particularly to the extensive observations of Colonel BEAUFOY; however, the maximum east and the maximum west in those to which I have referred, appear to have happened earlier than those, at the same time of the year, when the variation was 19° W; and in this respect they agree with the results in these experiments. The time of the evening minimum west, 11ʰ P. M., also coincides with the time at which Colonel BEAUFOY states it to have occurred. The observations which I made in 1823,* on the deviation of the needle when under the influence of magnets, are of too limited a nature to draw any very decided conclusions from them; but the course of the daily variation, as exhibited in the diagrams, agrees remarkably with the results in the table.

There is one circumstance in which all the observations on the daily variation in this latitude most strikingly correspond with the results in the table: little change takes place in the direction of the needle during the latter part of the evening until about five o'clock in the morning; and, from the results in the table, it appears that this would arise from there being a small maximum easterly deviation about 11ʰ P. M., and an

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* Philosophical Transactions, 1823.

intermediate minimum east about 4^h A. M., previous to the absolute maximum east about 9^h A. M. Another circumstance, common to all the observations in northern latitudes, namely, that the extent of the daily variation increases as the sun passes to the north of the equator, and decreases on its passage south, so clearly follows from the hypothesis that it is scarcely necessary to mention it.

Observations at Port Bowen. Mr. FOSTER states that the mean time of maximum west deviation, deduced from four month's observations, is 11^h 49^m A. M.; and that of the maximum east 10^h 01^m P. M. The corresponding times in the table are a little after 10 in the morning and 8 in the evening. So that the experiments differ considerably from the observations in the absolute times, but agree very closely in the interval of time between the two maxima. It is extremely probable that the forces brought into action by the heat of the sun are not directed, at the same instant of time, towards points absolutely fixed, whatever may be the situation of the needle on the surface of the earth; and it is easily seen that in such an extreme case as this, where the dip of the needle is 88°, this may have a considerable influence on the changes that take place in its direction: therefore, although the deviations in their general character may agree with the effects produced by the revolution of two poles, yet we ought not, perhaps, to expect that they should do so in every respect. Whether the times of the maxima arising from the rotation of these poles would precede those that would actually result from the hypothesis I have advanced, would be best determined by experiments on substances having different conducting powers, and whose contact, like that of the earth and the atmosphere, should be symmetrical. Another circumstance in which the experiments differ from the hypothesis, is this: that in these the needle is not only of finite dimensions compared with the distance of the revolving poles from the centre, but its length is even considerable in this respect, being a fifth of that distance; and this will modify all the results.

I have before mentioned that the intensity of the magnet whose north pole was upwards in these experiments, and which, for the sake of distinction, I will now call N, was considerably greater than that of the magnet whose south pole was upwards, and which I call S. Their intensities were such, that when they were severally placed to the west of the needle, so that their axes were in a horizontal line drawn through its centre at right angles to the magnetic meridian, and their nearest ends 4·2 inches from that centre, the deviations of the north end of the needle were: for the magnet N, 60° 40′ W., and 59° 40′ E., according as its north, or its south pole was that nearest to the needle; and for the magnet S, 26° 00′ E, when the south pole was nearest to the centre, and 31° 00′ W., when its north pole was nearest. I afterwards increased the intensity of the weaker magnet by passing the poles of the other alternately over it from centre to ends. After this the deviations observed as before were: for the magnet N, 60° 20′ W., 58° 20′ E.; for the magnet S, 39° 40′ E., 41° 30′ W. In this state the magnets were again adjusted to the instrument as before, excepting that the distance of their poles from the centre was 10 inches instead of 10·25 inches, as, at the latter distance, the stem of the support to the compass was liable, in some cases, to interfere with their revolution. The greatest east and west deviations, the times at which they occurred, and the times at which the needle passed zero, were alone in this instance determined in the several cases. The results are contained in the following table.

	Height of the needle's centre.	Angle between the poles.	Minimum East.		Maximum East.		Zero.	Maximum West.		Zero.	2d Maximum East.	
			Time.	Deviation.	Time.	Deviation.	Time.	Time.	Deviation.	Time.	Time.	Deviation.
	Inches.	°				° ′	h. m.	h. m.	° ′	h. m.		
Lat. 64° 28′ N; var. 36° 24′ E, as at Ft Enterprise, 1821.	12	130	The mininum does not occur here.		10 30 A.M.	6 00 E	1 35 P.M.	5 40 P.M	5 55 W	11 40 P.M	The minimum does not occur here.	
		170			10 20	6 15	1 55	8 15	5 00	1 34 A.M.		
	10	130			10 30	9 20	1 40	5 40	8 20	11 38 P.M.		
		170			10 30	9 25	1 56	8 20	6 40	1 34 A.M.		
	8	130			10 30	12 00	1 35	5 40	11 20	11 45 P.M.		
		170			10 30	12 45	1 55	8 00	9 15	1 34 A.M.		
			h. m.	° ′							h. m.	° ′
Lat. 51° 30′ N; var. 29° W, as in London, 1759.	12	130	3 20 A.M.	2 20 E	9 00 A.M.	4 30	11 05 A.M	1 40 P.M.	10 20	8 05 P.M.	10 30 P.M.	4 10 E
		170	5 00	3 15	9 00	4 30	11 10	1 30	9 50	10 24	1 30 A.M.	4 15
	10	130	3 30	3 05	9 00	6 20	11 05	1 30	15 30	8 08	10 40 P.M.	5 50
		170	5 00	5 00	9 00	7 30	11 08	1 20	14 40	10 20	1 20 A.M.	6 50
	8	130	3 30	4 15	9 10	9 15	11 03	1 20	20 25	8 05	10 50 P.M.	7 40
		170	5 00	6 45	9 10	10 25	11 08	1 20	19 35	10 25	1 30 A.M.	8 40
Lat. 51° 30′ N; var. 24° 40′ W, as near London, 1820.	12	130	3 30	2 15	8 40	3 55	10 50	1 30	19 35	7 52	10 30 P.M.	4 20
		170	5 20	3 15	8 40	4 00	10 54	1 30	10 20	10 18	1 20 A.M.	4 40
	10	130	3 45	3 15	8 55	6 15	10 50	1 20	16 10	7 50	10 30 P.M.	6 10
		170	5 00	4 45	9 00	6 10	10 54	1 20	15 20	10 05	1 20 A.M.	6 40
	8	130	3 10	4 15	9 00	7 50	10 46	1 10	21 30	7 53	10 40 P.M.	7 50
		170	5 00	6 40	9 00	8 45	10 52	1 10	20 00	10 07	1 30 A.M.	8 35
Lat. 73° 14′ N; var. 124° W. as at Port Bowen, 1825.	12	130	The minimum does not occur here.		8 00 P.M.	4 30	1 53 A.M.	10 10 A.M.	5 05	2 46 P.M.	The maximum does not occur here.	
		170			10 00	4 30	3 12	10 20	5 40	3 20		
	10	130			8 00	6 50	1 54	10 10	7 10	2 44		
		170			10 20	6 25	3 00	10 10	7 45	3 20		
	8	130			8 10	8 55	1 54	10 00	9 30	2 50		
		170			10 00	8 40	3 15	10 20	10 20	3 25		

Here the times do not, in any instance, sensibly agree more nearly with the observed times at the respective places, and in some they differ sensibly more widely, than when the intensity of the easterly pole was less. Likewise the variation from the morning maximum east to the maximum west, compared with the variation from the maximum west to the evening maximum east, does not so nearly agree with the ratio of the observed variations in London as in the former case. So that, an increase of intensity in the eastern magnet causes a greater discordance between the experimental results and the observations. By a small diminution in the intensity of this pole they might agree more nearly; but if it were removed altogether, that is, if we were to suppose only one pole to revolve in the equator, the evening maximum east would not obtain, with the latitude and variation corresponding to those of London, and the discordance in the results would be still greater.

I have not as yet noticed what may appear an incongruity in these experimental results, as compared with the observations in London and at Port Bowen, viz., that here the deviations corresponding to those in London are considerably greater than those corresponding to the observations at Port Bowen, whereas the diurnal changes at the latter place greatly exceed those at the former. This incongruity is however only apparent, and its cause is easily pointed out. The changes in the direction of the horizontal needle are dependant on those which would take place, under the same circumstances, in the direction of a needle freely suspended by its centre of gravity, in such a manner that, if we conceive their centres to coincide, and the disturbing force to act upon the dipping needle alone, the horizontal needle would always be found in the vertical plane passing through this imaginary dipping needle.* According to this, if equal disturbing forces act at places where the dips are different, the sines of the

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* I first stated this law of the dependance of the deviations of the horizontal needle on those of the dipping needle, in the form of an hypothesis, in the Cambridge Philosophical Transactions for 1820; but without adverting to that hypothesis, it may be considered as a most convenient method of embracing in one view, various phænomena observed with the horizontal needle. From whatever cause deviation in the direction of the horizontal needle may have arisen, except, possibly, in cases where the length of the needle bore a very sensible ratio to the distance of the disturbing body, I have met with no instance that was not quite consistent with this law; nor was I aware that any such was said to exist, until I heard the circumstance stated in a paper of Mr. BARLOW'S, recently read before the Royal Society. Not being in possession of Mr. BARLOW'S experimental results, however far I may be from being convinced by the arguments he has adduced against the law itself, independent of any hypothetical views, I will not venture to point out the nature of their fallacy: but as far as I could collect the facts during the reading of the paper, I am of opinion that they would result from my view of the subject. Thus supposing the centre of the shell, in his experiments, to be placed in the equator of the imaginary dipping needle passing through the centre of the horizontal needle, so that, if the branches of this needle were of equal intensity, no deviation would, according to my view, take place; then, if either branch be deteriorated, the centre or pivot of the needle will no longer be the centre of its magnetism, and a point in the horizontal needle, remote from its pivot, must now be considered as the centre of the dipping needle; and the centre of the shell being now above, or below the equator of this dipping needle, according to the circumstances of deterioration, unequal action will take place on the branches of the dipping needle, and consequently, according to the law in question, deviation of the horizontal needle will ensue. Although this law has been called in question, I will here mention an instance of its advantage in connecting different phænomena. By supposing the effect of a disturbing force to be produced on the dipping needle, and that in consequence the poles describe, in a certain time, a circle round their undisturbed places, connecting the observations on the dip, in this manner, with those on the horizontal needle, I found, as I have stated in my paper in the Cambridge Philosophical Transactions, 1820, that the resulting variations will agree, within less than half a degree, with the observed variations of the horizontal needle in London, during a period of 200 years. These results I never published; but I found very shortly afterwards, that, on this view, the same approximation might be made to the variations of the horizontal needle observed at Paris, during the same period.

greatest horizontal deviations will be to each other inversely as the cosines of the dips. In the experiments, although the variation and latitude were those at the different places of observation, the dip remained the same in all cases: so that in the deviations corresponding to those at Port Bowen, no increase, arising from an increase in dip, could here take place, but on the contrary, a decrease took place from the increased distance of the revolving poles.

This manner of viewing the effects on the horizontal needle, as depending on those produced on the dipping needle, will serve to explain the nature of the changes which take place in the intensity of the horizontal needle. The horizontal intensity varying as the cosine of the dip, when the dip is a *maximum* the horizontal intensity will be a *minimum*, and *vice versa*. Let us now consider what effects will be produced on a dipping needle with its centre in the same situation as that of the horizontal needle in the preceding experiments; the angle between the magnets being 130° and the instrument adjusted to correspond to the latitude and variation of London. It is manifest that the *maximum* dip will take place nearly when the *north* pole is on the *south* magnetic meridian; that is, nearly at the time when the needle passes zero from the easterly deviation in the morning towards the westerly, or a little before 11^h A. M.; and the *minimum* dip will happen nearly when the *south* pole is on the *south* mag-

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On the same view of the subject, Lieutenant Foster has recently been enabled to connect the changes observed in the horizontal intensity with changes in the dip. I must however say, that, should the deviations which are observed when one branch of the needle is deteriorated, not be consistent with this law, no one will be more ready than myself, to expunge from the philosophical code, a law which is found to be inconsistent with observed phænomena.

netic meridian, or nearly when the needle passes zero from the westerly deviation in the evening towards the east, that is before 8^h^ P. M.: the dip of the needle will be nearly undisturbed about 3 P. M. It is evident also, that, the intensity of the north pole being greater than that of the south, the maximum dip will exceed the undisturbed dip by a greater quantity than this last does the minimum. It appears then, according to this theory, that the horizontal intensity ought to decrease here until a little before 11 o'clock in the morning, when it should be a minimum; that it should increase from this time till nearly 8 o'clock in the evening, when it reaches its maximum; and that taking the intensity at 3 o'clock in the afternoon as the undisturbed intensity, this should exceed the minimum by a greater quantity than it is itself exceeded by the maximum.

I am not aware of any extensive series of observations having been made near London, on the diurnal changes in the intensity of the horizontal needle. Those which I made in May and June, 1823,* having been undertaken for the particular purposes of pointing out the cause of what had been considered an anomalous change in the direction of the needle, and of exhibiting and illustrating the particular mode of observation, were consequently of a limited nature; but as they are the only ones I can refer to, I shall compare the preceding conclusions with the results obtained from them. From a mean of these observations it appears, that the horizontal intensity was the least about $10^h\ 30^m$ A. M., and that the maximum happened nearer to $7^h\ 30^m$ P. M. than to 9^h P. M.: also that, taking the minimum as unity, the excess

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* Philosophical Transactions, 1825.

of that at 3^h P. M. above it was ·00177, and the excess of the maximum above this was ·00049. So that the theory agrees in all respects as nearly with these observations as can possibly be expected. I may also notice that the mean of seven months' observations by Professor HANSTEEN, at Christiania, where the variation was about 20° west, gives the minimum intensity at $10^h\ 30^m$ A. M., and the maximum of the recorded observations 7^h P. M.: the maximum appearing however to happen somewhat later. It may be proper to add, that the method I adopted for determining the diurnal changes in the intensity of the horizontal needle was totally dissimilar to that employed by Professor HANSTEEN.

Although I have hitherto but slightly noticed the observations made at Port Bowen, I nevertheless fully appreciate those observations, and the zeal and ability of the observers. As might have been anticipated from the law to which I have referred, as connecting the deviations of the horizontal needle with those of the dipping needle, the extent of the daily variation observed at Port Bowen was such, that had the changes taken place with any degree of regularity, the times at which the maximum east and maximum west deviation occurred, would have been determined with great precision. This however was by no means the case, and such irregularities take place, from day to day, in the extent of the variations, in the times of their maxima, and in the times of maximum and minimum intensity, that it appears to me, these irregularities must be the effects of local causes, equally irregular in their operation, unless we admit that from the peculiarity of the situation, the dip being above 88°, irregularities, which in other cases completely elude observation, are here, not only sensible, but very commonly have a preponderating influence. Such irregularities have been occasionally noticed by Canton and Colonel Beaufoy, but they are comparatively of extremely rare occurrence; and, certainly, irregularities that were not detected by the accuracy of Colonel Beaufoy's observations, with a dip of 70°, would not, simply from the increase of the dip to 88°, obliterate every trace of regularity, though they might undoubtedly become sensible. I have made several attempts to separate, in these observations, those which appeared to follow some general law, from those which appeared to be altogether irregular, but hitherto without success, possibly from my having been restrained from making any selection that might appear to favour the hypothesis I have advanced. Perhaps, if the deviations and intensities observed at Port Bowen at the several hours throughout the day, were exhibited as the ordinates of a curve, the times being the abscissæ, in the manner in which I have represented some observations in 1823, the variations and intensities having the true characters of maxima and minima might be separated from those accidentally the greatest or the least. I have not yet had sufficient leisure to accomplish this, but I think that when I have, I may be repaid for the time devoted to it, by the light that may be thus thrown upon these very interesting observations.

Admitting that the daily changes in the direction of the needle may be represented by the revolution of poles near the equator, it appears that the times at London, reckoning from the passage over the true meridian, of the *north* pole, that of greatest intensity, nearly agree with the times reckoned from the sun's passage: so that the *north* pole, to the *north* of the equator, would always have the sun nearly on its meridian; the *south* pole to the *north* of the equator would probably be nearly in the longitude 130° east from this meridian: and corresponding to these, a *south* and a *north* pole would be developed to the *south* of the equator. Supposing then that the place of greatest heat on the equator is in general 45° to the east of the sun, the poles of *greasest* intensity would be developed in the longitude of about 45° *west*, and those of *least* intensity in the longitude of about 85° *east* from the place of heat. The situation however of the place of heat, and the distances of the poles from it, must be greatly influenced by the nature of that part of the earth to which the sun is vertical. For there can be no doubt, that if, during the passage of the sun over the scorched desarts of Africa, the place of greatest heat is in general 45° behind the sun, the border of that continent on the west will continue the place of greatest heat, for some time after the sun has advanced 45° over the Atlantic; and that on the other hand, the eastern shores of the continent of America will become more heated than the Atlantic, even when the sun has scarcely passed the aquatic meridians. Besides this, owing to the different conducting powers of land and water, the intensities of the poles, and likewise their distances from the place of heat, may vary according as that place is on the land or on the water. Since then, when the sun is in the *northern* tropic, it passes over the greatest extent of land without intervening sea, and afterwards of sea scarcely intersected by land; and that the case is the reverse when it is in the *southern* tropic; this would produce a considerable effect on the diurnal variation at different times of the year. It is by no means improbable, that it is owing to circumstances of this kind that we have a maximum daily variation in April, and another in August. The situation of the poles will also be influenced by the elevation of the land to which the sun is vertical; and when it passes over the plains of Hindoostan, with the Himaleh mountains covered with perpetual snow, and the high and cold plains of Tartary to the north; and again, when it crosses the Andes; the situations of these poles, with respect to the place to which the sun is vertical, will, I consider, be different from their positions with regard to it, when the sun is vertical to the centre of Africa, or of the Pacific ocean. The great disturbances which take place in the atmosphere in the intertropical regions, and which when of considerable extent must influence the situation of the place of heat, will also become a source of anomalous action on the needle. All these circumstances, admitting the correctness of the hypothesis, must greatly modify the regular effects that would otherwise be produced, and must influence considerably the nature and extent of the daily variation when observed at different parts of the earth; and probably they are the causes of some apparent discrepancies in the preceding results.

On commencing the experiments with the compound plate of bismuth and copper, I had no expectation of reducing the deviations of the needle to so simple a law as that resulting from a polarising of the plate in a particular direction; but I considered that it would be essential to determine, whether the deviations near the outer edge of the copper were of the same character as those near the line of junction of the copper and bismuth, since these situations of the needle would correspond to those of a needle placed successively in the higher regions of the atmosphere, and on the surface of the earth. As the character of the deviations is the same in the two cases, it would follow, that the course of the daily changes in the direction of the needle would be the same at the level of the sea, and at the summit of the highest mountain, although the extent of those changes might be slightly different. At the time of making these experiments, and when I had drawn this conclusion, I was not aware that Lieut. FOSTER had made any observations at Port Bowen, with the view of comparing the changes which take place in such cases; but, previously to his sailing for Spitzbergen, he informed me, that at Port Bowen he had found them to be simultaneous.

I am perfectly aware that it would have been more satisfactory, had I drawn my conclusions from experiments on substances in contact more precisely in the manner of the earth and the atmosphere; that is, for example, from experiments on a copper shell filled with bismuth. This I proposed doing, and for the purpose had a six-inch copper shell cast, into which, after having it cleaned and soldered on its inner surface, I cast bismuth to fill it. In consequence however of inequalities in the thickness of the copper, and possibly from imperfect contact in some parts, the results, when different points had been heated, were not, in all cases, of such an uniform character as to enable me to determine the general magnetical phenomena that would ensue from applying heat to any part of the sphere. This being the case, I shall detail none of these experiments. As however one effect, which was invariably produced to whatever point of the equator heat was applied, bears immediately on the subject of the daily variation, and besides, coincides perfectly with the results obtained with the plate of copper and bismuth, I shall state it.

The sphere was adjusted on an axis, of which that part in contact with the copper was of wood, and this axis being adapted to the instrument to which I have already referred, could be inclined to the horizon at any angle, and could likewise be placed in any azimuth. When the axis was in the plane of the magnetic meridian, if the centre of the needle was placed vertically over the centre of the sphere, its position would correspond to that of a needle on the surface of the earth at a place having the latitude equal to the elevation of the pole of the sphere, and where the mean variation vanished. The angle of elevation of the pole of the sphere was 50°, as most convenient for making observations at a distance from the equator. A lamp being applied so that its flame was as nearly as possible on a point in the equator, the sphere was turned on its axis until the equatorial regions became nearly uniformly heated; and the lamp was allowed to remain for 5 minutes after the sphere was brought to rest; so that the point during this time over it became the place of greatest heat, and the poles of rotation the points of greatest cold. Now, whatever point in the equator was made the place of heat, I invariably found, that when the elevated pole was towards the *north*, the deviation of the *north* end of the needle was towards the *west*, when the place of heat was on the meridian *above* the horizon, that is to the south; and towards the *east*, when the place of heat was on the meridian *below* the horizon. This is precisely the character of the daily variation at places in *north* latitude, to which the above positions of the compass and sphere correspond. If the instrument was turned 180° in azimuth, so that the elevated pole was now to the south of the needle, the effects were reversed: that is, when the place of heat was on the meridian *above* the horizon, the deviation of the north end of the needle was towards the *east;* and it was towards the *west*, when the place of heat was on the meridian *below* the horizon. So that, *the deviation of the end of the needle of the same name as the latitude* was always towards the *west*, when the place of heat was on the meridian *above* the horizon, and towards the *east*, when it was on the meridian *below* the horizon. I am not aware of any observations having been made on the variation of the needle in a high *southern* latitude, but consider that the agreement of the theoretical results with such observations would be almost decisive of the correctness of the theory.

Besides the method which I have described, I adopted others for heating the equatorial part of the sphere; one of these, by which the results were greatly increased, was this: having covered a copper wire with very loosely spun string, I wetted it throughout with spirits of wine, and placed it round the sphere, somewhat below the equator, so that, when the spirit was inflamed, the equatorial part of the sphere was the hottest. The string being in largest quantity where the two ends of the wire were joined, and this joint being downwards during the combustion, the corresponding part of the sphere became the place of greatest heat. To whatever part of the equator this joint was adapted, the effects perfectly corresponded with, though they greatly exceeded those which I have before stated.

I have mentioned that, owing to the want of uniformity in the thickness of the copper, and possibly likewise in the contact of the two metals, some other results which I obtained with this sphere were not of the same uniform character. I have however not yet been able to make such alterations in the apparatus as shall remedy this defect. If, with an apparatus similar to that which I employed, but of much larger dimensions, and in which the copper sphere should be of uniform thickness, and the contact of the two metals perfect throughout, means were adopted by which the distribution of heat should nearly resemble that on the earth, and that observations were made with such adjustments of the instrument, that the positions of the needle should correspond to those of a horizontal and of a dipping needle on different parts of the earth's surface, I consider that much light might probably be thrown on the various phenomena of terrestrial magnetism.

S. H. CHRISTIE.

Royal Military Academy,
26th May, 1827.

XXIII. *On the ultimate composition of simple alimentary substances; with some preliminary remarks on the analysis of organized bodies in general.* *By* William Prout, *M.D. F.R.S.*

Read June 14, 1827.

The present being the first of several communications on the same subject which I hope to have the honour of laying before the Royal Society, a few observations on the origin and object of the whole series may not be deemed irrelevant.

Many years ago I published an anonymous Paper, containing some views, at that time new, connected with the doctrines of chemical proportions.* Though this Paper, for reasons which need not be here stated, was drawn up and published in a very hasty and imperfect manner, it attracted some notice; and the views therein advanced gradually gained ground, and at present appear to be generally admitted in this country.† When this Paper was published, it was my intention to have pursued the subject further, but I soon found my progress obstructed by insuperable difficulties. The first and chief of these was the want of accurate data; and the infinity of objects comprehended by chemistry prevented the hope of acquiring, by individual exertion,

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* Annals of Philosophy, vi. 321, and vii. 111. (O. S.) The object of the second Paper was simply to correct some oversights in the first.

† Dr. Thomson's Chemistry, and his Attempt to establish the first principles of chemistry by experiment. Also, Dr. Henry and Mr. Brande's Elements of Chemistry, &c.

however unremitting, a sufficiency for the establishment of general laws. Professional duties still further limited my exertions, and at length obliged me to relinquish chemistry in general, and confine my attention solely to the chemistry of organized substances; a subject that has occupied the greater portion of my leisure hours for the last ten or twelve years.

Organic chemistry is confessedly one of the most difficult departments of the science; and though much has been done, and more attempted on the subject, it is yet in a very imperfect and unsatisfactory state; and it must be frankly admitted that Physiology and Pathology have derived less advantage from this most promising and really powerful of the auxiliary sciences, than might have been expected. To explain this perhaps would not be difficult; but as the explanation would be misplaced here, I shall merely observe, that dissatisfied with the old modes of inquiry, I determined to attempt a different one, and keeping in view the notions I had originally formed respecting chemical combinations, proposed to myself to investigate the modes in which the three or four elementary substances entering into the composition of organized bodies are associated, so as to constitute the infinite variety occurring in nature.

With these views my first object was to determine the exact composition of the most simple and best defined organic compounds, such as sugar, and the vegetable acids, a point that had been several times before attempted, but, as it appeared to me, without complete success. About the same time also albumen and other animal products, as urea, lithic acid, &c. were examined with similar views. The subject of digestion, however, had for a long time occupied my particular attention; and by degrees I had come to the conclusion, that the principal alimentary matters employed by man, and the more perfect animals, might be reduced to three great classes, namely, the *saccharine*, the *oily*, and the *albuminous*: hence, it was determined to investigate these in the first place, and their exact composition being ascertained, to inquire afterwards into the changes induced in them by the action of the stomach and other organs during the subsequent processes of assimilation. In conformity with this plan, the object of the present communication is the consideration of the first class or family above-mentioned, namely, the *saccharine*.

Preliminary observations on the analysis of organised substances.

Vegetable substances contain at least two elements, hydrogen and carbon; and most generally three, hydrogen, carbon, and oxygen. Animal substances are still more complicated; and besides the above three, usually involve a fourth element, namely, azote, to which they appear to owe many of their peculiar properties. These general facts have been known ever since the elements themselves have been recognised as distinct principles, though the determination of the exact proportions in which they enter into any particular substance, has always proved a most difficult problem. To enumerate all that has been done on this subject would be loss of time; and it need only be mentioned, that all idea of separating the different elements from one another, so as to obtain them *per se*, has been long since abandoned, if indeed it was ever entertained, and the general principle on which the analysis of organic products has been usually conducted, has been to obtain their elements in the form of binary compounds, either by destructive distillation, as was formerly practised; or by combining the elements with some other element with which they possessed the property of forming definite binary compounds, from the quantity and known composition of which, those of the original elements might be readily obtained by calculation. For this latter purpose oxygen has been the principle usually employed, which, as is well known, forms water with hydrogen, and carbonic acid gas with carbon; two compounds not only as well understood as any in chemistry, but likewise, from their physical properties, well adapted for the purpose. When azote is involved other means must be adopted, which will be fully considered hereafter.

The modes in which chemists have attempted to combine oxygen with the hydrogen and carbon of vegetable substances have differed very considerably. The illustrious LAVOISIER attempted their union by burning the substance at once in oxygen gas, a method subsequently followed by SAUSSURE and others. Afterwards the metallic oxides were employed for the purpose; and BERZELIUS in particular informs us, that so early as 1807 he had tried the oxide of lead, but did not succeed with it.* In 1811, GAY LUSSAC and THENARD published the analysis of different organic substances made by means of the chlorate of potash; and, considering the nature of the apparatus they employed, they obtained admirable approximate results.† BERZELIUS, in 1814, published an

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* Annals of Philosophy, iv. 403.

† Recherches Physico-chimiques, ii. 265.

elaborate paper on the subject of vegetable analysis, in which he likewise employed the chlorate of potash, but in quite a different manner; and to this celebrated chemist I believe we are indebted for the improvement subsequently adopted by most of his successors, of introducing the mixture of the substance to be analysed, and the oxide, into a narrow tube, and submitting the different portions of it to heat in succession. The results of BERZELIUS were in general more accurate than those of his predecessors, especially as far as related to the quantity of carbon, but his method was not well adapted for determining the proportion of hydrogen.* In 1816, GAY LUSSAC seems to have thought of employing the oxide of copper for the purposes of analysis,† the introduction of which undoubtedly constituted one of the greatest improvements hitherto made in organic analysis; and the use of which has continued to the present time, and will perhaps never be entirely superseded. The oxide of copper has however some disadvantages, which it is one object of the present remarks to point out; another is, to propose a form of apparatus free from most of the objections to which those hitherto in use have been more or less liable.

There are two methods of arriving at the quantity of water formed during the combustion of an organized substance; either actually to collect and weigh it, as BERZELIUS did, or to estimate the quantity by the loss of weight sustained by the tube after the combustion. The latter in general is the best method, and was that adopted by me from the first: it has since been followed by Dr. URE, and others.‡ Whichever

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* Annals of Philosophy, iv. 323.

† Annales de Chimie, xcvi. 306.

‡ Phil. Trans. 1822. p. 457.

method is adopted, it obviously becomes necessary that no extraneous water be present; but all pulverulent substances, and oxide of copper among the rest, are more or less hygrometric, and rapidly attract moisture from the atmosphere. This circumstance seems to have struck the French chemists, and it occurred to me at a very early period. Dr. URE, however, was the first who published a method of obviating this difficulty; and his method, if this were the only difficulty to contend with, is capable of considerable precision. But there is another, and perhaps still more troublesome property, possessed by the oxide of copper, in common with many other powders, namely, that of condensing air as well as water;* and these two difficulties, taken together in conjunction with another mentioned in a note below, render great precision almost out of the question.† To conquer these,

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* See SAUSSURE's paper on the absorption of the gases by different bodies. Annals of Philosophy, vi. 241. Also GILBERT's Annalen der Physick, xlvii. 112.

† As I am unwilling that so much labour should be lost, particularly as it may be of some use to other inquirers, I have thrown into the form of a note a few of the principal circumstances connected with the inquiry mentioned in the text. In my earlier experiments tubes of iron, copper, &c. were employed instead of glass, and charcoal instead of spirits, as the medium of heat; and during this period most of the modifications of apparatus which have been since proposed as novelties or improvements, were tried and rejected. I first took the hint of employing a spirit lamp from Mr. PORRETT, and was certainly among the first that did so employ it. Various forms of lamps were tried, but at length I was induced to relinquish the use of the horizontal apparatus for the vertical one;‡ and this, I have no hesitation in saying, is by far the best form of apparatus hitherto proposed for the substances to which it is adapted; nor would any other have been required by me, at least, had it not been for the properties of the oxide of copper alluded to in the text, which render this and all other forms of apparatus depending on the

‡ Described in the Annals of Philosophy, xv. 190 (O. S.) and more completely in Dr. HENRY's Chemistry, ii. 167, ninth edition.

every means that could be thought of, as likely to succeed, were tried, but without effect, and I was obliged to relinquish

employment alone of that substance perfectly useless when great accuracy is required. It has been objected to the lamp that the heat produced by it is not sufficient; but this is a mistake; at least I have never met with any substance that resisted its action, provided the oxide of copper was well shaken up in the tube, or, if necessary, taken out of the tube and retriturated, and afterwards exposed to heat a second time, one or other of which *ought to be done in all instances*, whatever be the medium of heat employed; for no ordinary heat will induce the oxide to part with its oxygen to a combustible substance at some distance off, and not immediately in contact with itself. A great heat is also attended with some disadvantages, and among others, that of causing the oxide to adhere together in hard and solid masses, which thus becomes more difficult to be removed from the tube, and much less adapted for future experiments. In general organized bodies are more difficult of combustion, and require more heat than crystallized ones. The lamp described in the text I have only recently employed, and it answers the purpose in all respects better than any I have yet seen.

With respect to the sources of error above mentioned, it was found that 200 grs. of the oxide of copper, recently ignited, gained, after ten or fifteen minutes exposure to the air, a quantity varying from ·02 to ·05 gr. one half of which, or even more, was acquired before it became cold; that is to say before it had cooled down to 100°, considerably above which point it began to acquire weight. Of the increase of weight above mentioned, it was found that about $\frac{1}{3}$, or $\frac{1}{4}$, (for the proportion varied from causes that I could not discover) was due to the condensation of air, the rest was due to moisture. The oxide I employed was perfectly pure, and prepared by exposing metallic copper to heat. Dr. Ure states, "that 100 grs. of the oxide prepared from the nitrate of copper exposed to a red heat merely till the vapours of nitric acid were expelled, absorbed in the ordinary state of the atmosphere from ·1 to ·2 gr. in the space of an hour or two, and about half that quantity in a very few minutes."

In determining the quantity of water formed by the oxide of copper in the usual manner, there is yet another difficulty to contend with, to which we have alluded above, and which we shall here briefly notice. It has been stated, that complete combustion seldom or never takes place during one exposure to heat, and that in many cases the oxide ought to be removed from the tube and retriturated. Now it was found, almost invariably, that during the second exposure to heat, the tube, instead of *losing* additional weight, actually became *heavier*, sometimes to the

the matter in despair, and endeavour to contrive some other mode of analysis that should be free from these difficulties altogether. After a good deal of consideration I was induced to adopt a method which had occurred to me long before, but which I had never put in execution. This method is very simple, and founded on the following well known principles.

When an organic product containing three elements, hydrogen, carbon, and oxygen, is burnt in oxygen gas, one of three things must happen. 1. The original bulk of the oxygen gas *may remain the same,* in which case *the hydrogen and oxygen in the substance must exist in it in the same proportions in which they exist in water;* (for it is well known that oxygen gas by being converted into carbonic acid gas is not altered in its bulk): or, 2. The original bulk of the oxygen may be *increased,* in which case the oxygen must exist in the substance in a *greater* proportion than it exists in water; or, 3. The original bulk of the oxygen gas may be *diminished,* in which case the *hydrogen must predominate.*

Hence it is obvious, that in the first of these cases the composition of a substance may be determined by simply ascertaining the quantity of carbonic acid gas yielded by a known quantity of it; while in the other two, the same can be readily ascertained by means of the same data, and by noting the excess or diminution of the original bulk of the oxygen gas employed. Such are the simple and universally admitted

amount of ·03 gr. though often much less than this. I was a good deal puzzled to account for this anomaly at first, but believe that it arose chiefly from the reoxydation of the partially reduced oxide, by the air of the atmosphere.

Since this Paper was read before the Royal Society, I have observed one or two other singular facts connected with this subject, which will be noticed when we come to speak of the analysis of substances containing azote.

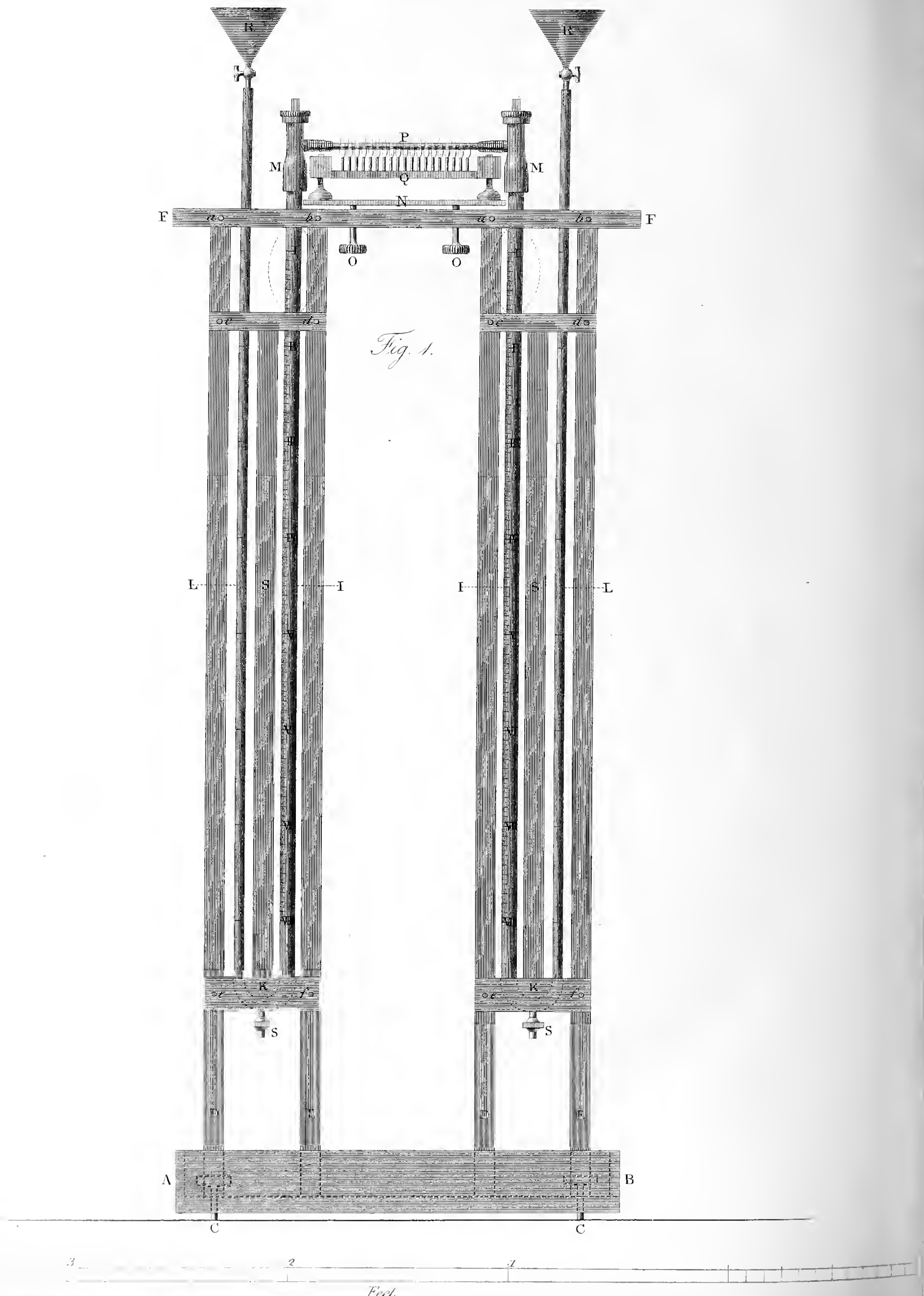

Fig. 1.

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principles in which the following method of analysis is founded; the only novelty in which, therefore, is the form of the apparatus; and of this it is hoped the following summary sketch, and annexed figures, will convey every requisite information.

Fig. 1, Plate XIV, represents a front view or elevation of the whole apparatus in the act of being employed. A B is a platform, two feet square, surrounded by a ledge about 2½ inches high, for preserving any mercury that may chance to fall about, and furnished with four adjusting screws (of which two, C C, are sectional views), by means of which it may be placed perfectly horizontal. Into this platform, in the manner represented, are fixed perpendicularly four square pillars, D E, D E, about four feet and a half high, at the top of which is placed another small platform, F F, about four inches wide, and which may be fixed or removed at pleasure by means of the brass pins *a b*, *a b*. I I are glass tubes graduated with the utmost care to hundredths of a cubic inch, and which are cemented at bottom into semicircular iron tubes enclosed in the blocks K K (as represented by the dotted lines). These iron tubes project a little below the wood at the lower part, where they are furnished with iron stop-cocks, S S, for drawing off the mercury when it may be necessary. Into the other end of these semicircular tubes are likewise cemented the glass tubes L L (of smaller dimensions, and a little longer than the tubes I I), and forming with them, when taken together, inverted syphons. The smaller tubes, L L, are represented as surmounted by funnels, R R, furnished with stop-cocks, the object of which is to permit the mercury to flow into them with any velocity that may be required. On the tops of the larger tubes, I I, are cemented the vertical stopcocks, M M, of which fig. 2. Plate XV, is a sectional view on a larger scale, and which renders the construction so obvious, that perhaps no further remark is necessary, than merely stating that the cup, *a*, is filled with oil, and that the plug, *b*, which is square at the upper part, and adapted to a key, is furnished with a shoulder, on which the screw-cap, *c*, rests, and by means of which it may be tightened at pleasure.*

On the platform, F F. (fig. 1.) is a thin piece of wood, capable of being raised or depressed at pleasure, by means of the screws, O O; on this the lamp Q is placed, which may thus be placed at any distance that may be required from the tube, P. Fig. 3, is an enlarged view of this lamp: it consists of two reservoirs, *d e*, for holding the spirit, connected together by means of the tube, *f*, into which are placed, at the distance from one another of about $\frac{1}{3}$ of an inch, a number of vertical burners, *g g*, &c. about $\frac{1}{12}$ of an inch in diameter, and $\frac{3}{4}$ inch long, and *made as thin* as possible, with the view of preventing the conduction of the heat. These burners are each furnished with a few threads of cotton, and are bent a little alternately like the teeth of a saw, in order that their flame may envelope the tube, P, (fig. 1.) more completely. *h* is a cover for the wick of the lamp when not in use. The

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* These syphons are fixed together independently of the general frame work, and may be removed at pleasure by taking out the pins *c d*, *c d*, and *e f*, *e f*. This admits of their being replaced by others of different sizes. Those of a larger size have balls near the top, as represented by the dotted lines, and may contain as much as 20 cubic inches of gas. It much facilitates the process of determining the exact quantity of gas contained in the apparatus, to have both legs of the syphon graduated, which may be easily done so as to obviate the effects of capillary attraction when the tubes are not both of the same calibre.

Fig. 2.

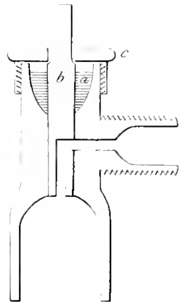


Fig. 3.

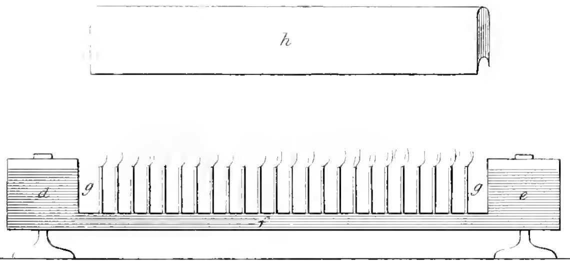


Fig: 5.

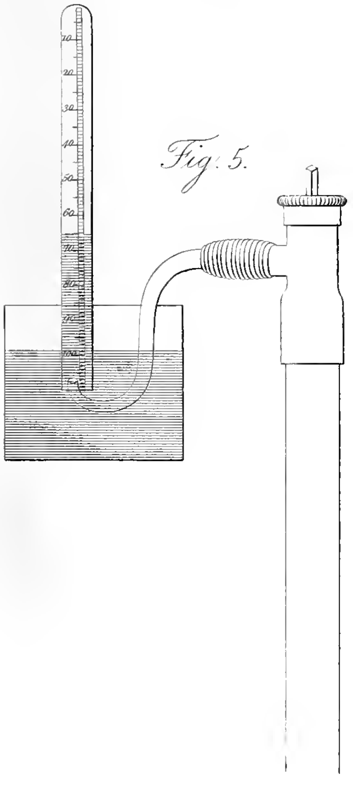
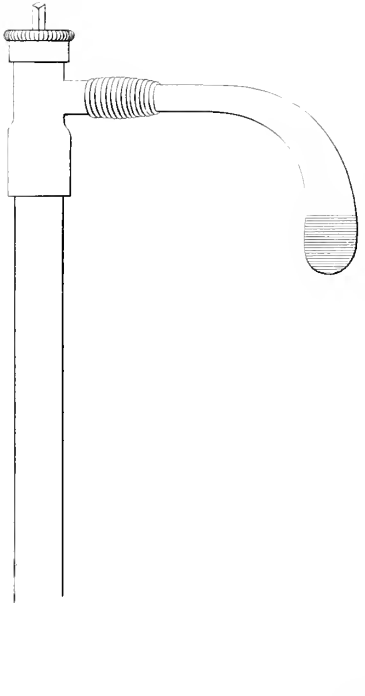


Fig: 4.



tube, P, (fig. 1.) is of green or bottle glass, moderately stout, and about $\frac{1}{5}$ of an inch internal diameter. It is fixed between the horizontal parts of the vertical cocks, M M, so as to be perfectly air-tight; and when required, the whole, or any part of it, may be heated by means of the lamp, Q, at the pleasure of the operator.

When the apparatus is to be employed, both the syphon gasometers, I L, I L, are to be filled with quicksilver, and a small green glass retort, containing the requisite quantity of chlorate of potash, (and which had been previously heated so as to completely expel the common air, and to fill it with oxygen gas) is to be attached to one of the cocks, as represented in fig. 4, by means of a caoutchouc tube. Heat is then to be applied, and any quantity of oxygen gas that may be required, introduced into the tube, I. After the whole has acquired the temperature of the atmosphere, the exact quantity of the gas is to be accurately noticed, as well as the state of the barometer and thermometer at the same time. The tube, P, containing the substance to be analysed, is then to be firmly fixed between the cocks M M,* and subjected to heat, during which the oxygen gas is to be transferred from one syphon to another, through the red hot tube, with any velocity that may be required, and which may be regulated by means of one of the stop-cocks of the funnels, R R, and the stop-cock, S, of the opposite syphon.

Such is a general view of the apparatus, and the principles

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* I have tried various modes of connecting the tube so as to ensure its being air-tight. Caoutchouc answers very well; but the best substance I have hitherto employed are slips of thin moistened hogs' bladder, tied on very tightly with fine *dry* twine. The twine is then to be moistened also, and the whole kept in this state till the end of the experiment.

of its operation; but perhaps a few practical remarks on some of the circumstances to be attended to during its employment, may not be deemed superfluous.

The substance to be analysed may be placed in a small tray made of platina foil, and introduced alone into the tube P, and gradually submitted to the action of heat and oxygen gas; but this does not answer well with organic compounds, as a portion of them is apt to escape combustion. Another method is to mix the substance with pure silicious sand, and to retain the mixture in the centre of the tube by means of asbestos. But this method often fails, except there be about an inch of the oxide of copper at each end of the tube, which must be kept red hot during the experiment, and in this case it succeeds completely with many substances. Another method, and that which the most generally succeeds, is to mix the substance with peroxide of copper, to heat these together in the tube in the first place, and afterwards to open the other stop-cock and send the oxygen gas through the ignited and partially reduced oxide, by means of which it again becomes peroxidized; and any portion of the substance that had escaped complete combustion in the first part of the experiment, is now completely burnt. This last method is also that employed when it is required to determine the quantity of carbonic acid gas yielded by a given quantity of any substance; only in this case, of course, oxygen gas is not required, and the contents of the tube P, must be taken out and well triturated, and subjected to heat a second time. If it should be required to analyse the gas formed, one method of removing it from the tube I, is represented at Fig. 5; and others will readily occur to the practical chemist.

The following are some of the advantages of this apparatus, and mode of analysing organic compounds. In the first and chief place, *there is nothing to be apprehended from moisture.* Whether the substance to be analysed be naturally a hydrate, or in whatever state it may be with respect to water, the results will not be affected; and the great problem, whether the hydrogen and oxygen exist in the substance in the proportions in which they form water, or whether the hydrogen or oxygen predominates, will be equally satisfactorily solved, and that (of course within certain limits), independently of the weight of the substance operated on.* When however it is the object to ascertain the quantities of carbonic acid gas and water yielded by a substance, it is, of course, necessary to operate on a known weight; but this being once determined, there is no fear, as in the common methods, of exposing the substance to the atmosphere as long as may be necessary. The hygrometric properties of the oxide of copper, as well as its property of condensing air, are also completely neutralised, for the whole, at the end of the experiment, being left precisely in the same state as it was at the commencement, the same condensation of course must take place, and any little differences that may exist are rendered quite unimportant from the bulk of oxygen gas operated on, which of course should, in all instances, be considerably greater than that of the carbonic acid gas formed. Another advantage of this method is, that more perfect combustion is ensured by it than by any other that I am acquainted

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* It is to be observed, that, throughout the experiments, great care is taken that the gases are *saturated with moisture*; the errors from this cause are thus rendered definite, and are easily corrected by tables calculated for the purpose from the most accurate data, and which will be given in a subsequent communication.

with. There is also no trouble of collecting or estimating the quantity of water, a part of the common process attended with much trouble, and liable to innumerable accidental errors, besides those already mentioned, and which there is no method of obviating or appreciating; here, on the contrary, the results are obtained in an obvious and permanent form, and, from the ease with which they are thus verified, comparatively very little subject to error.

It need scarcely be stated, that the form and principles of this apparatus render it well adapted for many other chemical operations besides the analysis of organized substances. Such, for example, as the reduction of oxides by hydrogen, and a variety of others that will readily occur to the practical chemist.

Of the Saccharine principle.

In the following observations, the word *Sugar*, is used in its ordinary acceptation; but the extended sense in which the term *saccharine principle* is employed, requires a few remarks.

Messrs. Gay Lussac and Thenard were induced to conclude, from their experiments on organized products, that when the hydrogen and oxygen of a substance exist in it in the proportions in which they form water, the substance is neither acid nor alkaline, as in sugar, starch, gum, &c.; that when the oxygen exceeds this proportion, the substance possesses acid properties; that when it is less, an oily or resinous character.* These conclusions are true to a certain extent, but by no means universally so, as will be shown hereafter. I shall however adopt this general distribution of

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* Recherches Physico-chimiques. ii. 321.

organized substances so far, as to confine my attention at present to those substances in which the first peculiarity above mentioned exists; and as sugar, on account of its crystalline form, appears to constitute the most perfect and definite of these substances, I have thought it best entitled to give a name to the whole class, or family, and hence have included, under the term *saccharine principle*, all those substances, whatever their sensible properties may be, into the composition of which the hydrogen and oxygen enter in the proportions in which they form water. Now it will be found, that the substances thus constituted may generally be employed as aliments, and as they are chiefly derived from the vegetable kingdom, they may be considered as representing vegetable aliments, properly so called; hence, *saccharine principle* and *vegetable aliment* may be regarded as synonymous terms, and they will be so employed throughout the present inquiry.

As a subject of general interest to chemists, as well as of considerable importance in the present inquiry, I shall also attempt to investigate the composition of a few of the compounds of the saccharine principle with oxygen, or what are usually denominated the *vegetable acids*.

Of Sugars.

Many analyses of sugar have been published by different chemists, no two of which agree with each other. These discrepances have doubtless arisen from various causes, though one cause has probably been some real or accidental difference in the composition of the sugars employed.* How

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* Some years ago I published an analysis of sugar, in which the proportions of carbon to water were stated to be to one another as 40 : 60. I was not aware at

many distinct varieties of sugar exist I do not pretend to know, but there are at least two, (independently of the sugar of milk, manna, &c. which belong to another series), and probably there are several others; and it is to the mixture or combination of these in different proportions, and the frequent presence of foreign bodies, that a good deal of the confusion respecting the composition of sugar has undoubtedly risen.

Cane Sugar. The strongest and most perfect sugar that I am acquainted with, is sugar candy carefully prepared from cane sugar. This, purified by repeated crystallizations from water and alcohol, and deprived of the little hygrometric

that time of the differences existing among sugars, and the results given were founded on the analysis of a specimen of remarkably fine looking sugar candy, a quantity of which I had purchased and kept by me for several years for the purposes of experiment.† At length my stock became exhausted, and I was surprised to find on analysing other specimens, that they in general contained upwards of one per cent. more of carbon than what I had before examined. This induced me to recur to the notes of my former experiments, but I could detect no material error in them; and though I readily admit that the apparatus I then employed was much less susceptible of accuracy than what I now use, I cannot help thinking that the candy itself was partly in fault, and that it was prepared from an imperfect sugar, probably from the East Indies.

There was also another circumstance which contributed to mislead me, not only in this but in all my other results, viz. an inaccuracy in the weight usually assigned to atmospheric air, at least as regarded *my weights.* I have long suspected the perfect accuracy of this datum as settled fifty years ago by Sir G. Shuckburgh, and have been accustomed for some time past to make an allowance for it; but I was not aware till recently of its exact amount, when I was induced to undertake a series of experiments on the subject, which I hope shortly to lay before the public.

† See Annals of Philosophy, iv. 424 (N. S.) I do not distinctly remember whether, at the time this paper was published, some of the original sugar candy existed or not, but I had then only made one or two experiments on the sugar of commerce.

moisture that usually adheres to it by exposure for some time to a temperature of 212°, was found to be composed of

Carbon - - -	42·85
Water - - -	57·15

Now, all the finest and purest specimens of loaf sugar of commerce that I have yet examined, give, when similarly treated, precisely the same results. They may therefore be considered as identical in their composition with sugar candy.* Cane sugar appears to undergo no change whatever at the temperature of boiling water; but at about 300° it begins to melt, and assume the form of a dark brown liquid. In one experiment, after exposure to this temperature for seven hours, it lost ·6 per cent. only of its weight, but its properties seem to have been permanently injured. BERZELIUS however has shewn that, on being combined with lead, sugar parts with about 5·3 per cent. of water without undergoing decomposition; for he has likewise shown that it may be obtained again from the lead in its original state. This saccharate of lead I have several times formed, and once by accident I obtained it in the state of beautiful crystals.

Sugar of Honey. The *lowest*† well defined sugar that I have yet examined, was first obtained from Narbonne honey, by means of a process formerly pointed out by me for obtain-

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[*] Dr. URE states that he has found sugar to contain upwards of 43 per cent. of carbon; but no such specimen has occurred to me, though I by no means deny its existence. Indeed I have hitherto met with no sugar as it occurs in commerce, yielding more than 42·5 per cent of carbon, and frequently it contains considerably less than this.

[†] In commerce, these imperfect sugars are denominated *weak* or *low* sugars, which last epithet is here employed in this sense.

ing diabetic sugar in a state of purity.* This, deprived of its hygrometric moisture by being placed under a receiver with sulphuric acid for several days, was found to consist of

Carbon - - -	36·36
Water - - -	63·63

This sugar in the ordinary state of the atmosphere usually contains more water than indicated by this analysis; that is to say, generally about 64·7 per cent. On the other hand, on exposure to a temperature considerably below that of boiling water, it rapidly loses about 3 per cent. of water, and begins to assume the fluid form; kept at the temperature of boiling water for 30 hours, it lost in one experiment upwards of 10 per cent. of its original weight, became of a deep brown colour, and seemed to be partially decomposed.†

Sugar prepared from *starch* evidently belongs to this variety, as is sufficiently indicated both by its sensible properties and composition. The same is true in general of *diabetic sugar*, and probably also of the *sugar of grapes, figs, &c.* When pure, all the varieties of this sugar are beautifully

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* Med. Chirurg. Trans. viii. 537. I have little doubt that honey contains a still lower sugar, and which is incapable in this country (at least during a great part of the year), of assuming the solid form. This is probably the liquid sugar of Proust.

† I observed that after this sugar had been cautiously melted it might be preserved in the state of a transparent fluid, if placed in a perfectly dry atmosphere, as under the receiver of an air pump with sulphuric acid; but that in a few hours after exposure to the air, it began to grow opaque and assume the crystalline form, by attracting moisture. Is not this precisely analogous to the deterioration which is known to take place in the sugars of commerce? See Mr. Daniell on this subject in the Royal Institution Journal, v. 32. Dr. Ure supposes that this deterioration depends on the absorption of oxygen; but I have hitherto met with no sugar containing an excess of oxygen.

white, crystallize in spherules, and are permanent under the ordinary circumstances of the atmosphere.

Between these two extremes, sugars occur of almost every grade, as the following table will show.

	Carbon.	Water.
Pure sugar candy - - -	42·85	57·15
* Impure sugar candy	41·5 to 42.5	58·5 to 57·5
East India sugar candy *(v)*	41·9	58·1
English refined sugar	41·5 to 42·5	58·5 to 57·5
East India refined sugar *(v)*	42·2	57·8
Maple sugar *(v)* - - -	42·1	57·9
Beet-root sugar *(v)* - -	42·1	57·9
East India moist sugar *(v)*	40·88	59·12
Sugar of diabetic urine -	36 to 40 ?	64 to 60 ?
Sugar of Narbonne honey	36·36	63·63
Sugar from starch - - -	36·2	63·8

On some of these it may be necessary to make a few remarks. The *sugar candies* of the shops frequently contain minute quantities of foreign fixed bodies, such as lime, &c., as well as others of a destructible character. Both the specimens of *India sugar candy* I examined were obviously impure to the eye, being of a brown colour and deliquescent; they contained, among other things, traces of potash. The *East India refined sugar* was perfectly white, but rather soft and friable, and it did not possess the fine and brilliant grain of the best refined sugars of commerce. For a specimen of the *maple* sugar I was indebted to Mr. FARADAY; this, when I received it, was very impure and deliquescent, but by treating it by the process above alluded to, a portion was separated that differed but little in its appearance from cane sugar. The *beet-root sugar* was made and refined in France;

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* In these results *fixed* bodies only have been allowed for, and those marked *(v)*, as occurring in commerce, are probably subject to slight variations in their composition.

it was perfectly white, but rather soft and fine in the grain. The *East India moist sugar* was of a very low kind, and known in commerce by the name of *Burdwan sugar;* it was deprived of its hygrometric moisture before analysis by exposure to sulphuric acid under a receiver. The *diabetic sugar* was prepared as above ; the results given were obtained many years ago, and I have had no opportunity of repeating the analysis with the present apparatus ; I believe however that diabetic sugars in general belong to the Honey variety. The *sugar of starch* was prepared by myself in the usual manner.

Of Amylaceous Principles.

Before we proceed to consider the analysis of amylaceous bodies, a few remarks on the nature of these and similar substances may not be deemed improper. It has been known from the very infancy of chemistry, that all organised bodies, besides the elements of which they are essentially composed, contain minute quantities of different foreign bodies, such as the earthy and alkaline salts, iron, &c. These have been usually considered as mere mechanical mixtures accidentally present ; but I can by no means subscribe to this opinion. Indeed, much attention to this subject for many years past has satisfied me that they perform the most important functions ; in short, that organization cannot take place without them. This point will be more fully investigated hereafter : at present it is sufficient merely to observe, that many of those remarkable changes which crystallized bodies undergo on becoming organized, are more apparent than real ; that is to say, their chemical composition frequently remains essentially the same ; and the only points of difference that can be traced, is the presence of a little more or less of water, or the intimate mixture of a minute portion of some foreign fixed body. There is no term at present employed which expresses this condition of bodies, and hence, to avoid circumlocution, I have provisionally adopted the term[*] *merorganized,* (*μέρος pars* vel *partim*) meaning to imply by it that bodies on passing into this state become partly, or to a certain extent, organized. Thus starch I consider as *merorganized* sugar, the two substances having, as we shall see presently, the same essential composition, but the starch differing from the sugar by containing minute portions of other matters, which we may presume, prevent its constituent particles from arranging themselves in the crystalline form, and thus cause it to assume totally different sensible properties.[†]

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[*] I am indebted to my friend Mr. LUNN for this term.

[†] When this subject first occupied my attention many years ago, I was at a loss to form any notion of the *modus operandi* of these minute admixtures of foreign bodies, except the mechanical one mentioned in the text, viz. that they operated by being interposed, as it were, among the essential elements of bodies, and thus by weakening or modifying their natural affinities. But the admirable Paper, published by Mr. HERSCHEL, in the Philosophical Transactions for 1824, " On certain motions produced in fluid conductors when transmitting the electric current," appeared to throw an entire new light on the subject. The facts brought forward in this Paper are of the most important kind, and seem to me to be evidently connected with a principle of a more general character, which when completely developed, will lead to the most unexpected results. " That such minute proportions of extraneous matter," says Mr. H. " should be found capable of communicating sensible mechanical motions and properties of a definite character to the body they are mixed with, is perhaps one of the most extraordinary facts that has yet appeared in chemistry. When we see energies so intense exerted by the ordinary forms of matter, we may reasonably ask what evidence we have for the imponderability of any of the powerful agents to which so large a part of the activity of material bodies seem to belong?"

Any substance may be supposed capable of performing the part of a merorganizing body; but, in a certain point of view, *water* appears to constitute the *first* and *chief*, at least in organized substances.

Wheat Starch. The most perfect form of the amylaceous principle is undoubtedly that derived from wheat. This has been analysed by different chemists with very different results. MM. Gay Lussac and Thenard state that they found it to contain as much as 43·55 per cent. of carbon; while Dr. Ure informs us that he only found 38·55 per cent. The following observations will sufficiently explain these differences.

A very fine specimen of wheat starch, which had been prepared expressly at my desire without the addition of the colouring matter commonly added to the starch of commerce, and which had been kept in a dry situation for many months, was found, in the ordinary columnar form in which it usually occurs, (abstracting foreign matters) to consist of

Carbon	37·5
Water	62·5.

One hundred parts of the same specimen reduced to a state of fine powder, and subjected to a temperature between 200° and 212°, for the space of 20 hours,* lost, in a mean of two experiments, 12·5 parts, and on being analysed in this state gave

Carbon	42·8
Water	57·2,

which very nearly coincides with what by calculation it ought to have given, on the supposition that the loss of weight was owing to the escape of water, a circumstance indeed of which there could have been little doubt. Starch however in this state still retains water, a portion of which may be separated

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* I have reason to believe from other experiments that six or eight hours, or even less, of steady exposure to the boiling temperature, will sometimes reduce both starch and arrow root, and even gum, to this state of desiccation.

by subjecting it to higher temperatures. Thus, after having been exposed as above for 24 hours to the temperature of 212°, on being further submitted to a temperature between 300° and 350° for six hours longer, it lost 2·3 per cent. more, and analysed in this state gave very nearly

Carbon 44
Water 56.

It had now acquired a slight yellow colour, and seemed to have suffered some change in its properties; hence, this is probably nearly the utmost quantity of water that starch is capable of parting with, short of decomposition.

Arrow root. This is another variety of the amylaceous principle, of which, like sugar, there seems to be a great variety. The specimen on which the following experiments was made was remarkably fine, and free from adventitious matters. It had been kept in the same drawer with the starch before mentioned, and under precisely similar circumstances of the atmosphere was found to consist of (abstracting foreign matters)

Carbon 36·4
Water 63·6.

One hundred parts, in the above state, exposed for twenty hours to a temperature between 200° and 212°, lost fifteen parts. Hence its composition, when thus dried, was very nearly the same as that of wheat starch similarly exsiccated; or it consisted of

Carbon 42·8
Water 57·2.

On being subjected to the full temperature of 212° for six hours longer it lost 3·2 per cent. more, and was then reduced to a state similar to that of starch dried between 300° and 350°, or it consisted very nearly of

Carbon 44·4.
Water 55.6.

When subjected to the temperature of 300° and 350° for six hours longer, it lost 1·38 per cent. more of its weight, but became of a deeper yellow colour than starch similarly exposed, and consequently shewed greater marks of decomposition. Hence, this form of the amylaceous principle, like the sugar of honey before-mentioned, seems to part with the whole of the water not essential to its composition at the temperature of 212°, or even perhaps below this point if exposed for a period sufficiently long.

It may not be deemed superfluous to notice here very briefly two or three circumstances resulting from the above analyses, which, though their importance may not be seen at present, should be constantly borne in mind, as they will enable us hereafter to throw light on many points connected with organization, which otherwise would be inexplicable.

In the first place, the identity of composition between the sugar of honey and arrow root, under the ordinary circumstances of the atmosphere, seems to show that the differences among the varieties of the amylaceous principles are precisely analogous to those existing among sugars, or in other words, that there are *low* starches as well as *low* sugars. Whether arrow root be the lowest that exists, I am unable to say; but I have met with none lower; and have reason to believe that the greater portion of the other varieties of the amylaceous principle known to exist, like the varieties of sugars above given, are intermediate in their composition between arrow root and wheat starch. The same remarks apply to other merorganized principles.

In the second place, the identity of composition between wheat starch and cane sugar, and between the sugar of honey and arrow root above mentioned, seems to show that, though merorganized bodies are not actually capable of assuming the crystalline form, yet that the original tendency among their essential elements to combine in certain proportions (and perhaps to assume certain forms) still continues to operate, though in a mitigated degree, and thus to exert, as it were, a feeble *nisus*, or endeavour toward the maintenance of certain definite modes of existence.

Thirdly, and lastly, crystallized bodies usually part with their water of crystallization with difficulty, and when they do, it is commonly *per saltum*, or in definite quantities. Merorganized bodies, on the other hand, retain water so feebly at all points, that within certain limits this fluid may be readily separated, or made to combine with them in every proportion. And this appears to be true, not only with respect to water, but with other substances capable of combining with merorganized bodies. It may be remarked also in general, that *low* varieties of principles resemble merorganized bodies in these and some other respects; thus, they usually part readily with all the water not essential to their composition at the temperature of 212°, or even less (provided they be submitted to it long enough,) above which point they rapidly undergo decomposition, &c.

Lignin, or *the woody fibre.*

Messrs. GAY LUSSAC and THENARD first showed that the hydrogen and oxygen in this principle exist in it in the proportions in which they form water, a result fully confirmed by my experiments. The variety of forms in which lignin occurs in different woods is so great, that an examination of them all would be quite out of the question; I therefore selected two, *viz.* the woods of the *Box* and *Willow*, which appeared to present the greatest contrast; the one being among the densest, the other the lightest of the woods. These were both treated exactly in the same manner, that is to say, they were first reduced to the form of a coarse powder by rasping, then well pulverized in a WEDGWOOD mortar, and afterwards sifted. Being by these means reduced to the form of impalpable powders, they were boiled in repeated portions of distilled water till that fluid came off unchanged: a tedious process, requiring several days to accomplish perfectly. After this they were similarly treated with alcohol, and finally again with distilled water. They were now exposed to the atmosphere, when in a dry and favourable state; and when they ceased to lose weight were submitted to analysis, and found to consist of (abstracting foreign matters)

	Box.	Willow.
Carbon	42·7	42·6
Water	57·3	57·4

A known weight of each was then exposed for twenty-four hours to a temperature of 212°, and afterwards for six hours longer (by means of an oil bath) to a temperature between

300° and 350°; and at the end of this time they were found to have lost, per cent.

Box.	Willow.
14·6	14·4

Analysed in this state of desiccation, they were found to consist of

	Box.	Willow.
Carbon - - -	50·0 - - -	49·8
Water - - -	50·0 - - -	50·2

showing that the loss of weight arose from the escape of water. These latter results nearly agree with those of MM. Gay Lussac and Thenard, as obtained from the analyses of the woods of the *Oak* and *Beech*, and seem to show beyond a doubt, that the composition of all of them is similar, or that they consist of equal weights of carbon and water; to which simple analogy this important principle probably owes its stability.

Lignin undoubtedly exists in many other forms besides the woody fibre; indeed it appears to constitute the skeleton or ground work on which most organic processes in the vegetable kingdom are carried on. To illustrate its properties as an *aliment*, the only point of view in which we have to consider it here, I shall briefly quote the experiments of Professor Autenrieth of Tubingen, who showed some years ago, that by proper management this principle might be rendered capable of forming bread. The following was the method he employed for this purpose. In the first place, every thing that was soluble in water was removed by frequent maceration and boiling. The wood was then reduced to a minute state of division, that is to say, not merely into fine fibres, but actual powder; and after being repeatedly subjected to the heat of an oven, was ground in the usual manner of corn. Wood thus prepared, according to the author, acquires the smell and taste of corn flour. It is however never quite white, but always of a yellowish colour. It also agrees with corn flour in this respect, that it does not ferment without the addition of leaven, and in this case sour leaven of corn flour is found to answer best. With this it makes a perfectly uniform and spongy bread; and when it is thoroughly baked, and has much crust, it has a much better taste of bread than what in times of scarcity is prepared from the bran and husks of corn. Wood flour also, boiled in water, forms a thick tough trembling jelly, like that of wheat starch, and which is very nutritious.*

It may be remarked that all the preceding principles are capable of being converted into oxalic acid by the action of the nitric acid, and into sugar by the action of dilute sulphuric acid.

Acetic Acid, or Vinegar.

This principle seems to have been more or less used as an aliment, either by accident or design, in every age and country. There have been various analyses of it published, by different chemists; but it is singular, that although some of them have given its exact composition, no one seems to have been struck with the most remarkable peculiarity of its composition,† viz.

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* See the Edinburgh Magazine for November 1817, p. 313, where an account is also given of the Lapland mode of making bread from the bark of trees, as described by Von Buch. It is not improbable that during the above processes the lignin combines with water, and forms an artificial starch.

† Berzelius, in his Paper On the definite proportions, in which the elements

that the hydrogen and oxygen exist in it in the proportions in which they form water. Some experiments which I made many years ago appeared to render this probable, but from the difficulties attending the analysis of this acid, and the uncertainty arising from the properties of the oxide copper formerly stated, I was unable to satisfy myself completely on the subject. On repeatedly burning, however, a very fine specimen of the acetate of copper, in a given bulk of oxygen gas, with the apparatus described at the commencement of this Paper, it was found that the volume of the gas underwent no change, and hence, that the above opinion was correct.

Acetic acid, freed from non-essential water, I find to be composed of

Carbon 47·05
Water 52·95,

results which almost exactly agree with those of other chemists.

Sugar of Milk.

The sugar of milk employed in these experiments was prepared by myself in the usual manner, and rendered as pure as possible by repeated crystallizations. It was then freed from its hygrometric moisture by confinement under a receiver with sulphuric acid, and was found to consist of

Carbon 40
Water 60,

results almost exactly agreeing with those of Berzelius.

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of organic nature are combined, assigns to vinegar this composition. See Annals of Philosophy, v. 174 (O. S.) Dr. Thomson also, in the last edition of his Chemistry, gives the same composition; though in his more recent work he has assigned to it another proportion of hydrogen.

Manna Sugar. The saccharine principle existing in manna has been long known to possess peculiar properties. That employed in the following analysis was separated by means of alcohol in the manner commonly described in chemical books, and was obtained in a state of perfect purity by repeated crystalizations from that fluid. It was then dried at 212°, and in this state was found to consist of

Carbon	38·7
Water	61·3

results very different from those of M. Theodore de Saussure.* This sugar seems to part with hygrometric water only at the temperature of boiling water; but a few degrees above this point it begins to suffer decomposition, and at 250° it assumes, without melting, the form of a brown powder, and acquires a strong empyreumatic odour.

Gum Arabic. A very fine specimen of gum arabic reduced to powder, and analysed as it existed under the ordinary circumstances of the atmosphere, was found (abstracting foreign matters) to consist of

Carbon	36·3
Water	63·7.

One hundred parts of the same gum, exposed to a temperature between 200° and 212°, for upwards of 20 hours, lost 12·4 parts. Hence its composition thus dried would be nearly

Carbon	41·4
Water	58·6.

Results confirmed almost exactly by actual analysis.

The same gum, further exposed to a temperature between 300° and 350° for six hours longer, assumed a deep brown

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* See Bibliothéque Britannique, 1814; also Annals of Philosophy, vi. 424.

colour, and seemed to have suffered decomposition, though it lost in weight only 2·6 per cent. more. Hence, gum probably parts with the whole of the water not essential to its composition at the temperature of 212°, provided it be exposed for a sufficient time to this degree of heat.

Substances belonging to this series appear in general to be of a weak or *low* kind, though they are probably very numerous. They may be readily distinguished by being converted into saclactic acid by the action of nitric acid.

The vegetable Acids.

Oxalic acid. Many years ago I ascertained that this acid in the crystallized state consists of

Carbon	19·04
Water	42·85
Oxygen	38·11,

a composition assigned to it long since by other chemists, and now I believe generally admitted, except by Dr. THOMSON, who informs us that he has met with a specimen containing as much as half its weight of water.* I have examined a great many specimens with the view of verifying this result, but hitherto have not been successful.

Citric acid. This and all the following acids, except the *malic*, were analysed at the same period as the oxalic acid above mentioned, and the results have been recently verified. I find the crystals of citric acid to consist of

Carbon	34·28
Water	42·85
Oxygen	22·87.

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* Attempt to establish the first principles of chemistry by experiment, ii. 103.

This composition has been approached very nearly by several chemists; but no one, so far as I know, has given it exactly.

Tartaric acid in crystals is composed of

Carbon	32·0
Water	36·0
Oxygen	32·0,

a composition assigned to it by Dr. Thomson in his work just quoted.

Malic acid. I am not acquainted with any analysis of malic acid except that of M. Vauquelin,* which has not, I believe, obtained much confidence among chemists, chiefly on account of the large proportion of hydrogen which he assigns to it. The acid I employed was obtained from the berries of the mountain ash by a process very similar to that of Mr. Donovan. It was not analyzed *per se*, but in combination with lead, with lime, and with copper, and was found, abstracting water not essential to its composition, to consist of

Carbon	40·68
Water	45·76
Oxygen	13·56,

This acid, in many points of view, may be regarded as one of the most interesting and important of all the vegetable acids.

Saclactic acid. The unexpected composition of this acid induced me to investigate its properties more fully than I had otherwise intended. What I first employed was obtained from the sugar of milk, and hence was tolerably pure, though not perhaps completely so. Latterly, I have preferred that

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* Annales de Chimie et de Physique, vi. 337.

prepared from gum, which, though exceedingly impure as first obtained, may be easily and completely purified by the following simple process.

Add ammonia in slight excess to the impure acid, and afterwards as much boiling distilled water as will dissolve the saclactate formed. Filter the solution while boiling hot, and then evaporate it very slowly nearly to dryness. The saclactate of ammonia will be separated in the form of crystals, which are to be washed with cold distilled water till they become quite white and pure. They are now to be again dissolved in distilled water, and the boiling saturated solution permitted to drop from a filter into cold diluted nitric acid. This latter of course decomposes the saclactate, and precipitates the saclactic acid in a state of perfect purity. Thus obtained, this acid was found to consist of

Carbon	33·33
Water	44·44
Oxygen	22·22

results differing a little from those of other chemists, who probably did not take the necessary pains to obtain this acid in a perfectly pure state.

In conclusion, I wish to observe, that I purposely abstain at present from making any further observations on the preceding results than those already given. I do this for several reasons: in the first place, such observations will appear with greater effect, when the whole of the facts in my possession are laid before the public; and secondly, I consider that data which lead to such important conclusions as these appear to do, cannot be too firmly established; I therefore, in the mean time, earnestly invite chemists in general to repeat them, and thus either to confirm them, or point out their errors; and for the sake of those who may be inclined to take this trouble, I shall close this part of the subject with the following remarks: 1. The multiples of hydrogen, carbon, and oxygen, are assumed in the preceding calculations as 1 : 6 : 8. 2. The results given are, on all essential points, the means of many experiments, the differences among which are either inappreciable, or at most vary from ·01 to ·03 of a cubic inch in from 5 to 8 cubic inches of carbonic acid or oxygen gas; the greatest differences in general, being for obvious reasons, found among merorganized bodies; and hence the analyses of these are usually stated to the first decimal figure only. 3. As rules to be observed, I would say, that a single result should never be registered, nor a single calculation made, till the operator has made himself complete master of his apparatus, and carefully studied the nature of the substance to be analyzed; for different substances often require very different management: that two or three results should never be relied on; the minute quantities here sought can be only obtained, like those of astronomy, by repeated observations: and lastly, the utmost care should be taken that the substances operated on be *pure*, a point of greater importance, and frequently of more difficult accomplishment than any other, and one that has caused me more trouble than all the rest put together.

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GEOGRAPHIE (La Soc. de) Recueil de Voyages et de Mémoires, publié par la Société de Géographie. Tome deuxième. 4° *A Paris* 1825.	The Geographical Society of Paris.
GEOLOGICAL SOCIETY. Transactions of the Geological Society of London. Second Series. Vol. II. parts 1 and 2. 4° *Lond.*	The Geological Society.
GMELIN (C. G.) Naturwissenschaftliche Abhandlungen. Herausgegeben von einer Gesellschaft in Würtemberg. Erster Band, Erster Heft. 8vo. *Tübingen* 1826. See Tiedemann.	Dr. C. G. Gmelin.
GOLDINGHAM (J.) Astronomical Observations made at the Honourable the East India Company's Observatory at Madras, by, and under the superintendance of, John Goldingham, Esq. Astronomer, and Fellow of the Royal Society. Printed by Order of the Government of Madras. Vols. 3 and 4. *Madras* 1824-25.	The Honourable the East India Company.
GRAHAME (J.) The History of the Rise and Progress of the United States of North America till the British Revolution in 1688. By James Grahame, Esq. 2 vols. 8vo. *Lond.* 1827.	John F. W. Herschel, Esq. M. A.
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HORTICULTURAL SOCIETY. Transactions of the Horticultural Society of London. Vol. VI. Part 4, and Vol. VII. Part 1. 4to. *Lond.*	The Horticultural Society.
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LITERATURE (Roy. Soc. of). Transactions of the Royal Society of Literature of the United Kingdom. Vol. I. Part I. 4° *Lond.* 1827.	The Royal Society of Literature.
LONGITUDES (Le Bur. de). Observations Astronomiques faites à l'Observatoire Royal de Paris; publiées par le Bureau des Longitudes. Tome premier. Folio. *A Paris* 1825. See Connaissance des Tems.	Le Bureau de Longitudes.
MAGAZINE. The Philosophical Magazine and Journal from No. 338 to 343 inclusive. 8° *Lond.* 1826.	Richard Taylor, Esq.
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ROYAL INSTITUTION. A Journal of Science and the Arts. Edited at the Royal Institution of Great Britain. Nos. 42, 43, and 44. 8° *Lond.*	The Managers of the Royal Institution.
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———— Tables Auxiliaires Astronomiques pour l'année 1827. Publiées par H. C. Schumacher. 8° *Copenhague.*	————
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THE ZOOLOGICAL JOURNAL. No. IX. July 1826—January 1827. (coloured plates). 8° *Lond.*	The Editors.

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TO THE

PHILOSOPHICAL TRANSACTIONS

FOR THE YEAR 1827.

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ERRATA, Part IV. 1826.

Page 4, line 1, *for* " 9 ten-thousandths," *read* " 9 thousandths."

In Plate VI. page 189, *insert* the letter " N " at the left hand extremity of the horizontal diameter in Fig. 4; and at the upper extremity of the vertical diameter in Fig. 5, *insert* the letter " e."

Directions to the Binder.

Plates I. II. and III. should face page 124, *instead of* 174.

From the Press of
W. NICOL,
Cleveland-row, St, James's.

METEOROLOGICAL JOURNAL,

KEPT BY THE ASSISTANT SECRETARY

AT THE APARTMENTS OF THE

ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL

for January, 1826.

1826 January.		Time.		Barometer corrected.	Therm. without.	Degree of Moisture by Daniell's Hygrom.	Sixe's Therm.	Rain.	Winds.		Weather.
		H.	M.	Inches.	°	°	°	Inches.	Points.	Str.	
☉	1	9	0	29,742	36	936	31	0.070	E	1	Rain.
		4	0	29,695	40	857	41		E	1	Cloudy.
☾	2	9	0	29,798	39	794	38		E	1	Cloudy.
		3	0	29,768	39	—	41		E	1	Fine.
♂	3	9	0	29,817	33	893	31		E	1	Fine.
		9	0	29,835	36	806	38		E	2	Fine.
☿	4	9	0	29,877	35	867	33		E	1	Hazy.
		3	30	29,840	36	871	36		E	1	Cloudy.
♃	5	9	0	29,844	35	900	35		E	2	Cloudy and dark.
		3	0	29,805	35	933	36		—	1,2	Rain.
♀	6	9	0	29,712	36	903	35		E	2,3	Rain.
		—		—	—	—	37		—	—	—
♄	7	9	0	29,778	36	968	35	0.055	E	1	Cloudy and hazy.
		3	0	29,779	35	967	36		E	2	Cloudy.
☉	8	9	0	29,892	33	786	31½		E	2	Fine.
		3	30	29,948	31	No deposition at 15°	33		E	2	Cloudy.
☾	9	9	0	29,987	28	612	25		E	2	Fine.
		3	0	29,963	28	569	30		E	1,2	Fine.
♂	10	9	0	29,797	24	774	33½		NE	1	Hazy.
		4	0	29,676	30	760	32		NNW	1	Fine.
☿	11	9	0	29,665	30	910	29		N	1	Snow.
		4	0	29,704	33	786	32		N	1	Fine.
♃	12	9	0	29,755	24	799	21		NNW	1	Dark and foggy.
		3	0	29,773	31	702	32½		W	1	Hazy.
♀	13	8	30	29,906	21	788	20		W	1	Hazy.
		—		—	—	—	—		—	—	—
♄	14	—		—	—	—	—		—	—	—
		—		—	—	—	—		—	—	—
☉	15	10	0	30,216	24	890	19½		N	1	Hazy.
		4	0	30,282	28	745	29		NE	1	Hazy.
☾	16	9	0	30,439	17	772	16		E	1	Thick fog.
		5	0	30,529	28	936	28		E	1	Cloudy and hazy.

METEOROLOGICAL JOURNAL
for January, 1826.

1826 January.	Time.		Barometer corrected.	Therm. without.	Degree of Moisture by Daniell's Hygrom.	Sixe's Therm.	Rain.	Winds.		Weather.
	H.	M.	Inches.	°	°	°	Inches.	Points.	Str.	
♂ 17	9	0	30,552	24	963	22		E	1	Fine, rather hazy.
	4	0	30,542	31	846	32		SE	1	Fine.
☿ 18	9	0	30,463	30	904	28		S	1	Cloudy.
	3	0	30,341	36	734	37		S	1	Fine.
♃ 19	9	30	30,078	41	849	34		N	1	Cloudy and hazy.
	3	0	30,030	42	842	43		N	1	Fine.
♀ 20	9	0	30,349	37	916	36		N	1	Cloudy and dark fog.
	3	0	30,144	40	886	41		WNW	1	Cloudy.
♄ 21	9	0	30,146	37	812	36		SW	1	Cloudy and hazy.
	3	0	30,095	40	943	41½		W	1	Rain.
☉ 22	9	30	30,164	34	966	34		—	1	Thick fog.
	3	0	30,159	39	941	39		WNW	1	Cloudy.
☾ 23	9	0	30,146	32	963	31		S	1	Cloudy.
	3	0	30,169	40	943	40		W	1	Cloudy.
♂ 24	9	0	30,363	35	900	35		N	1	Thick fog.
	3	0	30,351	35	1000	41		S	1	Thick fog.
☿ 25	9	0	30,295	35	900	34		E	1	Cloudy.
	3	0	30,258	34	966	35		E	1	Cloudy.
♃ 26	9	0	30,270	35	867	34		E	1	Cloudy.
	3	0	30,228	36	839	37		E	1	Cloudy.
♀ 27	9	0	30,241	31	933	30		E	1	Fog.
	3	0	30,208	35	783	35		SW	1	Fine.
♄ 28	9	0	30,201	33	812	31		E	1	Cloudy.
	3	0	30,142	39	882	39		S	1	Fine.
☉ 29	9	0	30,038	34	966	32½		E	1	Fine.
	3	0	29,949	43	709	44		SE	1	Fine.
☾ 30	9	0	29,750	41	849	36		SE	1	Cloudy.
	3	0	29,641	44	829	45		SSE	1	Cloudy.
♂ 31	9	0	29,716	43	861	41½		SSW	1	Cloudy.
	4	0	29,699	46	830	47		S	1	Fine.

Whole Month.

Greatest...	......	30,552	46	1,000	47	Rain 0.125 inches.
Least	......	29,641	17	0,000	16	
Mean	......	30,027	34,2	841	34	

METEOROLOGICAL JOURNAL

for February, 1826.

1826 February.		Time.		Barometer corrected.	Therm. without.	Degree of Moisture by Daniell's Hygrom.	Sixe's Therm.	Rain.	Winds.		Weather.
		H.	M.	Inches.	°	°	°	Inches.	Points.	Str.	
☿	1	9	0	29,796	41	877	40		—	—	Foggy.
		3	0	29,738	45	859	45		E	1	Cloudy.
♃	2	9	0	29,807	46	898	43		W	1	Cloudy.
		3	0	29,772	39	824	40		SW	1	Fine.
♀	3	9	0	29,658	48	904	45		SW	2	Fine.
		3	0	29,629	50	910	51		S	2	Rain.
♄	4	9	0	29,843	44	963	43		SW	1	Cloudy.
		4	0	29,837	48	840	49		—	1	Cloudy.
☉	5	9	0	29,844	45	929	42	0.010	SSE	1	Fine.
		3	30	29,779	47	901	49		SSE	2	Cloudy.
☾	6	9	0	29,484	51	937	46		W	3	Rain.
		3	0	29,457	52	935	53	0.045	WbyS	3	Rain.
♂	7	9	0	29,924	43	861	43	0.010	W	2	Fine.
		3	30	30,135	48	702	49		W	1	Fine.
☿	8	9	0	30,335	38	939	36		S	1	Cloudy.
		3	0	30,308	47	769	47		SW	1	Fine.
♃	9	9	0	30,254	38	970	36		SSE	—	Hazy.
		3	0	30,201	42	895	46		SSE	1	Hazy.
♀	10	9	0	30,220	33	1000	32		E	1	Very hazy.
		5	0	30,180	35	967	36		E	1	Cloudy.
♄	11	9	0	30,126	36	968	33½		SE	1	Cloudy.
		4	0	30,060	39	794	40		E	1	Fine.
☉	12	9	0	30,029	43	810	35		SE	1	Fine.
		3	0	30,040	46	864	47		S	1	Cloudy.
☾	13	9	0	30,145	41	904	40		—	1	Hazy.
		3	30	30,018	46	830	48		SE	1	Fine.
♂	14	9	0	29,900	45	824	41		S	1	Rain.
		3	0	29,909	46	864	47		S	1	Cloudy.

METEOROLOGICAL JOURNAL

for February, 1826.

1826 February.	Time.	Barometer corrected.	Therm. without.	Degree of Moisture by Daniell's Hygrom.	Sixe's Therm.	Rain.	Winds.		Weather.
	H. M.	Inches.	°	°	°	Inches.	Points.	Str.	
☿ 15	9 0	29,911	46	795	46	0.011	SE	1	Cloudy.
	3 0	29,840	52	710	53		SE	1	Fine.
♃ 16	—	—	—	—	—		S	2	Fine.
	3 0	29,653	50	790	53		S	2	Fine.
♀ 17	9 0	29,302	48	840	44		SSW	3	Cloudy.
	3 0	29,354	47	798	49		W	1	Rain.
♄ 18	9 0	29,700	40	914	37		SW	1	Cloudy.
	3 0	29,767	44	732	45		S	1	Hail shower.
☉ 19	9 0	29,487	44	927	38	0.125	S	3	Rain; a very stormy night.
	3 0	29,565	52	682	53		W	1	Cloudy.
☾ 20	9 0	29,755	45	859	44		W	2	Fine; a very stormy night.
	3 0	29,982	49	680	50		W	1	Fine.
♂ 21	9 0	30,279	40	914	38		W	1	Fine.
	3 0	30,291	47	769	48	0.165	SW	1	Cloudy.
☿ 22	8 30	29,952	49	907	46		S	2	Rain.
	3 0	29,867	52	935	53	0.078	W	2	Cloudy.
♃ 23	8 30	29,861	44	1000	44	0.145	W	1	Rain.
	3 0	29,785	45	929	46		W	1	Cloudy.
♀ 24	8 30	30,120	35	900	33½	0.030	W	1	Fine.
	3 0	30,131	45	753	45		W	1	Fine.
♃ 25	9 0	29,968	49	938	42	0.192	W	1	Cloudy.
	3 0	30,006	52	661	54		W	1	Fine.
☉ 26	9 0	30,343	40	914	38		W	1	Fine.
	3 0	30,400	48	808	49		WNW	1	Fine.
☾ 27	9 0	30,287	44	854	41		W	1	Cloudy.
	3 0	30,080	50	624	52		W	3	Cloudy.
♂ 28	8 0	30,179	45	930	43	0.020	NW	1	Cloudy and dark.
	3 0	30,182	53	847	45		W	1	Cloudy.

Whole Month.

Greatest . .	. . . . . .	30,335	53	1000	54	0.831 inches Rain.
Least	. . . . . .	29,302	33	624	32	
Mean	. . . . . .	29,936	45	857	44.2	

METEOROLOGICAL JOURNAL

for March, 1826.

1826 March.		Time.		Barometer corrected.	Therm. without.	Degree of Moisture by Daniell's Hygrom.	Sixe's Therm.	Rain.	Winds.		Weather.
		H.	M.	Inches.	°	°	°	Inches.	Points.	Str.	
☿	1	9	0	30.026	48	808	45		W	1	Cloudy.
		3	30	29.923	50	820	52		SW	2	Cloudy.
♃	2	8	30	29.719	50	940	47		W	1	Fine.
		3	0	29.699	50	880	52		SW	2	Rain.
♀	3	9	0	29.636	50	910	50		WSW	1	Rain.
		3	0	29.723	49	814	50	0.158	NW	1	Cloudy.
♄	4	9	0	29.620	50	790	42	0.080	W	3	Rain.
		3	0	29.647	54	765	55		S	2	Cloudy.
☉	5	9	0	29.798	43	835	42	0.030	W	1	Fine.
		4	0	29.918	49	814	54		SW	1	Fine.
☾	6	9	0	30.132	39	882	36		E	1	Cloudy.
		—		—	—	—	—		—	-	—
♂	7	9	0	29.809	57	763	39		SSE	3	Rain.
		3	0	29.793	54	791	55	0.020	W	2	Cloudy.
☿	8	9	0	30.025	50	970	49½		S	1	Cloudy.
		3	0	29.981	57	815	58		E	1	Fine.
♃	9	9	0	30.095	51	937	49		E	1	Fine.
		3	0	30.139	64	698	63		E	1	Fine.
♀	10	9	0	30.291	53	874	50		E	1	Fine.
		3	0	30.284	62	747	65		SE	1	Fine.

METEOROLOGICAL JOURNAL,

for March, 1826.

1826 March.	Time.		Barometer corrected.	Therm. without.	Degree of Moisture by Daniell's Hygrom.	Sixe's Therm.	Rain.	Winds.		Remarks.
	H.	M.	Inches.	°	°		Inches.	Points.	Str.	
♄ 11	9	0	30.287	44	963	42		E	1	Fine.
	3	0	30.272	51	850	52		E	1	Fine.
☉ 12	9	0	30.435	43	861	39		SE	1	Fine.
	3	0	30.382	50	500	51		SE	2	Fine.
☾ 13	9	0	30.420	39	912	36		E	1	Hazy.
	3	0	30.331	46	705	50		E	1	Fine.
♂ 14	9	0	29.974	44	890	39	0.108	SE	1	Fine.
	3	0	29.962	50	700	51		SW	1	Fine.
☿ 15	9	0	29.827	45	930	42		W	1	Cloudy.
	3	0	29.823	52	682	52		N	1	Fine.
♃ 16	9	0	30.136	39	882	36	0.075	N	1	Fine, rather hazy.
	3	0	30.235	46	568	52		N	1	Cloudy.
♀ 17	9	0	30.389	37	813	33		N	1	Cloudy and somewhat hazy.
	3	0	30.326	43	633	45		E	1	Cloudy.
♄ 18	9	0	30.232	35	900	32		W	1	Hazy.
	3	0	30.577	48	660	48		S	1	Fine.
☉ 19	9	0	29.855	42	816	34	0.065	W	1	Fine.
	3	0	29.836	49	660	50		N	2	Fine.
☾ 20	9	0	29.950	44	805	37		NW	1	Cloudy.
	3	0	29.916	46	750	49		N by E	2	Cloudy.

1826. April.	9 o'Clock, A. M.		3 o'Clock, P. M.		Dew Point at 9 A. M. in degrees of Fahr.	External Thermometer.				Rain,* in inches, read off at 9A.M.	Direction of the Wind at 9 A. M.	Remarks.
						Fahrenheit.		Self-registering.				
	Barom.	Attached Thermometer.	Barom.	Attached Thermometer.		9 A. M.	3 P. M.	Lowest.	Highest.			
♃ 6	30.158	54.8	30.166	57.5	51	54.2	58.8	48.8	61.0		WSW	Fine—cloudy.
● ♀ 7	30.166	56.7	30.183	59.7	51	56.6	62.5	50.0	64.0		NNW	Fine—light clouds.
♄ 8	30.215	57.0	30.141	61.0	51	54.0	62.0	50.0	63.0		W	Fine—broken clouds.
☉ 9	29.822	50.5	29.793	61.5	50	59.5	58.5	52.2	64.8		NW	A. M. nearly cloudless. P. M. cloudy.
☽ 10	29.956	56.0	29.944	57.9	39	50.8	57.0	42.8	58.0		WSW	A. M. cloudy. P. M. fine.
♂ 11	29.830	56.2	29.793	58.5	53	54.9	58.8	48.9	61.8		SW	Overcast—brisk wind. At 5 P.M. a hail storm.
☿ 12	29.218	55.0	29.222	56.0	45	52.5	49.5	48.5	54.3		SW	Dark and cloudy. Frequent showers.
♃ 13	30.130	52.2	30.144	56.0	39	48.0	57.5	45.3	58.5		N	Cloudy.
♀ 14	30.235	57.0	30.214	60.0	46	57.0	63.0	49.2	66.0		SW	A. M. cloudy. P. M. fine—light clouds.
♄ 15	30.303	59.8	30.220	62.0	51	58.5	65.3	49.8	68.0		W	A. M. fine. P. M. cloudy.
☉ 16	30.146	57.3	30.216	59.0	49	50.5	54.8	50.8	66.5		NNE	A.M. dark and overcast. Sleet. P.M. fine.
☽ 17	30.315	54.0	30.243	57.0	38	48.0	51.5	40.8	54.5		WNW	Fine.
♂ 18	30.229	54.0	30.220	56.0	38	45.0	57.5	40.5	59.0		SE	Fine.
☿ 19	30.134	55.8	30.033	58.5	42	54.2	57.5	44.0	60.5		SSE	Fine.
♃ 20	29.839	55.0	29.722	58.0	42	53.5	58.0	42.0	67.0		E	Fine.
♀ 21	29.707	57.8	29.639	60.0	47	53.5	63.0	44.5	65.0		S by W	Fine—light clouds.
○ ♄ 22	29.606	60.0	29.629	62.8	49	61.0	63.8	54.0	67.0		SE	Cloudy.
☉ 23	29.800	56.0	29.804	58.0	43	49.0	53.5	46.0	54.5		NW	Overcast.
☽ 24	29.911	53.8	29.899	56.0	39	47.0	53.2	38.0	54.5		W	A.M. cloudy. P.M. fine—broken clouds.
♂ 25	30.018	52.5	29.920	54.0	37	47.0	51.5	40.0	53.0		NW	Fine—overcast.
☿ 26	29.697	54.5	29.621	55.0	36	49.0	50.0	43.5	54.0		NW	Fine—cloudy.
♃ 27	29.419	51.0	29.402	51.0	42	45.0	47.0	42.5	47.5		SE	Overcast and slightly hazy.
♀ 28	29.895	51.0	29.913	50.0	28	40.0	43.0	33.5	44.5		NW	A. M. cloudy. P.M. fine—light clouds.
♄ 29	30.023	49.0	30.031	50.5	35	43.0	48.5	34.5	51.0		NW	Overcast and cloudy.
☉ 30	30.140	50.5	30.148	51.0	37	47.0	50.5	34.5	52.0		NNE	A.M. fine—light clouds. P.M. overcast.
	Mean 29.956	Mean 54.7	Mean 29.930	Mean 57.1	Mean 43.1	Mean 51.1	Mean 55.8	Mean 44.6	Mean 58.8			

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr.	9 A.M.	3 P.M.
	29.902	29.870

* The Rain Gage undergoing repair during the months of April, May, and June.

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge .... = 83 ft. 2½ in.
.................................above the mean level of the Sea (presumed about) = 95 ft.
The External Thermometer is 2 feet higher than the Barometer Cistern.
Height of the Receiver of the Rain Gage above the Court of Somerset House........... = 79 ft. 0 in.
The hours of observation are of Mean Time—the day beginning at Midnight.
The Thermometers are graduated by Fahrenheit's Scale.
The Barometer is divided into inches and decimals.

1826.	9 o'Clock, A. M.		3 o'Clock, P.M.		Dew Point at 9 A. M. in degrees of Fahr.	External Thermometer.				Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
May.						Fahrenheit.		Self-registering.				
	Barom.	Attached Thermometer.	Barom.	Attached Thermometer.		9 A.M.	3 P.M.	Lowest.	Highest.			
☽ 1	30.309	51.5	30.220	52.0	38	46.0	51.5	39.0	53.0		NNE	Fine—cloudy.
♂ 2	30.144	49.0	30.010	52.3	39	48.0	57.5	36.5	59.2		N	Fine—light clouds.
☿ 3	30.025	51.5	30.045	52.0	44	48.0	48.0	43.0	49.5		NNE	Overcast and hazy.
♃ 4	30.047	50.0	30.010	50.5	34	44.5	46.5	39.5	47.5		NE	Fine—cloudy.
♀ 5	30.010	49.0	30.095	53.0	43	45.0	50.0	40.0	52.2		NE	Cloudy.
♄ 6	30.041	50.0	30.020	51.0	35	45.0	48.0	39.5	49.8		NNW	Cloudy—showery.
☉ 7	30.018	52.0	30.004	52.0	40	49.0	51.0	39.0	55.0		NNE	Cloudy.
☽ 8	30.027	55.0	30.004	56.0	43	54.0	57.0	41.5	64.0		NE	Fine—strong wind.
♂ 9	30.095	54.5	30.008	56.0	36	51.5	60.5	39.0	62.0		NNE	Fine—thin scattered clouds.
☿ 10	30.095	56.5	30.012	57.7	44	55.5	61.0	44.5	63.5		NNW	Fine—cloudy.
♃ 11	30.117	54.5	30.126	58.0	44	50.5	58.0	49.0	61.5		ESE	Foggy and overcast.
♀ 12	30.223	54.7	30.229	56.7	39	51.0	52.0	42.5	54.5		ESE	Cloudy and overcast.
♄ 13	30.297	55.0	30.202	56.0	34	50.0	54.0	43.5	57.5		SE	Fine.
☉ 14	30.111	58.0	30.051	57.0	41	53.0	57.5	39.0	59.5		E	Fine—overcast.
☽ 15	30.146	58.8	30.136	58.3	39	54.0	57.0	40.5	59.0		N	Fine—overcast.
♂ 16	30.146	57.0	30.113	58.0	45	52.0	62.0	42.5	65.2		E	Overcast and cloudy.
☿ 17	30.115	63.0	30.142	62.5	45	51.0	66.5	50.5	69.5		W	Fine—light clouds.
♃ 18	30.105	65.0	30.105	63.0	52	66.0	63.5	55.8	70.5		SSW	A. M. fine. P. M. rain.
♀ 19	29.911	67.0	29.831	65.0	51	63.0	68.5	50.2	70.0		S	Fine—nearly cloudless.
♄ 20	29.730	61.5	29.833	63.5	50	56.5	63.0	53.5	63.4		E	A.M.overcast. P.M.fine—nearly cloudless
☉ 21	30.025	59.0	30.029	62.0	46	54.0	65.0	45.8	67.5		SE	A.M.cloudy. P.M. fine—nearly cloudless.
☽ 22	30.105	67.5	30.027	66.0	48	64.0	71.0	49.5	72.5		N	Nearly cloudless.
♂ 23	30.004	69.0	29.944	66.0	49	64.0	61.5	48.6	72.5		NNW	Cloudy.
☿ 24	29.818	61.0	29.820	64.0	50	56.5	57.5	52.0	67.0		N	Rain—overcast.
♃ 25	29.732	60.0	29.691	62.0	54	55.8	58.0	52.0	59.3		N	Light rain—cloudy and overcast.
♀ 26	29.691	60.0	29.720	63.0	53	55.0	60.0	53.2	62.8		E	Light rain—cloudy and overcast.
♄ 27	29.821	62.0	29.821	64.5	57	60.8	64.0	62.8	66.9		E	Cloudy—showery.
☉ 28	29.905	67.5	29.834	66.0	56	58.9	60.0	50.0	64.0		NE	Overcast.
☽ 29	29.746	58.5	29.839	59.5	54	52.0	54.0	51.0	55.0		NE	Continued rain—overcast.
♂ 30	29.924	58.5	29.938	59.5	55	53.0	54.8	51.0	55.5		W	Continued rain—overcast.
☿ 31	29.924	58.2	29.930	60.5	52	54.5	57.0	51.5	59.8		E	Dark and overcast.
	Mean 30.013	Mean 57.9	Mean 29.993	Mean 58.8	Mean 45.8	Mean 53.6	Mean 58.0	Mean 46.3	Mean 70.0			

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr. { 9 A. M. 29.950 | 3 P.M. 29.928 }

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge .... = 83 ft. 2½ in.

.... above the mean level of the Sea (presumed about) = 95 ft.

The External Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gage above the Court of Somerset House .... = 79 ft. 0 in.

The hours of observation are of Mean Time—the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided into inches and decimals.

1826. June.	9 o'Clock, A. M.		3 o'Clock, P. M.		Dew Point at 9 A. M. in degrees of Fahr.	External Thermometer.				Rain, in inches, Read off at 9A.M.	Direction of the Wind at 9 A. M.	Remarks.
						Fahrenheit.		Self-registering.				
	Barom.	Attached Thermometer.	Barom	Attached Thermometer.		9 A. M.	3 P. M.	Lowest.	Highest.			
♃ 1	29.909	59.5	29.841	62.0	53	54.0	59.5	52.0	61.0		SW	Overcast and cloudy.
♀ 2	29.928	59.0	——	—	53	53.5	—	52.5	62.0		E	Overcast.
♄ 3	30.130	65.5	30.126	64.0	49	61.0	64.0	49.8	68.0		NW var.	Fine—light clouds—brisk wind.
☉ 4	30.216	68.0	30.235	65.0	55	66.5	67.0	50.0	69.0		SW	A. M. fine, P. M. cloudy and overcast.
● ☽ 5	30.307	66.8	——	—	54	62.5	65.0	52.9	67.5		NE	Fine—cloudy.
♂ 6	30.307	65.5	30.307	66.0	49	60.0	69.0	53.0	71.8		S	Fine—cloudy.
☿ 7	30.245	70.5	30.223	68.0	58	62.5	62.5	58.0	67.0		S	Overcast and cloudy.
♃ 8	30.297	68.5	30.247	67.0	57	64.0	67.5	53.0	70.5		N	Cloudy—brisk wind.
♀ 9	30.049	72.0	30.010	70.0	58	69.5	74.0	53.8	75.2		E	Fine—light clouds—strong wind.
♄ 10	30.033	72.5	29.994	70.6	59	67.5	72.5	53.2	75.5		N	Fine—nearly cloudless.
☉ 11	30.122	67.0	30.196	70.0	57	63.0	73.0	55.2	75.0		N	Fine.
☽ 12	30.313	74.0	30.297	73.0	58	70.8	75.0	60.0	76.5		E	Fine—cloudy.
♂ 13	30.317	71.0	30.243	74.5	59	72.5	78.0	62.0	79.5		E	Fine—nearly cloudless.
☿ 14	30.251	72.5	30.223	74.5	60	71.0	77.5	61.5	80.8		W	Fine—light clouds.
♃ 15	30.239	76.0	30.136	74.0	57	71.5	77.7	60.0	79.5		W	Fine—nearly cloudless—imperfectly clear.
♀ 16	30.334	75.8	30.323	71.5	44	63.0	67.5	53.8	79.5		N	Fine—light clouds.
♄ 17	30.439	67.0	30.317	68.5	50	62.0	72.0	54.0	74.5		SW	Fine—light broken clouds.
☉ 18	30.326	76.5	30.336	74.8	59	72.5	76.0	62.0	77.5		WNW	Fine—light clouds.
○ ☽ 19	30.439	68.8	30.408	70.5	57	66.0	68.9	61.0	73.0		E	Fine—light broken clouds.
♂ 20	30.499	65.0	30.431	69.5	47	65.0	71.0	53.5	72.0		SE	Fine—cloudy.
☿ 21	30.425	62.6	30.427	65.0	50	60.0	64.5	50.5	66.4		S	Overcast and cloudy.
♃ 22	30.408	61.0	30.328	65.8	54	58.5	69.0	53.8	70.5		E	Fine—light clouds.
♀ 23	30.425	62.0	30.418	67.3	54	60.0	72.0	51.5	73.3		N	Fine and cloudless—imperfectly clear.
♄ 24	30.408	69.5	30.406	72.2	51	68.2	77.0	52.0	78.0		N	Fine and cloudless—imperfectly clear.
☉ 25	30.347	69.5	30.309	72.5	51	71.0	75.0	56.0	75.2		E	Fine and cloudless.
☽ 26	30.220	72.0	30.212	74.8	59	74.0	76.0	57.0	80.3		E	Fine and cloudless—imperfectly clear
♂ 27	30.051	75.0	30.010	77.0	62	77.2	82.0	65.0	87.5		SE	Fine and cloudless. Thunder at 11 P. M.
☿ 28	30.115	79.8	30.105	78.5	62	80.0	77.0	69.0	87.0		S	A.M. cloudless. P.M. dark and cloudy. At 2h. 35m. P. M. thunder with rain.
♃ 29	30.233	75.5	30.223	76.5	62	73.5	77.5	63.0	81.5		S	Fine—cloudy.
♀ 30	30.221	76.8	30.208	76.5	62	75.2	79.5	63.0	84.5		WSW	Fine—cloudy.
	Mean 30.252	Mean 69.5	Mean 30.233	Mean 70.7	Mean 55.3	Mean 66.5	Mean 72.0	Mean 56.4	Mean 74.6			

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr. 9 A. M. 30.159 3 P. M. 30.136

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge......... = 83 ft. 2½ in.

.... above the mean level of the Sea (presumed about) = 95 ft.

The External Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gage above the Court of Somerset House .... = 79 ft. 0 in.

The hours of observation are of Mean Time—the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided into inches and decimals.

326. ly.	9 o'Clock, A. M.		3 o'Clock, P. M.		Dew Point at 9 A. M. in degrees of Fahr.	External Thermometer.				Rain, in inches, Read off at 9A.M.	Direction of the Wind at 9 A. M.	Remarks.
						Fahrenheit.		Self-registering.				
	Barom.	Attached Thermometer.	Barom.	Attached Thermometer.		9 A. M.	3 P. M.	Lowest.	Highest.			
1	30.212	72.0	30.196	75.0	64	69.5	75.5	67.4	76.8	0.031	WSW	Overcast and cloudy.
2	30.297	76.2	30.239	75.0	56	75.5	79.0	62.4	81.2		NW	Fine and clear—a few broken clouds.
3	30.299	74.0	30.241	76.8	65	74.0	80.0	64.8	83.0		S var.	Fine—cloudy—brisk wind.
4	30.111	77.0	30.025	77.5	62	77.0	79.0	62.8	81.4		E	Fine—cloudy.
5	29.944	80.0	29.932	78.5	65	78.5	80.0	67.5	82.5		SW	Fine—nearly cloudless—strong wind.
6	29.942	67.5	29.903	74.5	64	67.0	78.5	63.8	81.2		S	Fine—cloudy.
7	29.778	——	29.712	——	64	75.5	78.0	——	——		SSW	Fine. N.
8	29.655	——	29.634	——	63	76.0	77.0	——	——		SW	Overcast—showery. N.
9	29.647	——	29.611	——	66	74.0	78.0	——	——	0.087	SW	Overcast. N.
10	29.764	——	29.757	——	64	71.0	76.0	——	——		W	Fine—cloudy. N.
11	29.842	——	29.866	——	64	70.0	73.0	——	——		W	Cloudy. Thunder at 11 A. M. N.
12	29.843	——	29.715	——	65	67.0	72.0	——	——		SW	Cloudy. N.
13	29.672	——	29.605	——	68	68.5	68.0	——	——	0.100	SW	Cloudy—showery. N.
14	29.739	——	29.764	——	67	68.0	73.0	——	——		W	Fine. N.
15	29.849	——	29.819	——	65	68.0	72.0	——	——		W	Fine. N.
16	29.796	——	29.729	——	63	63.0	68.0	——	——	0.045	W	Fine—cloudy—showery. N.
17	29.975	——	29.929	——	57	66.0	73.0	——	——		W by N	Fine. N.
18	29.951	——	29.876	——	64	67.0	74.0	——	——		WNW	Fine. N.
19	29.921	——	29.938	——	55	65.0	71.0	——	——		W by N	Fine. N.
20	29.990	——	29.976	69.0	55	69.0	67.5	——	80.5		NW	Very cloudy—brisk wind.
21	29.606	69.5	29.635	67.5	57	64.8	65.0	61.0	69.2	0.150	SW	Sky clear—dark clouds and strong wind—showery.
22	29.802	68.5	29.901	68.8	58	68.0	65.0	54.5	66.8	0.042	E	Cloudy—brisk wind.
23	29.944	61.0	30.029	62.0	59	56.5	57.5	55.8	61.0	1.300	N	Overcast and gloomy—strong wind—showery.
24	30.035	61.0	30.105	68.0	58	60.0	68.0	55.5	69.5	0.604	N	Fine—light clouds—strong wind.
25	30.136	64.0	30.136	70.5	57	62.0	72.0	55.4	73.8		N	Clear and cloudless.
26	30.301	66.5	30.322	70.0	56	66.0	70.1	56.0	72.0		ESE	Fine—light clouds—brisk wind.
27	30.332	66.0	30.317	67.0	54	63.5	70.5	52.3	72.4		N	Fine—light clouds.
28	30.251	69.8	30.212	70.0	53	65.0	71.5	57.0	73.5		E	Fine—nearly cloudless.
29	30.124	70.2	30.103	72.0	56	69.0	75.0	57.8	75.8		SE	Fine—nearly cloudless.
30	30.122	73.0	30.041	73.0	58	72.0	76.0	61.0	79.1		W	Cloudless.
31	30.031	76.5	30.004	78.0	59	75.0	80.0	67.5	82.4		SE	At 10 A.M. thunder and lightning, with a few drops of rain. P. M. cloudless.
	Mean 29.965	Mean 70.1	Mean 29.944	Mean 71.8	Mean 60.7	Mean 68.7	Mean 73.0	Mean 60.1	Mean 75.7	Sum 2.359		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr.	9 A.M.	3 P.M.
	29.871	29.845

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge........ = 83 ft. 2½ in.
.... above the mean level of the Sea (presumed about) = 95 ft.
The External Thermometer is 2 feet higher than the Barometer Cistern.
Height of the Receiver of the Rain Gage above the Court of Somerset House .... = 79 ft. 0 in.
The hours of observation are of Mean Time—the day beginning at Midnight.
The Thermometers are graduated by Fahrenheit's scale.
The Barometer is divided into inches and decimals.

1826. August.	9 o'Clock A. M.		3 o'Clock, P. M.		Dew Point at 9 A. M. in degrees of Fahr.	External Thermometer.				Rain, in inches, Read off at 9 A.M.	Direction of the Wind at 9 A. M.	Remarks.
						Fahrenheit.		Self-registering.				
	Barom.	Attached Thermometer.	Barom.	Attached Thermometer.		9 A.M.	3 P.M.	Lowest.	Highest.			
♂ 1	30.103	74.0	30.101	78.5	65	71.5	78.5	68.0	80.0		NNE	Fine—light clouds.
☿ 2	30.023	70.0	29.994	76.0	63	68.0	76.0	63.0	77.0		E	Fine—thin spreading clouds.
● ♃ 3	29.911	68.0	29.911	72.0	66	65.2	72.0	64.8	73.8	0.042	NNE	Overcast. At 10h 30m P.M. lightning and heavy rain.
♀ 4	29.994	65.2	30.008	72.6	61	63.5	71.0	59.2	73.2	0.125	N	Overcast and lowering.
♄ 5	30.041	63.3	30.081	68.0	62	61.5	67.0	58.0	69.2	0.166	N	Overcast and gloomy.
☉ 6	30.109	67.8	30.137	71.9	57	65.5	71.9	58.0	73.5		NW	Fine—light clouds.
☽ 7	30.216	70.0	30.198	73.0	55	67.8	73.5	63.0	74.8		SSW	Fine—light clouds.
♂ 8	30.119	71.0	30.095	74.6	65	69.8	74.6	62.0	75.7		SW	Fine—nearly cloudless.
☿ 9	30.029	69.0	29.994	74.0	59	67.9	74.0	62.4	74.6	0.031	N	Sky overspread with small clouds.
♃ 10	30.000	67.6	29.922	71.5	59	67.8	71.0	61.6	72.6		E	Fine—nearly cloudless.
♀ 11	29.905	65.0	29.831	65.4	61	62.0	63.0	60.0	65.5	0.125	SSW	Overcast—showery.
♄ 12	30.016	64.8	30.045	68.0	53	63.5	67.5	55.2	70.5	0.010	W by S	Fine—light clouds.
☉ 13	30.247	65.0	30.196	69.0	55	64.3	68.7	55.0	70.6		WSW	A.M. cloudless. P.M. cloudy and overcast.
☽ 14	29.994	67.0	29.920	72.5	56	66.5	72.2	56.5	75.5		SSW	Fine—cloudy—brisk wind. Boisterous wind during the night.
♂ 15	30.124	66.2	30.041	70.0	54	66.0	70.0	57.0	72.0		SSW	Fine and clear—strong breeze.
☿ 16	29.946	66.8	29.994	69.0	64	65.0	69.0	61.2	70.3	0.031	SSW	A.M. lowering—brisk wind—showery. P.M. fine—light clouds.
○ ♃ 17	30.136	65.6	30.134	70.5	57	64.5	71.0	56.6	73.0		WSW	Fine—light stretching clouds.
♀ 18	30.316	69.2	30.326	74.5	65	69.8	75.5	64.2	76.4		SSW	Fine—light fleecy clouds.
♄ 19	30.322	70.0	30.241	75.0	57	69.5	75.2	63.0	77.6		SW	Fine—nearly cloudless.
☉ 20	30.095	70.5	30.031	77.8	66	70.0	79.0	62.7	81.2		NE	Fine and clear—nearly cloudless.
☽ 21	29.948	65.0	29.938	71.2	59	63.0	70.5	61.2	72.5		NNE	Cloudy and foggy.
♂ 22	29.905	68.4	29.930	72.0	60	67.0	72.5	61.6	74.0		ESE	Fine. Strong unsteady wind during the night
☿ 23	29.837	68.0	29.735	70.0	62	66.5	69.0	62.0	71.8	0.005	SSW	Lowering—strong, unsteady wind.
♃ 24	29.716	70.0	29.738	70.6	60	65.8	70.5	61.0	73.8		SW	A.M. nearly cloudless. P.M. very cloudy.
♀ 25	29.798	70.4	29.720	73.0	62	69.5	74.5	64.8	75.9	0.005	SSW	Fine—brisk wind. At 8 P.M. thunder and lightning, with rain—sultry.
♄ 26	29.821	66.9	29.823	69.6	63	65.5	68.5	62.8	70.6		SSW	Fine—light broken clouds—brisk wind.
☉ 27	29.950	66.5	30.008	69.0	56	63.5	68.0	57.5	70.0		W by N	Cloudy.
☽ 28	30.004	66.2	30.014	71.2	63	65.0	69.9	58.5	72.5		ESE	Cloudy and lowering.
♂ 29	29 956	70.8	29.893	72.6	68	68.8	73.0	65.6	74.5	0.010	SE	Fine and clear—light clouds—showery.
☿ 30	29.764	70.5	29.706	74.0	66	70.4	74.5	64.9	77.5		SSE	Fine—cloudy.
♃ 31	29.849	67.8	29.848	70.5	60	65.0	69.0	63.0	71.5		W by N	Cloudy and overcast.
	Mean 30.006	Mean 67.9	Mean 29.985	Mean 71.8	Mean 60.6	Mean 66.4	Mean 71.6	Mean 61.2	Mean 73.6	Sum 0.550		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr. { 9 A. M. 29.917 — 3 P. M. 29.886 }

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge .... = 83 ft. 2½ in.

.... above the mean level of the Sea (presumed about) = 95 ft.

The External Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gage above the Court of Somerset House .... = 79 ft. 0 in.

The hours of observation are of Mean Time—the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided into inches and decimals.

826.	9 o'Clock, A. M.		3 o'Clock, P. M.		Dew Point at 9 A. M. in degrees of Fahr.	External Thermometer.				Rain, in inches, Read off at 9 A.M.	Direction of the Wind at 9 A. M.	Remarks.
ptember.	Barom.	Attached Thermometer.	Barom.	Attached Thermometer.		Fahrenheit.		Self-registering.				
						9 A. M.	3 P. M.	Lowest.	Highest.			
♀ 1	29.825	65.2	29.807	69.5	60	63.0	67.9	60.2	69.5		ESE	
♄ 2	29.720	62.7	29.701	65.4	63	62.0	62.0	58.7	64.8	0.011	N var.	Gloomy—unsteady wind—showery.
☉ 3	29.792	65.8	29.869	67.8	64	63.0	65.5	61.1	67.0		NE	Cloudy.
☽ 4	29.958	65.2	29.942	70.5	64	64.8	70.0	59.3	72.3	0.010	ESE	Cloudy—showery.
♂ 5	29.947	63.5	29.921	64.5	61	62.8	60.5	58.5	64.8	0.005	NNW	Overcast and foggy.
☿ 6	29.905	58.0	29.237	66.0	56	56.8	64.0	55.0	65.6		NE	Lowering—strong wind. Heavy rain.
♃ 7	29.166	57.8	29.452	58.7	56	55.0	56.0	52.9	57.5	0.198	WNW	Gloomy—slight fog. Continued rain.
♀ 8	29.631	57.0	29.635	63.1	55	56.5	64.8	50.0	66.6		SW	Overcast and cloudy. Heavy rain.
♄ 9	29.771	58.0	29.822	61.3	54	54.6	62.0	52.9	63.0	0.008	ESE	A. M. strong fog. P. M. fine—cloudy.
☉ 10	30.022	59.1	30.064	63.2	51	57.3	62.9	50.0	63.9	0.041	SW	Fine and clear—cloudy.
☽ 11	30.213	56.8	30.215	62.5	53	55.3	62.0	49.9	63.8		WNW	Fine—cloudy.
♂ 12	30.240	56.5	30.197	63.6	54	55.5	63.5	49.8	65.0		SW	A. M. cloudless. P.M. overcast—slight fog.
☿ 13	30.089	58.2	30.017	64.5	56	57.9	64.0	52.0	65.0		SSW	Clear and cloudless.
♃ 14	29.912	59.4	29.929	66.2	58	59.0	65.4	52.7	67.7	0.012	SW	A. M. clear and cloudless. P.M. cloudy. Showery.
♀ 15	30.226	53.0	30.270	61.8	51	53.0	61.2	47.2	62.4		N	Fine—light clouds.
♄ 16	30.290	55.4	30.213	62.9	54	56.0	62.1	48.0	66.9		E	Very fine—clear and cloudless. Heavy fog in the morning.
☉ 17	30.007	56.8	29.949	65.0	56	57.2	66.2	50.3	68.0		ENE	Fine—light clouds.
☽ 18	29.849	62.2	29.782	63.8	62	61.5	61.5	57.2	64.0	0.132	NE	Overcast and gloomy—brisk wind—showery.
♂ 19	29.903	64.3	29.901	66.5	63	63.0	67.0	61.0	68.0		SSW	Fine and clear—cloudy.
☿ 20	29.840	61.3	29.843	64.5	61	59.7	63.0	59.0	64.0		WNW var.	Overcast. Dense fog in the morning.
♃ 21	30.054	59.9	30.083	63.6	54	57.0	60.8	51.8	63.2		NE	Fine—cloudy.
♀ 22	30.135	53.0	30.126	58.5	51	52.5	58.9	46.2	58.5		ESE	Clear and cloudless—brisk wind.
♄ 23	30.055	51.0	——	——	48	51.6	——	43.8	——		E	Fine and cloudless.
☉ 24	——	——	29.641	——	—	——	63.0	53.0	63.0			N.
☽ 25	29.639	——	29.661	——	60	60.0	61.0	59.0	64.0		S	Showery. N.
♂ 26	29.802	——	29.808	——	60	61.0	63 0	57.0	66.0		SW	Showery. N.
☿ 27	29.968	——	30.008	——	61	62.0	64.0	60.0	67.0		W	Fine. N.
♃ 28	30.156	——	30.166	——	61	62.0	64.0	57.0	67.0		SW	Fine. N.
♀ 29	30.146	——	30.047	——	59	59.0	63.0	55.0	67.0		SW	Fine. N.
♄ 30	29.762	——	29.778	——	66	64.0	65.0	60.0	67.0		SW	Showery. N.
	Mean 29.932	Mean 59.1	Mean 29.899	Mean 64.2	Mean 57.6	Mean 58.7	Mean 63.2	Mean 54.3	Mean 65.3	Sum 0.417		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr.	9 A. M.	3 P. M.
	29.867	29.820

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge .... = 83 ft. 2½ in.

.... above the mean level of the Sea (presumed about) = 95 ft.

The External Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gage above the Court of Somerset House .... = 79 ft. 0 in.

The hours of observation are of Mean time—the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's scale.

The Barometer is divided into inches and decimals.

1826. October.	9 o'Clock, A. M.		3 o'Clock, P. M.		Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer.				Rain, in inches. Read off at 9A.M.	Direction of the Wind at 9 A.M.	Remarks.
	Barom.	Attached Thermometer.	Barom.	Attached Thermometer.		Fahrenheit.		Self-registering.				
						9 A. M.	3 P. M.	Lowest.	Highest.			
● ☉ 1	29.802	—	29.786	—	60	58.0	62.0	53.0	62.0		W	Rain. N.
☽ 2	29.944	—	29.938	—	55	54.0	60.0	48.0	61.0		SE	Fine. N.
♂ 3	29.909	—	29.893	—	55	54.0	59.0	48.0	59.0		W	Fine. N.
☿ 4	29.772	58.0	29.751	56.3	53	52.0	52.5	48.5	58.0		SW	A. M. cloudless. P. M. gloomy—very small rain.
♃ 5	29.787	50.0	29.821	55.0	44	46.0	52.2	40.9	54.8		SSW	Cloudy and hazy.
♀ 6	30.118	47.0	30.123	52.0	40	43.0	51.5	37.6	52.0		SW	Fine and cloudless—hazy.
♄ 7	30.105	50.0	30.055	56.2	47	49.0	55.5	41.8	57.5		S	Cloudy—brisk wind.
☉ 8	29.949	56.8	29.848	59.5	55	56.1	60.3	53.8	61.5		SSW	Fine and clear—brisk wind. Heavy rain at 8 P.M.
☽ 9	29.809	55.1	29.775	57.2	50	51.0	51.8	47.3	56.5		WNW	Cloudy and overcast—showery.
♂ 10	29.683	53.0	29.722	61.6	52	52.5	61.5	46.2	62.7	0.688	SSW	Overcast—showery.
☿ 11	30.079	61.0	30.098	65.0	59	60.2	63.8	57.6	66.3		WSW	Fine—light clouds.
♃ 12	30.133	61.8	30.104	64.0	61	60.2	62.8	59.2	63.8		SW	Fine—cloudy.
♀ 13	30.079	62.4	30.123	63.5	61	61.7	60.0	58.7	66.0		WSW	Overcast—brisk wind.
♄ 14	30.211	49.5	30.153	56.8	46	50.4	57.0	46.3	58.5		E	Overcast and foggy.
○ ☉ 15	29.944	58.0	29.809	61.2	56	57.8	62.5	54.1	64.0		ESE	A. M. foggy. P.M. fine and clear.
☽ 16	29.629	59.8	29.639	63.0	57	58.5	59.0	54.9	65.0	0.031	SSE	Cloudy. At 2 P.M. thunder, with heavy rain.
♂ 17	30.030	55.8	29.991	59.6	50	50.0	57.0	45.4	59.8		ESE	A. M. overcast. P. M. fine and clear.
☿ 18	29.994	56.8	30.004	59.1	54	54.5	58.5	49.6	59.5		ENE	Overcast and foggy.
♃ 19	30.017	59.5	30.000	61.3	58	58.8	59.0	57.7	60.5		E var.	Overcast and gloomy—brisk wind.
♀ 20	29.992	61.8	29.974	62.7	59	57.8	59.0	58.2	61.2		ESE	Overcast—faintly hazy.
♄ 21	30.000	61.4	29.978	65.0	60	58.8	65.2	57.8	67.8		SE	Fine—cloudy.
☉ 22	29.960	62.1	29.919	63.2	60	59.8	61.6	57.7	62.8		SE	Overcast and gloomy—showery.
☽ 23	29.921	62.1	29.912	63.5	60	58.7	61.3	58.4	63.0	0.031	E	Strong fog. Thunder and lightning with heavy rain, at noon.
♂ 24	29.961	61.1	29.872	63.2	57	61.8	59.4	55.0	61.8	1.031	SW	Fine and clear—light clouds.
☿ 25	29.425	60.3	29.409	61.3	58	56.2	55.0	55.0	58.4		SW var.	Fine. Violent gales during the early part of the morning.
♃ 26	29.434	54.3	29.496	54.7	43	45.3	50.0	44.0	51.3		WNW	Fine—light clouds—faint haze.
♀ 27	29.635	53.0	29.618	54.2	48	46.4	52.8	42.2	53.3	0.090	S by E	Overcast and foggy—showery.
♄ 28	30.119	51.0	30.114	53.9	43	44.7	52.6	41.8	53.0		WNW	Cloudy.
☉ 29	30.155	50.8	30.092	53.2	46	47.7	52.8	43.0	53.2		WSW	Overcast and hazy.
☽ 30	30.128	53.1	30.085	54.2	51	49.8	53.1	50.0	54.3		SE	Overcast and gloomy.
● ♂ 31	30.098	53.0	30.078	54.3	47	48.0	50.8	47.0	52.4		NW	Fine—cloudy.
	Mean 29.930	Mean 56.4	Mean 29.909	Mean 59.1	Mean 53.1	Mean 53.6	Mean 57.4	Mean 50.3	Mean 59.4	Sum 1.871		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr.	9 A.M.	3 P.M.
	29.871	29.844

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge........ = 83 ft. 2½ in.

...............................above the mean level of the Sea (presumed about) = 95 ft.

The External Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gage above the Court of Somerset House .... = 79 ft. 0 in.

The hours of observation are of Mean Time—the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided into inches and decimals.

1826.	9 o'Clock, A. M.		9 o'Clock, A. M.		Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer.				Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
ovember.	Barom.	Attached Thermometer.	Barom.	Attached Thermometer.		Fahrenheit.		Self-registering.				
						9 A.M.	3 P.M.	Lowest.	Highest.			
☿ 1	29.758	50.6	29.609	52.7	44	45.3	47.8	41.5	49.6		SW	Overcast—disjointed clouds.
♃ 2	29.792	46.6	29.825	50.0	39	42.0	47.8	40.0	48.8		NNW	Fine and cloudless—very faint haze.
♀ 3	29.908	48.0	29.825	51.6	46	46.1	50.2	43.0	51.5		NNE	Violent gales, with rain, in the evening and during the night.
♄ 4	29.767	50.2	29.782	50.9	49	47.8	47.0	46.6	47.5	0.007	NNE	Overcast and gloomy. Continued rain.
☉ 5	29.896	48.2	29.812	50.3	46	45.7	47.8	42.0	49.0		NNE	Overcast and gloomy. Showery.
☽ 6	29.789	48.5	29.778	48.4	46	45.0	42.8	43.8	42.4		W	Overcast and gloomy. Frequent showers.
♂ 7	29.931	41.3	29.982	42.8	30	34.1	40.8	30.3	41.3	0.083	W	Fine—light clouds.
☿ 8	30.092	40.8	30.085	42.6	33	35.2	40.6	32.3	40.6		W	Cloudless—faint haze.
♃ 9	30.155	40.0	30.172	43.1	36	36.8	42.2	32.5	42.2		NNW	Nearly cloudless.
♀ 10	30.071	40.0	29.920	42.5	33	35.6	44.1	31.2	47.5		SW	Overcast, gloomy, and foggy.
♄ 11	29.801	44.7	29.689	47.2	49	47.0	51.3	44.5	52.0		WSW	Cloudy.
☉ 12	29.644	47.0	29.529	49.8	46	44.0	48.9	42.0	52.3		SW	Overcast and hazy.
☽ 13	29.452	43.1	29.204	47.7	41	39.0	45.7	35.1	46.0		SW	Overcast and foggy.
♂ 14	29.083	44.8	29.145	46.0	39	39.7	43.2	38.4	43.4		NNE	Overcast and cloudy.
☿ 15	29.594	43.7	29.697	45.7	39	39.3	44.1	37.8	44.2		NW	Nearly cloudless.
♃ 16	29.983	41.0	29.988	44.0	34	33.8	41.2	31.7	41.2		SW	Clear and cloudless.
♀ 17	29.908	46.5	29.983	47.8	45	45.0	45.0	38.4	46.2		ESE	Overcast and lowering.
♄ 18	30.152	45.1	30.114	46.5	42	43.8	45.2	38.2	45.9		NNE	Overcast and foggy.
☉ 19	30.172	47.0	30.186	47.8	43	45.0	45.0	44.4	46.0		NNE	Overcast and gloomy. Showery.
☽ 20	30.381	46.3	30.420	47.6	43	43.9	45.5	43.1	45.5		N by E	Overcast, gloomy, and foggy.
♂ 21	30.503	45.9	30.468	47.0	34	43.8	44.2	41.9	44.8		NE by N	Partially overcast—faint haze.
☿ 22	30.433	45.0	30.358	46.8	37	41.8	44.2	40.0	44.5		N	Cloudy—dense fog.
♃ 23	30.114	46.3	—	—	41	43.8	—	42.6	47.2		N by W	Cloudy—dense white fog.
♀ 24	29.519	48.0	29.404	48.7	43	42.6	43.0	42.6	51.0		SW	Fine—light clouds and haze.
♄ 25	29.156	43.0	29.142	44.3	30	34.8	38.3	31.7	41.2		SW	A.M. clear and cloudless—brisk wind. P.M. overcast.
☉ 26	29.313	38.8	29.361	39.9	24	27.2	33.9	26.8	34.0		WSW	A. M. strong white fog and hoar frost.
☽ 27	29.681	37.2	29.768	38.9	28	30.6	35.6	29.5	36.0		W	A. M. white fog. P. M. fine and cloudless
♂ 28	29.790	37.6	29.648	40.2	36	36.0	43.2	30.8	48.6		SW by S	Overcast and gloomy. Continued rain.
☿ 29	29.384	43.2	29.345	46.7	44	46.5	46.8	43.5	48.5		SW	Cloudy. Showery.
♃ 30	29.371	46.2	29.417	47.8	45	44.2	45.7	41.9	46 2	0.367	SSE	Cloudy and foggy.
	Mean 29.820	Mean 44.5	Mean 29.781	Mean 46.0	Mean 39.5	Mean 40.8	Mean 44.2	Mean 38.3	Mean 45.5	Sum 0.457		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr.	9 A. M.	3 P. M.
	29.792	29.749

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge........ = 83 ft. 2½ in.

.... above the mean level of the Sea (presumed about) = 95 ft.

The External Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gage above the Court of Somerset House .... = 79 ft. 0 in.

The hours of observation are of Mean Time—the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided into inches and decimals.

1826. December.	9 o'Clock, A. M.		3 o'Clock, P. M.		Dew Point at 9 A. M. in degrees of Fahr.	External Thermometer.				Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A. M.	Remarks.
						Fahrenheit.		Self-registering.				
	Barom.	Attached Thermometer.	Barom.	Attached Thermometer.		9 A.M.	3 P.M.	Lowest.	Highest.			
♀ 1	29.492	44.2	29.316	45.8	39	38.8	41.8	35.6	43.5		SW	Overcast. Showery.
♄ 2	29.166	42.0	29.108	46.3	41	40.5	45.0	40.5	45.8		SW	Very cloudy—brisk wind.
☉ 3	29.452	44.0	29.389	45.3	39	38.5	40.8	37.5	43.2		W	Hazy.
☽ 4	29.419	41.1	29.544	42.3	36	35.9	37.8	32.8	38.5		WNW	Fine—broken clouds and light haze.
♂ 5	29.732	41.0	29.675	41.3	38	38.3	37.3	35.0	39.2		WNW	Overcast and foggy.
☿ 6	29.695	40.0	29.742	41.8	38	36.4	41.7	34.0	51.2		ESE	Overcast and foggy.
♃ 7	29.625	46.1	29.517	49.0	48	50.8	52.2	42.0	52.8		SW	Overcast—brisk wind. Continued rain.
♀ 8	29.294	51.0	29.391	52.6	48	49.0	50.5	47.2	51.8		SW	Lowering—brisk wind.
♄ 9	29.893	49.2	29.893	50.2	41	41.2	45.0	40.5	45.2		WSW	Sky overspread with fleecy clouds.
☉ 10	29.893	51.0	29.877	53.5	50	51.0	52.5	45.5	53.0		S	Cloudy. N.
☽ 11	29.871	52.5	29.887	54.0	50	51.0	51.0	49.0	51.0		SSW	Fine—showery. N.
♂ 12	29.881	52.0	29.685	52.0	49	49.5	50.0	46.0	50.0		S	Cloudy. N.
☿ 13	29.540	51.5	29.540	53.0	47	48.0	49.0	47.8	52.8		SSW	Fine. N.
○ ♃ 14	29.623	50.0	29.609	51.4	47	45.0	47.5	43.0	48.8		S by W	Cloudy—brisk wind.
♀ 15	29.657	50.3	29.602	50.8	46	45.8	46.8	44.0	46.8		ESE	Overcast— light fog.
♄ 16	29.592	50.0	29.637	50.8	47	46.7	47.6	45.0	47.8		E	Overcast and foggy,
☉ 17	29.861	48.8	29.907	48.8	45	44.0	43.5	44.0	44.2	0.099	NE	Overcast and hazy.
☽ 18	29.999	46.2	29.994	46.3	50	41.0	41.8	40.8	41.8		NNE	Dark and overcast—strong haze
♂ 19	30.078	45.2	30.050	45.7	35	40.8	41.2	40.2	41.2		NNE	Overcast and foggy.
☿ 20	29.902	45.0	29.712	46.1	41	40.9	40.0	39.3	41.8		SSE	Overcast and foggy.
♃ 21	29.681	44.2	29.852	44.5	38	39.2	40.8	38.0	41.0		WNW	Fine and cloudless – strong haze.
♀ 22	30.276	41.4	30.230	42.8	30	33.4	39.9	32.2	44.2		WNW	Cloudy, overcast, and hazy.
♄ 23	30.253	44.2	30.242	45.8	43	44.1	47.0	40.1	47.2		WSW	Overcast and hazy.
☉ 24	30.305	47.0	30.301	47.3	46	45.8	45.0	45.8	45.8		NNW	Overcast and foggy.
☽ 25	30.354	47.0	30.355	47.0	44	44.3	45.1	44.3	45.1		N by E	Overcast and foggy.
♂ 26	30.449	46.8	30.464	46.8	43	42.5	42.4	42.0	42.7		NE	Overcast and foggy.
☿ 27	30.550	45.8	30.535	46.4	41	41.2	42.8	40.3	43.2		NNE	Overcast and hazy.
● ♃ 28	30.571	43.2	30.564	43.4	37	36.6	38.3	36.4	38.0		N by E	Overcast and hazy.
♀ 29	30.419	43.0	30.297	44.0	38	40.0	44.0	33.0	44.0		WNW	Cloudy. N.
♄ 30	30.206	45.5	30.172	47.0	42	45.5	48.0	43.0	48.0		NNW	Fine. N.
☉ 31	30.229	46.0	30.180	47.5	44	45.0	48.0	41.0	48.0		W	Cloudy. N.
	Mean 29.902	Mean 46.3	Mean 29.879	Mean 47.4	Mean 42.6	Mean 42.9	Mean 44.6	Mean 40.8	Mean 45.7	Sum 0.099		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr. { 9 A.M. 29.869 — 3 P.M. 29.844 }

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge .... = 83 ft. 2½ in.
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